Peano's Axioms in their Historical Context

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Peano's Axioms in their Historical Context

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The question for the ultimate foundations and the ultimate meaning of mathematics remains open; we do not know in which direction it will find its final solution nor even whether a final objective answer can be expected at all.

HERMANN WEYL

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Introduction

As recently as a century ago, most learned people still believed that mathematics (and science in general) were an eternal, objective wisdom containing clear and indubitable truth, obtained by means of a process of rigorous proof based on self-evident knowledge.[1] Geometry, in particular, was regarded by many as the firmest and most reliable branch of mathematics, and mathematical analysis was based on and legitimized by geometry.

This view spread particularly at the time of the scientific revolution, and eventually predominated in mathematics as the final decades of the last century witnessed a climax in the quest for rigor. This climax, however, was not an indicator of the success of rigorism, but a symptom of its decline. In fact, during the nineteenth century serious breaches started appearing in the "rigorous" mathematical edifice: one of the major "disasters" was the discovery of

[1] This is the so-called "Euclid Myth." See DAVIS and HERSH, *The Mathematical Experience*, pp. 322–330. *Cf.* GIORGIO ISRAEL, "Il mito della certezza," *Prometeo*, December 1984, pp. 46–57. For a broad survey see KLINE, *Mathematics: The Loss of Certainty* (regrettably without references).

non-Euclidian geometry, which undermined the previous incontestable trust in Euclidian geometry.[2] Another "disaster" was the development of analysis beyond geometrical intuition, as in the case of the discovery of space-filling curves and continuous, nowhere differentiable curves. Mathematicians reacted instinctively by increasing the quest for rigor, in the belief that a more thorough rigor would sooner or later solve the problems. If intuition were banned from mathematics and proofs were made totally rigorous, they thought, the lost certainty would be re-established.

The effort to establish a basis for mathematical indubitability is called foundationism, and has dominated the philosophy of mathematics since 1890. It has taken different forms and given rise to rival schools of thought, the major ones being Logicism, Formalism, and Intuitionism. It would be difficult to give

GIUSEPPE PEANO (Courtesy of AUGUSTO PEANO)

[2] On mathematical monsters in the nineteenth century, see VOLKERT, *Die Krise der Anschauung*, pp. 99–157.

a clear-cut definition of these schools, since their many adherents had different approaches to the study of the foundations of mathematics, producing a broad spectrum of views.

The general story of foundationism is well-known. Its major streams underwent a crisis, culminating in KURT GÖDEL's "incompleteness theorems," and only a few mathematicians today still hold that mathematical indubitability can be achieved.

The present work will concentrate on a famous, but little-studied example of early foundationism: the case of GIUSEPPE PEANO (1858–1932). PEANO is best known for his set of axioms in arithmetic and his work is at times presented as being at the core of foundationism.

PEANO was, indeed, an influential figure in mathematics at the turn of the century, yet he is nearly always mentioned incidentally, mainly in relation to the streams or schools that developed in his day.[3] His fame in history of mathematics comes primarily from the enthusiastic acknowledgement of the leading Logicist philosopher, BERTRAND RUSSELL. As a result of this acknowledgement, PEANO's work has been (incorrectly) linked to RUSSELL's view that mathematics is an elaboration of logic. Hence PEANO became famous mainly for his contributions (both true and alleged) to mathematical logic, rather than for his many, no less important, contributions to other branches of mathematics.

PEANO's interesting, broad, and complex work has thus been the subject of relatively little historical study. A selection of his writings was published by his pupil, UGO CASSINA, some thirty years ago, but this comprises only a minor part of the more than 230 books and articles he published. CASSINA has also written a number of articles on PEANO, conveying important information and indicating some central themes in PEANO's mathematical work. CASSINA — unlike most writers on PEANO — rightly emphasized the importance of PEANO's contribution to the various branches of mathematics, and did not concentrate primarily on the latter's mathematical logic. Yet CASSINA's contribution remains that of a mathematician with little historical insight, and cannot be regarded as history of mathematics — at least not by the standards of history of science.

A series of insightful studies on PEANO was written by HUBERT C. KENNEDY. KENNEDY has also translated a small selection of PEANO's works into English, and has written the only monograph existing on PEANO, presenting a chronological outline of PEANO's life and work.

In all, there are relatively few articles on PEANO, many of them concentrating on the relations between him and his contemporaries.

[3] KENNEDY remarks that, "In this day of the French Nicolas Bourbaki and the Swedish H. Gask, when the latest writers cavalierly strip Peano of all credit for the postulates for the natural numbers and whenever mention is made of the space-filling curve the reference is usually to Hilbert's explanation, it would be no surprise if some people wondered whether there actually *was* a Giuseppe Peano, or at least whether he was more than a spokesman for another collective." "Giuseppe Peano at the University of Turin," p. 703.

The present research concentrates on what is regarded as his major contribution to arithmetic: the set of axioms he produced in 1889. They were presented for the first time in a curious, short work under the title *Arithmetices principia nova methodo exposita* — "*The principles of arithmetic, presented by a new method*" written (*sic*) in Latin.[4] Possibly PEANO wrote his work in Latin because he considered it of universal importance, but what, in fact, was the true importance of the axioms? I believe that their real significance is still to a great extent ignored.

PEANO's axioms (or "postulates," as they are alternatively called), are normally considered to be part and parcel of the logical foundations of mathematics. I would like to argue that PEANO did not intend to contribute to the logical foundations of mathematics, my theses being as follows:

1. PEANO formulated his axioms in the hope of giving a clear and rigorous presentation of arithmetic, and in consequence, of mathematics in general. Like many contemporary mathematicians, he thought that mathematics was perfect and that this could be shown by rigorous attention to precision in the formulation of axioms and proofs: he believed that a "hygienic" presentation of mathematics would suffice to avoid errors and ease its development.

2. PEANO was not interested in reducing mathematics to logic. As a matter of fact, he believed that basic entities, such as "zero," "number," and "successor" *cannot* be defined. He did not belong — and did not want to belong — to any of the foundationist streams of his day. He was a foundationist inasmuch as he endeavored to establish the basis for the indubitability of mathematics. He also anticipated to the three foundationist streams mentioned above, and shared some common elements with each of them. But he did not propose a general, rigid, mathematical system as was done by Logicists, Formalists, and Intuitionists.

3. Even limited to the rigor of symbolic presentation, PEANO's foundationism was a failure. Though many of his signs were adopted by modern mathematics, his proposed "mathematical language" was not adopted; on the contrary, it often exasperated students, colleagues and readers. PEANO's contribution, therefore, may be taken as yet another instance of the general failure of foundationism.

I propose to study the historical context of PEANO's axioms by following, step by step, the process of their formulation and by considering them in relation both to other analogous contemporary works and to contemporary streams of thought in mathematics (of particular interest is the analogous and independent work of RICHARD DEDEKIND). A study of this kind has not, to the best of my knowledge, been undertaken in history of mathematics.

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[4] *Opere scelte*, **2**:20–55. English translation in PEANO, *Selected Works*, pp. 101–134.

This study has eight chapters:

Chapter 1 outlines the history of the concept of mathematical rigor from ancient times to the days of CAUCHY. The concept of mathematical rigor has, up to now, not been adequately studied, and has certainly varied from period to period and from mathematician to mathematician. I do not want here to delve into the meaning of this concept, but only to point out how the quest for rigor in mathematics increased considerably in the wake of GEORGE BERKELEY's criticism of NEWTONian calculus. The chapter follows the various attempts to overcome this criticism and shows how the main outcome was to increase the quest for rigor – in calculus in particular and in mathematics in general.

Chapter 2 is devoted to CAUCHY's "rigorous calculus." CAUCHY is particularly important for the present study because the classical literature in history of mathematics often presents him as the mathematician who succeeded in giving the correct rigorous formulation to calculus. Without belittling CAUCHY's contribution to mathematics, this chapter takes a critical view of his work and pinpoints one of its main flaws, namely his assumption that convergent series of continuous functions must converge to a continuous function. This is an example of analysis going beyond intuition, ultimately questioning CAUCHY's rigor. Thus, even CAUCHY's efforts did not provide a satisfactory answer to BERKELEY and other criticisms. In response to the difficulties in CAUCHY's rigorous calculus, WEIERSTRASS tried to refine CAUCHY's work but encountered new problems related to the nature and definition of irrational numbers. This led both him and DEDEKIND, on the eve of PEANO's formulation of his axioms, to study the nature of number. Here we reach PEANO himself.

Chapter 3 is a brief presentation of PEANO's life and work. There is no need to cover the subject more fully, since KENNEDY's biography is already very detailed. I have tried, however, to emphasize PEANO's adherence to the rigorist tradition.

Chapter 4 presents the state of the art in mathematical logic on the eve of PEANO's formulation of his axioms. It follows some of the developments of the mathematization of logic from LEIBNIZ to PEANO. It indicates the originality of PEANO's approach: unlike his predecessors — the most prominent of whom was GEORGE BOOLE — PEANO did not attempt to mathematize logic, but tried to use it as an instrument in mathematics.

Chapter 5 is central and presents in detail the consecutive stages in PEANO's work that led him to the formulation of his axioms. It shows how the quest for rigor and, more specifically, an attempt to avoid mistakes, led PEANO to formulate his axioms. The curious *Arithmetices principia*, presenting PEANO's axioms for the first time, is only 20 pages long. It begins with a list of four undefined terms and nine axioms. Five of them were later slightly modified and presented by PEANO as an independent set of axioms. In their final form, these are:[5]

1. Zero is a number.
2. The successor of any number is another number.

[5] This formulation was given by PEANO in 1898: see abstract in *Opere scelte*, 3: 215–231, p. 216. Translation from KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," p. 262.

3. There are no two numbers with the same successor.
4. Zero is not the successor of a number.
5. Every property of zero, which belongs to the successor of every number with this property, belongs to all numbers.

PEANO introduced the concepts of addition, subtraction, maximum, minimum, multiplication, powers, division, some theorems concerning open and closed intervals and real numbers. Having formulated the basic axioms and theorems of arithmetic, PEANO went on to attempt to formulate the basic axioms of geometry and, eventually, of logic. PEANO's view of the relation between logic and mathematics is, however, hard to define. He certainly did not regard mathematics as part of logic, but considered logic as an instrument in mathematical research, saying that arithmetic and geometry are constructed in analogy to logic, yet he did not specify what exactly this analogy is.

Chapter 6 deals with the period immediately after the formulation of PEANO's axioms during which PEANO consolidated his view that mathematics should be approached rigorously and axiomatically. The chapter presents the mathematical details of PEANO's curve (1890), which against intuition went through every point of a unit square, showing how unreliable intuition in mathematics may be. This demanded further rigor, and PEANO's article concerning the integration of first-order differential equations, published in the same year, made full use of symbolism to ensure maximum rigor.

Chapter 7 looks into the basic concepts of PEANO's axioms, particularly that of number. It attempts to expound PEANO's concept of number and explains why PEANO refused to define it. It shows that PEANO's proposed system was qualitatively different from other contemporary systems, in particular DEDEKIND's, rendering the hoary question of priority redundant. At the end of the chapter I present PEANO's "definition by abstraction," a none-too-clear concept which might relate him to Logicism.

Chapter 8 deals with the relation of PEANO's work to the foundationist traditions that grew up in his day. PEANO is, in some ways, the predecessor of all these traditions, but when they underwent a crisis he avoided taking part in the controversy. Although definitely a foundationist, PEANO's aim was limited and concentrated on the didactic aspects of mathematics. He naively thought that a clear and rigorous presentation of mathematics would suffice to solve its problems. This approach is interesting, and has a fascination of its own, but, as I point out in the *Conclusion*, like all other foundationist approaches it was an utter failure.

The primary and secondary literature presented in the footnotes is relatively wide-ranging. The bibliography at the end is only a selection of the items presented in the footnotes, namely works that are cited repeatedly or works that I found of particular importance for this research. Works of PEANO are referred to by their number in PEANO's chronological list of publications, quoted in square brackets (*e.g.* [16] refers to PEANO's *Arithmetices principia*). Translations are mine unless otherwise stated.

Chapter 1: The Quest for Rigor

Introduction

This work is about rigor in mathematics, and one of its aims is to show that despite the increasing quest for rigor as a basic prerequisite in modern mathematics, it is very doubtful that mathematical rigor can be attained, and that at times the quest for rigor can even hinder progress in mathematics.

Although the term "rigor" appears frequently in modern mathematical works — especially in those written during the second half of the last century — the notion of mathematical rigor is complex, manifold, and not easily definable.[1] Generally speaking, mathematical rigor is a step-by-step precise logical deduction in mathematical proof. But logic is not a unique science — many logical systems have been suggested since ARISTOTLE's day — and occasionally rigor has varied accordingly.[2] Moreover, what is a "step" in mathematical proof? How big should it be? On what basic premises should it rely?

Since "mathematical rigor" has varied with time, with logic, and from one mathematician to another, a general historical study of the concept would be a major enterprise, and most studies of the subject concentrate on limited periods, mainly on the last century.[3] Whatever the concept of mathematical rigor, however, one general statement can be made: The quest for rigor certainly increased considerably after the seventeenth-century scientific revolution. I shall present here a few examples of the quest for rigor before and after the scientific revolution and suggest that the modern quest for rigor was mainly an

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[1] An overview of how rigor varied is offered by KITCHER, "Mathematical Rigor — Who Needs It?" A discussion of the question whether mathematical rigor is absolute or changeable is presented by SPALT. *Vom Mythos der Mathematischen Vernunft*, §. 1.4.

[2] For examples of how logic varied, see HENRI POINCARÉ, "The New Logics," *The Monist* **22** (1912): 243–256; AGASSI, "Logic and Logic of," and "Presuppositions for Logic."

[3] On change of standards of rigor in history see MICHAEL J. CROWE, "Ten Misconceptions About Mathematics," in ASPRAY & KITCHER, *History and Philosophy of Modern Mathematics*, 260–277; see pp. 269–271 under the title, "Standards of Rigor are Unchanging".

The concept of mathematical rigor in the nineteenth century has been discussed by GIORGIO ISRAEL in "'Rigore' ed 'assiomatica' nella matematica moderna." ISRAEL attempts a general definition of rigor as (p. 432) "giving a central importance to logical deduction in mathematical reasoning, seen as the only procedure able to guarantee the truth of the results obtained." In another article, "'Rigor' and 'Axiomatics' in Modern Mathematics," ISRAEL distinguishes between the "rigor" movement and the "axiomatic" movement: whereas (p. 211) "the nineteenth century exponents of the 'rigor' movement considered the *autonomy of mathematical reasoning* to be indispensable but not so the *autonomy of mathematics* either from intuition or experimental sciences . . . the 'autonomy' of the supporters of the axiomatic movement involves the total separation of mathematics both from intuition and experimental sciences." ISRAEL claims that the two movements are separate things. In this study, however, I try to show that PEANO is a borderline case and could belong to both movements. *(Continued)* outcome of BERKELEY's critique of the (NEWTONian) calculus.[4] Although a certain quest for rigor existed in different forms, albeit sporadically, before BERKELEY, it grew and became insistent after BERKELEY, through the works of mathematicians such as MACLAURIN and LAGRANGE.

It is, of course, possible to show the implicit existence of a "modern" quest for rigor throughout the history of mathematics, beginning with ancient Greece.[5] Traces of a quest for rigor can be found as early as ARCHIMEDES, more precisely in his *Method* (or, to quote the full title — *The Method of Archimedes Treating of Mechanical Problems – to Eratosthenes*) — a work in the form of a letter to ERATOSTHENES, the manuscript of which was discovered by JOHAN L. HEIBERG in 1906 in a library in Constantinople. In this letter ARCHIMEDES tells ERATOSTHENES how he found areas and volumes by means of a particular method, the "mechanical method," emphasizing, however, that this method is only a heuristic tool and not a proper mathematical proof. In ARCHIMEDES' own words:

> Certain things first became clear to me by a mechanical method, although they had to be demonstrated by geometry afterwards because their investigation by the said method did not furnish an actual demonstration.[6]

ARCHIMEDES declared demonstration by means of "geometry" the only legitimate way to demonstrate a theorem. In antiquity the procedure used to find areas and volumes was the "method of exhaustion:" an unknown area (or volume) was shown to be neither greater nor smaller than a certain known area (or volume). Although this was an *a posteriori*, laborious method, ARCHIMEDES and other ancient mathematicians seem to have regarded it as satisfactorily rigorous.[7] One must, of course, be careful in attributing such a consideration to the ancients, since their mathematical understanding differed from ours.[8] But, as we shall see later in this chapter, the method of exhaustion was certainly regarded as an ideal of rigor by a certain number of mathematicians.

ISRAEL bases his ideas, *inter alia*, on CARLO BOLDRIGHINI and FEDERICO MARCHETTI, "Lo sviluppo della matematica alla fine del secolo XIX: il problema dei fondamenti e la formalizzazione hilbertiana," in *Matematica e fisica: struttura e ideologia*, edited by ELISABETTA DONINI, ARCHANGELO ROSSI and TITO TONIETTI (Bari: De Donato, 1977), pp. 109–122. This article attempts to give an outline of different ways of viewing mathematics in France and Germany during the nineteenth century in relation to contemporary socio-political developments.

[4] For an outline of the controversies over the basis of infinitesimal calculus see DAVIES & HERSH, *The Mathematical Experience*, pp. 237–254.

[5] POINCARÉ even claims that this quest was diffused in ancient Greece: "Du Role de l'intuition et de la logique," p. 118. POINCARÉ uses the term analytical (in opposition to intuitive), to denote a mathematician emphasizing rigor.

[6] HEATH (ed.), *The Method of Archimedes*, p. 13.

[7] This, at least, is the interpretation given by HEATH, *ibid.*, p. 7.

[8] For ARCHIMEDES' quest for rigor, see SCHNEIDER, *Archimedes*, pp. 43–63. For the various views concerning mathematics in the time of the Greeks see VOLKERT, *Die Krise der Anschauung*, pp. 1–18.

Although ARCHIMEDES' *Method* was lost until the beginning of the present century, the method itself was known, or at least was assumed to have existed, and Renaissance mathematicians tried to find traces of it, or to reconstruct it.[9] One such attempt was made by BONAVENTURA CAVALIERI (*ca.* 1598–1647) who, in 1635 in BOLOGNA, formulated the "Theory of Indivisibles" — a general method of measuring surfaces and volumes. Generally speaking, CAVALIERI let a straight line move over a surface (or, in the three-dimensional case, a surface move through a volume), and the "trace" left behind by the line (or surface) was used to find the surface (or volume).[10] CAVALIERI's theory was very intuitive, and used, *inter alia*, superposition of forms. In fact it soon came under criticism (between 1635 and 1641) by the Jesuit mathematician PAUL GULDIN in his work *Centrobaryca*. GULDIN complained that CAVALIERI's approach was too intuitive, and claimed that the latter had violated classical (Euclidian) orthodoxy when he considered general geometrical objects without presenting specific constructions. GULDIN also criticized CAVALIERI's use of proof by superposition of geometric figures: "Who will be the judge," he asked, "the hand, the eye or the intellect?"[11] More criticism soon followed, showing the growing awareness, even at this early stage of the scientific revolution, of the necessity for well-defined foundations for mathematical theories. EVANGELISTA TORRICELLI (1608–1647), who used and extended the Theory of Indivisibles in his own mathematical work, did not feel completely at ease with it, and avoided using indivisibles wherever possible, preferring the traditional, Euclidian, geometrical method of proof.[12] Later, even ISAAC NEWTON implicitly criticized the theory by noting that

[9] A monumental study on the work of ARCHIMEDES in the Middle Ages is MARSHALL CLAGETT, *Archimedes in the Middle Ages*, 5 vols. (Vol. 1. Madison: The University of Wisconsin Press, 1964. Vols. 2–5. Philadelphia: The American Philosophical Society, 1984). ARCHIMEDES in the Renaissance (before GALILEO and CAVALIERI) is discussed, *inter alia*, by PAUL LAWRENCE ROSE, *The Italian Renaissance of Mathematics: Studies on Humanists and Mathematicians from Petrarch to Galileo* (Geneva: Droz, 1975).

[10] In his *Geometria indivisibilibus continuorum* (Bologna, 1635). For a modern presentation of CAVALIERI's theory of indivisibles see KIRSTI ANDERSEN, "Cavalieri's Method of Indivisibles," *Archive for History of Exact Sciences*, **31** (1985): 291–367.

[11] The controversy between CAVALIERI and GULDIN is described in ENRICO GIUSTI, *Bonaventura Cavalieri and the Theory of Indivisibles* (Rome: Cremonese, 1980), pp. 55–65. The quotation from GULDIN's *Centrobaryca* is taken from p. 63. See also my *In the Wake of Galileo*, chapter 4.

[12] Using CAVALIERI's theory of indivisibles, TORRICELLI achieved a series of impressive results published in his *Opera geometrica* (Florence, 1644) (he showed, *inter alia*, that the area of a hyperbolic solid is finite). In the same work, however, TORRICELLI tried, whenever he could, to avoid the use of indivisibles, as, for example, in his "De sphaera et solidis sphaerlibus," in the first part of the *Opera geometrica*. See EVANGELISTA TORRICELLI, *Opere*, edited by GINO LORIA and GIUSEPPE VASSURA, Vol. 1, Part 1 (Faenza: Montanari, 1919).

"The hypothesis of indivisibles seems somewhat harsh, and therefore that method is reckoned less geometrical."[13]

All these arguments could be regarded as quests for rigor, although in these cases, too, one must be careful when considering concepts which may well differ from our own. Altogether, these instances were still relatively timid and implicit. A more explicit quest for rigor, as we understand it today — namely as a step-by-step, rigid and coherent logical deduction in mathematical proof — gradually emerged only after the advent of infinitesimal calculus — formulated during the second half of the seventeenth century by NEWTON and LEIBNIZ independently.

Early Criticism of the Calculus

History of mathematics rightly emphasizes the remarkable success of infinitesimal calculus in its various forms during the eighteenth century; less attention is paid to the question of whether this new, powerful mathematical tool was, in fact, also satisfactorily grounded.[14]

In reality, both NEWTON and LEIBNIZ based their calculus on concepts which were not entirely defined. NEWTON called a varying function "fluent," and denoted its rate of change as "fluxion." In simple terms, if one takes the function $f(x)$ and increases x by h, then for a sufficiently small h, the rate of change, or fluxion, of $f(x)$ would be given by $\frac{f(x+h)-f(x)}{h}$. But — and here lies the problem — NEWTON let h tend to zero and conceived it as an "evanescent," or "diminished without end" quantity, so that the increment h was both an existing and a non-existing quantity. NEWTON himself was aware of the problem. He said:

> Perhaps it may be objected, that there is no ultimate proportion of evanescent quantities; because the proportion, before the quantities have vanished, is not the ultimate, and when they are vanished, is none.[15]

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[13] NEWTON, *Principia*, Book I, Lemma XI, Scholium. Translation from MOTTE-CAJORI, Vol. 1, p. 38. (As I. B. COHEN points out in *Introduction to Newton's 'Principia'*, *passim* — *e.g.* p. 329 — this translation is at times misleading; it is nevertheless adequate for the quotations in the present work). NEWTON's leading follower, COLIN MACLAURIN, too, in the first page of the Introduction to his *A Treatise of Fluxion* (Edinburgh, 1742), criticized CAVALIERI's method of indivisibles for its lack of rigor: "But as this doctrine was inconsistent with the strict principles of geometry, so it soon appeared that there was some danger of its leading them into false conclusions."

[14] On the success of calculus, as well as its unsatisfactory grounding, see SPALT, *Vom Mythos der Mathematischen Vernunft*, §. 2.14.1; GRABINER, "Is Mathematical Truth Time-Dependent?", pp. 356–358. Foundations, says GRABINER (p. 358), were "relegated to Chapter I of textbooks, or found in popularizations." As A. P. YOUSCHKEVITCH rightly points out, "There is a very widespread opinion according to which eighteenth-century mathematics paid little attention to justification of the infinitesimal calculus, the period (in F. KLEIN's terms) having been a creative and uncritical one. That opinion is erroneous." YOUSCHKEVITCH, "Lazare Carnot," p. 151.

[15] NEWTON, *Principia*, Book I, Lemma XI, Scholium. MOTTE's trans., Vol. 1, p. 38.

He tried to overcome the problem by giving a physical explanation related to velocity:

> But by the same argument it may be alleged that a body arriving at a certain place, and there stopping, has no ultimate velocity; because the velocity, before the body comes to the place, is not its ultimate velocity; when it has arrived there is none.[16]

This argument, however, was not totally satisfactory, and later in this chapter we shall discuss the objections raised against NEWTON's calculus.

LEIBNIZ's formulation was no better grounded. He based his calculus on infinitesimal quantities; he did not claim that they really existed, but thought one could reason correctly as if they did. Some mathematicians would certainly not find this sufficiently rigorous.

The development of rigorous mathematics after NEWTON has been studied by PHILIP KITCHER.[17] KITCHER distinguishes between rigorous reasoning and rigorous mathematics. "Central to the idea of rigorous reasoning," he says, "is that it should contain no gaps, that it should proceed by means of elementary steps."[18] But rigorous mathematics cannot simply be identified with rigorous arguments: one can, for instance, devise a rigorous substitute for an unrigorous argument by expanding the set of premises. Both NEWTON and LEIBNIZ, for example, already had their own rigorous reasoning. NEWTON's rigorous reasoning was geometrical, and he sought to ground his calculus on geometry; for LEIBNIZ, on the other hand, rigorous reasoning was algebraic.[19] KITCHER raises the question why mathematicians (and philosophers) ask for rigorous proofs of statements which they already know to be true.

The question has more than one answer. With regard to the controversy that followed NEWTON's calculus, the concern for rigor was rationally motivated, since mathematicians were unable to find a way to reconstruct successful reasoning in terms of inferences which they regarded as elementary and premises they accepted as true.[20] Rigor grew in response to criticism, in the belief that, by increasing the precision or rigor of a theory, one could render criticism harmless. It is precisely this controversy that impelled the quest for rigor, and, as I shall try to show, it is this quest that led to the modern investigations into the foundations of mathematics.

The controversy was initiated by two philosopher-mathematicians: BERNARD NIEUWENTIJT (1654–1718) raised the problem as early as 1694 (when NEWTON and LEIBNIZ were still alive), and GEORGE BERKELEY (1685–1753) later gave it particular impetus.[21]

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[16] *ibid.*, pp. 38–39.
[17] KITCHER, "Mathematical Rigor — Who Needs It?"
[18] *ibid.*, p. 469.
[19] *ibid.*, pp. 478–479.
[20] *ibid.*, p. 482.
[21] For a recent outline of the controversy on the question of the existence and nature of revolutions in mathematics, see GIULIO GIORELLO, "The 'Fine Structure' of

NIEUWENTIJT was a physician, a mathematician, a philosopher, and a civil servant in the northern Netherlands (he was the burgomaster of Purmerend). His early works are in mathematics, though he is known mainly for his later works in theology.[22]

NIEUWENTIJT's contribution to mathematics, despite its importance, has not been widely studied.[23] His criticism of the calculus was first expressed in a short work, *Considerationes circa analyseos*, published in 1694.[24] In this work, NIEUWENTIJT admitted that calculus led to correct results, but pointed out its obscurities, criticizing the work of mathematicians such as BARROW, NEWTON, BERNOULLI and LEIBNIZ. For instance, he regarded NEWTON's evanescent quantities as too vague a concept, and could not really understand how NEWTONian terms could both tend to equality and be equal.[25] As to LEIBNIZ, he asked how a sum of infinitesimals in his calculus could be a finite quantity.[26] A year later NIEUWENTIJT published a long technical treatise on calculus, *Analysis infinitorum*, attesting his previous criticism.[27] LEIBNIZ tried to defend his own views in *Acta eruditorum* in the same year, but this did not satisfy NIEUWENTIJT, who in 1696 wrote a *Considerationes secundae*, raising further criticism and explicitly appealing to the rigor in ARCHIMEDEan works.[28]

Mathematical Revolutions: Metaphysics, Legitimacy, and Rigour. The Case of the Calculus from Newton to Berkeley and Maclaurin," in *Revolutions in Mathematics*, edited by DONALD GILLIES, pp. 134–168.

[22] A detailed bibliography of NIEUWENTIJT's work is given by HANS FREUDENTHAL in Vol. **10** of the *Dictionary of Scientific Biography* (1974), p. 121. (One of his works in theology, translated into English under the title *The Religious Philosopher, or the Right Use of Contemplating the Works of the Creator* . . . , is monumental). On NIEUWENTIJT's theology, see HANS FREUDENTHAL, "Nieuwentijt und der teleologische Gottesbeweis," *Synthese* **9** (1953–1955): 454–464.

[23] NIEUWENTIJT is mentioned in many histories of mathematics, without, however, presenting the details of his work. The only study of Nieuwentijt's (philosophy of) science I was able to find is E. W. BETH, "Nieuwentyt's Significance for the Philosophy of Science," *Synthese* **9** (1953–1955): 447–453. It concentrates on NIEUWENTIJT's *Gronden van Zekerheid* (*Fundaments of Certitude*) (Amsterdam, 1720).

[24] *Considerationes circa analyseos ad quantitates infinitè parvas applicatae principia, & calculi differentialis usum in resolvendis problematibus geometricis* (Amsterdam, 1694).

[25] *ibid., Sectio secunda.*

[26] *ibid., Sectio tertia.*

[27] *Analysis infinitorum seu curvilineorum proprietas ex polygonorum natura deducta* (Amsterdam, 1695).

[28] G. W. LEIBNIZ, "Responsio ad nonnullas difficultates a Dn. Bernardo Niewentiit circa methodum differentialem seu infinitesimalem motas," in LEIBNIZ, *Mathematische Schriften*, Vol. **5**, 320–328. LEIBNIZ's article is a good presentation of NIEUWENTIJT's criticism. The full title of NIEUWENTIJT's work is *Considerationes secundae circa calculi differentialis principia et responsio ad virum nobilissimum Leibnitium* (Amsterdam 1696); see, in particular, p. 3.

It may well be that NIEUWENTIJT deserves particular credit as one of the forerunners of the modern quest for rigor. However, judging from contemporary literature, the greatest credit for fostering this quest seems to belong to a non-professional mathematician: GEORGE BERKELEY. BERKELEY raised the basic question: Are we certain that we know exactly what the mathematical objects we speak of are? The question is still open, despite two and a half centuries of attempts to answer it.

Berkeley's Criticism

BERKELEY was an Irish philosopher and a bishop. His fame derives mainly from his philosophy, which denies the existence of matter. Yet the value of his work has also been recognized by mathematicians interested in the history of their subject.[29] There may, of course, have been a direct relation between BERKELEY's general philosophy and his mathematical philosophy, since he also questioned the existence of certain mathematical entities.

BERKELEY's interest in mathematics began in his youth. More or less at the time when NIEUWENTIJT was producing his criticism of calculus, BERKELEY wrote two Latin tracts, *Arithmetica absque Algebra aut Euclide demonstrata*, and *Miscellanea Mathematica*, which for a long time were considered to be youthful writings of no particular value.[30] In this century, however, the late J.O. WISDOM pointed out that these works may be more important than previously thought, not only as mathematical exercises, but also as containing some remarks concerning the logic of mathematics.[31] The remarks referred to may have been the nucleus of BERKELEY's general criticism of calculus, developed in full forty years later in his best known mathematical work, *The Analyst*, published in 1734, less than a decade after NEWTON's death.[32]

The Analyst was presented as a polemic against the materialists, mathematicians and scientists of his day: it bears the subtitle *A Discourse addressed to an Infidel Mathematician.* The "infidel mathematician" referred to was probably NEWTON's friend, EDMUND HALLEY, and BERKELEY suggested that just as infidels thought they had the right to express their doubts concerning religion, so believers, like BERKELEY, had the right to claim that mathematics rested on

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[29] Many works have been written on BERKELEY. A concise outline of his life and works is J. O. WISDOM, "An Outline of Berkeley's Life," *The British Journal for the Philosophy of Science*, 4 (May 1953 — February 1954): 78–87. For a recent general presentation of BERKELEY's life and work see BREIDERT, *Berkeley.*

[30] *The Works of George Berkeley*, 4: 157–230.

[31] J. O. WISDOM, "The *Analyst* Controversy: Berkeley's Influence on the Development of Mathematics," p. 4, and "The *Analyst* Controversy: Berkeley as a Mathematician." BERKELEY's two early mathematical works have been studied in BREIDERT, *Berkeley*, pp. 83–90.

[32] *The Works of George Berkeley*, Vol. 4, pp. 53–102.

a shaky foundation.[33] Moreover, as ERIC SAGENG points out, BERKELEY's object was to demonstrate that mathematicians accepted things on faith.[34] And he showed how infinitesimal calculus was by no means as exact and rigorous as one would expect from a major mathematical field.

From the very beginning of *The Analyst*, BERKELEY advanced a plea for rigor:

> It hath been an old remark, that Geometry is an excellent Logic. And it must be owned that when the definitions are clear; when the postulata cannot be refused, nor the axioms denied; when from the distinct contemplation and comparison of figures, their properties are derived, by a perpetual well-connected chain of consequences, the objects being still kept in view, and the attention ever fixed upon them; there is acquired an habit of reasoning, close and exact and methodical: which habit strengthens and sharpens the mind, and being transferred to other subjects is of general use in the inquiry after truth.[35]

BERKELEY then presented in brief outline the objects of NEWTONian analysis: points, lines, planes, solids, velocities, fluxions and moments. "The foreign mathematicians," he said (probably hinting at LEIBNIZ, NIEUWENTIJT and the MARQUIS L'HÔPITAL), "are supposed by some, even of our own, to proceed in a manner less accurate, perhaps, and geometrical, yet more intelligible."[36]

As mathematics historian NICCOLÒ GUICCIARDINI points out, BERKELEY's criticism was both ontological and logical. In the ontological criticism, BERKELEY questioned the existence of the objects of infinitesimal calculus and it was therefore important for NEWTON's followers that mathematics had some empirical foundations. This ontological aspect is relevant to later controversies, in PEANO's day, related to the foundations of mathematics and to the degree to which mathematics should rely on intuition.[37]

As to the logical criticism, BERKELEY argued that the principles of mathematical analysis include error and false reasoning. BERKELEY's criticism, like NIEUWENTIJT's, was that NEWTON's concept of a limit was self-contradictory.[38] BERKELEY held that NEWTON's vanishing quantities were variously considered, for the sake of convenience, at one stage as a finite quantity and at another stage as zero, and that their effects remained after they vanished. BERKELEY argued that NEWTON could not have it both ways, and produced a series of arguments or examples which were so simple as to appear trivial, though in fact

[33] See the Editor's introduction to *The Analyst*, *ibid.*, pp. 56–57.
[34] SAGENG, *Colin MacLaurin*, p. 342.
[35] BERKELEY, *The Analyst*, §. 2.
[36] BERKELEY, *The Analyst*, §. 5.
[37] NICCOLÒ GUICCIARDINI, *The Development of Newtonian Calculus in Britain*, Chap. 3, see p. 39.
[38] BERKELEY's criticism is presented, *inter alia*, by WISDOM's two articles on "The *Analyst* Controversy;" I have relied here on these articles.

they were not. Two arguments were particularly strong and impressive: the fluxion of a product and that of a power.

The first argument, against NEWTON's method of obtaining the fluxion of a product, was so simple, straightforward and short that it may be quoted as written:[39]

> Suppose the product or rectangle AB increased by continual motion: and that the momentaneous increments of sides A and B are a and b. When the sides A and B were deficient, or lesser by one half of their moments, the rectangle was $(A - \frac{1}{2}a) \times (B - \frac{1}{2}b)$, *i.e.* $AB - \frac{1}{2}aB - \frac{1}{2}bA + \frac{1}{4}ab$. And as soon as the sides A and B are increased by the other two halves of their moments, the rectangle becomes $(A + \frac{1}{2}a) \times (B + \frac{1}{2}b)$ or $AB + \frac{1}{2}aB + \frac{1}{2}bA + \frac{1}{4}ab$. From the latter rectangle subduct the former, and the remaining will be $aB + bA$. Therefore the increment of the rectangle generated by the intire increments a and b is $aB + bA$.[40]

Up to this point BERKELEY followed NEWTON,[41] but then he made the following objection:

> But it is plain that the direct and true method to obtain the moment or increment of the rectangle AB, is to take the sides as increased by their whole increments, and so multiply them together, $A + a$ by $B + b$, the product whereof $AB + aB + bA + ab$ is the augmented rectangle; whence, if we subduct AB the remainder $aB + bA + ab$ will be the true increment of the rectangle, exceeding that which was obtained by the former illegitimate and indirect method by the quantity ab.

The argument speaks for itself.

As to the second argument, the fluxion of a power, BERKELEY argued against NEWTON's attempt, in his *De quadratura*, to avoid infinitely small quantities in the finding of the fluxion of x^n.[42] NEWTON's reasoning was as follows: he gave x an increment $\boldsymbol{o}$ and expanded $(x + \boldsymbol{o})^n$ by the binomial theorem; he then subtracted x^n from the expansion, obtaining the increment in x^n; finally, he divided this increment by $\boldsymbol{o}$ and let $\boldsymbol{o}$ become evanescent. BERKELEY's argument in this case, too, was quite simple. (For brevity's sake let us consider the case x^2.) The incrementary ratio is $(2x \cdot \boldsymbol{o} + \boldsymbol{o}^2)/\boldsymbol{o}$ or $2x + \boldsymbol{o}$. BERKELEY noted that $\boldsymbol{o}$ was supposed to be "something". In the next step, however, $\boldsymbol{o}$ was allowed to become zero, so as to produce the fluxion $2x$. On this BERKELEY said that it introduces a supposition contrary to the first, namely, that there was no longer an increment of x (or that $\boldsymbol{o}$ is nothing), so that it was invalid to retain the result $2x$. In short, if $\boldsymbol{o}$ were something, what was obtained would have been not $2x$

[39] BERKELEY, *The Analyst*, §. 9.

[40] In the original text underlining is used instead of brackets.

[41] NEWTON, *Principia*, Book II, Lemma II.

[42] BERKELEY, *The Analyst*, §. 13–14.

but $2x + o$ where o was not zero, since, if o were zero, nothing would be obtained.

The Analyst contains many such similar ingenious examples. BERKELEY also asked himself how the method of fluxions, assumed to be faulty, could produce correct results. He observed that

> The conclusion comes out right, not because the rejected square of dy was infinitely small; but because this error was compensated by another contrary and equal error. I observe in the second place, that whatever is rejected, be it ever so small, if it be real and consequently makes a real error in the premises, it will produce a proportional real error in the conclusion.[43]

BERKELEY gave detailed examples to support this. He showed, for instance, that the calculation of the subtangent could be set out in such a way as to contain two finite quantities that cancel each other out. NEWTON had let these quantities "vanish." BERKELEY argued that this was illegitimate and that in NEWTON's method two canceling quantities were really ignored.[44]

To sum up, BERKELEY dismissed fluxions as "ghosts of departed quantities," and showed that the classical problem concerning the relation between finite and infinite which the new analysis had tried to solve was also the heart of the flaw in the NEWTONian (and LEIBNIZian) presentation. This was, without a doubt, a major achievement of logical criticism.

BERKELEY is often said to have contributed little to mathematics over and above his criticism.[45] Yet his criticism posed the modern version of the problem of the relation between finite and infinite, and stimulated a series of fundamental mathematical investigations. More explicitly than anyone before him, BERKELEY raised the question of what mathematical objects we use in calculus. Are they true, and if so how can they have a double meaning? How, and by means of what basic terms are they defined? Are they only symbols without essence, acquiring a meaning according to the circumstances? These are deep and significant questions, still valid today. No wonder, then, that the reaction to BERKELEY was immediate, with the controversy spreading to many mathematicians.[46]

There were three main trends of reaction to BERKELEY: First, the classical, appealing to the rigor existing — or thought to have existed — in Greek geometry; then the algebraic, trying to divert the problems from geometry to

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[43] BERKELEY, *The Analyst*, §. 23.

[44] BERKELEY, *The Analyst*, §. 24.

[45] See, for instance, J. O. WISDOM, "The *Analyst* Controversy: Berkeley's Influence on the Development of Mathematics" p. 3.

[46] An outline of BERKELEY's criticism and the reaction to it is given by THOMAS THOMSON in *History of the Royal Society from its Institution to the End of the Eighteenth Century* (London, 1812), pp. 297–302. See also YOUSCHKEVITCH, "Lazare Carnot."

algebra and solve them as algebraic problems; and, finally, the attempt to solve the problem by studying and rendering "rigorous" the concept of limit. We shall see how the various attempts to overcome BERKELEY's criticism, regardless of their success or failure to solve the basic problems, certainly augmented the quest for rigor.

The Return to the Ancients: Maclaurin

The best counter-move to criticism is to show that some move of the critic is not legitimate. An alternative counter-move is to increase the theory's precision or rigor, so as to render the critic's move superfluous. COLIN MACLAURIN (1698–1746), the first major advocate of NEWTON's calculus, attempted to make both moves in one by appealing to the authority and legitimation of classical mathematics.[47]

In 1742, eight years after the publication of *The Analyst*, MACLAURIN published the first extensive reply to BERKELEY, a book entitled *A Treatise of Fluxions*, in which he endeavored to put the theory of fluxions on a "sound basis" in answer to BERKELEY's criticism. This is also the first great work on mathematical rigor: it states plainly:

> When the certainty of any part of geometry is brought into question, the most effectual way to set the truth in a full light, and to prevent disputes, is to deduce it from axioms or first principles of unexceptionable evidence, *by demonstrations of the strictest kind*, after the manner of the antient geometricians.[48]

MACLAURIN endeavored, then, to derive the rules of fluxions from the "rigorous methods of the ancients" (*i.e.* the method of exhaustion). His *Treatise* is a broad mathematical work, written in English, with relatively few mathematical notations, using ancient methods, though in as modern a presentation as possible. It is a thorough "classical" treatment of calculus and of additional problems in physics: It studies the logic of fluxions better than any other previous work on the subject. It solves a large number of mathematical and physical problems. Most important, it contains a discussion of infinite series including the test of convergence and the series for the expansion of a function of x which bears his name.[49] His work with series is a prelude, as we shall soon

[47] An outline of MACLAURIN's life and work is given by HERBERT WESTERN TURNBALL in *Bi-centenary of the Death of Colin Maclaurin (1698–1746)* (Aberdeen: The University Press, 1951). A recent study of MACLAURIN's reply to BERKELEY is SAGENG's *Colin MacLaurin.*

[48] *A Treatise of Fluxions*, pp. 2–3, my italics.

[49] *ibid.*, §. 751.

see, to an important trend in the study and development of the theory of calculus.[50]

Admittedly MACLAURIN's *Treatise* was written on the defensive and its insistence on the classical approach kept alive an allegedly old standard.[51] But as far as rigor is concerned, a most important outcome of MACLAURIN's work was to accentuate the quest for rigor — without really overcoming the central difficulty relating to the value of a ratio whose constituents vanish. This, perhaps, questions the success of his attempt: the controversy, in any case, remained open.

The Algebraic Solution

By the end of the eighteenth century, many more mathematicians expressed a quest for rigor, despite the fact that rigor, as a solution, did not, and does not — as I claim in the present work — necessarily solve the problem of inconsistencies in calculus. How could one render calculus more rigorous? Another suggested solution was to turn to algebra.

NEWTON himself viewed algebra as a "universal arithmetic," in which the operations of ordinary arithmetic are applied to letters instead of numbers.[52] LEIBNIZ had produced a system of notation for his calculus, and this could be regarded as a first step in linking algebra and calculus. Since in eighteenth century algebra the quest for foundations had not yet been raised, its "rigor" could perhaps be used in some way to find the rigor in calculus. Moreover, a major algebraic challenge in the seventeenth century — one of NEWTON's and LEIBNIZ's concerns — had been the study of infinite series. Algebra was perhaps more appropriate than other mathematical fields to deal with infinite mathematical processes.

It is at this point that the work of LEONHARD EULER (1707–1783) concerning the infinite and infinitesimals should be mentioned. EULER contributed more than any contemporary mathematician to making calculus a coherent discipline.

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[50] See *ibid.*, §. 745.

[51] J. F. SCOTT, the author of the article on MACLAURIN in the *Dictionary of Scientific Biography* (Vol. 8, p. 611), also says that MACLAURIN's defense had harmful consequences for the progress of mathematics in Great Britain. According to SCOTT, his work, combined with national pride, induced Englishmen to follow the geometrical methods which NEWTON had employed in the *Principia*, rather than the analytical methods which were being pursued with such conspicuous success on the Continent. As a result, for over a century English mathematicians came to believe that the calculus was not really necessary. This view is generally true, although SAGENG (*Colin Maclaurin*, pp. 341–352), and GUICCIARDINI, (*The development of Newtonian Calculus*) have shown that there were also continental influences in eighteenth century Britain.

[52] See "The 'First Book of Universal Arithmetic'," in D. T. WHITESIDE (ed.) *The Mathematical Papers of Isaac Newton*, Vol. 5 (Cambridge: At the Univ. Press, 1972), pp. 538–624. See pp. 538–539.

In 1748 he published his *Introductio in analysin infinitorum*, a work on functions, curves, infinite series, infinite products and infinite continued fractions. He presented, for example, infinite-series development for all standard functions of the time, such as quotients of polynomials, exponentials, logarithms, sines and cosines. He showed how functions could be represented by infinite series, with no need of infinitesimal quantities. Deriving series was viewed as part of algebra: one could therefore speak of analysis of the infinite in algebraic terms.

EULER had a peculiar, "formalistic" approach to mathematics: the formal representation mattered to him more than the conceptual relationship.[53] (Although EULER did not share LEIBNIZ's general mathematical and philosophical approach, the latter's notations suited his approach.[54]) In his *Introductio*, EULER defined function as an analytic expression composed of variables, numbers, or constant quantities.[55] He used the same approach to infinitesimal calculus in his later *Institutiones calculi differentialis* (1755). EULER regarded infinitesimal quantities as zeros, and held that their peculiarity is that their ratio (0:0) could represent a finite number (he argued $n \cdot 0 = 0$, hence $n : 1 = 0 \cdot 0$).[56] He also held that dx^2 vanishes more quickly than dx so that $dx \pm dx^2 \colon dx = 1$.[57]

Thus, for EULER the determination of the ratio of evanescent increments had a different meaning than for NEWTON and LEIBNIZ. Take, as an example, the function x^2 (EULER wrote xx). If the increment of x is ω, then the increment of the function is $2x\omega + \omega^2$ (EULER wrote $2x\omega + \omega\omega$). The ratio between the increment of the function and the increment of x is $2x \times \omega$. The smaller the value of ω, the closer the ratio approaches $2x$. However, these increments are absolutely zero and nothing can be inferred from them other than their mutual ratio.[58] In other words, according to EULER there are quantities that are absolutely zero but whose ratios are finite numbers.

So much for EULER's understanding of infinitesimal quantities. Even more interesting is his attitude to the ambiguities in calculus: in the Introduction to his *Institutiones* EULER openly admitted, as BERKELEY had done two decades earlier, that in calculus there are errors that cancel each other out.[59] EULER can therefore hardly be considered as part of the reaction to BERKELEY; in fact one could say that he tried to free analysis from geometrical rigor.[60] Yet EULER

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[53] Formalism has acquired particular significance in the twentieth century: I will deal with twentieth-century Formalism in detail later in this work.

[54] EULER rejected LEIBNIZ's concepts of mathematical atomism or monadology. See his *Institutiones calculi differentialis, Leonhardi Euleri Opera Omnia*, Series I, Vol. **10** (Leipzig: Teubner, 1913), p. 67.

[55] LEONHARD EULER, *Introductio in analysin infinitorum, Leonhardi Euleri Opera Omnia*, Series I, Vol. **8** (Leipzig: Teubner, 1922), p. 18.

[56] *Institutiones, Opera omnia*, Series I, **10**: 69–71.

[57] *ibid.*, p. 71.

[58] *ibid.*, p. 7.

[59] *ibid.*, p. 6.

[60] For a comparison between EULER's and CAUCHY's rigor, see KITCHER, "Mathematical Rigor — Who Needs It?," pp. 488–490.

inaugurated a line of thought which would soon return to rigor: his idea that there was an algebra of infinite power series eventually stimulated JOSEPH LOUIS LAGRANGE (1736–1813) to originate and promulgate the program of reducing calculus to algebra.[61]

LAGRANGE — one of the most influential and respected of modern mathematicians — was born in Turin. In 1766 he left for Berlin where he worked for the Berlin Academy, and in 1787 he moved to Paris where he remained till his death. His philosophy of mathematics is of central importance as a bridge between the early, "non-rigorous" calculus criticized by BERKELEY, and the nineteenth-century "rigorous," calculus, formulated mainly by CAUCHY.[62]

LAGRANGE expressed criticism similar to BERKELEY's, but believed that calculus was, after all, a rigorous discipline, even though still not properly formulated. His proposed solution to express calculus in term of series was a gradual process, influenced by LAGRANGE's need to teach and to provide his students with a textbook which would present correctly the foundations of calculus. (In fact, as JUDITH GRABINER points out, the need to teach enhanced the quest for rigor;[63] the same need, as we shall see, motivated PEANO a century later.) The nucleus of his program may be found in a letter to EULER of 1759, in which LAGRANGE expressed his concern for the foundations of calculus. LAGRANGE was teaching at that time at the Turin military school, and said that he had worked out the elements of differential and integral calculus for the use of his pupils. He also claimed "to have developed the true metaphysics of their principles, in so far as this is possible."[64]

LAGRANGE returned to the issue of the foundations of calculus in 1760, when philosopher and Barnabite friar, later cardinal, HYACINTH SIGISMUND GERDIL wrote a work claiming that there was no "absolute infinite" in mathematics, and that ancient geometrical rigor allowed only "potential infinite," — as a limit of a possible increase (or decrease) in size.[65] GERDIL argued, *inter alia*, against the method proposed by GUILLAUME DE L'HÔPITAL (1661–1704) to determine the asymptote

[61] EULER, in general, had considerable influence on LAGRANGE. On the interaction between the two mathematicians, see MARIA TERESA BORGATO & LUIGI PEPE *Lagrange: Appunti per una biografia scientifica* (Turin: La Rosa editrice, 1990), in particular pp. 7–20. On the influence of EULER on LAGRANGE, see GRABINER, *The Origins of Cauchy's Rigorous Calculus*, in particular pp. 117–120.

[62] LAGRANGE's role in the rigorization of calculus is emphasized by GRABINER: see *The Origins of Cauchy's Rigorous Calculus*, and "Changing Attitudes toward Mathematical Rigor."

[63] GRABINER, "Is Mathematical Truth Time-Dependent?", pp. 359–360.

[64] *Oeuvres de Lagrange*, Vol. **14** (Paris, 1892), pp. 170–174. Quotation from p. 173.

[65] "De l'infini absolu, Consideré dans la Grandeur", *Miscellanea taurinensia* **2** (1760–1761): 1–45. See pp. 1–2. GERDIL's work is an interesting critical outline of several views concerning infinity. I rely also on GRABINER, *The Origins of Cauchy's Rigorous Calculus*, p. 38.

to a hyperbola by considering it as a tangent at infinity.[66] GERDIL denied that the infinite was really involved in the argument, despite the correct results L'HÔPITAL had obtained following his assumption. LAGRANGE soon responded with a note which throws light on his general opinion concerning rigor in calculus. He agreed with GERDIL that calculus did not really need infinity, but argued that calculus, at least NEWTONian calculus, was rigorous.[67] It was with LEIBNIZ's method of infinitesimals that correct results were obtained due to compensation of errors. LAGRANGE claimed that L'HÔPITAL made an error in assuming that the hyperbola and its asymptotes met, but that the error was compensated for by an opposite error in treating differentials as though they were zeros. However, NEWTON's method of evanescent quantities was, according to LAGRANGE, totally rigorous, albeit more complicated.

In an article written in 1772, "Sur une nouvelle espèce de calcul," LAGRANGE claimed that rigor came to calculus through algebra.[68] LAGRANGE, like EULER, endeavored to present calculus in terms of expansions of power-series.[69] Given a function u of three variables x, y, z, and taking $x + \xi$, $y + \psi$, $z + \zeta$ for x, y, z, one can develop the function into a series which will have the form

$$\begin{aligned} &u + p\xi + q\psi + r\zeta \\ &\quad + p'\xi^2 + q'\xi\psi + r'\psi^2 + \alpha'\xi\zeta + \beta'\psi\zeta + \gamma'\zeta^2 \\ &\quad + p''\xi^3 + q''\xi^2\psi + r''\xi\psi^2 + s''\psi^3 + \alpha''\xi\zeta^2 + \dots . \end{aligned}$$

According to LAGRANGE

> The differential calculus considered in all its generality, consists in finding directly, and by means of easy and simple procedures, the functions $p\ p'\ p'' \dots, q\ q'\ q'' \dots, r\ r'\ r'' \dots$, derived from the function u; and the integral calculus consists in finding back the function u by means of these last functions.
>
> This notion of differential and integral calculus seems to me the clearest and simplest: it is, as one can see, independent of all metaphysics and of all theory of infinitesimally small or vanishing quantities.[70]

The issue of rigor remained important for LAGRANGE in later years also, when he concentrated on other topics, and in 1784 he suggested to the Berlin

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[66] G. DE L'HÔPITAL, *Traité analytique des sections coniques et de leur usage pour la résolution des équations dans les problèmes tant déterminés qu'indeterminés* (Paris, 1720), §. 108.

[67] "Note sur la métaphysique du calcul infinitésimal," *Oeuvres de Lagrange*, 7: 597–599. This note, like GERDIL's, was also originally published in *Miscellanea taurinensia*, 2.

[68] The full title is "Sur une nouvelle espèce de calcul, relatif a la différentiation et a l'intégration des quantités variables," *Oeuvres de Lagrange*, 3: 439–476.

[69] LAGRANGE, this time, did not acknowledge EULER, but only LEIBNIZ and JEAN BERNOULLI: see, *ibid.*, p. 441.

[70] *ibid.*, p. 443.

Academy that they award a prize for a work which would advance the foundations of calculus. The official announcement of the prize began by saying:

> The utility derived from mathematics, the esteem it is held in, and the honorable name of 'exact science' *par excellence* justly given it, are all the due of the clarity of its principles, the rigor of its proofs, and the precision of its theorems.
>
> In order to ensure the perpetuation of these valuable advantages in the elegant part of knowledge, there is needed *a clear and precise theory of what is called Infinite in Mathematics.*[71]

The quest for rigor is self-evident here.

Finally, in 1797 LAGRANGE published his *Théorie des Fonctions analytiques*, a work which endeavored to solve the problem of putting calculus on a rigorous basis by means of the algebra of infinite series.[72] The Introduction to this work reveals LAGRANGE's philosophy of mathematics. It includes a general critique of previous attempts to found calculus, including EULER's work. Echoing BERKELEY, LAGRANGE writes that infinitesimally small quantities are not a sufficient basis for differential calculus and admitted that there is in differential calculus compensation of errors. NEWTON's calculus of fluxions was clearer, but it introduced the concept of speed which, according to LAGRANGE, was foreign to a mathematical domain and cannot be regarded as rigorous.[73] LAGRANGE repeated the hoary claim that NEWTON's method has "the great inconvenience of considering quantities in the state in which they cease, so to speak, to be quantities; for though we can always properly conceive the ratios of two quantities as long as they remain finite, that ratio offers to the mind no clear and precise idea, as soon as its terms both become nothing at the same time."[74]

LAGRANGE's solution, namely to present calculus as an algebraic process, is interesting but has disadvantages. As CARL B. BOYER, in his *The History of the Calculus*, says: "Lagrange's method is based on the unwarranted supposition that every function can be so represented and handled. Moreover, the escape from the infinitely large and the infinitely small, as well as from the limit concept, is only illusory, inasmuch as these notions enter into the critical

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[71] YOUSCHKEVITCH, "Lazare Carnot," p. 155. *Cf.* GRABINER, *The Origins of Cauchy's Rigorous Calculus*, p. 41.

[72] *Oeuvres de Lagrange*, Vol. **9**.

[73] *ibid.*, p. 17.

[74] *ibid.*, p. 18, translation from KLINE, *Mathematical Thought*, p. 430. An interesting criticism of LAGRANGE's "speculative" and "rigorous" approach was expressed by TOMMASO VALPERGA-CALUSO in "Sul paragone del calcolo delle funzioni derivate coi metodi anteriori," in *Memorie di Matematica e di Scienze Fisiche della Società Italiana delle Scienze*, (1809, Part 1): 201–224. VALPERGA-CALUSO argued that NEWTON's "natural" method is more advantageous than LAGRANGE's.

question of convergence which Lagrange did not adequately consider. Furthermore, his method lacks the operational suggestiveness and facility which the Leibnizian ideas and notations afforded."[75] In fact, the existence — against LAGRANGE's belief — of functions which cannot be expanded in a Taylor series was shown by CAUCHY in 1822.[76] Despite these criticisms, however, one thing is certain: LAGRANGE heightened even further the search for rigor.

The problems indicated by BOYER were well presented in LAZARE CARNOT's *Réflexions sur la métaphysique du Calcul infinitésimal* (1797; the work had been submitted for the 1784 competition of the Berlin academy), an outstanding outline of infinitesimal calculus, presenting the various approaches to it with their advantages and disadvantages. CARNOT admitted that calculus was a short-cut to the method of exhaustion where errors compensated each other.[77]

A further step towards rigor in calculus — perhaps the most important — was made by CAUCHY. At the core of his work was yet another approach, based on the theory of limits. But when speaking of limits, one should mention one more prominent contemporary mathematician, who made a contribution in this area before CAUCHY : JEAN LE ROND D'ALEMBERT (1717–1783). It was he who proposed founding differential calculus on the notion of limit and contended that differential calculus does not treat infinitely small quantities, but limits of finite quantities.

D'ALEMBERT did not criticize NEWTON. In his article on "*Différentiel*" in the *Encyclopédie* which he co-edited, D'ALEMBERT says that NEWTON "has never regarded the differential calculus as a calculus of infinitesimals, but as a method of prime and ultimate ratios, that is to say, a method of finding the limits of these ratios."[78] D'ALEMBERT favored the derivative as a limit: He believed that the theory of limits is the true metaphysics of calculus. In his pursuit of limits he, like EULER, argued that $0/0$ may be equal to any quantity one wishes.

D'ALEMBERT, too, was vague on a number of points; for example, he defined the tangent to a curve as the limit of the secant when two points of intersection become one. This vagueness, especially in his statement on the notion of limit, caused considerable debate on whether a variable can reach its limit. But here we reach CAUCHY, and enter the most important period in the history of mathematical rigor.

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[75] p. 253.

[76] In "Sur le développement des fonctions en séries et sur l'intégration des équations différentielles ou aux différences partielles," *Oeuvres*, 2nd series, vol. 2, pp. 276–282.

[77] PEANO, by the way, also wrote (in 1912) a historical critique on the concepts of derivative and differential: "Derivata e differenziale," *Opere scelte*, 1: 369–388 [159]. In this article, PEANO claims that the pioneers of differential calculus thought in derivatives, rather than in differentials.

[78] Vol. 4 (1754), pp. 985–986. Translation from KLINE, *Mathematical Thought*, pp. 432–433.

Chapter 2: The Rigorous Calculus

Many histories of mathematics contend that during the nineteenth century infinitesimal calculus reached a satisfactory, systematic and rigorous formulation, implying that this formulation solved its basic problems. The main credit — they say — goes to CAUCHY, and if the latter did not solve all the problems, the last touch was given in the second half of the nineteenth century by WEIERSTRASS.[1] It is, *inter alia*, against this general view that the present work argues: in this and the following chapters I propose to show that the basic problems of calculus — so vividly described by BERKELEY — did not find their final solution either in CAUCHY's work or in the works of later mathematicians. At most, infinitesimal calculus was presented more clearly, but the problem of its foundations remained, and still exists in today's mathematics.

This chapter will outline the attempts made during the nineteenth century to solve the problems of infinitesimal calculus by means of an increasingly rigorous formulation based on the concept of limit. The first section will sketch CAUCHY's life and work and discuss the impact of his contribution. Though he hoped to give calculus a final, rigorous presentation, flaws soon appeared in his formulation. The next section will present some of the inconsistencies in his work and analyze how mathematicians reacted to them. Not only was there an increase in the quest for rigor, but the emphasis on rigor at times led to stagnation in mathematical progress. Mathematicians — CAUCHY in particular — instead of modifying their "rigorous" system, at times refused even to acknowledge the existence of mistakes. The quest for rigor ultimately led to a calculus which leaned — again — more and more towards algebra, a direction of research already noticeable in the work of CAUCHY's contemporary,

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[1] CAUCHY's work is, indeed, considered an apogee by classical histories of science. FELIX KLEIN regarded him as the "founder of the exact infinitesimal calculus:" KLEIN, *Elementarmathematik vom höheren Standpunkte aus*, 3rd ed. (Berlin: Springer, 1924), p. 229. E. T. BELL, *Men of Mathematics* (New York: Simon and Schuster, 1937), p. 271, says: "Modern mathematics is indebted to CAUCHY for two of its major interests, each of which marks a sharp break with the mathematics of the eighteenth century. The first was the introduction of rigor into mathematical analysis." BOYER, in the last chapter of his *History of Calculus*, under the title "The Rigorous Formulation," says (p. 282): "With CAUCHY, it may safely be said, the fundamental concepts of the calculus received a rigorous formulation." WISDOM, mentioned in the previous chapter for his presentation of BERKELEY's criticism of NEWTONian calculus, ends his article on "The *Analyst* Controversy" (1939) by saying (p. 28): "It may fairly be said that the line of thought initiated in *The Analyst* reached its crowning achievement in CAUCHY's definition of continuity." The more recent *The History of Mathematics — A Reader*, edited by FAUVEL & GRAY (1987), p. 556, cites "some of the definitions that he [CAUCHY] presented, which show how he built up a new calculus, the first to be adequately rigorous." *Cf.* SPALT, *Vom Mythos der Mathematischen Vernunft*; this view is upheld by the "authoritarian" interlocutor in the dialogue, Anna; p. 35.

BERNARD BOLZANO.[2] The main credit for refining CAUCHY's formulation and basing it on algebra belongs to WEIERSTRASS. However, WEIERSTRASS encountered new problems related to the nature of irrational numbers which were still not clearly defined and had to be taken into account. Thus the whole problem shifted to a study of the nature of numbers. This opened a new era in mathematical research, which leads us to DEDEKIND, CANTOR and PEANO.

Cauchy

AUGUSTIN-LOUIS CAUCHY, the mathematician who allegedly formulated the "rigorous calculus," was born in Paris in 1789. He served under Napoleon as a military engineer, and later became professor of mathematics at the École Polytechnique. He had reactionary ideas, and in 1816 took the place of GASPARD MONGE at the Académie des Sciences, after the latter had been expelled because of his Bonapartism. With the accession of LOUIS-PHILIPPE, CAUCHY went into exile, living in various European cities. Finally, in 1838, he returned to Paris and remained there until his death in 1857.[3]

CAUCHY's mathematical output is impressive: no fewer than 789 papers, some fifty written during the last two decades of his life. More than any mathematician before him, and perhaps also after him, he gave a general, systematic, clear and elegant presentation of calculus, giving the impression of a system without flaws. Among his major contributions are: *Cours d'analyse de l'École Royale Polytechnique* (1821); *Résumé des leçons données à l'École Polytechnique sur le calcul infinitésimal* (1823) and *Leçons sur le calcul différentiel* (1829).[4] CAUCHY's main concern, in these and other works, was rigorous formulation: in the Introduction to the *Cours d'analyse* he said: "As for methods, I have sought to give them all the rigor that one demands in geometry, in such a way as never to revert to reasoning drawn from the generality of algebra."[5] This sentence is doubly important: it directly confirms CAUCHY's plea for rigor, and also indicates that CAUCHY looked for rigor in *geometry* rather than algebra (the end of this chapter will clarify further the importance of this second point). Where do we find his geometrical rigor?

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[2] For a general outline of this process see HANS NIELS JAHNKE & MICHAEL OTTE, "Origins of the Program of 'Arithmetization of Mathematics,'" in MEHRTENS, BOS & SCHNEIDER, *Social History of Nineteenth Century Mathematics*: 21–49.

[3] For a recent biography of CAUCHY, see BRUNO BELHOSTE, *Augustin-Louis Cauchy: A Biography* (New York: Springer, 1991).

[4] A concise outline of these ideas is presented by J. M. DUBBEY, "Cauchy's Contribution to the Establishment of the Calculus," *Annals of Science* **22** (1966): 61–67. My outline relies on this work.

[5] CAUCHY, *Cours d'analyse* (1821), pp. ii–iii. *Oeuvres*, Series 2, Vol. **3** (Paris, 1897). Translation from BOTTAZZINI, *The Higher Calculus*, p. 102.

CAUCHY's starting point was his classical definition of limit:

> When the values successively attributed to a variable approach indefinitely a fixed value, so as to end by differing from it by as little as one wishes, this latter is called the *limit* of all the others.[6]

CAUCHY's main contribution could be summarized by saying that he constructed the whole edifice of calculus, as rigorously as he could, on the above definition.[7] He began by defining the concept of infinitesimal more concretely than his predecessors, as a variable whose values converged towards zero as a limit. In his own words:

> A variable quantity becomes *infinitely small*, when its numerical value decreases indefinitely so as to converge towards the limit zero.[8]

Similarly, CAUCHY defined the bounded function $f(x)$ as being continuous between two given limits of the variable x

> if, between these limits, an infinitely small increase of the variable always produces an infinitely small increase of the function itself.[9]

What is important in the above definition is that continuity is defined in terms of limits, rather than *vice versa* as mathematicians before CAUCHY had attempted to do.

Having defined continuity, CAUCHY reached the concepts of quotient of infinitesimals and of derivative. He first says:

> When the two terms of a fraction are infinitely small quantities whose numerical values decrease indefinitely with those of the variable α, the singular value that received this fraction, for $\alpha = 0$, is sometimes finite, sometimes zero or infinite.[10]

This is a general definition of a limit of the form 0/0. CAUCHY then considered the particular fraction (*i.e.* derivative)

$$\frac{f(x+\alpha)-f(x)}{\alpha}$$

[6] CAUCHY, *Oeuvres*, Series 2, Vol. 3, p. 19.

[7] According to BOYER, the above definition "appealed to the notions of number, variable, and function, rather than to intuitions of geometry and dynamics:" *The History of the Calculus*, p. 273. This statement seems not to be entirely accurate — we have just seen that CAUCHY himself was looking for rigor in geometry.

[8] CAUCHY, *Oeuvres*, Series 2, Vol. 3, p. 37, translation from GRATTAN-GUINNESS, *From the Calculus to Set Theory*, p. 110.

[9] CAUCHY, *Oeuvres*, Series 2, Vol. 3, p. 43.

[10] CAUCHY, *Oeuvres*, Series 2, Vol. 3, p. 64, translation from BOTTAZZINI, *The Higher Calculus*, p. 117.

whose two terms converge at the limit zero with the variable α, whenever one assigns to the variable x a value in whose neighborhood the function $f(x)$ remains continuous.[11]

Apart from continuity and derivatives CAUCHY made many other important contributions. For instance, he treated the convergence of series (which will be discussed in detail later), and in integral calculus he defined integration by the traditional method of exhaustion, or, to use a more modern term, as "Riemann sums," *i.e.* a converging series of rectangles under the curve.[12] He also adopted FOURIER's handy notation for definite integral, with the boundaries at the top and bottom of the integral sign

$$\int_b^a f(x)\,dx$$

rather than EULER's, where the boundaries were written after the dx in square brackets:[13]

$$\int f(x)\,dx \begin{bmatrix} x = b \\ x = a \end{bmatrix}$$

CAUCHY's elegant presentation appeared at first sight to be a definitive formulation of calculus. Unfortunately, even his extreme "rigor" did not suffice to ensure perfection. Further development led to inconsistencies, or, at least, inconsistencies were sooner or later spotted. The most famous of these concerned one of the central topics of his work: the convergence of series.

Cauchy's Mistake

Series, convergent or divergent, are necessary to determine limiting processes, and in his *Cours d'analyse*, CAUCHY stated the conditions for their convergence and divergence.[14] Unfortunately, this basic topic caused a major problem in CAUCHY's analysis, and what is generally called "CAUCHY's mistake" appears in his very first theorem on the limit of series of continuous functions. The theorem considers the series (1) $u_0, u_1, u_2, \ldots, u_n, u_{n+1}$, and says:

> When the different terms of the series (1) are functions of the same variable x, continuous with respect to that variable in the vicinity of

[11] CAUCHY, *Oeuvres*, Series 2, Vol. 3, p. 65.

[12] CAUCHY, *Oeuvres*, Series 2, Vol. 4 (1899), pp. 122–127. On CAUCHY's contribution to integral calculus see PHILIP E. B. JOURDAIN, "The Origin of CAUCHY's Conceptions of Definite Integral and of the Continuity of a Function," *Isis*, 1 (1913–1914): 661–703; NICOLAS BOURBAKI, *Éléments d'histoire des mathématiques* (Paris: Hermann, 1960), pp. 218–219; BOTTAZZINI, *The Higher Calculus*, Chapter 4.

[13] See BOURBAKI, *ibid.*

[14] CAUCHY, *Oeuvres*, Series 2, Vol. 3, pp. 114–115.

a particular value for which the series is convergent, the sum s of the series is also, in the vicinity of a particular value, a continuous function of x.[15]

This theorem is wrong, since it is possible to produce examples of series of functions that are continuous in a certain vicinity but have a limiting function which is discontinuous. What is striking is that although such examples were known long before he published his theorem, CAUCHY actually proved (*sic*) his theorem.[16]

The mistake in CAUCHY's proof has been studied and analyzed by many historians, and I do not intend to discuss it further.[17] I shall concentrate on the way CAUCHY and his contemporaries reacted to the existence of counter-examples to his theorem. CAUCHY's reaction, in particular, demonstrates how rigor not only leaves the problem unsolved but can even retard mathematical research. For this purpose let us go back a few years to JOSEPH FOURIER (1768–1830), who formulated a number of counter-examples to CAUCHY's theory.

FOURIER was a mathematician, an Egyptologist and an administrator.[18] In 1798 he accompanied NAPOLEON on his expedition to Egypt, and throughout his career made notable contributions to mathematics and mathematical physics, the most original and best-known being the expansion of functions into trigonometric series.

In 1807 FOURIER submitted to the *Institut de France* a long paper on heat diffusion between disjoint masses and in special continuous bodies.[19] The paper was not published at the time but in 1812, when a contest on the diffusion of heat was announced by the *Institut*, FOURIER submitted a revised version, and won. The paper, however, had to wait another few years for publication, as part of *Théorie analytique de la chaleur* (1822).[20]

FOURIER's *Théorie analytique de la chaleur* has particular relevance where rigor in mathematics is concerned. According to GIORGIO ISRAEL, this work opened a new era in the history of mathematical rigor by establishing a close

[15] CAUCHY, *Oeuvres*, Series 2, Vol. **3**, pp. 115 and 120.

[16] My presentation of the steps of CAUCHY's proof is taken from GRATTAN-GUINNESS, *ibid.*, pp. 78–79. *Cf.* SPALT, *Vom Mythos der Mathematischen Vernunft*, §. 2.2.

[17] For a detailed presentation of CAUCHY's mistakes see ENRICO GIUSTI, "Gli 'errori' di CAUCHY." On p. 26, note 6, GIUSTI lists a series of works that have discussed the subject. CAUCHY's mistake in its broad philosophical context is also discussed by SPALT, in *Vom Mythos der Mathematischen Vernunft*. See also PIERRE DUGAC, *Sur la théorie des séries au XIX^e Siècle* (Paris: Centre National de la Recherche Scientifique, Centre de Documentation Sciences Humaines, 1978).

[18] For a biography of FOURIER see JOHN HERIVEL, *Joseph Fourier: The Man and the Physicist* (Oxford: Clarendon Press, 1975).

[19] A critical edition of the paper was published in GRATTAN-GUINNESS, *Joseph Fourier*.

[20] *Oeuvres de Fourier*, Vol. **1**, §. 178.

link between mathematical analysis and the (experimental) study of nature (a link influenced by the new relation between science and techniques that emerged in the wake of the French Revolution).[21] CAUCHY, according to ISRAEL, moved along the same path, by emphasizing the numerical calculability of analysis.

ISRAEL claims that eighteenth century rigor was not the predecessor of the axiomatic movement that arose at the end of the century. I myself disagree, though at this stage I do not propose to argue against this claim; my opinion will become clear later in this work when I argue that PEANO was a rigorist, and at the same time one of the pioneers of axiomatics. I would like, however, to point out a more specific aspect of FOURIER's work, always related to rigor, and to show that it actually contradicted CAUCHY's rigor. In his paper of 1807, FOURIER treated at length the following function:[22]

$$\cos x - \tfrac{1}{3}\cos 3x + \tfrac{1}{5}\cos 5x - \ldots .$$

He showed that as the number of terms tended to infinity the function took the constant value of $\frac{1}{4}\pi$ over the open interval $(-\frac{1}{2}\pi, +\frac{1}{2}\pi)$, and of $-\frac{1}{4}\pi$ over the open interval $(\frac{1}{2}\pi, +\frac{3\pi}{2})$; at $\frac{1}{2}\pi$ and $-\frac{1}{2}\pi$, it took the value zero.[23] This is an example of a convergent series of continuous functions which tend to a discontinuous function.

Although FOURIER's result was published a year later than CAUCHY's *Cours d'analyse*, one may assume that FOURIER's analysis, including this cardinal counter-example, was well known to CAUCHY. Was CAUCHY's mistake just a small, correctable error, or did it lay CAUCHY's work in general open to question?[24]

Admittedly, contemporary mathematicians seem to have regarded the mistake as relatively unimportant. FOURIER himself at first did not present it as a counter-example to CAUCHY's work. In his published version of 1822, when discussing the continuity of the limiting function, he even contradicted himself: at first he claimed that the limiting function was discontinuous, but in the following section he said it was continuous. In the first case he discussed the series by taking y as the abscissa: looking at:[25]

$$\frac{\pi}{4} = \cos y - \tfrac{1}{3}\cos 3y + \tfrac{1}{5}\cos 5y - \tfrac{1}{7}\cos 7y$$
$$+ \tfrac{1}{9}\cos 9y - \tfrac{1}{11}\cos 11y + \ldots .$$

[21] ISRAEL, "'Rigor' and 'Axiomatics' in Modern Mathematics." *Cf.* BOLDRIGHINI & MARCHETTI, "Lo sviluppo della matematica alla fine del secolo XIX," (see Chapter 1).

[22] GRATTAN-GUINNESS, *Joseph Fourier*, Chapter 7.

[23] *ibid.*, p. 146.

[24] For an attempt to classify types of experimental errors see GIORA HON, "Towards a Typology of Experimental Errors: An Epistemological View," *Studies in History and Philosophy of Science* (1989), **20**: 469–504. The same typology could also be applied, or at least adapted, to mathematics.

[25] *Oeuvres de Fourier*, Vol. **1**, §. 177. *Cf.* FREEMAN's translation, p. 144.

Fourier commented:

> The second member is a function of y that does not change its value when the variable y is given a value between $-\pi/2$ and $+\pi/2$. It is easy to prove that this series always converges; in other words, when one substitutes y by any number and goes on calculating coefficients, one approaches more and more a fixed value; so that the difference between this fixed value and the value of the sum becomes smaller than any value given. Without stopping at this demonstration which the reader can find for himself, we would like to point out that the fixed value which we approach continually is $\pi/4$, if y is given a value between 0 and $\pi/2$, but is $-\pi/4$ if y is between $\pi/2$ and $3\pi/2$. In fact, in the second interval, every term changes its sign. In general, the limit of the series is alternatively positive and negative; as for the rest, the convergence is not sufficiently rapid to produce an easy approximation, but is enough for the truth of the equation.

In other words, Fourier's series does not always converge, and according to Cauchy's definition of continuity, the limiting function is discontinuous. But then, in the next section Fourier presents the same function with x as abscissa:[26]

> The equation
>
> $$y = \cos x - \tfrac{1}{3}\cos 3x + \tfrac{1}{5}\cos 5x - \tfrac{1}{7}\cos 7x + \dots .$$
>
> belongs to a line which, having x as abscissa and y as ordinate, is composed of separate straight lines, each of which is parallel to the x-axis and equal to half the circumference. These parallel lines are located alternately above and below the axis, with a distance $\frac{\pi}{4}$ between any two, and are *joined by perpendicular lines* which themselves make part of the line. In order to get an idea of the exact nature of this line, one should assume that the number of terms of the function
>
> $$\cos x - \tfrac{1}{3}\cos 3x + \tfrac{1}{5}\cos 5x - \dots$$
>
> receives first of all a determined value. In the latter case, the equation
>
> $$y = \cos x - \tfrac{1}{3}\cos 3x + \tfrac{1}{5}\cos 5x \dots$$
>
> belongs to a curve passing alternatively above and below the axis, and cuts it each time that the abscissa x becomes equal to one of the quantities
>
> $$\pm \tfrac{\pi}{2}, \pm \tfrac{3\pi}{2}, \pm \tfrac{5\pi}{2}, \dots .$$
>
> As the number of the terms of the equation grows, the curve tends to be confused with the preceding lines.

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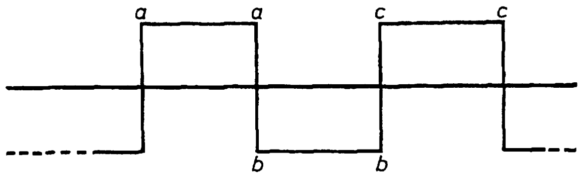
26 *Oeuvres de Fourier*, Vol. 1, §. 178. *Cf.* Freeman's translation, p. 144.

(FOURIER does not draw the function, but a diagram of it is presented by the modern editors of the paper of 1807.)[27] Here we have the same functions rotated through 90 degrees, converging to a *continuous function*, in contradiction to the previous assertion that they converged to a discontinuous one.



Fourier's function.

Why did FOURIER present a function first with the variable y and then with the variable x? And how did he get a discontinuous function (in the first instance) from a continuous one (in the second instance) by a mere rotation of the function by 90 degrees? One thing is certain: FOURIER makes no mention of CAUCHY's definition of continuity, despite the fact that his manuscripts seem to show that as early as 1809 he used the term "discontinuous" in a modern sense.[28] Perhaps FOURIER considered "CAUCHY's mistake" not relevant to his topic (the diffusion of heat); perhaps he did not mention it out of respect for CAUCHY, or perhaps he had still not made up his mind about continuity and was not certain whether there was a mistake at all. Or perhaps FOURIER simply did not notice the mistake. Whatever the reason, FOURIER ignored CAUCHY's mistake.

The mistake was, however, noticed by other mathematicians. In 1826 NIELS HENRIK ABEL (1802–1829) wrote a short paper concerning the series[29]

$$1 + m.x/1 + m.(m-1).x^2/1.2 + m.(m-1).(m-2).x^3/1.2.3 + \dots,$$

and in a footnote argued that there are exceptions to CAUCHY's theorem, *e.g.* the series

$$\sin\phi - \tfrac{1}{2}\sin 2\phi + \tfrac{1}{3}\sin 3\phi - \dots.$$

Three years later, in 1829, LEJEUNE DIRICHLET (1805–1859) wrote a famous article on FOURIER's series, criticizing CAUCHY and arguing that convergent series of continuous functions can converge to discontinuous functions.[30]

[27] GRATTAN-GUINNESS, *Joseph Fourier*, p. 158. Emphasis in the quotation is mine.

[28] See IMRE LAKATOS, "Cauchy and the Continuum," pp. 48–49, note 3.

[29] N. H. ABEL, "Untersuchungen über die Reihe ...," *Journal für die reine und angewandte Mathematik* **1** (1826): 311–339, p. 316.

[30] LEJEUNE DIRICHLET, "Sur la convergence des séries trigonométriques qui servent à représenter une fonction arbitraire entre des limites données," *Journal für die reine und angewandte Mathematik* **4** (1829): 157–169.

Finally, in 1848, DIRICHLET's pupil, PHILIP SEIDEL (1821–1896), corrected CAUCHY's theorem by proving the following: If one has a convergent series of continuous functions that express a discontinuous function of a quantity x, then one must be able to assign, in the immediate vicinity of the point where the function jumps, values of x for which the series converges *arbitrarily slowly.*[31] SEIDEL introduced the concept of uniform convergence.

What is amazing here is CAUCHY's attitude. He totally disregarded FOURIER's counter-example and did not admit having made a mistake: not only did he "prove" his theorem, but he repeated it in a paper read to the *Académie des Sciences* as late as 1853.[32]

I have mentioned "CAUCHY's mistake" as an example of a flaw in his presentation. Contemporary mathematicians, despite their plea for rigor, considered CAUCHY's mistake to be little more than a minor error, and disregarded it.[33] Some modern historians, on the other hand, hold different views, and consider the mistake important and symptomatic. Among the many attempts to understand the context of CAUCHY's mistake, one rather radical interpretation of it is worth considering. It was expressed by the philosopher of science, IMRE LAKATOS, who argued that CAUCHY's mistake was a major mathematical error.[34]

Error and Rigor

At the International Colloquium on logic held in 1966 in Hanover, LAKATOS delivered an important paper on CAUCHY's continuum and its relation to non-standard analysis. Non-standard analysis is a very interesting modern attempt, mainly by the mathematician, ABRAHAM ROBINSON (1918–1974), to define infinitesimals rigorously as part of a "non-standard universe".[35] In his paper, LAKATOS *inter alia* assessed the importance of CAUCHY's mistake for the history and philosophy of science. LAKATOS argued that there were too many peculiar facts surrounding this mistake, and indicated the dangers in the existence of rigor.[36]

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[31] PHILIP L. SEIDEL, "Note über eine Eigenschaft der Reihen, welche discontinuirliche Functionen darstellen," in Ostwald's Klassiker N. 116, edited by HEINRICH LIEBMANN (Leipzig: Engelmann 1900): 35–45, p. 37 (first published in *Abhandl. der Math. Phys. Klasse der Kgl. Bayerischen Akademie der Wissenschaften* 5 (1847): 381–394).

[32] See LAKATOS, "Cauchy and the Continuum," p. 47.

[33] This view is also shared by modern historians. See, for instance, GIUSTI, "Gli "errori" di Cauchy . . . ", pp. 25–26.

[34] On LAKATOS' life, work, and philosophy of mathematics see DAVIS & HERSH, *The Mathematical Experience*, pp. 345–359.

[35] ROBINSON, incidentally, regarded CAUCHY's work as the origin of non-standard analysis: see pp. 259–267 of his *Non-Standard Analysis.* On ROBINSON and his contribution to logic, see ANGUS J. MACINTYRE, "Abraham Robinson, 1918–1974," *Bulletin of the American Mathematical Society* **83** (July 1977): 646–666.

[36] LAKATOS' paper, "Cauchy and the Continuum" (*op. cit.*) was published *post mortem*, see pp. 45–47.

First of all LAKATOS insisted that FOURIER's counter-examples were all known by 1822: "It seems that Cauchy proved a theorem which many people, including himself, knew to be false or at least problematic. Abel's footnote to the effect that Cauchy's theorem 'suffers exceptions' only put into print part of the 'folklore' of the experts: as he [ABEL] himself says, after giving an example from Fourier's published work, 'it is *well known* that there are many series with similar properties'."[37]

If a counter-example was well known, asks LAKATOS, why was the proof not immediately checked, the hidden lemma not discovered and made explicit, the validity of the proof not restored, and, by incorporating the lemma into the original theorem, a more correct one not formulated? Why did ABEL make no effort to find out what was wrong with the proof, confining himself to quoting CAUCHY's theorem? "Abel, a typical rigorist, was ready to forego difficult terrain rather than risk his standards of rigor."[38] Even DIRICHLET, who must have seen the problem, did not mention it in his celebrated paper on the convergence of the FOURIER series in which he showed some subtle details of how convergent series of continuous functions converge, despite CAUCHY's theorem, to discontinuous functions. It was left to his pupil SEIDEL to solve the problem. LAKATOS asks polemically: "Why this delay of twenty-six years? Today, if one gave CAUCHY's false proof to a bright undergraduate, it would not take him long to put it right ... What inhibited a whole generation of the best minds from solving an easy problem?"[39]

LAKATOS also had his own explanation of CAUCHY's mistake, based on the different mathematical traditions of the day. LAKATOS perhaps attached too much importance to the problem: CAUCHY certainly made a mistake, and not a small one, though probably not a "fatal blemish on the whole of the new 'rigorous' mathematics" (as LAKATOS says); after all, the introduction of uniform convergence by SEIDEL corrected things.[40] Yet LAKATOS' criticism remains of particular interest as far as the history and methodology of mathematics is concerned, because he pointed out the self-destructive pattern of response which rigorism induced. And, in general, there were many other similar flaws in CAUCHY's calculus, clearly indicating that his formulation still needed improvement.[41]

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[37] *ibid.*, p. 46.
[38] *ibid.*
[39] *ibid.*, pp. 46–47.
[40] LAKATOS, *Proofs and Refutations*, p. 131.
[41] "CAUCHY's mistake" was only one example of a flaw in his calculus, further indicating how precarious a solution "rigor" was. There were other, perhaps even more important flaws. For instance, as LAKATOS also noticed, if a convergent series of continuous functions did not converge to a continuous function, then CAUCHY's integral, too, could not exist for all continuous functions ("Cauchy and the Continuum," p. 45). Another instance, cited by EDWARDS in *The Historical Development of the Calculus* (p. 329), concerns the validity of the "bounded monotone sequence property" which was assumed by CAUCHY to be evident on geometrical ground, but not verified. The main problem was the lack of precise definition of real numbers.

The CAUCHY revolution, like the work of MACLAURIN and other mathematicians, was motivated by an attempt to apply Euclidian rigor to calculus, and thereby, in ABEL's words, to dispel the "tremendous obscurity that reigns in analysis."[42] CAUCHY proceeded systematically, step by step, always attempting as far as possible to avoid intuition. But this "strategy" not only did not solve problems — it sometimes even had a paralyzing effect, retarding the progress of mathematics. Mathematicians, however, continued to believe that the problem was soluble through further rigor.[43] Paradoxically, CAUCHY's treatment of a variable approaching a limit was criticized for being still too "intuitive," and affected by the physical concept of motion. Indeed, one could easily ask for more specific and rigorous definitions of some of the expressions which CAUCHY used, such as "approach indefinitely," or "as little as one wishes," or "last ratios of infinitely small increments."

The proposed solution, namely to appeal to rigor in arithmetic rather than in geometry, was offered, already in CAUCHY's day, by BERNARD BOLZANO (1781–1848), who worked independently of and in parallel with CAUCHY; and a full-scale arithmetization of calculus was presented by KARL WEIERSTRASS in the second half of the century.

The Arithmetization of Calculus

BOLZANO was a priest, a mathematician and a philosopher.[44] His wide-ranging and original work cannot be described in a few lines. Always stressing rigor, he independently obtained many results very similar to CAUCHY's.[45] One of the problems that kept BOLZANO busy after 1815 was the old question of division by zero: he believed that a function has no determined value at a point if it reduces to 0/0, but may have a limiting value as this point is approached. BOLZANO indicated that by adopting the limiting value as the

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[42] In his letter to HANSTEEN (1825), in *Oeuvres complètes de Niels Henrik Abel*, edited by L. SYLOW & S. LIE, Vol. 2 (Christiana, 1881), p. 263.

[43] See BOYER, *The History of the Calculus*, p. 284.

[44] For a biography of BOLZANO see EDUARD WINTER, *Bernard Bolzano: ein Lebensbild* (Stuttgart: Frommann, 1969); see also CURT CHRISTIAN (ed.), *Bernard Bolzano: Leben und Wirkung* (Vienna: Österreich. Akademie der Wissenschaften, 1981), dealing in particular with Bolzano's philosophy, in religion and in science. For BOLZANO's work with foundations see VOJTĚCH JARNÍK (ed.), *Bolzano and the Foundations of Mathematical Analysis* (Prague: Society of Czechoslovak Mathematicians and Physicists, 1981).

[45] On BOLZANO's contribution to calculus see, O. STOLZ, "B. BOLZANO's Bedeutung in der Geschichte der Infinitesimalrechnung," *Mathematische Annalen* **18** (1881): 255–279. A more recent thorough discussion of BOLZANO's approach to mathematics is PHILIP KITCHER, "Bolzano's Ideal of Algebraic Analysis," *Studies in History and Philosophy of Science* **6** (1975): 229–269.

meaning of 0/0, the function may be made continuous at this point.[46] This led him, in his *Rein analytischer Beweis* (written in 1817, four years *before* CAUCHY published his *Cours d'analyse*), to give a definition of continuity and derivative very similar to CAUCHY's, namely, $f(x)$ is continuous in an interval if at any x of the interval the difference $f(x+\omega)-f(x)$ can be made as small as one wishes by making ω sufficiently small;[47] it follows that the derivative of $f(x)$, $f'(x)$, is the limit of the ratio $f(x+\Delta x)-f(x)$ divided by Δx where Δx is made sufficiently small. BOLZANO emphasized that dy/dx was not a division of zero by zero, but should rather be interpreted as a symbol for a function.[48]

BOLZANO, however, was also a step ahead of CAUCHY: he seems to have understood the basic weaknesses one finds in CAUCHY's presentation. In 1834, for instance, he produced a specific function which was continuous in an interval but had no derivative at any point of the interval, in contradistinction to CAUCHY's belief that every continuous function is differentiable except at isolated singular points. This important example, unfortunately, did not become widely known, and the credit for producing this function went, as we shall soon see, to WEIERSTRASS.[49]

BOLZANO, more than any other contemporary mathematician, tried to arithmetize calculus. He said: "It is just as clear that it is an insufferable offense against the *right method* to want to derive the truths of *pure* (or general) mathematics (that is, arithmetic, algebra, or analysis) from considerations that belong to a purely *applied* (or special) part of it, namely to geometry."[50] This statement contradicts CAUCHY's declared attempt to render calculus rigorous through geometry. BOLZANO maintained that in science, demonstrations cannot be simple procedures for fabricating evidence, but rather foundations, or "presentations of every objective reason which the truth to be proved has."[51] BOLZANO was little known, and his work was overlooked. The credit for having made calculus as rigorous, as precise and as formal as possible went to WEIERSTRASS (1815–1897) who "rediscovered" many of BOLZANO's results.

WEIERSTRASS published very little, his influence being felt through his teaching in Berlin, and his program became known mainly through the work of students who attended his lectures.[52] One of these was SALVATORE PINCHERLE

[46] BOLZANO, *Funktionenlehre*, pp. 25–26. I have also relied on the *Dictionary of Scientific Biography* **2**: 274, and on BOYER, *The History of Calculus*, pp. 268–269.

[47] BOLZANO, *Rein analytischer Beweis*, pp. 7–8.

[48] BOLZANO, *Paradoxien des Unendlichen*, pp. 66–68.

[49] BOLZANO's function was "rediscovered" in his *Nachlass* only in 1921; see GERHARD KOWALEWSKI, "Über Bolzanos nichtdifferenzierbare stetige Funktion," *Acta Mathematica* **44** (1923): 315–319.

[50] BOLZANO, *Rein analytischer Beweis*, pp. 4–5. Translation from BOTTAZZINI, *The Higher Calculus*, p. 98. BOTTAZZINI's emphasis.

[51] *ibid.*, BOLZANO, p. 5; BOTTAZZINI, p. 98.

[52] For a general presentation of WEIERSTRASS' life and work as well as some notes of mathematicians who attended WEIERSTRASS' lectures see DUGAC, "Eléments d'analyse

(1853–1936), who attended WEIERSTRASS' classes in the academic year 1877–1878 and wrote a detailed presentation of his teacher's work.[53]

WEIERSTRASS' program with regard to rigor consisted of two stages: First, shifting from geometry to arithmetic, thereby continuing to ban intuition from mathematics, and second, defining the concept of number as the basis of arithmetic. As an example of the danger of relying on intuition, WEIERSTRASS cited the contemporary belief (based on intuition) that any continuous curve necessarily possessed a tangent, except perhaps at certain isolated points. From this it would follow that the corresponding function should in general possess a derivative. In 1872 WEIERSTRASS presented a paper at the Royal Academy of Sciences in Berlin in which he showed that a function which was continuous throughout an interval need not have a derivative at any point of the interval. He did this by producing the following example: If x is a real variable a and odd integer and b a positive constant smaller than 1 such that $ab > 1 + \frac{3\pi}{2}$,[54] then

$$f(x) = \sum_{n=0}^{\infty} b^n \cos(a^n x\pi)$$

is a nondifferentiable function.

The proof is relatively simple and runs as follows: Assume x_0 to be a value of x, and m to be any positive integer. There is an integer α_m such that the difference $a^m x_0 - \alpha_m$, labelled $x_m + 1$, is $> -1/2$, but $\leqq 1/2$. Take

$$x' = (\alpha - 1)/a^m,$$

$$x'' = (\alpha + 1)/a^m,$$

so that

$$x' - x_0 = -(1 + x_{m+1})/a^m,$$

$$x'' - x_0 = (1 - x_{m+1})/a^m$$

hence $x' < x_0 < x''$. One can however assume m to be as great as one wishes so that x' and x'' are as near as one wishes to x_0.

Now, look at

$$\frac{f(x') - f(x_0)}{x' - x_0}$$

de Karl Weierstrass." For an overview of WEIERSTRASS' work see POINCARÉ, "L'Oeuvre mathématique de Weierstrass."

[53] SALVATORE PINCHERLE, "Saggio di una introduzione alla teoria delle funzioni analitiche secondo i principii del prof. C. WEIERSTRASS."

[54] "Über continuirliche Functionen eines reellen Arguments, die für keinen Werth des letzteren einen bestimmten Differentialquotienten besitzen," in WEIERSTRASS, *Mathematische Werke*, **2**: 71–74. *Cf.* POINCARÉ, "L'Oeuvre Mathématique de Weierstrass," p. 5.

WEIERSTRASS substituted his function for this and, by means of trigonometrical identities, showed that the quotient could be put in the following form:

$$\frac{f(x') - f(x_0)}{x' - x_0} = (-1)^{\alpha_m}(ab)^m.\eta\left(\frac{2}{3} + \varepsilon\frac{\pi}{ab-1}\right),$$

where η is positive and greater than 1, and ε is between -1 and $+1$. Similarly:

$$\frac{f(x'') - f(x_0)}{x'' - x_0} = -(-1)^{\alpha_m}(ab)^m.\eta_1\left(\frac{2}{3} + \varepsilon_1\frac{\pi}{ab-1}\right),$$

where η_1 is also positive and greater than 1, and ε_1 is also between -1 and $+1$. If one chooses a, b so that

$$ab > 1 + \tfrac{3\pi}{2}, \text{ or } \tfrac{2}{3} > \pi/(ab-1),$$

then

$$\frac{f(x') - f(x_0)}{x' - x_0}, \quad \frac{f(x'') - f(x_0)}{x'' - x_0}$$

have opposite signs and grow infinitely with m. Thus, although the function is continuous at the point x_0, it has neither a finite, nor even an infinite derivate. This, of course, is against intuition.

So much for WEIERSTRASS' nondifferentiable continuous function. To ban intuition, WEIERSTRASS tried to refine the definitions given and the topics expounded by CAUCHY, which he regarded as still too intuitive, and more than anyone before him he made use of symbolism in order to make mathematical relationships as clear and straightforward as possible. WEIERSTRASS believed that rigor could be achieved by establishing the theory of functions upon the concept of number alone, and accordingly he defined the limit and continuity of a function in terms of epsilons and deltas. CAUCHY had said that a function $f(x)$ tends to a limit when $f(x+h) - f(x)$ tends to zero as h tends to zero. WEIERSTRASS improved this definition by saying that given any arbitrarily small number ε, another number δ could be found such that for *all* values of h whose absolute value is smaller than δ, the absolute value of $f(x+h) - f(x)$ is smaller than ε.[55] The definition of continuity of a function follows immediately: $f(x)$ is continuous within certain limits of x, if, taking any value x_0 within these limits and an arbitrarily small positive number ε, it is possible to find an interval about x_0, such that for every value x' taken within these limits, the difference $f(x') - f(x_0)$ in absolute value is less than ε.[56]

Yet, since WEIERSTRASS had used numbers to define limits, he had to know exactly how numbers, both rational and irrational, behave. CAUCHY had defined irrational numbers by means of limit, and if he wanted his formulation to

[55] This definition was recorded in a manuscript by H. A. SCHWARZ: see DUGAC, *Éléments d'analyse de Karl Weierstrass*, pp. 119. *Cf.* OTTO STOLZ, *Vorlesungen über Allgemeine Arithmetik*, Part 1 (Leipzig, 1885), pp. 156–157 and 339 (Note 4).

[56] PINCHERLE, *Saggio*, p. 246.

survive, WEIERSTRASS had to find a definition which was independent of limit.[57] WEIERSTRASS was therefore forced to concentrate on a study of arithmetic and the topology of the real line, particularly with respect to the theory of irrational numbers.

The Definition of Number

We are now approaching the focal point of this study. WEIERSTRASS' solution was to define numbers as aggregates of units having a particular property in common. The number $3\frac{2}{3}$, for instance, was regarded as composed of 3α and 2β, where α is the principle unit and β is an aliquot part, $\frac{1}{3}$ being regarded as another element. An irrational number was said to be composed of an infinite aggregate of elements (rather than the limit of a sequence): The irrational number $\sqrt{2}$ would not be defined as the limit of the sequence 1, 1.4, 1.41, ..., but as the aggregate of 1α, 4β, 1γ, ... where α is the principle unit, and β, γ ... are certain of its aliquot parts.[58] WEIERSTRASS' solution to the problem of defining the limit of a convergent sequence was to consider the sequence itself (which is actually an unordered aggregate) to be the number or limit. Thus the logical error arising from CAUCHY's theory of numbers and limits was corrected.

Another interesting, analogous and more elegant attempt to define number was made by DEDEKIND (1831–1916). DEDEKIND, one of GAUSS' last students in Göttingen, taught first in Zurich, and then at the Polytechnic in Braunschweig.[59] He and WEIERSTRASS differed slightly in the direction of their solutions. Whereas the latter studied the nature of real numbers as an indispensable step in constructing his theory of analytic functions, DEDEKIND concentrated more on the nature of continuity as a way to find the rigorous arithmetical foundation of calculus.[60] In *Stetigkeit und irrationale Zahlen*

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[57] The definition of number has, of course, a long history beginning with the Greeks. For its history, see GERICKE, *Geschichte des Zahlbegriffs*.

[58] See KARL WEIERSTRASS, *Einleitung in die Theorie der analytischen Funktionen. Vorlesung Berlin 1878* (Braunschweig/Wiesbaden: Deutsche Mathematiker-Vereinigung/Vieweg, 1988), Chapter 2. The example is taken from BOYER, *The History of the Calculus*, p. 286. For a detailed presentation see PINCHERLE, *Saggio*, pp. 179–213.

[59] For DEDEKIND's life and work see PIERRE DUGAC, *Richard Dedekind et les fondements des mathématiques* (Paris: Vrin, 1976). On DEDEKIND's contribution to algebra see HERBERT MEHRTENS, "Das Skelett der modernen Algebra. Zur Bildung mathematischer Begriffe bei Richard Dedekind," in CHRISTOPH J. SCRIBA, *Zur Entstehung neuer Denk- und Arbeitsrichtungen in der Naturwissenschaft* (Göttingen: Vandenhoeck & Ruprecht, 1979), pp. 25–43.

[60] On DEDEKIND's work on foundations see R. BUNN, "Developments in the Foundations of Mathematics, 1870–1910," in GRATTAN-GUINNESS, *From the Calculus to Set Theory*: 220–255.

(*Continuity and Irrational Numbers*), published for the first time in 1872, which focuses on the nature of continuity, he wrote:

> The statement is so frequently made that the differential calculus deals with continuous magnitude, and yet an explanation to this continuity is nowhere given; even the most rigorous expositions of the differential calculus do not base their proofs upon continuity but, with more or less consciousness of the fact, they either appeal to biometric notions or those suggested by geometry, or depend upon theorems which are never established in a purely arithmetic manner.[61]

Thus, without recourse to geometrical evidence, he searched for the basis of the differential calculus in the elements of arithmetic, hoping "at the same time to secure a real definition of the essence of continuity."[62]

DEDEKIND produced purely arithmetical definitions both of continuity and irrational number. He said: "If all points of the straight line fall into two classes such that every point of the first class lies to the left of every point of the second class, then there exists one and only one point which produces this division of all points into two classes, this severing of the straight line into two portions."[63]

Any rational number, according to DEDEKIND, produces a resolution of the system of rational numbers into two classes, A_1 and A_2, in such a way that each number a_1 of the class A_1 is smaller than each number a_2 of the second class A_2. He calls this division of the system of rationals a "cut"; it is represented by (A_1, A_2). A rational number is either the maximum of class A_1 or the minimum of class A_2.

But rational numbers are discontinuous and an infinite number of cuts exist that are not produced by rational numbers: "Whenever, then, we have to do with a cut (A_1, A_2) produced by no rational number, we create a new, an *irrational* number α, which we regard as completely defined by this cut (A_1, A_2); we shall say that the number α corresponds to this cut, or that it produces this cut."[64]

DEDEKIND's definition of irrational numbers, like WEIERSTRASS', was independent of geometry and avoided the circularity of defining irrational numbers in terms of limits, and vice versa: The cut determined the real number, and the real number determined the limit. The circle could be said to have been closed: calculus seemed to have been rigorously presented, relying on a concept of number.

But the new definition of number, both by WEIERSTRASS and by DEDEKIND, was far from being trivial. Numbers were now defined in terms of infinite aggregates, the entire behavior of which had to be studied. Clarity had been

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[61] DEDEKIND, *Essays on the Theory of Numbers*, p. 2.

[62] *ibid.*

[63] *ibid.*, p. 11.

[64] *ibid.*, p. 15.

achieved, but at the price of ambiguity on a higher level. The challenge now was far greater, namely to learn how to deal with infinite aggregates, and DEDEKIND's *Was sind und was sollen die Zahlen* (1888) was an important step in this direction. Furthermore, the need to define the concept of number produced new challenges. Mathematicians, first and foremost GEORG CANTOR (1845–1918), began to investigate the nature of aggregates, which led to unexpected results and even greater problems.[65] Thus the need to find rigor in analysis which followed BERKELEY's criticism of NEWTONian calculus, produced the need to investigate the nature of numbers. It is within this context that one should study the work of PEANO.

[65] On CANTOR's work see JOSEPH WARREN DAUBEN, *Georg Cantor: His Mathematics and Philosophy of the Infinite* (Cambridge, Mass.: Harvard University Press, 1979).

Chapter 3: Peano's Quest for Rigor

The previous chapters described a widespread tradition in calculus and in modern mathematics in general, based on the belief that mathematical inconsistencies or errors can be solved by an increase of rigor. It is within this tradition that the origins of PEANO's axioms should be sought, and the present chapter, therefore, will outline his early mathematical investigations, before he formulated his axioms.[1] None of the works I found dealing with this early period of his career emphasizes the fact that PEANO formulated his axioms while doing research in calculus. Traditionally, his work is presented as part and parcel of the mathematical trend that related mathematics to logic, ending with the Logicist program pursued by GOTTLOB FREGE, BERTRAND RUSSELL, and ALFRED NORTH WHITEHEAD (the origins of this program will be presented in detail in the next chapter.)[2] One of the theses of this work is that PEANO was only marginally related to the latter trend, and that although he certainly influenced it, he disagreed with its principles.

Like LAGRANGE, PEANO's quest for rigor emerged while he was teaching (as assistant to the chair of calculus) at the University of Turin; he found significant mistakes in contemporary textbooks and corrected them. He gradually became a mistake-hunter and sought for a method both to correct and avoid mistakes. He assumed that mistakes could be avoided by increasing rigor, and this led him, in the contemporary trend of WEIERSTRASS and DEDEKIND, to investigate the logical foundations of mathematics in the hope of reaching a rigorous and coherent presentation. Before discussing this trend of thought, a brief look at his life and work would be useful.

Giuseppe Peano

PEANO was one of the leading figures in the mathematics of his day. He created a school and influenced many mathematicians; RUSSELL, for instance, acknowledges a great debt to him.[3]

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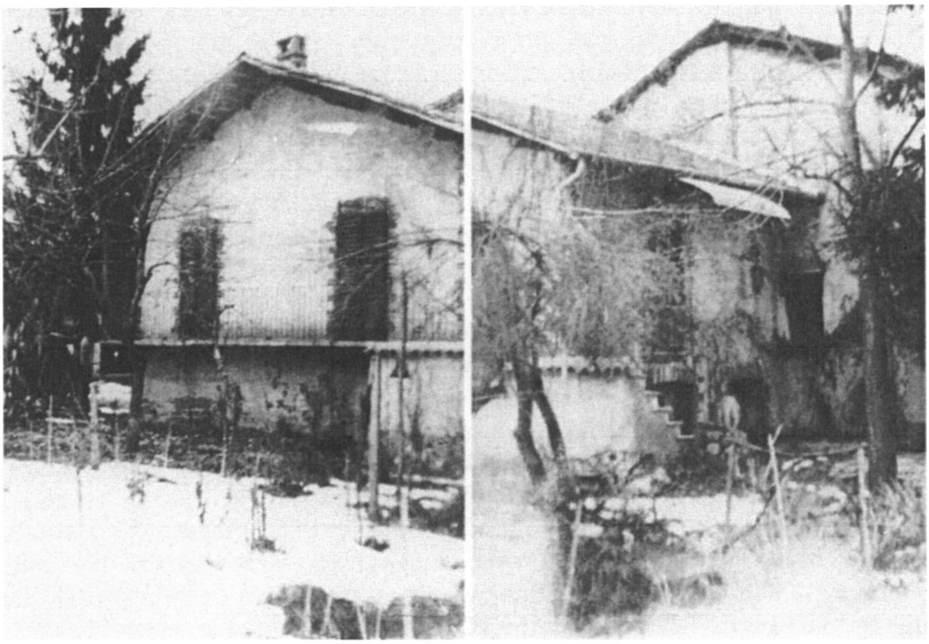
[1] In the discussion in the form of a dialogue concerning the nature of rigor in SPALT, *Vom Mythos der Mathematischen Vernunft*, §. 1.4, it is precisely Alexander, the interlocutor for axiomatics, who argues that rigor is absolute.

[2] See, for instance, MAX BLACK, *The Nature of Mathematics*, p. 8; CARL G. HEMPEL, "On the Nature of Mathematical Truth," in JAMES R. NEWMAN (ed.), *The World of Mathematics*, Vol. 3 (London: George Allen and Unwin, 1960), pp. 1619–1634; PAOLO FREGUGLIA, "La logica matematica di Peano: un'analisi," *Physis* **23** (1981): 325–336, p. 326.

[3] RUSSELL expressed his debt to PEANO on several occasions. See, for instance, chapter 2 of his *The Principles of Mathematics*. Admittedly, PEANO was more acclaimed abroad than in Italy: see EVANDRO AGAZZI's preface to BORGA, FREGUGLIA and PALLADINO, *I contributi fondazionali della scuola di Peano.*



"Tet Galant", the house where GIUSEPPE PEANO was born in Spinetta (Cuneo), Italy. (Courtesy of AUGUSTO PEANO)

PEANO's life and career have been described in KENNEDY's detailed biography.[4] He was born on May 27, 1858 near Cuneo — a small, neat city in southern Piedmont, in the hilly country at the foot of the Maritime Alps. As a youth he moved to Turin, capital of Piedmont and the leading city of the *Risorgimento*, the movement for the unification of Italy under the rule of the House of Savoy. In 1876, PEANO enrolled at the University of Turin, to which he remained attached for the rest of his life, first as a student, then as an assistant, and finally, till his death, as a professor. The University of Turin is in the historic heart of the city, a few steps from the Royal Palace and CAVOUR's office. It was in this small neighborhood, a few years earlier, that the most important decisions of the *Risorgimento* were made; in the same neighborhood PEANO contributed considerably towards turning Italy into a major center of mathematical research.[5]

[4] I also rely on KENNEDY's article on PEANO in the *Dictionary of Scientific Biography*, **10**: 441–444.

[5] On the "Renaissance" of Italian mathematics in this period see UMBERTO BOTTAZZINI, "Mathematics in a Unified Italy," in MEHRTENS, BOS and SCHNEIDER, *Social History of Nineteenth Century Mathematics*, pp. 165–178. According to BOTTAZZINI the unification of Italy contributed considerably to the rebirth of Italian mathematics, mainly by facilitating contacts between Italian mathematicians.

The first step in this successful career came in 1880, soon after he graduated in mathematics, when he was appointed assistant to ENRICO D'OVIDIO, the professor of algebra and analytic geometry. From 1881 to 1889 he was assistant to ANGELO GENOCCHI, incumbent of the chair of infinitesimal calculus, and during this period he wrote no fewer than three textbooks and some twenty articles, most of them on infinitesimal calculus.[6] In 1886 PEANO was appointed professor at the Royal Military Academy of Turin, and continued with his university teaching; in 1890 he became associate professor of infinitesimal calculus at the University of Turin, and, in 1895, full professor. He was active as a professor until his death.

Perhaps the most important period in PEANO's life was roughly a century ago, between 1889 and 1891. In 1889 he published his best known and classic work, written in Latin — *Arithmetices principia, nova methodo exposita* — containing the earliest version of his five famous axioms. This work was written with the aid of a set of mathematical and logical signs, to make the presentation as simple, as clear and as straightforward as possible. PEANO gradually amplified his system of symbols and used it in his writings, hoping that it would be adopted by the mathematical community. Later on he applied the same method, *i.e.* a short, clear presentation based on axioms and using his particular symbols, to various fields of mathematics, in particular to geometry.[7]

In 1891 PEANO founded the *Rivista di matematica*, a journal that published the results of his researches and those of his followers. In 1892 he announced in the *Rivista* his "*Formulario*" project[8] — which became his life work — aimed at collecting all possible mathematical theorems and presenting them in terms of his notations: the *Formulario* came out in five editions.

A great moment in PEANO's career was the First International Congress of Mathematicians in Zurich, in 1897, where he presented his system of logic. Three years later, he and his followers dominated the Paris Philosophical Congress, an event described by RUSSELL in the following words:

> The Congress was a turning point in my intellectual life, because I there met PEANO. I already knew him by name and had seen some of his work, but had not taken the trouble to master his notation. In discussions at the Congress I observed that he was always more precise than anyone else, and that he invariably got the better of any argument upon which he embarked.[9]

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[6] CASSINA, in his *Vita et opera de Giuseppe Peano* (Milan: Ex Schola et Vita, 1932), p. 17, says that PEANO assisted GENOCCHI only during the academic year 1881–1882. But from Chapter 3 of KENNEDY's PEANO it is clear that PEANO assisted GENOCCHI for a much longer period.

[7] On PEANO's symbolism see CASSINA, "Sul 'Formulario Matematico' di Peano."

[8] See *Rivista di Matematica* **2** (1892): 76.

[9] *The Autobiography of Bertrand Russell, 1872–1914*, p. 144. PEANO's influence on RUSSELL is studied by FRANCISCO RODRIGUES CONSUEGRA, "Russell's Logicist

Apart from the expression of high esteem, one can see how a neutral observer noted that precision was a key factor in PEANO's approach.

As KENNEDY relates in detail the second part of PEANO's life, in the first three decades of the present century (he died on April 20, 1932), was devoted more to encouraging an international auxiliary language and editing his *Formulario* than to mathematical research. In addition to his research in mathematics, he was interested in an international auxiliary language, especially to assist scientists, and this interest grew over the years into one of his major concerns. PEANO noted that scientists speaking different languages could understand each other through a vocabulary mainly of Latin origin. He tried to select forms of

PEANO's gravestone in Cuneo, Italy. (Courtesy of AUGUSTO PEANO)

Definitions of Numbers, 1898–1913: Chronology and Significance," *History and Philosophy of Logic* (1987) **8**: 141–169, and *The Mathematical Philosophy of Bertrand Russell*.

words which would be easily understood by anyone whose mother-tongue was either English or a Romance language. This research led him to investigate the algebra of grammar and philology. In 1903 he proposed an "Interlingua," — an international language for scholars, using a *latino sine flexione*, "Latin without grammar." PEANO believed that such a language did not need any grammar and showed how easily grammatical structures may be eliminated. By 1915 this project produced the *Vocabulario commune ad latino-italiano-français-english-deutsch*: it had 14,000 entries, each given the form to be adopted by Interlingua. KENNEDY finds this second period of PEANO's career, if less important in scientific achievement, more interesting in human terms. It is apparently in this period that PEANO showed the gentle and tolerant personality that attracted lifelong disciples, despite the fact that in his mathematics teaching he often exasperated his students by an excessive use of his basic notation to express mathematical concepts.[10] PEANO always maintained a simple lifestyle stemming from his country origins, which characterized his uncomplicated relations with his students and people in general. KENNEDY ends his biography of PEANO by saying: "PEANO may not only be classified as a 19th century mathematician and logician, but because of his originality and influence, must be judged one of the great scientists of that century."[11]

Peano's Early Mathematical Work

There is something a little odd about PEANO's early mathematical work: He produced his axioms quite suddenly, in 1889, having up to then published very little on logic. In fact, his only earlier contribution to mathematical logic was a minor one in the introduction to his book, *Calcolo geometrico secondo l'Ausdehnungslehre di H. Grassmann*, published in 1888 (GRASSMANN's influence on PEANO will be discussed later). This publication, which is chronologically late — number fourteen in the list of PEANO's works — was basically on calculus.[12] Then, a year later, he produced his axioms. This rather surprising fact may throw some light on his general approach to mathematics, and help to clarify the origin of his axioms. The picture becomes clearer if one considers PEANO neither as a logician nor as a mathematician who attempted to investigate the logical foundations of mathematics, but simply as a mathematician who believed that mathematics is perfect, rigor is absolute, and that all mistakes can be corrected simply by keeping mathematics rigorous. How, then, did PEANO arrive at his axioms? What follows is merely a short outline indicating that PEANO's

[10] KENNEDY, *Peano*, p. 175.

[11] *ibid.* For PEANO's lifestyle see ROMANO, "Lo spirito creativo . . . .

[12] In fact, the second volume of PEANO's works, containing his contribution to logic, begins with this work, which is the first chapter of PEANO's *Calcolo geometrico*.

starting point was a quest for rigorous calculus. The details of the formulation of the axioms will be presented in Chapter 5.

PEANO started his mathematical research in the way most assistants do, by working in the same domain as the professors whom he assisted. His first published works [1–4] were in algebra, and followed the work of D'OVIDIO and other teachers on the theory of connectivity of forms. The first work [1], "Costruzione dei connessi (1, 2) e (2, 2)" was published in 1881, when he was 23 years old, and was related to D'OVIDIO's work in geometry. The most interesting article was "Formazione invariantive delle corrispondenze," ([3], 1881), where he made the first systematic use of mathematical induction to prove theorems concerning invariant formations of algebraic correspondences. These early articles, however, tell us little about PEANO's general approach to mathematics.

The first traces of a particular approach in PEANO's work — an attempt to render mathematics rigorous — appeared during his teaching. On May 22, 1882, while replacing GENOCCHI in the classroom, PEANO noticed an error in the commonly used textbook by J. A. SERRET on integral calculus,[13] concerning the definition of area of curved surfaces. Though not the only contemporary mathematician to notice it and propose a solution (see Chapter 5), this was the first in a long series of mistakes that he detected while investigating calculus, which ultimately led him to look for a presentation of mathematics which would avoid such mistakes.

The year 1883 saw the publication of PEANO's first major article in a series of works on calculus: "Sulla integrabilità delle funzioni:" [5]. In this article, using notions of least upper bound and greatest lower bound, PEANO presented for the first time his own definition of integral, and of a related topic, the concept of area. This was one of PEANO's major mathematical contributions, which he developed in later works. The details of PEANO's integral are less relevant here than the fact that his presentation of integral and area are explicitly part of his program to render calculus rigorous (PEANO's definition of integral and area will be discussed later). He begins the article by saying:

> The existence of the integral of functions of one variable is not always proved with the rigor and simplicity desired in such questions. Geometrical considerations are brought in and, in general, the mode of reasoning of the major textbook writers is not satisfactory. The analytic proofs are long and complicated; conditions are introduced in them which are either too restrictive or in part useless. I propose, in the present note, to prove the existence of the integral by introducing a quite simple condition of integrability. The reasoning will be analytic, but every part can be given a geometric interpretation.[14]

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[13] J. A. SERRET, *Cours de calcul différential et intégral, tome second: calcul intégral* (Paris, 1868), pp. 295–298.

[14] PEANO, *Selected Works*, p. 37.

The quest for rigor is self-evident. A similar attitude is expressed at the end of the paper when speaking of areas:

> Many authors demonstrate the existence of the integral using geometrical considerations, but these arguments are not entirely satisfactory. Moreover, they exclude some integrable functions from consideration, and usually consider the area of the figure without defining it. It seems to me that the area, considered as a quantity, of a curvilinear plane figure is precisely one of those geometrical magnitudes which, like the length of the arc of a curve, is often conceived, or thought to be conceived, by our mind quite clearly, but which, before being introduced into analysis, needs to be well defined.[15]

The quest for rigor was accompanied by error hunting: In 1884, in a letter in *Nouvelles annales de mathématiques* [7], PEANO criticized the proof of an intermediate-value theorem for derivatives as given in CAMILLE JORDAN's *Cours d'analyse de l'Ecole Polytechnique* and GUILLAUME JULES HOÜL's *Cours de calcul infinitesimal.* PEANO noted that the mean value of the formula

$$f(x_0 + h) - f(x_0) = hf'(x_0 + \theta h),$$

can be proved very easily without assuming the continuity of the derivative. This correction gave rise to a minor, at times humorous, controversy, among several contemporary mathematicians.[16] Again, one can clearly see PEANO's ability to discover errors in calculus.

Shortly afterwards, PEANO published a whole book based on error hunting [8]. His initial purpose was to edit a course by GENOCCHI, *Calcolo differenziale e principii di calcolo integrale*, but he introduced so many corrections, additions, modifications and annotations that GENOCCHI ultimately disclaimed the book. ALFRED PRINGSHEIM, in the *Enzyclopädie der mathematischen Wissenschaften*, presents a long list of PEANO's important corrections, pointing out errors in texts then in use:[17] a generalization of the mean-value theorem for derivatives; a theorem on uniform continuity of functions of several variables; theorems on the existence and differentiability of implicit functions; an example of a function whose partial derivatives do not commute; conditions for expressing a function of several variables with a TAYLOR's formula; a counter-example to the current theory of minima; and rules for integrating rational functions when roots of the denominator are not known.[18] (Among the topics that PEANO presented in his *Calcolo differenziale* was his definition of integral, already presented in 1883.) As KENNEDY remarks, "PEANO had discovered one of his strong points: the ability to uncover flaws in the teaching of calculus, to single out the difficulties in

[15] *ibid.*, p. 42

[16] KENNEDY, *Peano*, pp. 15–16.

[17] PRINGSHEIM, "Grundlagen der Allgemeinen Funktionenlehre," *passim.*

[18] The translation of the list is from KENNEDY, *Peano*, p. 14.

theorems and definitions (and to manufacture counter-examples simple enough to be understood and convincing), and to make theorems rigorous while simplifying their statements. The calculus text demonstrated all this."[19] This was the first major appearance of PEANO's approach as a mistake-hunter.

PEANO's variations were quite important and the book received much praise: it was, *inter alia*, listed by PRINGSHEIM among the most important mathematical textbooks (including EULER's and CAUCHY's).[20] PEANO's learned attempt to put calculus on a rigorous basis was described by the mathematician ADOLPH MAYER in his introduction to the German translation of the work (1899; in 1922 the book was also translated into Russian), *inter alia*, as "a model of accurate presentation and rigorous argument, whose propitious influence unmistakably entered into nearly all the textbooks of differential and integral calculus that were published thereafter."[21]

PEANO's next works were devoted to specific problems of calculus. In spring 1886 he presented to the Academy of Science in Turin a note on the integrability of first-order differential equations, in which he proved for the first time the existence of a solution of the first-order differential equation $y' = f(x, y)$ on the sole condition that $f(x, y)$ is continuous [9]. PEANO's proof is elementary, and he later generalized it and presented it in his symbolism.[22] A year later, for the first time, he used the method of successive approximations for the solution of linear differential equations,[23] and in the same year he published his second book, *Applicazioni geometriche del calcolo infinitesimale* (*Geometrical Applications of Infinitesimal Calculus* [11]), based on a course he gave at the university in 1885. In 1888 be published his third book, *Calcolo geometrico*, mentioned above. It is in this book that PEANO for the first time introduced some elements of logic: in the first chapter he pointed to the equivalence of the calculus of sets and the calculus of propositions. This chapter shows that PEANO, after having delved into the foundations of calculus and investigated the continuum and its related set theory, felt he had to turn to logic to render calculus rigorous and coherent. However, his exact purpose in writing this chapter is not entirely clear (I will discuss this problem in detail later), nor, at this stage, is his view of the role of logic in mathematics. It is nevertheless a fact that his axioms appeared in 1898.

Two additional works, published after the axioms, should be mentioned, both for their importance and their relationship to the axioms. One is a short

[19] KENNEDY, *Peano*, p. 15.

[20] PRINGSHEIM, "Grundlagen der Allgemeinen Funktionenlehre," p. 2.

[21] ANGELO GENOCCHI, *Differentialrechnung und Grundzüge der Integralrechnung*, herausgegeben von GIUSEPPE PEANO, Übersetzung von G. BOHLMANN und A. SCHEPP (LEIPZIG, 1899), p. III.

[22] See CASSINA, "Sul 'Formulario Matematico' di Peano," pp. 88–91.

[23] There was a controversy over priority for the use of this method with the mathematician EMILE PICARD. See KENNEDY, *Peano*, pp. 17–18.

note which appeared in 1890 in the *Atti* of the Lincean Academy — a correction of the mistake he had found in a lecture of 1882 in SERRET's textbook [23]. The second work, also a short note, is perhaps his most spectacular counterexample: the "PEANO curve", which shows that a continuous curve cannot always be enclosed in an arbitrarily small region [24].

Although this short outline of PEANO's work before he formulated his axioms does not go into detail, even this relatively superficial overview shows quite clearly that PEANO belonged to that tradition of mathematicians who emphasized rigor in the process of mathematical discovery, and that his axioms were an outcome of this approach. In the next chapters we shall see in more detail how PEANO arrived at his axioms. But first we must consider more closely the state of the art in mathematical logic on the eve of the formulation of the axioms.

Chapter 4: Logic and Mathematics: Prelude

The previous chapters presented the mathematical tradition to which, in my view, PEANO belonged, namely the tradition which maintained that the basic problems of calculus could be solved through its rigorous formulation. It is now time to consider another prominent tradition, with which PEANO is generally associated — the tradition relating mathematics to logic.

This is an older and far more complex field. The history of logic and its relation to mathematics goes back to the dawn of classical philosophy, in pre-Socratic times.[1] The basis of logic was elaborated by PLATO and ARISTOTLE, whose contribution, together with that of their followers in the classical age and during the Middle Ages, has been widely expounded.[2] In the Aristotelian tradition, formal logic and mathematics were two completely separate disciplines, and the relationship between logic and mathematics was at most regarded as being concerned with methods of proof. In modern times, a new era in logic was opened by LEIBNIZ, who tried to find a universal language of symbols which would make it possible to convey ideas.[3]

Many mathematicians and philosophers during the two centuries after LEIBNIZ dealt with the subject and proposed a variety of different logical systems. In the nineteenth century, prominent work was done by the mathematicians AUGUSTUS DE MORGAN, GEORGE BOOLE, and CHARLES SANDERS PEIRCE, who investigated the many possible relations between mathematics and logic. Roughly speaking they tried to mathematize logic, creating the so-called "algebraic logic." However, a clear-cut logicist approach, namely a systematic attempt to reduce mathematics to logic, began to emerge only at the end of the century with the work of FREGE, culminating with that of RUSSELL and WHITEHEAD at the beginning of the present century.[4] As PEANO's follower,

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[1] On the complicated relation between logic and mathematics see AGASSI, "Logic and Logic of," and "Presuppositions for Logic."

[2] *Inter alia*, by the following works: FEDERIGO ENRIQUES, *The Historic Development of Logic*, Trans. from Italian into English by JEROME ROSENTHAL (New York: Russel & Russel, 1968); JOSEPH M. BOCHENSKI, *Formale Logik* (Freiburg: Karl Alber, 1956); William KNEALE & MARTHA KNEALE, *The Development of Logic* (Oxford: Clarendon Press, 1962). See also STYAZHKIN, *History of Mathematical Logic from Leibniz to Peano*. A work concentrating more on the nineteenth century is JOURDAIN, "The Development of the Theories of Mathematical Logic and the Principles of Mathematics." On PEANO's place in the history of logic see VAILATI, "La logique mathématique."

[3] At the First International Congress of Mathematicians, held in 1897 in Zurich, PEANO is reported to have mentioned a long series of mathematicians from ARISTOTLE and LEIBNIZ to his own day. See FERDINAND RUDIO (ed.), *Verhandlungen des ersten internationalen Mathematiker-Kongresses*, Zürich, 9–11 August 1897 (Leipzig, 1898), p. 299. *Cf.* the list of sources produced by PEANO in *Formulaire de mathématique Vol. 2, §. 1: logique mathématique* [93], p. 18.

[4] See FRANCISCO RODRÍGUEZ–CONSUEGRA, *The Mathematical Philosophy of Bertrand Russell*, Chapter 1, in particular p. 8.

ALESSANDRO PADOA, says: "*Logic* (without adjectives) is an unfortunate *science*, for while most philosophers prefer to *speak* of it rather than to learn *using* it, most mathematicians prefer to avoid *speaking* about it, or even to avoid *hearing speaking* about it."[5]

PEANO, as I argue in this work, did not really belong to any of the above streams: he regarded himself, of course, as a follower of LEIBNIZ but was unsatisfied with nineteenth century work in algebraic logic, and did not share the contemporary logicist view.[6] In his first work on logic, in the introduction to his book, *Calcolo geometrico*, he acknowledged as his direct predecessors, *inter alia*, WILLIAM ROWAN HAMILTON and the brothers HERMANN and ROBERT GRASSMANN, mathematicians only marginally or vaguely concerned with logic. His own contribution, on the other hand, was acknowledged by RUSSELL as very useful to his work.[7]

A detailed outline of the complicated development of logic and its relation to mathematics before PEANO is beyond the scope of this research.[8] I will therefore consider only briefly some of the major protagonists: LEIBNIZ, whom PEANO mentions as an early predecessor; BOOLE, the leading mathematician in algebraic logic; HAMILTON and the GRASSMANN brothers, who were among PEANO's inspirers; and FREGE, the major representative, though unrecognized at the time, of Logicism. My general claim is that PEANO is probably less related to the work in logic of these prominent mathematicians than is generally thought.

Leibniz's Universal Language

LEIBNIZ believed logic to be the basis of metaphysics.[9] He was inspired by a number of streams of philosophical scholasticism, materialism, Cartesianism,

[5] PADOA, "Il contributo di G. Peano," p. 17.

[6] [14], p. VII. PEANO mentions LEIBNIZ early in the beginning of the preface of his *Calcolo geometrico* [14], pp. iii and x, and cites him often in his work; see, for instance, the introduction to the second volume of his *Formulaire de mathématiques* [88], in which he quotes LEIBNIZ at length; see PEANO, *Opere scelte*, 2: 196–199. KENNEDY believes, however, that although to cite LEIBNIZ gradually became usual and an inspiration for PEANO's *Formulario* project, probably that was not his original inspiration. KENNEDY, *Peano*, p. 46.

[7] See the next chapter for further details.

[8] Such an outline is given, *inter alia*, by BECKER, *Grundlagen der Mathematik in geschichtlicher Entwicklung*.

[9] This at least is the thesis of COUTURAT in *La logique de Leibniz d'après des documents inédits*. There are, of course, many works on LEIBNIZ and his philosophy, for example BERTRAND RUSSELL, *A Critical Exposition of the Philosophy of Leibniz* (London: George Allen & Unwin, 1967, first published in 1900). For the place of logic in LEIBNIZ's thought see HEINRICH SAUER, "Über die logischen Forschungen von Leibniz,"

and by BARUCH SPINOZA (1632–1677), who thought it might be possible to resolve all disputes and conflicts of opinions and achieve human perfection by a process of logic, and believed that problems of ethics could be solved by geometrical-type proofs.[10] (LEIBNIZ's ambitions, however, led him to distance himself from SPINOZA since the latter's work attracted opprobrium). SPINOZA's hope was that philosophers would one day be able to convert their philosophical disputes into simple pencil calculations, and he searched for some kind of alphabet that would be helpful in presenting and communicating thoughts and ideas in a concise form. LEIBNIZ's own aims were almost as utopian: he believed that logic was an important tool for the advancement not only of mathematics and physics, but of all branches of knowledge, including metaphysics, ethics, and even politics and jurisprudence.[11] He wanted this alphabet to be the basis of a universal language providing definitions and an elementary terminology to encompass knowledge: a system of rigorously defined symbols that could be used in logic and other deductive sciences to denote simple elements of the objects under investigation. This alphabet, he hoped, would facilitate the process of thought and help the growth of knowledge. PEANO, especially later in his career, was attempting to reach much the same goal.

As early as 1666, in his *Dissertation on the Combinatorial Art* (*Dissertatio de Arte Combinatoria*, subtitled *Logicae Inventionis semina*, "The Seeds of the Logical Invention"), LEIBNIZ discussed the concept of the "universal characteristic." He wrote:

> Primitive terms will be designated by notes or letters as in an alphabet since from a mastery of them all other things are to be established ... If these are properly and clearly established, then this language will be universal, equally easy and common, and able to be read without any sort of dictionary, and at the same time a basic knowledge of all things will be assimilated.[12]

In other words, a universal alphabet of thought was to be composed of symbols containing maximum information in minimum space, and having an isomorphic correspondence to the objects they denoted. Once obtained, the analysis of the truth of a statement should reduce itself, according to LEIBNIZ, to an analysis of basic concepts, namely to definitions. To give a very simple example: To prove logically the proposition $2 + 2 = 4$, LEIBNIZ proceeded as follows: defining

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in *Gottfried Wilhelm Leibniz: Vorträge der aus Anlaß seines 300. Geburtstages in Hamburg abgehaltenen Wissenschaftlichen Tagung* (Hamburg: Heitmann, 1946): 46–78. The article claims, *inter alia*, that LEIBNIZ's contribution to logic has been somewhat overstressed.

[10] In his *Ethica ordine geometrico demonstrata* (Amsterdam, 1677) SPINOZA attempted a presentation of ethics in EUCLID's style, with definitions, axioms, and theorems.

[11] See, for instance, *Opuscules et fragments inédits*, pp. 153–157.

[12] G. W. LEIBNIZ, *Dissertatio de Arte Combinatoria* (Leipzig, 1666), in *Mathematische Schriften*, 5: 50. The English translation, by DENNIS C. KANE, O.P., is taken from PEANO, *Selected Works*, ed. KENNEDY, p. 153.

i. $2 = 1 + 1$, ii. $3 = 2 + 1$ and iii. $4 = 3 + 1$, then: $2 + 2 = 2 + 2$. But, by definition i. $2 + 2 = 2 + (1 + 1)$, and $2 + 2 = (2 + 1) + 1$. By definition ii. $2 + 2 = 3 + 1$, and by definition iii. $2 + 2 = 4$.[13]

Although LEIBNIZ realized that fundamental ideas in the sciences are far from being simple and clear and that his project to construct a logical calculus was extremely arduous, he persisted with it throughout his life. His various attempts at creating a universal language of science were expressed in several writings and fragments and can roughly be divided into three consecutive stages, starting from the presentation of basic concepts in terms of numbers, developing from this a higher degree of algebraic abstraction.[14] In the first stage, in 1679, LEIBNIZ used prime numbers to symbolize elementary concepts, and products of elementary concepts to represent complex ones.[15] Assume, for instance, that the number 6 corresponds to the concept of "human," 3 to the concept "animal," and 2 to the concept "reasoning." Thus the equation $6 = 2 \cdot 3$ corresponds to the concept "human."[16] LEIBNIZ tried to present this in general syllogistic forms, and encountered difficulties when dealing with complex predicate expressions. He attempted to overcome these difficulties in a second stage, in the years 1685-6, replacing the arithmetical by an algebraic calculus, treating the identity of concepts and the inclusion of one concept in another.[17] Finally, in 1690, LEIBNIZ attempted to extend his algebraic calculus even further but did not succeed in his ambitious aim to reduce all meaningful human thought to a finite number of formal mathematical calculi.[18] Even his attempt to restrict all meaningful mathematics to the narrow frame of formal logic was doomed to failure. But in 1714, two years before his death, he was still expressing the wish to produce an

> artificial form of speech [*manière de Spécieuse Générale*] where all reasoned truths would be reduced to a sort of calculus. It could be at the same time a form of language or of universal writing, but infinitely different from all those created up to that time, since the letters and the words themselves would guide reason, and the errors, with the exception of errors of fact, would be errors in calculation only. It would be very difficult to form or invent this language or characteristic, but very easy to learn it without the aid of any dictionary. In cases of missing data, it would also help to estimate the probability of certain truths. This

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[13] LEIBNIZ, *Mathematische Schriften*, 7: 31.

[14] A detailed presentation with manuscript sources is given by COUTURAT, *La logique de Leibniz*, Chapter 8. For an outline see STYAZHKIN, *History of Mathematical Logic from Leibniz to Peano*, pp. 82–92.

[15] COUTURAT, *ibid.*, pp. 323–344.

[16] In "Calculi universalis elementa," in COUTURAT (ed.), *Opuscules et fragments inédits de Leibniz*, p. 60.

[17] COUTURAT, *La logique de Leibniz*, pp. 344–358.

[18] *ibid.*, pp. 358–387.

prediction would be important for practical decisions in everyday life, when estimations often lead to misjudgements.[19]

LEIBNIZ's ideas, though they did not materialize, inspired many mathematicians and philosophers who created different logical systems for different purposes, more or less related to mathematics (FREGE, PEANO, RUSSELL, and GÖDEL were all LEIBNIZian).[20] Philosophers and mathematicians increasingly asked themselves what the relation between mathematics and logic was and subsequently, in the nineteenth century, algebraic logic was created.

Algebraic Logic: George Boole and his School

In 1847 the relation between mathematics and logic was debated by the philosopher WILLIAM HAMILTON (not to be confused with the mathematician, WILLIAM ROWAN HAMILTON) and the mathematician DE MORGAN. They had done parallel work in logic, but HAMILTON claimed that mathematicians dealt only with form, whereas logicians dealt with essence, hence the former had nothing to contribute to logic. For GEORGE BOOLE (1815–1864) this dispute was the starting point for his work in algebraic logic: In *The Mathematical Analysis of Logic*, published in the same year, he argued against HAMILTON, claiming that logic had to be associated with mathematics rather than with metaphysics, and showing how logical forms could be expressed using algebraic calculus.

The argument went beyond the question of disciplines. Before BOOLE, nobody had envisioned the possible existence of a plurality of algebras (similar to the plurality of geometries) and mathematicians had considered solely the algebra of real numbers. The first step towards the discovery of new algebras was taken by WILLIAM ROWAN HAMILTON (1805–1865), who in 1843 produced the Algebra of "quaternions", an algebra of quadruplets in which products are not commutative (see below). One of BOOLE's contributions (though he himself may not have been fully aware of it) was to consider an algebra which was different from the common algebra of real numbers.

BOOLE's program was to reduce logic to algebraic formulae. He used the letters x, y, z, to represent objects from a subset of things selected from a universal set, designated by the symbol 1. For instance, if 1 represents all Bavarians, x might represent all the inhabitants of Munich, y all the blue-eyed Bavarians, z all the Bavarians above the age of 40. The symbol 0 represents the

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[19] Letter to REMOND DE MONTMORT (1714), in G. W. LEIBNIZ, *Opera philosophica*, ed. J. E. ERDMANN (1840. Reprint. Scientia Aalen, 1959), p. 701. I have used the source quoted by PEANO: *Opere Scelte*, **2**: 197.

[20] PEANO mentions his sources on many occasions; for instance, in *Calcolo geometrico* [14], pp. viii-x. On the surge of LEIBNIZ's influence see JOSEPH AGASSI, "Leibniz's Place in the History of Physics," *Journal of the History of Ideas* (1969) **30**: 331–344.

empty set. $x + y$ represents the union of the subset x and y, *i.e.* the set made up of all elements x or y. (In spoken language, "or" can have both an inclusive and an exclusive meaning: "either ... or ... and possibly both," and "either ... or ... but not both". BOOLE chose the exclusive sense, and so did not allow the symbolism $x + y$ unless the sets x, y were mutually exclusive.) xy represents a successive selection, and the sign $=$ represents the relation of identity. Given these definitions, the five fundamental laws of algebra — associativity and commutativity for sum and product, and distributivity — are valid for Boolean algebra. (Not all rules of ordinary algebra are however valid under the above conditions: in particular $xx = x^2 = x$ in Boolean algebra is not acceptable in ordinary algebra.)

BOOLE obtained all these results despite having no formal training in mathematics and no university degree. In the wake of his achievements he was appointed in 1849 professor of mathematics at Queen's College, County Cork.[21] He extended his work in his classic *The Laws of Thought* (1854), in which he sought above all to understand the method of logic mainly as the basis of a more general method in the mathematical theory of probability, which would make it possible from the given probabilities of any system of events to determine the consequent probability of another logically connected event.[22] This implied a whole program:

> To investigate the fundamental laws of those operations of the mind by which reasoning is performed; to give expression to them in the symbolical language of a Calculus, and upon this foundation to establish the science of Logic and construct its method; to make that method itself the basis of a general method for the application of the mathematical doctrine of Probabilities; and, finally, to collect from the various elements of truth brought to view in the course of these inquiries some probable intimations concerning the nature and constitution of the human mind.[23]

With regard to the relation between mathematics and logic, BOOLE says:

> There is not only a close analogy between the operations of the mind in general reasoning and its operations in the particular science of Algebra, but there is to a considerable extent an exact agreement in the laws by which the two classes of operations are conducted. Of course the laws must in both cases be determined independently; any formal agreement between them can only be established *a posteriori* by actual comparison.

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[21] For a biography of BOOLE, see DESMOND MACHALE, *George Boole: His Life and Work* (Dublin: Boole Press, 1985).

[22] The full title is: *An Investigation of the Laws of Thought on Which are Founded the Mathematical Theories of Logic and Probabilities* (Reprint. New York: Dover, 1958).

[23] *ibid.*, p. 1.

> To borrow the notation of the science of Number, and then assume that its new application the laws by which its use is governed will remain unchanged, would be mere hypothesis.[24]

The relation between logic and BOOLEan algebra was thus in some way an analogy.[25]

The logical calculus of propositions has a certain analogy to that of classes, with implication in the former corresponding to inclusion in the latter. This is fully justified by TARSKI's definition of the content class of a proposition: the content class of a proposition is the class of all and only those propositions that follow from it. Thus, for propositions p, q, r: if p implies q and q implies r, then p implies r. For classes A, B, C, the dual statement is that if A is contained in B and B is contained in C, then A is contained in C. In *The Laws of Thought*, BOOLE was aware of this duality but did not always make clear which interpretation of his symbolic calculus he was using, because he employed symbols less manageable than the ones in use today (in particular he preferred exclusive disjunction whereas we prefer disjunction).

BOOLE's work in algebraic logic was continued by a series of mathematicians more or less contemporary with PEANO. One of them was WILLIAM STANLEY JEVONS (1835–1882), the British economist and logician, who improved on BOOLE in some important details: he redefined, for instance, the symbol " + " from BOOLE's mutually exclusive "either or," to mean "either one, or the other, or both," so that it was true to say $x + x = x$.[26] His Philosophy was presented in his *Principles of Science* (1873), in which he also expressed his view on the relation between logic and mathematics, in particular on the relation between number and measurement. "Number" he said, "is logical in its origin, and quantity is but a development of number."[27] At the same time, he thought there could be no absolute precision: "All results of the measurement of continuous quantity can be only approximately true. Were this assertion doubted, it could readily be proved by direct experience."[28] JEVONS was speaking here of measurements in empirical science, but the spirit expressed differs from the spirit of rigor expressed by PEANO.

JEVONS' interpretation of " + " was also given by the American logician and engineer, CHARLES SANDERS PEIRCE (1839–1914).[29] PEIRCE extended BOOLE's

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[24] *ibid.*, p. 6.

[25] As RODRÌGUEZ–CONSUEGRA points out, BOOLE has been interpreted differently as far as the relation between mathematics and logic is concerned: at times he was thought to believe that mathematics is part of logic, and at times vice versa: *The Mathematical Philosophy of Bertrand Russell*, p. 8.

[26] See "Pure Logic or the Logic of Quality Apart From Quantity" (1864), in JEVONS, *Pure Logic and Other Minor Works*, pp. 1–77, see p. 25.

[27] P. 274.

[28] *ibid.*, p. 357.

[29] C. S. PEIRCE, "On an improvement in Boole's Calculus of Logic," (1867) in *Collected Papers of Charles Sanders Peirce* 3: 3–15. See pp. 3–4.

work and made a vast contribution. In 1870 he introduced a new abstraction called "inclusion in," (or "being as small as") which he denoted by the symbol " $-\!\!<$ " (a variation of the sign " $\leqq$ ", which PEIRCE did not like since it was composed of two relations of different kinds).[30] The logical meaning of "$a -\!\!< b$" is "every a is b", or "if a then b," and it links the idea of logical dependence and the idea of the sequence of a quantity. In general, PEIRCE tried to extend ordinary logic of classes of objects brought together by the relation of similarity, to a logic of relatives, treating "systems" in which objects had any kind of relation.[31] PEIRCE treated multitudes — considering, *inter alia*, systems within successive systems — from the smallest multitude (zero) to an infinite one. He regarded the transition to continuity as a matter of supreme importance for the theory of scientific method and also said it was not very complicated.[32]

PEIRCE influenced ERNST SCHRÖDER (1841–1902), who perfected BOOLE's method for reducing a logical expression to a normal form and introduced the term *Logikkalkül*.[33] Unlike BOOLE, SCHRÖDER placed at the foundation of the calculus of classes not the relation of equality, but the relation of set inclusion. The sign " + " in SCHRÖDER's calculus, as in JEVONS and PEIRCE, indicates the union of classes without exclusion of their common elements.[34]

If one compares all these contributions to that of PEANO, the first impression is of an essential difference. The work done in the wake of BOOLE ended up in many treatises dealing at length with problems of logic and trying, in most cases, to "mathematize" it. PEANO was not only dissatisfied with these works, but was aiming at something different: he tried to apply logical symbolism as much as possible in his mathematical work. Who, then, were PEANO's real forerunners?

Peano's Forerunners

In his earliest work in logic, PEANO mentions WILLIAM ROWAN HAMILTON and the GRASSMANN brothers as being among his forerunners.[35]

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30 "Description of a Notation for the Logic of Relatives, Resulting from an Amplification of the Conceptions of Boole's Calculus of Logic," *ibid.*, pp. 27–98. See p. 28.

31 See "The Logic of Relations," *ibid.*, pp. 195–209, 288–345. I rely in the article on PEIRCE in the *Dictionary of Scientific Biography*, **10**: 485.

32 *ibid.*, p. 401.

33 SCHRÖDER wrote more than forty mathematical works. His major one as far as logic is concerned is *Vorlesungen über die Algebra der Logik* (*Exakte Logik*), 3 vols. (Leipzig, 1890–1905, second edition, New York: Chelsea, 1966).

34 *ibid.* See, in particular, Vol. **1**, Dritte Vorlesung.

35 [14], p. vii.

It seems at first a little surprising that PEANO should mention HAMILTON, since the latter's work was chiefly concerned with complex numbers and not with mathematical logic. HAMILTON considered the presentation $i=\sqrt{-1}$ meaningless, and consequently was not comfortable with an expression such as $a+ib$, composed of a real number and an "imaginary" one. Instead of the algebra of complex numbers he proposed, in 1833, an algebra for handling couples of numbers (a, b) obeying the same rules.[36] This algebra had applications beyond complex numbers, for instance in the geometry of the plane. The idea of putting arithmetics and geometry on the same level originated with KANT, who considered both as synthetic, *a priori* truths. HAMILTON endeavored to produce a "science of pure time" (*i.e.* algebra), as a parallel to a "science of pure space" (*i.e.* geometry).

In 1843, after much effort, HAMILTON discovered the "quaternions," namely numbers of the form $a+bi+cj+dk$, describing a complex number in three-dimensional space, in such a way that $i^2+j^2+k^2=ijk=-1$, and $ij=k$, $ji=-k$, $jk=i=-kj$ and $ki=j=-ik$ (the algebra of the quaternions is non-commutative).[37] PEANO says: "The analogy between the calculus of logic and that of the quaternions lies in the fact that the symbols of each science satisfy laws which are special and analogous to, although not identical with, those of ordinary algebra."[38] Perhaps PEANO perceived the importance of HAMILTON's work in the direction of a new algebra; he may have been looking for an *analogy* between the various algebraic systems and logic. According to THOMAS L. HANKINS, one of HAMILTON's aims was "an attempt to place the algebra of real numbers on a more secure logical foundation."[39] HAMILTON, says HANKINS, believed that

> negative and imaginary numbers had no *meaning* in ordinary algebra, and that until they could be adequately defined, algebra rested on a very shaky base. In the middle section of the "Essay" Hamilton attempted to

[36] Published in 1837 in "Theory of Conjugate Functions, or Algebraic Couples: With a Preliminary and Elementary Essay on Algebra as the Science of Pure Time," *The Mathematical Papers of Sir William Rowan Hamilton.* Vol. 3: *Algebra*, edited by H. HALBERSTAM & R. E. INGRAM (Cambridge: Cambridge Univ. Press, 1967), pp. 3–100.

[37] HAMILTON expounded this algebra in his *Elements of Quaternions*, published posthumously in 1866. GRASSMANN's *Ausdehnungslehre* was an extension of HAMILTON's work from the three-dimensional space to the *n*-dimensional one.

[38] PEANO, *Opere Scelte*, 2: 100. A translation of the sentence is given in PEANO, *Selected Works* (ed. KENNEDY), pp. 153–154. I have slightly modified KENNEDY's translation.

[39] THOMAS L. HANKINS, "Algebra as Pure Time: William Rowan Hamilton and the Foundations of Algebra," in PETER K. MACHAMER & ROBERT G. TURNBULL, *Motion and Time, Space and Matter* (Columbus; Ohio: Ohio State Univ. Press, 1976), pp. 327–359. Quotation from p. 328. HANKIN's work is based, *inter alia*, on unpublished manuscripts.

state the properties of the real number system and to define negatives by the use of steps in the ordered relations of time. Thus he attempted to base algebra on the *ordinal* character of the real numbers.[40]

PEANO could have been interested in this aspect of HAMILTON's work.

The influence of the GRASSMANN brothers is more evident, though at first not entirely clear. HERMANN GÜNTHER GRASSMANN (1809–1877), mathematician, physicist and linguist from Stettin, Germany, is best known for his *Ausdehnungslehre*, the "theory of extension," or "theory of extensive magnitudes."[41] This general calculus of vectors in any number of dimensions appeared at first in 1844, under the title *Die lineale Ausdehnungslehre*,[42] and was later presented by Peano in his *Calcolo geometrico*. GRASSMANN believed that there are "formal" sciences and "real" sciences. Logic and pure mathematics are formal sciences, whereas geometry is a real one, *i.e.* an application of a formal science to reality. The purpose of his *Ausdehnungslehre* was to provide this formal branch of mathematics.

In 1861 GRASSMANN also published his *Lehrbuch der Arithmetik*, intending to give "the first rigorous scientific presentation of this discipline," and his method, "the only possible method for a rigorous, consequential and natural treatment of this science."[43] He emphasized the importance of his presentation for teaching and as a method of discovery. GRASSMANN's book begins by defining mathematics in general and such signs as " = ", "(", ")", " + " in particular; he deals with the totality of all integers, positive, negative and zero, introduces recursive definition for addition and multiplication, and on this basis proves ordinary laws by mathematical induction.[44] Although GRASSMANN did not, in fact, present his development in axiomatic form, his formulation does bring us very close to PEANO.

So much for HERMANN GRASSMANN: more about the relation between his *Ausdehnungslehre* and PEANO's work will follow in the next chapter. So far, while we concentrate on logic, the relation still appears vague, because the main drive in PEANO's work was not the development of logic but the quest for rigor.

HERMANN GRASSMANN mentions that his work was carried out together with his brother, ROBERT GRASSMANN (1815–1901). The latter is little known: between 1841 and 1848 he was a mathematics teacher, after which he founded and ran

[40] *ibid.*, pp. 328–329.

[41] For a biography, see A. E. HEATH, "Hermann Grassmann," *The Monist* **27** (1917): 1–21.

[42] It was reprinted in the first volume of GRASSMANN's *Gesammelte Werke*. For the development of GRASSMANN's *Ausdehnungslehre* see CROWE, *A History of Vector Analysis*, Chapter 3. See also NAGEL, "Modern Conceptions of Geometry," pp. 168–174.

[43] Parts of this work included in HERMANN GRASSMANN's *Gesammelte mathematische und physikalische Werke*, Vol. **2**, Part 1, pp. 295–349. Quotation from p. 295.

[44] On GRASSMANN's contribution see HAO WANG, "The Axiomatization of Arithmetic," pp. 146–150.

a publishing house in Stettin.[45] His book, *Die Formenlehre oder Mathematik* (*The Theory of Forms or Mathematics*), written for the lay reader and published in 1872, sketched what he and his brother believed to be the foundations of mathematics.[46] Without going into detail about ROBERT GRASSMANN's theory, its general approach is similar to that of PEANO.[47] The aim was to give a rigorous presentation of the foundations of mathematics:

> This work should be a rigorous scientific presentation of the elements (*Anfangszweige,* basic branches) of mathematics. All the hitherto existing presentations of mathematics rely on logic, without deriving it or constructing it scientifically. Nearly all of them attempted to overcome early difficulties through phrases or faulty considerations and only later proceeded in a rigorous scientific way.[48]

Like PEANO, ROBERT GRASSMANN wanted to avoid this, and in his book attempted to present "laws of rigorous scientific thinking."[49] This, he said, had been the goal of his and his brother's work for many years. PEANO certainly shared HERMANN and ROBERT GRASSMANN's plea for rigor, but probably paid less attention to their proposal to relate mathematics to logic. The pioneer of this approach was, rather, FREGE.

Frege

GOTTLOB FREGE (1848–1925), a trained mathematician, made a monumental contribution to logic. He was professor at the University of Jena, and shared several of PEANO's views. Like PEANO, he found ordinary language insufficient to express ideas; and like PEANO, he objected to intuition in mathematics. He was a particularly vigorous opponent of any definition of concept which contained a reference to intuition.[50]

FREGE's first and most important work, *Begriffsschrift* (translated variously as "conceptual notation," "ideography," or "concept-writing"), came out in

[45] A short biography of ROBERT GRASSMANN is presented by J. E. HOFFMANN at the beginning of the reprint of the latter's *Die Formenlehre oder Mathematik.*

[46] *ibid.*, p. 2.

[47] ROBERT GRASSMANN is also mentioned by SCHRÖDER as having anticipated several of FREGE's results. See ERNST SCHRÖDER's review of FREGE's *Begriffsschrift* in *Historisch-literarische Abteilung der Zeitschrift für Mathematik und Physik,* (1880) **25**, p. 90.

[48] R. GRASSMANN, *Die Formenlehre*, p. 3.

[49] *ibid.*, p. 5

[50] There is a considerable body of literature on FREGE. I cite the works I had at my disposal: JEREMY D. B. WALKER, *A Study of Frege* (Oxford: Blackwell, 1965); MARIO TRINCHERO, *La filosofia dell'aritmetica di Gottlob Frege* (Turin: Giappichelli, 1967); JEAN LARGEAULT, *Logique et philosophie chez Frege* (Paris: Nauwelaerts, 1970); DUMMETT, *The Interpretation of Frege's Philosophy*; BAKER & HACKER, *Frege: Logical Excavations.*

1879, ten years before PEANO published his axioms. It is a booklet of eighty-eight pages, and in the introduction to his translation into English of the work, JEAN VAN HEIJENOORT describes it as "perhaps the most important single work ever written in logic."[51] In this classic work, FREGE makes an attempt to find the true essence of language (in the LEIBNIZian tradition) through a symbolic representation of pure thought. The book's rather complicated sub-title indicates that this new script is to be modelled, through FREGE's particular symbolism, on the language of arithmetic, describing theorems related to the logic of propositions and to the logic of predicates.[52] Here are some instances of his ideography:[53]

"⊢——" (a basic symbol): the assertion sign; it is composed of the judgment stroke, "|", and the content stroke, "—". Thus "⊢——A" means "A is a fact."
"—┬—": the conditional sign; "—┬—A" means "implies B."
⊢┬—: the sign for negation └—B

FREGE showed that the propositional connections "and" and "or" can be stated by means of "no" and "if," and developed a propositional logic on the basis of a small number of axioms.[54] He then presented an early treatment of his notion of function and developed a quantification theory.[55]

Since FREGE's contribution is regarded as fundamental in mathematical logic, why did PEANO not come across it until over ten years after it was published? There are several reasons.

First of all, FREGE was at times atrociously difficult to follow and few people read him (FREGE is still not fully understood and the study of his work goes on). PEANO himself, whose ability to grasp was extraordinary, commented in a review of FREGE: "This book must have cost its author much labor; reading it is also quite tiring."[56] Commenting on FREGE's *Begriffsschrift* in his autobiography, RUSSELL says: "I possessed the book for years before I could make out what it meant. Indeed, I did not understand it until I had myself independently discovered most of what it contained."[57]

Secondly, FREGE's claim that logic is stronger than mathematics met with objections among BOOLE's followers. Indeed, in his review of FREGE's *Begriffsschrift* (1880) SCHRÖDER's main criticism was that the latter had

[51] JEAN VAN HEIJENOORT, *From Frege to Gödel*, p. 1.

[52] The full title is: *Begriffsschrift, eine der arithmetischen nachgebildete Formelsprache des reinen Denkens* (*Conceptual Notation, a Formula Language, Modelled upon that of Arithmetic, for Pure Thought*).

[53] For a detailed illustration, see BAKER & HACKER, *Frege*, Chapter 4 and 5.

[54] FREGE, *Begriffsschrift* §.7.

[55] *ibid.*, §. 9–12.

[56] PEANO was reviewing FREGE's *Grundgesetze der Arithmetik*, [82], p. 127. I have used KENNEDY's translation, from PEANO, p. 75.

[57] *The Autobiography of Bertrand Russell: 1872–1914*, p. 68.

disregarded the work done in previous years by BOOLE and his followers.[58] KENNEDY suggests that since FREGE's *Begriffsschrift* was so roundly condemned in this review probably few people read the book.[59] But FREGE was intentionally doing the opposite of BOOLE's followers. In his reply to SCHRÖDER he wrote:

> My intention was not to represent an abstract logic in formulas, but to express a content through written signs in a more precise and clear way than it is possible to do through words. In fact, what I wanted to create was not a mere *calculus ratiocinator* but a *lingua characteristica* in Leibniz's sense.[60]

The *Begriffsschrift* is the basis of FREGE's *Grundlagen der Arithmetik* (*The Foundations of Arithmetic*, 1884) in which he attempts to define number.[61] In the Introduction he explains:

> The present work will serve to show that even inferences which on the face of it are peculiar to mathematics, such as that from n to $n + 1$, are based on the general laws of logic, and that there is no need of special laws for combinative thought. It is possible, of course, to operate with figures mechanically, just as it is possible to speak like a parrot: but that hardly deserves the name of thought. It only becomes possible at all after the mathematical notation has, as a result of genuine thought, been so developed that it does the thinking for us, so to speak.[62]

FREGE criticizes the current theories of numbers and attempts to show that the concept of number can be defined in purely logical terms. "The concept of number, as we shall be forced to recognize" he said, "has a finer structure than most of the concepts of the other sciences, even although it is still one of the simplest in arithmetic."[63]

FREGE argues that number is a thing connected with an assertion concerning a concept, and he attempts to define number in such a way that its definition should be independent of any concepts other than those already related to the interpretation of his calculus as a logical system. At the core of FREGE's definition lies his use of the concept of one-to-one correspondence. FREGE's definition of number relies on a profound study, which cannot be given in

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[58] *Historisch-literarische Abteilung der Zeitschrift für Mathematik und Physik*, **25** (1880): 81–94. SCHRÖDER also imputes to FREGE excessive pedantry: see p. 90.

[59] KENNEDY, *Peano*, p. 73.

[60] I quote from VAN HEIJENOORT, *From Frege to Gödel*, p. 2.

[61] The full title is *Die Grundlagen der Arithmetik: Eine logisch mathematische Untersuchung über den Begriff der Zahl* (*The Foundations of Arithmetic: A Logico-Mathematical Enquiry into the Concept of Number*). The Austin translation also includes the German text.

[62] *ibid.*, p. iv[e]

[63] *ibid.*

a short outline. I quote the definition in his own words, without further comment:[64]

The expression
"the concept F is equal to the concept G"
is to mean the same as the expression
"there is a relation ϕ which correlates one to one the objects falling under the concept F with the objects falling under the concept G."
We now repeat our original definition:
the Number which belongs to the concept F is the extension of the concept "equal to the concept F"
and add further:
the expression
"n is a Number"
is to mean the same as the expression
"there is a concept such that n is the Number which belongs to it."

We shall see later in detail the difference between FREGE's and PEANO's approach to the concept of number.

FREGE's quest for rigor seems to be even stronger than PEANO's:

> No less essential for mathematics than the refusal of all assistance from the direction of psychology, is the recognition of its close connection with logic . . . In this direction, too, I go, certainly, further than is usual. Most mathematicians rest content, in enquiries of this kind, when they have satisfied their immediate needs . . . Yet it must still be borne in mind that the rigor of the proof remains an illusion, however flawless the chain of deductions, so long as the definitions are justified only as an afterthought, by our failing to come across any contradiction. By these methods we shall, at bottom, never have achieved more than an empirical certainty, and we must really face the possibility that we may still in the end encounter a contradiction which brings the whole edifice down in ruins. For this reason I have felt bound to go back rather further into the general logical foundations of our science than perhaps most mathematicians will consider necessary.[65]

FREGE's aims as one can see, were similar to PEANO's, with the addition that he also tried to investigate the very essence of number. It is partly thanks to PEANO, who considered and quoted his work, that FREGE attained his fame.

Thus far a general description has been given of the main streams that considered the relation between mathematics and logic on the eve of the formulation of PEANO's axioms. The key difference lay in the answer to the

[64] *ibid.*, §. 72, AUSTIN's translation (slightly modified: I have written "there is" for "*there exists*").

[65] *ibid.*, p. ix[e].

question: which is stronger, mathematics or logic? BOOLE's followers and most eighteenth century mathematicians believed mathematics to be stronger; FREGE, and possibly also the GRASSMANN brothers, thought that logic was stronger. PEANO's contribution needs evaluating in relation to this question. Where did he stand? The next chapter will consider PEANO's contribution in greater detail, and Chapter 7 will attempt to answer the question.

Chapter 5: From Rigor to Axiomatics

A consideration of the consecutive stages of PEANO's work illustrates in greater detail how his quest for rigor led him to formulate his axioms. The starting point was his special ability to produce counter-examples and to disprove theorems or mathematical statements which, though intuitively accepted, had not been "rigorously" proved.[1] He proceeded in four stages covering less than a decade:

i) In 1882 he first corrected a mistake in a widely used geometry textbook. A year later, also for the first time, he presented his notion of integral as an alternative to the contemporary notions, which he found either non-rigorous (*i.e.* too geometrical) or analytical and too complicated.

ii) In 1884 he extended this approach with a long series of additions and corrections to the published lectures of his teacher, GENOCCHI. On this occasion, he emphasized the importance of the concept of number as the basis of analysis, possibly hinting that a clearer and more precise definition of it would help to prevent mistakes.

iii) In 1888 he devoted the first chapter of his book, *Calcolo geometrico*, to logic, but with no apparent connection to the rest of the book.

iv) A year later he presented the first formulation of his axioms, and soon after attempted to extend his results to geometry, suggesting that rigor in arithmetic and geometry could be achieved if one considered them as analogous to logic.

1882: Serret's Definition of the Area of a Surface

PEANO's correction of a mistaken definition of the area of a curved surface, contained in the widely used textbook of differential and integral calculus by J. A. SERRET, has no particular importance as a general contribution to mathematics, but it is relevant to the present work as a first step in PEANO's ambitious program to give mathematics a general rigorous presentation. On p. 296 of his book, SERRET gave the following definition of the area of curved surfaces:[2]

> Let a portion of curved surface be bounded by a contour C; we shall call the area of this surface the limit S to which tends the area of an inscribed polyhedral surface formed by triangular faces and bounded by a polygonal contour line Γ with C as limit.

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[1] For a study of PEANO's counter-examples in relation to their context see BOTTAZZINI, "PEANO e la logica dei controesempi."

[2] J. A. SERRET, *Cours de Calcul différentiel et intégral, tome second: calcul intégral* (Paris, 1868), pp. 295–298. "Soit une portion de surface courbe terminée par un contour C; nous nommerons aire de cette surface la limite S vers laquelle tend l'aire d'une surface polyédrale inscrite formée de faces triangulaire et terminée par un contour polygonal Γ ayant pour limite le contour C."

PEANO noticed that this was not applicable to general surfaces and seemed difficult to apply in particular cases. GENOCCHI (with whom PEANO was working as assistant) was already aware of the difficulty, which had been noticed for the first time in 1880 by H. A. SCHWARZ, and communicated by him to GENOCCHI and to CHARLES HERMITE.[3] PEANO published his own correction eight years later: In a note in the *Atti* of the Accademia dei Lincei [23, 1890], he gave a lucid explanation of why the definition is fallacious — much clearer than the explanation given (in the same year) by SCHWARZ.[4]

In his note, PEANO faces the problem historically, and cites ARCHIMEDES' postulates of the length of a convex arc and of the area of a convex surface (PEANO often tackled mathematical problems historically, exhibiting an excellent knowledge of history of mathematics; he points out that in ARCHIMEDES's case, postulates are equivalent to definitions). The length of a convex arc, according to ARCHIMEDES, is the common value of the least upper bound (l.u.b) of the length of the inscribed polygonal curves, and the greatest lower bound (g.l.b.) of the circumscribed ones. Similarly, the area of a convex surface is the common value of the l.u.b. of the areas of the inscribed polyhedral surfaces and the g.l.b. of the circumscribed ones. ARCHIMEDES proved the coincidence of these two limits for the curves and surfaces he studied. PEANO corrects ARCHIMEDES, pointing out that the latter's definition of a curved arc cannot be applied to non-planar lines. It can, however, be generalized by omitting the circumscribed curves. (PEANO remarks, in a footnote, that his corrected definition is simpler than the original one.)

Areas, however, present a bigger problem: the definition given by SERRET (and repeated by other authors) regarding the limit towards which the inscribed polyhedral surfaces tend, is not valid; such a polyhedral surface may tend, depending on the way it varies, towards any limit which is larger than the area of the surface. CARL GUSTAV HARNACK and HERMITE proposed their own solutions to the problem, but PEANO found both unsatisfactory. HARNACK added the condition that the faces of the polyhedral surface should be triangles whose angles do not tend indefinitely to zero; PEANO points out that this does not solve the problem. HERMITE defined area as the limit of a system of noncontiguous polygons, tangent to the surface; PEANO finds this definition rigorous enough but remarks that it must explicitly consider axes of references.

In his solution PEANO considers areas as bivectors (products of two vectors, a topic that PEANO treated exhaustively in his *Calcolo geometrico* of 1888). PEANO defines the length of an arc as the upper limit of the sum of the magnitudes of the vectors of its parts. The area of a portion of a surface is the upper limit of the sum of the magnitudes of the bivectors of its parts. This early

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[3] The simultaneous discovery of the mistake is described by KENNEDY, *Peano* pp. 9–10.

[4] H. A. SCHWARZ's correction was published in 1881 and 1883 in a textbook by HERMITE and finally enclosed, under the title "Sur la définition erronée de l'aire d'une surface courbe" in SCHWARZ's *Gesammelte mathematische Abhandlungen*, Vol. **2** (Berlin, 1890), pp. 309–311.

example (though published later than the axioms) is clearly part of PEANO's conception of integral, which he implicitly produced in 1883 in his "On the Integrability of Functions" [5].

1883: Peano's Integral

The earliest presentation of PEANO's integral runs as follows. I present only the main stages of the treatment and rely here on KENNEDY's translation. KENNEDY simplified and improved PEANO's proof using more up-to-date knowledge. While the advantage of this presentation is obvious, it has the disadvantage of being ahistorical and, as HANS FISCHER pointed out to me, depriving the reader of the opportunity to savor the subtlety of PEANO's original and his accent on rigor:[5]

Let $y = f(x)$ be a function defined on an interval (a, b) of x. Assume that a and b are finite and that y is bounded on the interval. Let A and B be the l.u.b. and the g.l.b. respectively, of y on the interval. Divide (a, b) into parts of length $h_1, h_2, \dots, h_n$. Let y_s be any value whatever assumed by y as x varies in the interval h_s.

$$\text{Let } u = \sum h_s y_s$$

If, with all h diminishing indefinitely, u tends towards a limit S, the function is said to be integrable on the interval (a, b) and this limit is said to be the value of the integral (PEANO explains in a footnote that by "u tends towards a limit S" he means that, having fixed a quantity ε as small as one wishes, one may determine a σ such that for every partition of (a, b) for which the length h of each interval is less that σ, and for every choice of y_s in the intervals, one always has $|S - u| < \varepsilon$.)

Let p_s and q_s be the l.u.b. and the g.l.b., respectively, of y in the interval h_s. Set $P = \sum h_s p_s$, $Q = \sum h_s q_s$. Since $A \geqq p_s \geqq y_s \geqq q_s \geqq B$, multiplying by h_s and summing, one has $A(b - a) \geqq P \geqq u \geqq Q \geqq B(b - a)$, where P and Q are upper and lower bounds, respectively, for u.

By varying the partition of (a, b), one may vary P and Q. But every value of P is greater than or equal to every value of Q. Let, in fact, $h_1, h_2, \dots, h_n$ and $h'_1, h'_2, \dots, h'_{n'}$ be the length of the respective subintervals of two partitions of (a, b), and consider the partition formed by superimposing these two partitions. Label the length of the subintervals of the new partition $k_1, k_2, \dots, k_m$. Every interval k_α is contained in h_β and in h'_γ, and every h and every h' is equal to the sum of one or more intervals k. Since from the original partition one had

$$P = \sum_{r=1}^{n} h_r P_r, \quad Q' = \sum_{s=1}^{n'} h'_s q'_s$$

[5] PEANO, *Selected Works*, I. The disadvantages of correcting errors in history of science are discussed in AGASSI, *Towards an Historiography of Science*, in particular on pp. 51–54. I am most indebted to HANS FISCHER for his enlightening discussion of the

one has, after appropriate substitutions,

$$P = \sum_{\alpha=1}^{m} k_\alpha p_\beta, \quad Q' = \sum_{\alpha=1}^{m} k_\alpha q'_\gamma, \quad \text{and} \quad P - Q' = \sum_{\alpha} k_\alpha (p_\beta - q'_\gamma) .$$

But p_β is the l.u.b. of y on the interval h_β, which contains k_α, and q'_γ is the g.l.b. of y on the interval h'_γ, which contains k_α. Hence

$$p_\beta > q'_\gamma, \quad P - Q' > 0, \quad \text{and} \quad P > Q', \quad \text{Q.E.D.}$$

Thus the quantities P, which are all finite, have a g.l.b. M and the quantities Q have l.u.b. N, such that

$$P \geqq M \geqq N \geqq Q .$$

PEANO discusses some necessary conditions for integrability and then proves the following theorem:

The function $f(x)$ is integrable on the interval (a, b) if $M = N$, and the common value S is the value of the integral.

The proof runs as follows: let $h_1, h_2, \ldots, h_n$ be any partition whatever of (a, b) such that every h is less than σ, a quantity to be determined. Let $u = \sum h_s y_s$.

Since the g.l.b. of the values P is S, given an arbitrary ε one can make a partition $h'_1, h'_2, \ldots, h'_{n'}$ of (a, b) for which, if we let $P' = \sum h'_s p'_s$, one has $P' - S < \varepsilon$. Now consider the partition of (a, b) derived from the superposition of the preceding partitions, and an interval k_α of this contained in h_β and in h'_γ. One will have $P' = \sum_\alpha k_\alpha p'_\gamma$, $u = \sum_\alpha k_\alpha y_\beta$, and $P' - u = \sum k_\alpha (p'_\gamma - y_\beta)$. PEANO remarks that several of the intervals h_β can each be contained in some intervals h'_γ. For them, $p'_\gamma > y_\beta$, and the corresponding terms in $P' - u$, are positive. The other intervals h_β contain some point of the second division. There are less than n' of them and, since $h_\beta < \sigma$, their total length is less than $n'\sigma$. To these intervals can correspond, in $P' - u$, negative terms, but, seeing $p'_\gamma - y_\beta < A - B$, their sum will be less numerically than $n'\sigma(A - B)$. From this we have

$$P' - u > -n'\sigma(A - B), \quad \text{or} \quad S + \varepsilon + n'\sigma(A - B) > u.$$

Analogously, since S is the least upper bound of the values of Q, one can find a partition $h''_1, h''_2, \ldots, h''_{n''}$ for which, setting $Q'' = \sum h'' q''$, one has $S - Q'' < \varepsilon$. Thus, considering the quantity $u - Q''$, one may show in the same way that

$$u > S - \varepsilon - n''\sigma(A - B).$$

Given an arbitrarily small α, one may in the preceding argument suppose that $\varepsilon < \alpha/2$, and that $n'\sigma(A - B)$ and $n''\sigma(A - B)$ are less than $\alpha/2$, because it is sufficient to take $\sigma < \alpha/[2n'\sigma(A - B)]$ and $\sigma < \alpha/[2n''\sigma(A - B)]$. Then

$$S + \alpha > u > S - \alpha.$$

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subject and regret my inability to take up his suggestion and follow PEANO closely. This work belongs to the history of attitudes to mathematics rather than to the history of mathematics itself.

or, fixing a quantity α as small as one likes, one may determine σ such that for every partition of (a, b) for which each $h < \sigma$, and for any set of values of y_s, we always have $|S - u| < \alpha$, and hence u tends toward the limit S as the h decrease indefinitely, Q.E.D.

From the above theorem PEANO deduces another condition for integrability by proving the following theorem:

The function $f(x)$ is integrable if, given an arbitrarily small ε, one can find a value P and a value Q (corresponding, or not, to the same partition of (a, b)) whose difference is less than ε. The value of the integral lies between these two values.

If, in the above theorem, one makes the unnecessary hypothesis that P and Q correspond to the same partition of (a, b), then recalling $P - Q = D$ one has the following theorem:

The function $f(x)$ is integrable if the g.l.b. of the absolute values of D is zero.

Finally PEANO proves the following two theorems:

Every continuous function is integrable.

Every bounded function which is discontinuous for a finite number of values of x in the interval (a, b) is integrable.

So much for PEANO's integral. These early examples, namely the definition of areas of a curved surface (though published later than the axioms) and PEANO's integral (though still not presented with the utmost logical rigor) are typical of PEANO's quest for precision in the early stages of his work. The same quest appeared again, much more extensively, in his edition of GENOCCHI's lectures on calculus.

1884: Genocchi's Lectures

Much has been written on the famous PEANO additions to the GENOCCHI lectures on differential and integral calculus [8].[6] Some historians feel that PEANO, in this instance, may possibly have shown a lack of respect for his

[6] See, for instance, UGO CASSINA, *Dalla geometria egiziana alla matematica moderna* (Rome: Cremonese, 1961), pp. 407–419; KENNEDY, *Peano*, pp. 11–16. CASSINA also published some related correspondence: "Alcune lettere e documenti inediti sul trattato del calcolo di Genocchi–Peano," *Rendiconti dell'Istituto Lombardo di Scienze e Lettere* **85** (1952): 337–362.

teacher, but all agree that he turned his teacher's book into a very important and innovative work. The literature in the fields of mathematics and history of science devotes more attention to the many ingenious counter-examples given by PEANO than to the relation between this book and his work on foundations. In this context, however, some of PEANO's footnotes, published at the beginning of the book, are particularly illuminating, because they indicate the importance already ascribed by PEANO to the concept of number.

Pages 1 to 3 of the book are subtitled "On numbers and quantities." In §. 2, PEANO explains, for the benefit of his students, what commensurable and incommensurable numbers are:

> A commensurable number divides all the commensurable numbers into two categories: numbers which are smaller than a and numbers which are not smaller than a (*i.e.* numbers that are not greater, and numbers that are greater than a), and any number of the first category is smaller than any number of the second. *Vice versa*, if all the commensurable numbers are divided into two categories, so that every number of the first is smaller than every number of the second, we have to admit that, extending the concept of number, there is a number which is neither smaller than any of those in the first category nor bigger than any of those in the second and if no number has this property, we call it *incommensurable.*[7]

This definition is essentially that of DEDEKIND. The fact that PEANO begins his book by speaking of numbers clearly indicates the fundamental importance he attached to the concept in analysis. This is further strengthened by his footnotes. He says, for instance:

> Analysis bases itself, without any postulate, on the concept of number. And although this concept should already be [familiar] from arithmetic and algebra, it was considered beneficial to present here the definition of incommensurable numbers, so that the subsequent proofs would be clear (*e.g.* that in §. 14).[8]

In §. 14, in fact, PEANO presents a theorem which was a major difficulty in CAUCHY's analysis. The theorem states that "if, with the continuous increase of x, y increases continually, though having a value always lower than a quantity A, y tends to a limit which is either A or a quantity smaller than A."[9] CAUCHY and BOLZANO had proved that every bounded sequence of numbers, either increasing or decreasing, is convergent, but they did not verify this for real

[7] [8], p. 1. The German translation of the book under the title *Differentialrechnung und Grundzüge der Integralrechnung* (Leipzig, 1899) speaks of rational and irrational, rather than commensurable and incommensurable numbers. See p.1.

[8] [8], p. VII.

[9] [8], p. 7.

numbers. This was the kind of vagueness in the foundations of analysis that PEANO tried to clarify. DEDEKIND, incidentally, dealt with the same problem in 1872 at the end of his *Stetigkeit*.

PEANO's footnotes also throw light on his sources. It is interesting to note that although he mentions DEDEKIND's *Stetigkeit* he probably had not yet seen it; in fact his footnote continues: "The concept of incommensurable number introduced here is the simplest, the most natural and the most common of all. For a broader treatment, see DINI, *Fondamenti per la teorica delle funzioni di variabili reali*, Pisa 1878, pp. 1–14."[10] PEANO then mentions DEDEKIND and MORITZ PASCH (1843–1930): "Substantially identical is DEDEKIND's way of thinking in *Stetigkeit und irrationale Zahlen*, Braunschweig 1872, quoted by PASCH, *Einleitung in den Differential- und Integral-Rechnung*, Leipzig 1882." He then describes the work of these two mathematicians as follows: "Out of the two categories [of numbers] which we introduce, these authors consider only the first, which they call *Zahlenstrecke*, and according to which they define the definitions of their arithmetical operations."[11] This last sentence makes little sense, for PEANO's presentation (based on DINI's) seems identical to that of DEDEKIND. A comparison of PASCH's and DEDEKIND's books makes it clear that PEANO had relied solely on DINI and PASCH. Whereas DINI followed DEDEKIND closely, PASCH gave a somewhat different (more geometrical) and more complicated presentation. And PASCH alone, not DEDEKIND, used the term *Zahlenstrecke*. PEANO had DINI and PASCH in front of him, and assumed that PASCH followed DEDEKIND more closely than DINI, and that DINI had elaborated and simplified this alleged presentation. This fact is important in establishing the exact relation between the work of PEANO and that of DEDEKIND: when PEANO edited GENOCCHI's lectures he still had not read DEDEKIND's *Stetigkeit*.

In any case, DEDEKIND, DINI, and PEANO all attribute primary importance to the concept of number and consider it fundamental in infinitesimal calculus. This, in my view, is the most important point to note at this stage. Moreover, PASCH is an important source for PEANO: as the *Dictionary of Scientific Biography* points out, he was the initiator, in his *Vorlesungen über neuere*

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[10] [8], p. VII. ULISSE DINI (1845–1918), whose work PEANO mentions, was a professor of analysis and higher geometry at the University of Pisa (he later became rector of the University), who, like PEANO, belonged to the tradition that sought for rigor in analysis. His book begins with the following recollection (p. iii): "As early as twelve or thirteen years ago, I began to suspect that some of the fundamental principles of analysis, in their propositions or demonstrations, did not present all the rigor that mathematics indeed involves." Referring to the definition of numbers, DINI remarks (p. vi): "In the first chapter I deal with incommensurable numbers, using the memoirs of Dedekind and the notes kindly received from Schwarz." He does not say which "memoirs" he used, but a reading of Chapter 1 of his book leaves little doubt that he was relying on DEDEKIND's *Stetigkeit* of 1872. In fact the chapter is a very clear and detailed presentation of DEDEKIND's definition of real numbers.

[11] [8], p. VII.

Geometrie (also published in 1882), of the "axiomatic method" as it is understood today. PASCH and his influence on PEANO will be discussed again in this chapter.[12]

1888: The Introduction of Logic

UGO CASSINA, PEANO's faithful pupil, relates that at an early stage PEANO studied the works of BOOLE and SCHRÖDER in calculus of classes, and PEIRCE and MACCOLL on the calculus of proposition.[13]Unfortunately there are no documents showing PEANO's thoughts in this period, and logic made its appearance for the first time in 1888 in his book, *Calcolo geometrico secondo l'Ausdehnungslehre di H. Grassmann, preceduto dalle operazioni della logica deduttiva (The geometrical calculus according to the Ausdehnungslehre of Grassmann, preceded by the operations of deductive logic)* [14]. The main object of the book was to present GRASSMANN's *Ausdehnungslehre* more clearly than its inventor had done, and this fits well into PEANO's program to try to render mathematics as clear and as rigorous as possible.[14] The most peculiar chapter is the introductory one, entitled "The Operations of Deductive Logic," which has no apparent connection with the rest of the book.[15] In the foreword (before the introductory chapter on logic) PEANO says:

> The geometrical calculus, in general, consists of a system of operations to be performed on geometrical entities analogous to that which algebra performs on numbers. It permits one to express in formulae the results of geometrical constructions, present geometrical propositions with equations, and to substitute a transformation of equations for an argument [*ragionamento*]. The geometrical calculus has analogies with analytical geometry; it differs in that, while in analytical geometry one calculates in

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[12] *The Dictionary of Scientific Biography*, **10**: 343. A comparison between PASCH and PEANO is presented by BARTEL L. VAN DER WAERDEN in "Les contributions de Peano aux théories axiomatiques de la géométrie, " *Celebrazioni in memoria di Giuseppe Peano nel cinquantenario della morte*, pp. 61–71. For a more general outline, FREUDENTHAL, "Die Grundlagen der Geometrie um die Wende des 19. Jahrhunderts," or "The Main Trends in the Foundations of Geometry in the 19th Century."

[13] CASSINA, "L'Oeuvre philosophique di G. Peano," pp. 484–485.

[14] The mathematician BEPPO LEVI describes his enthusiasm when, as a young student, he read PEANO's *Calcolo Geometrico*, and, in contrast, his difficulty in reading GRASSMANN's abstract *Ausdehnungslehre*: BEPPO LEVI, "L'Opera matematica di Giuseppe Peano", *Bollettino della Unione Matematica Italiana* (1932) **11**: 253–262, see pp. 254–255.

[15] As TORRETTI points out in *Philosophy of Geometry*, p. 202, "Grassmann had no thought of associating the formal or abstract nature of mathematics with the mathematician's search for logical consequences of the principles assumed by him."

numbers which determine the geometrical entities, in this new science, calculations are made on the entities themselves.[16]

This paragraph hints at PEANO's view of the possible general relation between logic and mathematics. Later in the foreword PEANO remarks that the operations of deductive logic "present a great analogy with those of algebra and geometrical calculus."[17] PEANO was probably thinking of an analogy such as logic/algebra/geometry, rather than a reduction of mathematics to logic, and was at an advanced stage of elaborating its details. This is confirmed, as we shall see, in some of PEANO's later writings.

At this point PEANO mentions the works of BOOLE, SCHRÖDER, PEIRCE and a number of other mathematicians. He does so, however, with a certain disappointment: "Deductive logic, which is part of mathematical sciences, has not up to now made much progress, though it has been the object of study by Leibniz, Hamilton, Cayley, Boole, H. R. Grassmann, Schröder, *etc.*"[18] PEANO's disappointment is probably linked to the fact that his predecessors were mathematicians who tried to mathematize logic, whereas PEANO was just about to produce a totally different approach, namely to use logic to analyze mathematics.

The introductory chapter on logic is a development of calculus of classes, using the symbols $=$ for equal, $\cup$ for union, $\cap$ for intersection (it was PEANO who introduced these two symbols), ●[a full circle] for "all" and ○ [an empty circle] for "nothing." $-A$ or $\bar{A}$, is the class of all entities not belonging to A. $AB = \bigcirc$ means that no A is B, and $A \cup B = \bullet$ means every entity is either A or B. PEANO, in the main, synthesizes results which were obtained before him, in the attempt to apply arithmetic to logic. But W. QUINE regards the *Calcolo geometrico* as a break in the BOOLEAN tradition which used the notations of addition and multiplication to express the union and intersection of classes.[19] QUINE remarks that since PEANO wanted to apply logic to arithmetic, he needed to distinguish the notations to avoid confusion, and therefore introduced the signs $x \cap y$ and $x \cup y$ to denote intersection and union of classes respectively. Another novelty in PEANO's introductory chapter, according to QUINE, was his adoption of an explicit notation for class abstraction. "$x : Fx$" denoted the class of all values "x" that fulfill the condition "Fx."[20] PEANO insisted on the distinctive status of "x" as a bound variable., and pointed out the equivalence between the calculus of classes and that of conditions.[21]

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[16] [14], p. III.

[17] [14], p. VII.

[18] *ibid.*

[19] QUINE, "Peano as a Logician," p. 15. *Cf.* [14], p. X.

[20] PEANO *Opere scelte*, **2**: 8–9; *Selected Works* pp. 80–81.

[21] For a presentation of PEANO's logic see COUTURAT, "La logique Mathématique de M. Peano." See also PADOA, "Il contributo di G. Peano" and "Ce que la logique doit à Peano." A more recent study of PEANO's contribution to logic is E. A. ZAITSEV, *From*

All this does not, however, fully explain the relation between GRASSMANN's *Ausdehnungslehre* and "The Operations of Deductive Logic;"[22] as PEANO's admirer and critic LEVI says, the mathematical text "can be read ignoring almost completely the first chapter (on logic)."[23] Historians have studied the various aspects of the *Ausdehnungslehre* in an attempt to find some further clue to its relation to PEANO's work. According to BOTTAZZINI, it was the particular abstraction in GRASSMANN's work that appealed to PEANO.[24] PAOLO FREGUGLIA says that "In Grassmann there is a sense of *mathesis universalis* that one finds in Peano: mathematical abstraction as the basis of rigor."[25] And PEANO's follower, GIOVANNI VAILATI, says that the first chapter was written to prepare the reader for the use of logic in the geometrical presentation in the rest of the work.[26] It seems to me that the explanation is strictly related to PEANO's general belief that clarity can be achieved through rigor, namely through a step-by-step logical reasoning. As well illustrated by MICHAEL CROWE, GRASSMANN's *Ausdehnungslehre* was paid little attention — *inter alia* because it was not sufficiently clear.[27] PEANO, who understood the importance of GRASSMANN's work, endeavored to present it more clearly. This point is important for an understanding of PEANO's general approach: for him, rigor and clarity were two sides of the same coin.

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the History of Mathematical Logic at the End of the 19th – beginning of the 20th centuries: the Logical Theory of Giuseppe Peano (Ph.D. Thesis, Moscow, 1989). ZAITSEV's work is in Russian, and I was not able to read it fully, though a helpful English summary is given on pp. 62–63. ZAITSEV considers PEANO in the context of the work done in logic at the turn of the 19th century, and points out a confusion between the meta-theory of a system and the system itself in the work of PEANO as well as in that of other logicians.

[22] In 1894 PEANO wrote a review of GRASSMANN's *Gesammelte mathematische und physikalische Werke* [69], but this review does not offer any indication as to the relation between his logic and GRASSMANN's *Ausdehnungslehre.*

[23] LEVI, "Intorno alle vedute di G. Peano circa la logica matematica," p. 66.

[24] UMBERTO BOTTAZZINI, "Dall'analisi matematica al calcolo geometrico: Origini delle prime ricerche di logica di Peano," *History and Philosophy of Logic* **6** (1985): 25–52. The article has an important appendix containing PEANO's handwritten notes in his own copy of the book. PEANO, indeed, emphasized the importance of abstraction for geometry: see his "Importanza dei simboli in matematica" [176], *Opere scelte*, **3**: 389–396.

[25] FREGUGLIA, "Il calcolo geometrico ed i fondamenti della geometria," p. 198. According to HEATH, this abstraction caused the neglect of GRASSMANN's work: A. E. HEATH, "The Neglect of the Work of H. Grassmann," *The Monist* **27** (1917): 22–35.

[26] VAILATI, "Le logique mathématique et sa nouvelle phase de développement dans les écrits de M. J. Peano," p. 94. *Cf.* MAINO PREDAZZI, "Giuseppe Peano e la sua definizione assiomatica di spazio vettoriale (lineare)," *Cultura e scuola* (1976), Anno **15**, n. **58**, pp. 216–223.

[27] CROWE, *A History of Vector Analysis*, pp. 77–94.

PEANO's *Calcolo geometrico* paved the way for his mathematical logic: a year later he produced his axioms.

The Axioms

In 1889, PEANO published his most important work — *Arithmetices principia, nova methodo exposita (The Principles of Arithmetic, presented by a new method)* — containing his famous axioms. The beginning of this work gives a clear account of PEANO's general approach (and confirms my thesis):

> Questions pertaining to the foundations of mathematics, although treated by many these days, still lack a satisfactory solution. The difficulty arises principally from the ambiguity of ordinary language. For this reason it is of the greatest concern to consider attentively the words we use. I resolve to do this, and am presenting in this paper the results of my study with applications to arithmetic.[28]

This passage speaks for itself: PEANO's aim was to achieve clarity; this clarity, which we have seen in PEANO's work in the preceding years, was part and parcel of his rigor. The preface continues:

> I have indicated by signs all the ideas which occur in the fundamentals of arithmetic, so that every proposition is stated with just these signs. The signs pertain either to logic or to arithmetic ... With this notation every proposition assumes the form and precision equations enjoy in algebra, and from the propositions so written others may be deduced, by a process which resembles the solution of algebraic equations. That is the chief reason for writing this paper.[29]

Towards the end of the preface he emphasizes that he distinguishes between the symbol ε to indicate set membership and Ɔ to indicate inclusion (C inverted). At the end of the preface, PEANO acknowledges two works he had used: H. GRASSMANN's *Lehrbuch der Arithmetik*, and DEDEKIND's *Was sind und was sollen die Zahlen* (this time there is no reason to believe that PEANO had not seen them).

There follows an introductory note presenting the signs he uses divided into three groups: signs of logic, signs of arithmetic, and composite signs.

The first group includes signs such as: P for proposition; K for class; $\cap$ for and; $\cup$ for or; $-$ for not; Λ (V for *verum* turned upside down) for "false" or "nothing;" ε for is; [ ] is a sign for the inverse; з (inverted ε) means "such that" or [ε] *etc.* The arithmetical signs include $1, 2, \ldots, =, >, <, +, -, \times$, in

[28] PEANO, *Selected works*, p. 101.

[29] *ibid.*, pp. 101–102.

their usual meaning; and signs such as N for positive integers or R for positive rational numbers. There are four composite signs: $-<$ for "is not less than," $= \cup >$ for "is equal to or greater than," ɜD for "is a divisor," and MɜD for "is the greatest divisor."

Peano then explains how his notations work; he defines the punctuation between signs, and shows how propositions are built. Points in a formula have the following meaning: $ab.cd$ means $(ab)\,(cd)$; and $ad.cd{:}ef.gh \therefore k$ means $(((ab)\,(cd)\,((ef)\,(gh)))k$.

He then presents a series of propositions of logic, and explains the concepts of class, inverse, and function.

The "Principles of Arithmetic" proper begin with four "explanations" presenting the basic concepts of "number," "unity," "successor," and "equality":

The sign	N	means		*number* (positive integer).
„	„	1	„	*unity*
„	„	$a+1$	„	the *successor of* a, or a *plus* 1.
„	„	$=$	„	*is equal to.* This must be considered as a new sign, although it has the appearance of a sign of logic.

Then come nine axioms. Five of them (1, 6, 7, 8, 9) are "Peano's axioms" in their early form (later they were slightly varied).[30] The four others refer to the symbol " = ", which was taken as an undefined term. (Later the sign " = " was omitted from the "explanation," and the remaining explanations became "primitive ideas.") The axioms referring to " = " were separated, and the five remaining axioms were slightly varied. Here are the nine axioms:

1.	$1 \varepsilon N.$	(1 is a natural number)
2.	$a \varepsilon N . \supset . a = a.$	(If a is a natural number then a equals a)
3.	$a, b, \varepsilon N . \supset{:} a = b . = . b = a.$	(If a and b are natural numbers then $a = b$ equals $b = a$)
4.	$a, b, c,\ \varepsilon N . \supset \therefore a = b . b = c{:} \supset . a = c.$	(If $a, b,$ and c are natural numbers then if $a = b$ and $b = c$ implies $a = c$)
5.	$a = b . b \varepsilon N{:} \supset . a \varepsilon N.$	(If a equals b and b is a natural number then a is a natural number).
6.	$a \varepsilon N . \supset . a + 1 \varepsilon N.$	(Every natural number has a successor, $a + 1$)
7.	$a, b \varepsilon N . \supset{:} a = b . = . a + 1 = b + 1.$	(Two natural numbers a and b are equal if their successors are equal, *i.e.*, if $a + 1 = b + 1$.)

[30] See Peano, *Opere scelte*, **3**: 216.

8. $a \varepsilon \mathrm{N} . \supset . a + 1 - = 1$. (1 is not the successor of any natural number.)
9. $k \varepsilon \mathrm{K} \mathrel{.\cdot.} 1 \varepsilon \mathrm{k} \mathrel{.\cdot.} x \varepsilon \mathrm{N} . x \varepsilon \mathrm{k} : \supset_x . x + 1 \varepsilon \mathrm{k} :: \supset . \mathrm{N} \supset \mathrm{k}$.

(If a class k of natural numbers includes 1 and if, when k includes any number x it also contains a successor, then k contains all the natural numbers.)

By using these — and only these — notations, PEANO defines $2 = 1 + 1$, $3 = 2 + 1$, $4 = 3 + 1$ (*etc.*). He introduces subtraction, maximum, minimum, multiplication, powers, division, theorems from number theory, rationals, irrationals, and various theorems concerning open and closed intervals of real numbers. In brief, all arithmetic is logically deduced from five axioms.

The Principles of Arithmetic were only the beginning of PEANO's contribution to mathematical logic, but can be said to have included all the premisses for his future work. Shortly after, in fact, PEANO published *The Principles of Geometry*, a work which throws considerable light on his approach to mathematical research.

The Principles of Geometry

I principii di geometria logicamente esposti (The Principles of Geometry Logically Presented) was an attempt to extend the previous system to geometry [18]. In construction it is analogous to *The Principles of Arithmetic*, and in the preface PEANO explicitly states his approach: "One must rigorously keep the rules: Use in our propositions only terms with fully determined value, and make precise what is meant by definition and by proof."[31]

As shown by the notes at the end of the work, PEANO relies on PASCH's *Vorlesungen über Geometrie*. PASCH is considered by FREUDENTHAL to be the father of rigor in geometry,[32] and his *Vorlesungen* are important, *inter alia*, because of the axiomatic approach. Like PEANO, PASCH attempted to isolate concepts that could not be defined and theorems that were accepted without proof, as a basis for proving other theorems by using only logical arguments. It is quite obvious how close all this is to PEANO's approach.[33]

PEANO constructs his rigorous geometry as follows: he defines "The sign 1 reads [*leggassi*, *i.e.* "is to be understood as"] point; The sign = between two points indicates their identity (coincidence); If *a*, *b* are points, *ab* means the

[31] PEANO *Opere Scelte*, **2**: 56. *Cf.* KENNEDY, *Peano*, p. 27.

[32] FREUDENTHAL, "The Main Trends in the Foundations of Geometry in the 19th Century,", p. 619. For a general outline of contemporary foundation research in geometry see TORRETTI, *Philosophy of Geometry*, Chapter 3.

[33] For an illustration of PEANO's *Principii di geometria*, and a comparison with PASCH's work, see PAOLO FREGUGLIA, "Il calcolo geometrico ed i fondamenti della geometria," pp. 183–211.

class made of the internal points of the segment *ab*. Hence the formula $c \varepsilon ab$ means 'there is a point internal to the segment *ab*' "[34]

PEANO then states the five preliminary axioms, referring to "non-defined entities:"[35] He distinguishes between three axioms referring to the symbol " = " and two "axioms on segments." This is a step towards separating his five axioms in arithmetic from the nine original ones. In fact, the axioms referring to the symbol " = " are

1. $a = a.$
2. $a = b. = .b = a.$
3. $a = b.b = c : \supset a. = c.$

(This is equivalent to separating axioms 2, 3, and 4 in arithmetic from the other nine axioms; PEANO also says explicitly that these three axioms cannot be further reduced.[36])

There remain the two "axioms on segments" (it would be difficult to establish a direct analogy between these two axioms and the axioms in arithmetic); they are

4. $a, b \varepsilon 1 . \supset . ab \varepsilon \mathrm{K}1.$ (If *a* and *b* are points, then *ab* is a class of points (a figure).)[37]

5. $a, b, c, d \ \varepsilon 1 . a = b . c = d : \supset . ac = bd.$ (if *a*, *b*, *c*, *d*, are points and *a* coincides with *b*, *c* with *d*, then *ac* is identical to *bd*.)[38]

As in the case of arithmetic, a series of definitions follows in the form of logical statements, and then "propositions which depend only on the definitions and the logical axioms,"[39] namely, geometrical axioms and theorems.

PEANO's remarks at the end of the work are very revealing and help one to understand his general approach to mathematics; let me quote them in full despite their length. Speaking of the nature of mathematical proof, he says:[40]

> It is known that scholastic logic is of little use in mathematical proofs, seeing that in it the classification and rules of the syllogism are never mentioned, while on the other hand, one uses arguments that are entirely convincing, but not reducible to the forms considered in traditional logic. For this reason, several mathematicians, Descartes among them, declared that evidence is the only criterion to recognize the exactness of a reasoning.
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> But this principle is unsatisfactory. A demonstration may be more or less evident; it may be evident to one person, but doubtful to another; and it may have happened to anyone to find unsatisfactory proofs which

[34] PEANO, *Opere scelte*, **2**: 61.
[35] *Cf. ibid.*, p. 57.
[36] *ibid.*, p. 77.
[37] *ibid.*, p. 61, *cf.* p. 77.
[38] *ibid.*
[39] *Cf. ibid.*, p. 57.
[40] *ibid.*, pp. 80–81. Part of the translation is from KENNEDY, *Peano*, p. 28.

were already regarded as exact. It is particularly unsatisfactory in our research, which refers to propositions to which we are very accustomed and which may appear to many as almost self-evident.

But this question can be given an entirely satisfactory solution. In fact, reducing the propositions, as is done here, to formulas analogous to algebraic equations and then examining the usual proofs, we discover that these consist in transformations of propositions and groups of propositions, having a high degree of analogy with the transformations of simultaneous equations. These transformations, or logical identities, of which we make constant use in our arguments, can be stated and studied.

The list of logical identities we use was made already in my [*Arithmetices principia*]; many of them were listed by Boole. Their number is large. It would be an interesting study, and it has not yet been made, to distinguish the fundamental identities, which must be accepted from the beginning, from the remainder, which are contained in the fundamental ones. This research would lead to a study of logic, analogous to that made here of geometry, and in the preceding booklet on arithmetic.[41]

This is one of PEANO's most explicit presentations of his approach dating from the period in which he formulated his axioms, though it does not make entirely clear how he then viewed the relationship between logic and mathematics. At this stage he seems to have seen some kind of analogy between logic, arithmetic and geometry, but was the study of logic conceived as an instrument for the understanding of the structure of mathematics more than as the foundation of mathematics itself? If so, PEANO's point of view was different from RUSSELL's, who sought to reduce mathematics to logic. As we shall see in the next chapter, PEANO's mathematics, at this stage at least, developed in a more "formalistic" direction.[42]

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[41] In a related article, "Sui fondamenti della geometria," (1894, [64], *Opere scelte*, 3:115–257), PEANO said *inter alia* (p. 116):

"In this note I propose to treat briefly, precisely those points in which one can really reach both rigor and simplicity...

It is well known that in geometry not everything can be defined; this has been said by various authors.

Yet by the various authors the number and the nature of undefined entities varies considerably.

To make it clear what entities should or should not be defined, note that the terms of a treatise belong partly to the general grammar or logic, *e.g.*, *is*, *are*, *and*, *or*, *no* ... By classifying them one reconstructs mathematical logic ...

One should first differentiate when these terms or the ideas they represent are *primitive* ideas, which are not defined, and *derived* ideas, which are defined."

[42] For an overall discussion of the role of logic in the work of PEANO and his followers, see BORGA, "La logica ..."

Chapter 6: Against Intuition

With the formulation of his axioms PEANO took the basic step toward what he hoped would be a rigorous, clear and flawless, mathematics. In the following years he set out to develop, modify, clarify, apply, and spread his program. His objectives were classical for rigorism: Firstly, to ban intuition from mathematics. Secondly, to express mathematical proofs in a step-by-step deduction, rendered clear and more precise by the use of symbols. Two works, published immediately thereafter (1890) in the *Mathematische Annalen*, present these aims. The first is a note on the "Peano curve" — a curve filling a unit square — showing that, contrary to contemporary belief based on intuition, it was not always possible to enclose a continuous curve in an arbitrarily small area. The second article, "Démonstration de l'integrabilité des équations différentielles ordinaires" [27], developed his previous results by making the utmost use of symbolism – to achieve the utmost rigor.

Peano's Curve

The importance of the first article, "Sur la courbe, qui rempli une aire plane," is described by HUBERT KENNEDY in the following words:

> PEANO's next publication was a bombshell — the first example of a space-filling curve. It is a landmark in the history of the study of dimensionality. Already, in 1878, Georg Cantor had shown that it was possible to establish a one-to-one correspondence between the points on a line and the points on a plane, thus dispelling the common notion that there are 'more' points in two-space than in one-space. E. Netto showed shortly afterwards that such a correspondence was necessarily discontinuous. It was then thought that a continuous curve in two-space, i.e. one given by continuous parametric functions of a single variable, $x = f(t)$ and $y = g(t)$, was such that it could be enclosed in a region of arbitrarily small area. In four pages of the *Mathematische Annalen*, Peano gave expressions for function f, g, such that as t varies over the unit interval, the curve described goes through every point in the unit square. Thus, as Peano remarks in his first paragraph, 'given an arc of a continuous curve, without any other hypothesis, it is not always possible to enclose it in an arbitrarily small region'.[1]

How did PEANO construct his "monster" curve?[2] PEANO's aim was to determine two single-valued and continuous functions x and y of a (real) variable t which, as t varies throughout the interval $(0, 1)$, take on all pairs of values

[1] KENNEDY, *Peano*, pp. 31–32.
[2] On "monsters" in mathematics, see VOLKERT, *Die Krise der Anschauung*, I. 6.

such that $0 \leqq x \leqq 1$, $0 \leqq y \leqq 1$.[3] If one considers the locus of points whose coordinates are continuous functions of a variable to be a continuous curve, then one has an arc of a curve which goes through every point of a square.

PEANO used the number 3 as a base of numeration and referred to each of the numerals $0, 1, 2$ as a digit. He considered the infinite sequence of digits a_1, a_2, $a_3, \ldots$, and wrote

$$T = 0 . a_1 a_2 a_3 \ldots .$$

If a is a digit, designate $\mathbf{k}a$ as the digit $2 - a$ (the complement of a), *i.e.* let $\mathbf{k}0 = 2$, $\mathbf{k}1 = 1$, $\mathbf{k}2 = 0$.

$$\text{If } b = \mathbf{k}a, \text{ then } a = \mathbf{k}b, \text{ and also } \mathbf{k}a \equiv a \pmod 2 .$$

PEANO designates by $\mathbf{k}^n a$ the result of the operation $\mathbf{k}$ repeated n times on a. If n is even, then $\mathbf{k}^n a = a$; if n is odd, then $\mathbf{k}^n a = \mathbf{k}a$. If $m \equiv n \pmod 2$, then $\mathbf{k}^m a = \mathbf{k}^n a$.

Now PEANO let two sequences correspond to the sequence T

$$X = 0 \cdot b_1 b_2 b_3 \ldots ,$$

$$Y = 0 \cdot c_1 c_2 c_3 \ldots ,$$

where the digits b and c are given by the relations

$$b_n = \mathbf{k}^{a_2 + a_4 + \cdots + a_{2n-2}} a_{2n-1}$$

$$c_n = \mathbf{k}^{a_1 + a_3 + \cdots + a_{2n-1}} a_{2n} .$$

Thus b_n, the n^{th} digit of X, is equal to a_{2n-1}, the n^{th} digit of uneven rank in T, or to its complement, according to whether the sum $a_2 + \ldots + a_{2n-2}$ of digits of even rank which precede it, is even or odd, and analogously for Y. One may thus write these relations in the form:

$$a_1 = b_1, \quad a_2 = \mathbf{k}^{b_1} c_1, \quad a_3 = \mathbf{k}^{c_1} b_2, \quad a_4 = \mathbf{k}^{b_1 + b_2} c_2, \ldots ,$$

$$a_{2n-1} = \mathbf{k}^{c_1 + c_2 + \cdots + c_{n-1}} b_n, \quad a_{2n} = \mathbf{k}^{b_1 + b_2 + \cdots + b_n} c_n .$$

If the sequence T is given, then X and Y are determined, and if X and Y are given, then T is determined.

Give the name *value* of the sequence T to the quantity (analogous to a decimal number having the same notation)

$$t = \operatorname{val} T = \frac{a_1}{3} + \frac{a_2}{3^2} + \cdots + \frac{a_n}{3^n} + \cdots .$$

[3] I rely heavily on KENNEDY's translation, PEANO, *Selected works*, X.

To each sequence T corresponds a number t such that $0 \leqq t \leqq 1$. For the converse, the numbers t in the interval $(0, 1)$ divide into two classes:

(α) Those numbers, different from 0 and 1, which give an integer when multiplied by a power of 3. They are represented by two sequences, the one

$$T = 0 . a_1 a_2 \ldots a_{n-1} a_n 222 \ldots, \quad \text{where } a_n \text{ is equal to 0 or 1.}$$

The other

$$T' = 0 . a_1 a_2 \ldots a_{n-1} a_n' 000 \ldots, \quad \text{where } a_n' = a_n + 1 \ .$$

(β) The other numbers. These are represented by only one sequence T.

The correspondence established between T and (X, Y) is such that if T and T' are two sequences of different form, but $\operatorname{val} T = \operatorname{val} T'$, and if X, Y are the sequences corresponding to T, and X', Y' those corresponding to T', one has

$$\operatorname{val} X = \operatorname{val} X', \quad \operatorname{val} Y = \operatorname{val} Y'$$

Indeed, consider the sequence

$$T = 0 . a_1 a_2 \ldots a_{2n-3} a_{2n-2} a_{2n-1} a_{2n} 2222 \ldots,$$

where a_{2n-1} and a_{2n} are not both equal to 2. This sequence can represent every number of class α. Letting

$$X = 0 . b_1 b_2 \ldots b_{n-1} b_n b_{n+1} \ldots$$

where one has

$$b_n = \mathbf{k}^{a_2 + \cdots + a_{n-2}} a_{2n-1}, \quad b_{n+1} = b_{n+2} = \cdots = \mathbf{k}^{a_2 + \cdots + a_{n-2} + a_{2n}} 2 .$$

If one lets T' be other sequences whose values conicide with $\operatorname{val} T$, one has

$$T' = 0 . a_1 a_2 \ldots a_{2n-3} a_{2n-2} a'_{2n-1} a'_{2n} 0000 \cdots,$$

$$X' = 0 . b_1 b_2 \ldots b_{n-1} b_n' b_{n+1}' \ldots .$$

The first $2n-2$ digits of T' coincide with those of T; hence the first $n-1$ digits of X' coincide also with those of X. The others are determined by the relations

$$b_n' = \mathbf{k}^{a_2 + \cdots + a_{2n-2}} a'_{2n-1},$$

$$b'_{n+1} = b'_{n+2} = \cdots = \mathbf{k}^{a_2 + \cdots + a_{2n-2} + a'_{2n}} 0 .$$

One can distinguish two cases, according to whether $a_{2n} < 2$ or $a_{2n} = 2$. If a_{2n} has the value 0 or 1, then:

$$a'_{2n} = a_{2n} + 1, \quad a'_{2n-1} = a_{2n-1}, \quad b_n' = b_n,$$

$$a_2 + a_4 + \cdots + a_{2n-2} + a'_{2n} = a_2 + \cdots + a_{2n-2} + a_{2n} + 1,$$

whence

$$b'_{n+1} = b'_{n+2} = \cdots = b_{n+1} = b_{n+2} = \cdots = \mathbf{k}^{a_2 + \cdots + a_{2n}} 2 .$$

In this case the two series X and X' coincide in form and value.

If $a_{2n} = 2$, one has $a_{2n-1} = 0$ or 1, $a'_{2n} = 0$, $a'_{2n-1} = a_{2n-1} + 1$, and on setting

$$s = a_2 + a_4 + \cdots + a_{2n-2}$$

one has

$$b_n = \mathbf{k}^s a_{2n-1}, \quad b_{n+1} = b_{n+2} = \cdots = \mathbf{k}^s 2,$$

$$b'_n = \mathbf{k}^s a'_{2n-1}, \quad b'_{n+1} = b'_{n+2} = \cdots \mathbf{k}^s 0.$$

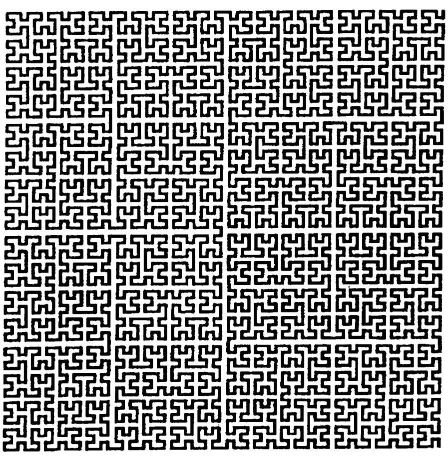
Since $a'_{2n-1} = a_{2n-1} + 1$, the two fractions $0 \cdot a_{2n-1}222\ldots$ and $0 \cdot a'_{2n-1}000\ldots$ have the same value. Operating on the digits with the same operation $\mathbf{k}^s$ one obtains the two fractions $0.b_n b_{n+1} b_{n+2}\ldots$ and $0.b'_n b'_{n+1} b'_{n+2}\ldots$, which also have the same value, as may be seen. Hence the fraction X and X', although differing in form, have the same value.

Analogously one can show that $\operatorname{val} Y = \operatorname{val} Y'$.

Therefore, if one sets $x = \operatorname{val} X$, and $y = \operatorname{val} Y$, one deduces that x and y are two single-valued functions of the variable t in the interval $(0, 1)$. Indeed, if t tends to t_0, the first $2n$ digits of the development of t finally coincide with those of the development of t_0 if t_0 is a β, or with those of one of the two developments of t_0 if t_0 is an α; and so the first n digits of the x and y corresponding to t will coincide with those of x and y corresponding to t_0.

Finally, to each pair (x, y) such that $0 \leqq x \leqq 1$, $0 \leqslant y \leqq 1$ corresponds at least one pair of sequences (X, Y) which express that value; to (X, Y) corresponds a T, and to this a t. Thus one may always determine t in such a fashion that the two functions x and y take on any arbitrarily given values in the interval $(0, 1)$.

This was how PEANO constructed his curve, proving against geometrical intuition that it was possible to draw a line through every point of the square,



Peano's curve

i.e. a line that fills a planar region. What was intuitively believed to be true was proved by PEANO to be false. This confirmed his belief that "good" mathematics is rigorous and the rigor can best be achieved by strict logical deduction.

Symbolism Takes Over

The second article [27], "Démonstration de l'integrabilité des équations différentielles ordinaires," was a generalization of the 1886 paper "Sull'integrabilità delle equazioni differenziali del primo ordine" [9], mentioned in Chapter 3, proving the existence of a solution to the first order differential equation $y = f'(x, y)$ on the sole assumption that the function is continuous. In 1888, PEANO wrote to FELIX KLEIN, then editor of the *Mathematische Annalen*: "Concerning my other note "integrabilità delle equazioni differenziali," it will certainly be a great honor for me if you consider publishing it. But I do not have any addition to make, having attempted in vain to extend the theorem thereby proved to equations of higher order."[4]

But by 1890, PEANO had overcome the difficulties and extended his theorem to the general case. The article, also published in the *Mathematische Annalen*, contributes, *inter alia*, to logic: PEANO for the first time distinguishes between an individual and the class composed of this individual alone, introducing a new logical symbol: the Greek letter iota (ι). Thus, if b is an individual and ιb is the set containing it, the identity $a = b$ may be written as $a \varepsilon \iota b$.[5] PEANO also anticipates the axiom of choice — later, in 1908, included by ERNST ZERMELO (1871–1953) in his system of axioms in set theory — saying that in a given collection of sets, one may form a set consisting of precisely one element from each set of the given collection. PEANO, however, on this occasion rejected this possibility, saying: "But since we may not apply an infinite number of times an *arbitrary* law whereby to a class a is made to correspond an individual of that class, we have formed here a *determined* law whereby to each class a, under certain hypotheses, we make correspond an individual of that class."[6]

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[4] Letter of PEANO to KLEIN, 8 April 1888. Cod. Ms. KLEIN, 189 Niedersächsische Staats- und Universitätsbibliothek, Göttingen.
"Per quanto si riferisce all'altra mia nota "integrabilità delle equazioni differenziali", certo sarà per me un alto onore, se ella crede di pubblicarla; ma io non avrei ad essa alcuna aggiunta da fare, avendo inutilmente tentato di estendere il teorema ivi dimostrato alle equazioni d'ordine superiore".

[5] On the importance of this distinction, see PADOA, "Ce que la logique doit à PEANO," pp. 34–35. PADOA explains that *a match* is not the same as *a box containing only this match.*

[6] Translation from KENNEDY, *Peano*, p. 33. For the historical context of the axiom of choice see GREGORY H. MOORE, *Zermelo's Axiom of Choice* (New York: Springer, 1982).

An odd aspect of PEANO's article is the way it is presented, using logical symbolism (PEANO devotes the first half of the article to explaining his symbolism). The proof of the theorem has its importance, but the way it is presented is superfluous, long and cumbersome.[7] Unfortunately, PEANO applied this kind of presentation to other works and to his teaching, thereby exasperating his students. This was one of the major drawbacks to his work and will be discussed in the Conclusion.

An interesting question would be: to what extent did PEANO's symbolism help him reach this result? Was it relevant only to the presentation, or did it advance his work intrinsically? This question cannot really be answered, since PEANO tells us nothing about this specific stage of his work. Yet he formulated his axioms precisely in this period, and it is quite possible that he found it easier to work creatively with a cumbersome but "rigorous" system of symbols, rather than using an "intuitive," but not entirely rigorous method.

So much for PEANO's rigorous proof of integrability. A third article published in the same year (1890) in the French journal *Mathesis* — "Les propositions du cinquième livre d'Euclide réduites en formules" [25] — , was written along the same lines: all 25 propositions of the Fifth Book of Euclid were presented in logical terminology. PEANO's purpose was to "find the minimum of signs and conventions necessary to express the 25 propositions of the Fifth Book of Euclid."[8]

Thus far simplicity and rigor may have helped, but later on they ended by complicating matters. When PEANO began to apply his symbolism to teaching, he exasperated his students.[9] Still, in the years 1890–1892 PEANO developed his work and turned it into a real program which included the appropriate tools for its dissemination. The first issue of the *Rivista di Matematica* appeared in 1892 presenting a more or less definitive formulation of PEANO's mathematical logic and announcing the steps in his scientific research. It also included an important article on the concept of number, to which the next chapter is devoted.

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[7] LEVI emphasizes the fact that PEANO used his symbolism only as an ideography, and not as an algebra. This is important to determining the role of logic in PEANO's mathematical work, which was strictly instrumental. See LEVI, "Intorno alle vedute di G. Peano circa la logica matematica," p. 66. A more straightforward proof was published by GUSTAV MIE in "Beweis der Integrirbarkeit gewöhnlicher Differentialgleichungssysteme nach Peano," *Mathematische Annalen* **43** (1893): 555–568.

[8] *Mathesis* (1980) **10**, p. 73.

[9] See KENNEDY, *Peano*, pp. 100–101.

Chapter 7: The Concept of Number

Thus far we have dealt with the growth of the quest for rigor in modern mathematics and shown how this quest led PEANO to formulate his axioms. Let us now concentrate on the axioms themselves — their meaning within their mathematical and philosophical context.

The basic concept in PEANO's axioms is, of course, the concept of number, and one would expect him to have expounded it, or at least to have defined it. Surprisingly, not only did he not define the concept, he even declared it undefinable. Nor did he define the other two basic concepts of his axioms: "successor," and "1" (later to be substituted by "0", also undefined), or mathematical induction in general. This attitude throws more light on his general approach to mathematics, and may help us understand the philosophy behind his axioms.

PEANO was interested in a rigorous and clear presentation of mathematics and not in the nature of its foundations, and he believed that clarity would enhance the correct development of mathematics. This can be seen, very explicitly, in the first issue of the *Rivista di Matematica* (1891), a journal edited by him and devoted mainly to spreading the results of his studies. Of particular interest are the main differences between PEANO's and DEDEKIND's conceptions of number.

The Rivista di Matematica

The first issue of the *Rivista di Matematica*, a 270-page volume, contained no less than five articles by PEANO, summarizing his work up to that point. PEANO's view of the role of logic in mathematics becomes more explicit in this first issue.

The issue begins with a short summary (ten pages) of PEANO's work in mathematical logic, "Principii di logica matematica" ("Principles of Mathematical Logic") [31]. PEANO's aim in this introduction was not to add anything particular to logic, but to "present these theories in a summary way, with the purpose of attracting the reader to this type of interesting studies, and of preparing for myself a tool which is almost indispensable for future research."[1] PEANO presents himself as a follower of LEIBNIZ, and, after a brief historical outline, says:

> One of the most notable results reached is that, with a very limited number (7) of signs, it is possible to express all imaginable logical relations, so that with the addition of signs to represent the entities of algebra, or geometry, it is possible to express all the propositions of these sciences."[2]

[1] *Opere scelte*, 2: 92, *cf. Selected Works*, p. 154.

[2] *Selected Works*, p. 154.

The seven signs, mentioned in previous works as well as at the end of this one, are:
"ε (*is*), $=$ (*is equal*), $\supset$ (*implies* or *is contained*), $\cap$ (*and*, usually indicated by juxtaposition, $\cup$ (*or*), $-$ (*not*), and Λ *absurd* or *empty*)."[3] Thus the emphasis, from the very beginning, is on ideography.

As always in PEANO's writings, his footnotes convey valuable information; in this case they indicate his ultimate aim. The first footnote quotes the passage from LEIBNIZ's *Dissertation on the Combinatorial Art* proposing a universal language. Footnote 4 quotes PEANO's *Principles* of arithmetic and geometry and then says: "It thus results that the question proposed by LEIBNIZ has been completely, if not yet perfectly, resolved."[4] This is an early indication of PEANO's main purpose: a general language of science, in the LEIBNIZ an sense, more than the actual foundations of mathematics. In this context, footnote 5 cites different mathematicians for their use of varying signs of deduction, mentioning SCHRÖDER and, for the first time in print, FREGE.[5]

Apart from PEANO's "Principles of Mathematical Logic" the first issue of the *Rivista di Matematica* also fully displays his concern for rigor. In a review of a textbook by FRANCESCO D'ARCAIS on infinitesimal calculus [34], for instance, PEANO praised the author's rigor. And even more striking is a controversy conducted in a series of notes and letters between PEANO and the mathematician CORRADO SEGRE, where PEANO reproaches SEGRE for his lack of rigor.[6]

Another article, "Formole di logica matematica" ("Formulas of Mathematical Logic") [35], repeats, in the main, his principles of mathematical logic, in a concise and perhaps more systematic way. As far as the development of the axioms is concerned, PEANO again presents separately, as he had done in the *Principles of Geometry*, the three axioms referring to the symbol "$=$".[7] These axioms are:

1. $a = a$
2. $a = b . \supset . b = a$
3. $a = b . b = c : \supset a . = c.$

These three axioms were originally numbers 2, 3 and 4, in the set of nine axioms in *The Principles of Arithmetic* (see the previous chapter), with the difference that here axiom 2 has an inclusion sign instead of the original equality sign.

[3] *ibid.*, p. 161.
[4] *ibid.*, p. 154.
[5] *ibid.*, p. 155. PEANO, however, mentions FREGE in the handwritten notes on his copy of *Calcolo geometrico*; see BOTTAZZINI, "Dall'analisi matematica al calcolo geometrico," pp. 48–49.
[6] The articles and notes concerned are in the *Rivista di Matematica* **1** (1891): 42–69, 154–159. *Cf.* KENNEDY, *Peano*, pp. 38–39.
[7] *Opere scelte*, **2**: 110.

The next, most important, PEANO article in the volume, "Sul concetto di numero" ("On the Concept of Number" [37]), is a further step in the development of the axioms. It also contains an explicit statement of his attitude to the definition of the concept of number.

Peano's Concept of Number

PEANO's five axioms ("Proposizioni primitive") are presented in "On the Concept of Number" for the first time on their own and not as part of 9 axioms as in the *Principles of Arithmetic.*[8]

For the purpose of his new presentation, PEANO introduces, *inter alia*, the sign "$\backslash$" as "a sign which placed after any entity of the class *a*, transforms it into a *b*."[9] Thus, for instance, "$!\varepsilon N\backslash N$ means 'The sign ! positioned after a positive integer produces a positive integer'."[10]

PEANO then introduces signs for three basic, undefined, concepts, namely "number," "1" and "successor:"[11]

The sign N should be read *number* (*integer and positive*),
The sign 1 should be read *one.*
a being a number, $a+$ should be read *the successor of a.*

(On the following page he says: "These concepts cannot be obtained by deduction; one has to obtain them by induction (abstraction)." I will soon return to the significance of this statement). These concepts had been introduced as "explanations" in the *Principles of Arithmetic* (see Chapter 5). Here the sign for equal — the fourth "explanation" in the *Principles of Arithmetic* — is omitted, and so are the related axioms. Immediately thereafter PEANO states his five axioms, as "Primitive Propositions." This is the first time that the axioms are presented in a group of five. This is also the most formal presentation up to now, though not yet the final form: PEANO still begins counting from "1," rather than "0."

Primitive Propositions

1. $1\ \varepsilon \mathrm{N}$
2. $+\varepsilon \mathrm{N}\backslash \mathrm{N}$
3. $a, b \varepsilon \mathrm{N} . a+ \ = b + : \supset . a = b$
4. $1 - \varepsilon \mathrm{N} +$
5. $s \varepsilon \mathrm{K} . 1 \varepsilon s . s + \supset s : \supset . \mathrm{N} \supset s.$

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[8] *Opere scelte*, 3: 84.
[9] *ibid.*, pp. 81–82.
[10] *ibid.*, p. 82.
[11] *ibid.*, p. 84.

Immediate Consequences

6. $a \varepsilon \mathrm{N} . \supset . a + \varepsilon \mathrm{N}$
7. $a, b \varepsilon \mathrm{N} . a = b : \supset . a + \ = b +$
8. $a, b \varepsilon \mathrm{N} . \supset : a = b . \ = . a + \ = b +$
9. » $: a - = b : \supset . a + - = b +$
10. $s \varepsilon \mathrm{K} . 1 \varepsilon s . \mathrm{N} - s - = \Lambda : \supset \therefore x \varepsilon s . x + - \varepsilon s : - =_x \Lambda$

These may be read:[12]

1. One is a number.
2. The sign + placed after a number produces a number.
3. If a and b are two numbers, and if their successors are equal, then they are also equal.
4. One is not the successor of any number.
5. If s is a class containing one, and if the class made up of the successors of s is contained in s, then every number is contained in the class s.
6. If a is an N, $a +$ is also an N.
7. If a and b are N and [are] equal, their successors are also equal.
8. Since a and b are two numbers, to say that $a = b$ is like saying that $a + \ = b +$.
9. If a and b are unequal numbers, their successors, too, are unequal.
10. If s is a class containing the unit but not all numbers (in other words, if there exist N which are not s), then there is a member (*individuo*) x of the class s whose successor $x +$ is not in this class.

(I was unable to find out where axiom 5 in the *Principles of Arithmetic* "disappeared," or how it evolved. This axiom says: $a = b . b \varepsilon \mathrm{N} : \supset . a \varepsilon \mathrm{N}$ (a equals b and b is a natural number then a is a natural number). I did not find it repeated in this form in any of PEANO's writings that I was able to see; its disappearance is a riddle I am unable to solve.)

Having stated his axioms, PEANO makes some basic remarks of particular importance as far as foundationism is concerned. First and foremost: "*The number cannot be defined.*"[13] He explains that, presupposing the ideas represented by his previously stated signs only ($\cup$, $\cap$, $-$, ε, etc.), however one combines them, one will never get an expression similar to number. This reasoning is important since it disagrees with the Logicist attempt to define number by means of logic. On the other hand, PEANO says that "number," as well as "unit" (1), and "successor," can be obtained by means of induction, or abstraction. But what exactly does he mean? "Abstraction," in mathematics, is not an easy concept, and at this stage PEANO does not elucidate further. However, he does

[12] *ibid.*, pp. 85–86. English translation of the first five propositions from KENNEDY, *Peano*, p. 37.

[13] *Opere scelte*, 3: 85.

so in later writings, beginning from 1894,[14] and I shall be returning to the problem at the end of the present chapter and in Chapter 8.

Another important remark is that his propositions express necessary and sufficient conditions so that the entities of a system can be made to correspond univocally to the series of positive integers (N).[15] In other words, his axioms characterize general entities and not, specifically, natural numbers.[16] This may remind one of DAVID HILBERT's famous assertion: "One must be able to say at all times — instead of points, straight lines, and planes — tables, chairs, and beer mugs."[17] Later in this work I argue that PEANO's and HILBERT's approaches may have had more in common than generally thought. Herein lies, in any case, the main difference between PEANO (and, perhaps, also HILBERT), and DEDEKIND, FREGE, and RUSSELL. PEANO deals with general systems having the same characteristics as numbers; DEDEKIND, FREGE, and RUSSELL, as we shall see, want more, a definition of number.

Having asserted that number cannot be defined, PEANO states his five axioms in the following words:[18]

> 1. To a particular entity of the system shall be given the name 1.
> 2. Define an operation by which to each entity a of the system there corresponds another, $a+$, also of the system.
> 3. And two entities whose correspondents are equal, are equal.
> 4. The entity called 1 is not the correspondent of any [entity].
> 5. And finally it shall be the class common to all the classes s which contain the individual 1, and which, if they contain an individual, contain its correspondent.

He goes on to prove the independence of his axioms:

> It is easy to see that these conditions are independent. Of the two first there is no doubt. [Axiom] 3 is not verified for every operation, there being operations (like squaring, integration, *etc.*) that do not satisfy it.
>
> That [axiom] 4 is not a consequence of the previous [axioms] results from the fact that the class of integers, positive, negative and including zero, satisfies the first three and not 4.
>
> To form a class of entities which satisfy [axioms] 1, 2, 3 and 4, but not [axiom] 5, it would suffice to add to the system N another system of entities which satisfies conditions 2, 3 and 4; thus the class formed by the

[14] In his "Notations de logique mathématique," [66].

[15] *Opere scelte*, **3**: 87.

[16] *Cf.* HUBERT C. KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," *Philosophy of Science* (1963) **20**: 262–266. p. 263.

[17] See REID, *Hilbert*, p. 57.

[18] *Opere scelte*, **3**: 87. *Cf.* JOURDAIN, "The Development of the Theories of Mathematical Logic," *The Quarterly Journal of Pure and Applied Mathematics* (1912) **43**, p. 294.

positive integers N, and by the imaginary numbers having the form $i + N$, namely those obtained by adding to the imaginary unit a positive integer, satisfies the conditions preceding 5, but not the latest.

In this article PEANO develops his system just as he did in his *Principles of Arithmetic* and on other occasions. He defines addition, zero and inversion, the integers, product and powers, division and finally, he produces rational and real number.[19] He acknowledges DEDEKIND's priority: "Among the various methods to define the irrational numbers, the most interesting, in my opinion, is that proposed by DEDEKIND in his booklet *Stetigkeit und irrationale Zahlen*."[20]

Authors repeatedly emphasize the similarity between PEANO's and DEDEKIND's treatment of number, raising the hoary and unimportant question of priority.[21] What is the relation between PEANO's and DEDEKIND's systems? It is true that in his *Was sind und was sollen die Zahlen*, DEDEKIND practically formulated PEANO's axioms one year before PEANO; but as PEANO himself modestly points out, there is a slight difference between DEDEKIND, who defines number according to the conditions it satisfies, and PEANO, who does not want to define number and only states its basic properties.[22] Let us now examine this relation more closely.

Dedekind's Concept of Number

To understand DEDEKIND's concept of number one must first understand his particular terminology:[23]

DEDEKIND uses the symbol "϶" to express enclosure of systems: $A϶S$ means that a system A is part of a system S, *i.e.* that every element of A is also an element of S (§. 3 in *Was sind und was sollen die Zahlen*). *Transformation* [*Abbildung*] ϕ of a system S, is a law according to which to every determinate element s of S there belongs a determinate thing which is called the *transform*

[19] For a presentation of these developments see KENNEDY, "Peano's Concept of Number," pp. 389–395.

[20] *Opere scelte*, **3**: 105.

[21] The question of priority between PEANO and DEDEKIND has been much debated. HAO WANG, in "The Axiomatization of Arithmetic," says (p. 149) that "Peano borrowed his axioms from Dedekind." A similar, though milder, opinion is expressed in GILLIES, *Frege, Dedekind, and Peano*, p. 66. KENNEDY, in his "The Origins of Modern Axiomatics," p. 135, on the other hand, claims PEANO's priority. But PEANO's remark, quoted by both WANG and KENNEDY, renders the whole debate superfluous. In any case it clears PEANO from any alleged plagiarism; If PEANO based himself on DEDEKIND he fully acknowledged him. If he did not, as KENNEDY claims, he displayed a remarkable generosity.

[22] *Opere scelte*, **3**: 88.

[23] I rely on or quote from DEDEKIND, *Essays on the Theory of Numbers*, trans. WOOSTER WOODRUFF BEMAN.

of s and denoted by $\phi(s)$ (§. 21). A transformation ϕ of a system S is said to be *similar* [*ähnlich*], when to different elements a, b of the system S there always correspond different transformation $a' = \phi(a)$, $b' - \phi(b)$ (§. 26). The systems R, S are *similar* when there is such a similar transformation ϕ of S that $\phi(S) = R$ (§. 32). Given a transformation ϕ of S into itself, a part K of S is called a *chain* [Kette] if $K' \ni K$ (§. 37). If A is any part of S, A_0 denotes the community of those chains (*e.g.*, S) of which A is part (§. 44). And a system S is said to be *infinite* when it is similar to a proper part of itself (§. 64). The notion "chain" is of central importance. DEDEKIND uses it to prove his Theorem of Complete Induction (§. 59):

> In order to show that the chain A_0 is part of any system Σ — be this latter part of S or not — it is sufficient to show,
> ρ. that $A \ni \Sigma$, and
> σ. that the transform of every common element of A_0 and Σ is likewise element of Σ.

In §. 71 DEDEKIND presents, under the title *Simply Infinite Systems. Series of Natural Numbers*, the following definition of number:

> A system N is said to be *simply infinite* when there is a similar transformation ϕ of N in itself such that N appears as chain of an element not contained in ϕ(N). We call this element, which we shall denote in what follows by the symbol 1, the *base-element* of N and say the simply infinite system N is *set in order* [*geordnet*] by this transformation ϕ. If we retain the earlier convenient symbols for transforms and chains then the essence of a simply infinite system N consists in the existence of a transformation ϕ of N and an element 1 which satisfy the following conditions, α, β, γ, δ:
> α. $N' \ni N$
> β. $n = 1_0$
> γ. The element 1 is not contained in N′
> δ. The transformation ϕ is similar.
> Obviously it follows from α, γ, δ that every simply infinite system N is actually an infinite system because it is similar to a proper part N′ of itself.

The above presentation is indeed very similar to PEANO's system of axioms. But the two systems were constructed for different purposes, which makes all attempts to look for priority redundant. In §. 73, DEDEKIND says:

> Definition. If in consideration of a simply infinite system N set in order by a transformation ϕ we entirely neglect the special character of the elements; simply retaining their distinguishability and taking into account only the relations to one another in which they are placed by the order-setting transformation ϕ, then are these elements called *natural numbers* or *ordinal numbers* or simply *numbers*, and the base element 1 is called the *base-number* of the *number-series* N. With reference to this

> freeing the elements from every other content (abstraction) we are justified in calling numbers a free creation of the human mind. The relations or laws which are derived entirely from conditions α, β, γ, δ in (71) and therefore are always the same in all ordered simply infinite systems, whatever names may happen to be given to individual elements, form the first object of the *science of numbers* or *arithmetic.*

DEDEKIND, in other words, defines, or at least attempts to define, natural number by presenting a set theoretical structure which has the properties of natural numbers, and then obtains natural numbers by what he calls "mental abstraction." Such a development may not be an entirely explicit definition of natural number,[24] but it goes beyond PEANO. A most vivid comparison between the two is made by RUSSELL in his *The Principles of Mathematics* (1903):[25]

> Dedekind *proves* mathematical induction, while Peano regards it as an axiom. This gives Dedekind an apparent superiority, which must be examined.

RUSSELL explains:

> As regards the proof of mathematical induction, it is to be observed that it makes the practically equivalent assumption that numbers form the chain of one of them. Either can be deduced from the other, and the choice as to which is to be an axiom, which a theorem, is mainly a matter of taste. On the whole, though the consideration of chains is most ingenious, it is somewhat difficult, and has the disadvantage that theorems concerning the finite class of numbers not greater than *n* as a rule have to be deduced from corresponding theorems concerning the infinite class of numbers greater than *n*. For these reasons, and not because of any logical superiority, it seems simpler to begin with mathematical induction. And it should be observed that, in Peano's method, it is only when theorems are to be proved concerning *any* number that mathematical induction is required ... In Dedekind's method, on the other hand, propositions concerning particular numbers, like general propositions, demand the consideration of chains. Thus there is, in Peano's method, a distinct advantage of simplicity, and clearer separation between the particular and general propositions of Arithmetic. But from a purely logical point of view, the two methods seem equally sound.

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[24] RUSSELL, as a matter of fact, ridiculed DEDEKIND's procedure, in *Introduction to Mathematical Philosophy*, p. 71, calling it the method of "postulating," and saying that its advantages "are the same as the advantages of theft over honest toil." As GILLIES explains (*Frege, Dedekind* ..., p. 61), by "honest toil" RUSSELL meant giving to numbers explicit definitions in terms of set.

[25] §. 241.

Although PEANO and DEDEKIND belonged to the same rigorist tradition, there were two main differences between their systems of axioms.[26] The first difference is fundamental: Whereas DEDEKIND attempted to define mathematically the concepts of both number and induction, PEANO regarded the two concepts as axiomatic. The second difference is technical and stated by RUSSELL: PEANO's system is simpler. All this does not exclude the possibility that PEANO "borrowed" his axioms from DEDEKIND, but there are all the connotations of a simultaneous discovery (CASSINA used to say that PEANO was a loner in his work and did not blindly follow others' teaching, preferring to elaborate everything anew by himself).[27] Considering that PEANO himself acknowledged DEDEKIND's priority, to discuss the question of priority further becomes redundant.

What has been said about DEDEKIND, as far as PEANO is concerned, can be repeated, even more emphatically, regarding FREGE. In his *Grundlagen der Arithmetik* FREGE states some principles that are analogous to DEDEKIND's conditions α, β, γ, δ.[28] For example, FREGE says that

> The relation of *m* to *n* which is established by the proposition
> "*n* follows in the series of natural numbers directly after *m*"
> is a one-one relation.[29]

As DONALD GILLIES remarks, this "corresponds to Dedekind's condition δ, and also that every number except 0 is a successor of a number, which contains Dedekind's condition γ (with 0 instead of 1, since Frege begins the sequence of natural numbers with 0)." But FREGE explicitly defines natural number, successor, 0, 1 in terms of logic. Thus the principles involving them become propositions which he has to prove, which takes him even further than PEANO.[30]

PEANO was not interested in a study of the foundations. He claimed that number was an undefinable concept, at least in terms of deductive logic, and used it as a corner-stone to build his system of axioms and of arithmetic. PEANO desired to achieve rigor in order to present mathematics in as clear and flawless a manner as possible. His "didactic" orientation was even more explicitly expressed in an indicative sentence in "On the Concept of Number." Taking as

[26] A comparison between PEANO, DEDEKIND and FREGE is presented in GILLIES, *Frege, Dedekind, and Peano*, Chapter 10, by AIMONETTO, "Il concetto di numero naturale in Frege, Dedekind e Peano," and BORGA, "La logica . . . ," pp. 105–122.

[27] See CASSINA, "L'opera scientifica di Giuseppe Peano," pp. 324–325; "L'oeuvre philosophique de G. Peano," p. 482.

[28] §. 78. I rely here heavily on GILLIES, *Frege, Dedekind, and Peano*, p. 61.

[29] AUSTIN's translation, p. 91[e].

[30] GILLIES, *Frege, Dedekind, and Peano*, p. 61. For a comparison between PEANO and FREGE see PAOLO FREGUGLIA, "Frege et Peano: Affinités et différences," *Epistemologia* 4 (1981): 56–76. For a broad comparison between PEANO's logic and that of his contemporaries see CELLUCI, "Gli scopi della logica matematica."

an example the "Euclidian" definition of number as an aggregate of several units, he says:

> "A child, at a young age, uses the words, *one*, *two*, *three*, *etc*.; later he uses the word *number*; only much later does the word *aggregate* appear in his vocabulary. And as philology teaches us, the development of these words occurs in the same order in the Arian languages. Thus, from the practical point of view, the question seems to me to be solved; namely, there is no advantage, in teaching, in giving a definition to number. This idea is very clear to the pupils, and any definition would only confuse it."[31]

All this seemed to PEANO quite simple. But for the reader of PEANO's work, there remains one puzzle: it concerns the "number of a class."

The Number of a Class

In "On the Concept of Number," after having defined the various operations on entire numbers, PEANO deals with "enumeration," and defines "num*a*," "the number of individuals of the class *a*."[32] The formal definition is as follows:

1. $a\,\varepsilon\,\mathrm{K}\,.\,\Im : \mathrm{num}\,a = 0\,.=.\,a = \Lambda$
2. $a\,\varepsilon\,\mathrm{K}\,.\,m\,\varepsilon\,\mathrm{N} : \Im :: \mathrm{num}\,a = m\,.=\therefore\ a - = \Lambda : x\,\varepsilon\,a\,.\,\Im_x\,.\ \mathrm{num}\,(a - \iota x) = m - 1.$

[ι, iota means "equal;" ιa means equal to a and represents the class of entities equal to a; ε means "is;" so $\varepsilon\iota$ means "is equal," and writing $b\,\varepsilon\,\iota a$ means $b = a$].

Expressed in words, this would be as follows:

1. If a is a class, saying the number of a is 0 means saying that the class is empty.

2. If a is a class, saying the number of a's is a number m, means that the class is not empty and that, taking any individual x in the class a, the number of a's not equal to x is $m - 1$.

According to RODRÍGUEZ-CONSUEGRA, who has recently studied the influence of PEANO on RUSSELL, there is here, after all, a beginning of what he calls "logicist arithmetic," namely a first definition of "number" in logical terms.[33] "However," says RODRÍGUEZ-CONSUEGRA, "though Peano afterwards offers several propositions about what he calls 'enumeration,' including the operations $+$ and $<$ between the numbers of a class, the sum of the logical product of classes, and the univocal correspondence, it seems he does

[31] *Opere scelte*, **3**: 84.
[32] *ibid.*, **3**: 100.
[33] RODRÍGUEZ-CONSUEGRA, *The Mathematical Philosophy of Bertrand Russell*, §. 3.2.4.

not yet realize that the equality between two numbers of a class can be expressed by means of a univocal correspondence."[34]

So much for PEANO's "number of a class." I discuss it further in the next chapter, when I compare PEANO's and RUSSELL's views concerning the definition of number.

All PEANO's efforts in the following years were oriented towards expressing all mathematical theorems in terms of his rigorous symbolism, and developing a language which would permit scientists to communicate better.[35]

The Formulario *Project*

PEANO ends his article on the concept of number with these words: "I have thereby presented those methods to treat the foundations of arithmetic which, in my opinion, are best. I will see that from now on the reader will be informed concerning new studies which will be published on this subject."[36] The idea was to *present* the foundations, not to enquire into their nature. He then hints at the next step — the *Formulario* project — publishing a collection of all possible mathematical theorems presented with the aid of the notations in mathematical logic:

> It would also be very useful to collect all the known propositions referring to certain parts of mathematics, and publish these collections. Limiting ourselves to arithmetic, I do not believe there would be any difficulty in expressing them in logical symbols. Then, besides *acquiring precision*, they would be concise, so much so, probably, that the proposition referring to certain subjects in mathematics could be contained in a number of pages not greater than that required for the bibliography.
>
> The transformation into symbols of propositions and proofs expressed in the ordinary form is often an easy thing. It is a very easy thing when treating propositions of the more accurate authors, who have already analyzed their ideas. It is enough to substitute, in the works of these authors, for the words of ordinary language, their equivalent symbols. Other authors present greater difficulty. For them one must completely analyze their ideas and then translate into symbols. Not rarely it is the

[34] *ibid.*, p. 111.

[35] In the preface (written in French) to the first edition of his *Formulario* of 1895 [71], p. III, PEANO drew up a series of rules telling how the *Formulario* should be compiled. The first rule was: "The only law which regulates the notations of the *Formulaire* is that they should be the *most simple and the most precise*, in order to represent the proposition concerned." (My italics.) The quotation speaks for itself.

[36] *Opere scelte*, **3**: 109.

case that a pompously stated proposition is only a logical identity or a preceding proposition, or a form without substance.

The *Rivista di Matematica* will try in the coming year to publish collections of this type. Hence, we invite readers to write them and to send them to us.[37]

It is in connection with his *Formulario* project that PEANO gave his explicit view of the role of mathematical logic. In two letters (to the best of my knowledge, unpublished) written in 1894 to FELIX KLEIN, he announced his project and said:

Mathematical logic, with a very limited number of signs (7 are used, and they are still reducible among themselves), was able to express all the conceivable logical relations between classes and proportions; or, to put it better, the analysis of these relations has brought into use those signs, with which everything is expressed, even the most complicated relations, that can hardly and tediously be explained with ordinary language.

Its advantage, however, is not limited to simplifying writing. It is particularly useful in analyzing the ideas and the reasoning made in mathematics.[38]

In the second letter, PEANO stated his views even more explicitly:

The role of mathematical logic is to analyze and develop the ideas and the reasoning which are represented especially by mathematical sciences. The analysis of the ideas allows one to find the fundamental ideas, with which all other ideas are expressed; and to find the relations between the various ideas, namely the logical identities, which are many forms of reasoning. The analysis of the ideas leads also to denoting the simplest ones by means of conventional signs, which, if conveniently combined, then represent the composed ideas.[39]

This can be taken as a summary of PEANO's philosophy and approach to mathematics.[40] By now the central principles of his mathematics had achieved

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[37] *ibid.* Translation from KENNEDY, *Peano*, p. 44. My italics.

[38] Letter of PEANO to FELIX KLEIN, 29 August 1894. Niedersächsische Staats- und Universitätsbibliothek, Göttingen, Cod. Ms. KLEIN, 11, 190/2.

[39] Letter of PEANO to FELIX KLEIN, 19 September 1894. Niedersächsische Staats- und Universitätsbibliothek, Göttingen, Cod. Ms. KLEIN, 11, 190A.

[40] For PEANO's general philosophical approach to mathematics and linguistics, see GABRIELE LOLLI, "Quasi Alphabetum: Logic and Encyclopedia in G. Peano," *Atti del Convegno Internazionale di Storia della Logica, San Gimignano, 4–8 dicembre* 1982, edited by V. M. ABRUSCI, E. CASARI, M. MUGNAI (Bologna: CLUEB, 1983), pp. 133–155.

their main formulation, and his chief concern became the spreading of his ideas through the *Rivista di Matematica* and the *Formulario*.[41]

The axioms, too, were later subject to minor changes only (*inter alia* by introducing 0 instead of 1). Let me quote again here the final formulation of PEANO's axioms:

1. Zero is a number.
2. The successor of any number is another number.
3. There are no two numbers with the same successor.
4. Zero is not the successor of a number.
5. Every property of zero, which belongs to the successor of every number with this property, belongs to all numbers.[42]

If one looks at the selection of PEANO's works (*Opere scelte*) edited by CASSINA, one notices a change which began in 1891: from then on PEANO's work became increasingly repetitious.[43] There remains, however, one topic already mentioned, related to the definition of number, which PEANO treated later, in 1894: definition by abstraction.

Definition by Abstraction

In 1894 PEANO published a small book, *Notations de logique mathématique* [66], as an introduction to his *Formulario*. In it he dealt with the concept of mathematical definition and presented his definition "by abstraction."[44] "There are ideas," he remarked, "that are obtained by abstraction and that continually enrich the mathematical sciences."[45]

"Let u be an object; one deduces by abstraction a new object ϕu; one can form the equality

$$\phi u = \text{a known expression},$$

since ϕu is an object of different nature from all those which have been considered up to now."[46]

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[41] For a history of the the *Formulario*, see CASSINA, "Storia e analisi del 'Formulario completo' di Peano."

[42] From KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," p. 262. This formulation was given by PEANO in 1898 in his *Formulaire*: see abstract in *Opere scelte*, **3**: 215–231, p. 216. On this occasion PEANO repeated that he was speaking of general systems that satisfy the same properties of numbers, rather than of numbers only: see p. 218.

[43] As CASSINA explicitly says, in 1891 PEANO had completed the first cycle of studies in mathematical logic: "Storia ed analisi del 'Formulario completo' di Peano," p. 246.

[44] *Opere scelte*, **2**: 166–176.

[45] *ibid.*, p. 167.

[46] *ibid.*, pp. 167–168.

Let v be another object, for which ϕ also holds. If ϕ is transitive, symmetrical and (within the field) reflexive, then $\phi u = \phi v$.

Symmetry, reflexitivity and transitivity are defined respectively as follows:

1. $\phi u = \phi u$,
2. $\phi u = \phi v \,.\supset.\, \phi v = \phi u$,
3. $\phi u = \phi v \,.\, \phi v = \phi w \,.\supset.\, \phi u = \phi w$.

PEANO does produce a few examples, but that is more or less all he says about definition by abstraction at that period; in any case he does not explain how to apply it to the concept of number.[47] PEANO's definition was explained and further developed by RUSSELL in *The Principles of Mathematics.*[48] The legitimacy of PEANO's process, says RUSSELL, "requires an axiom, namely the axiom that if there is any instance of the relation in question, then there is such an entity as $\phi(u)$ or $\phi(v)$." This axiom is RUSSELL's principle of abstraction:

> Every transitive symmetrical relation, of which there is at least one instance, is analyzable into joint possession of a new relation to a new term, the new relation being such that no term can have this relation to more than one term, but that its converse does not have this property.

This is all that can be said concerning PEANO's definition of abstraction at this stage, and basically his view remained that number cannot be defined.[49] Bearing this in mind, we can begin to evaluate his work in its historical perspective.

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[47] PEANO later wrote a series of articles concerning definitions, and, in 1915, one article entirely devoted to definition by abstraction [177]. These articles nevertheless add little to the understanding of his concept of natural number. As a matter of fact, in that article he said: "One thus defines the equality of two numbers, and not number itself; and this because this definition may be placed before arithmetic, and also because the number that results is not the finite number of arithmetic." *Opere scelte,* **2**: 404. Translation from KENNEDY, "Peano's concept of number," p. 403.

[48] §. 210.

[49] For more on the principle of abstraction by PEANO and RUSSELL, see EUGENIO MACCAFERRI, "Le definizioni per astrazione e la classe di Peano," *Rendiconti del Circolo Matematico di Palermo,* tomo **35** (I semestre 1913): 165–171.

Chapter 8: Peano and Foundationism

We have now reached a point where we can begin evaluating the more general, historical meaning of PEANO's contribution, with particular attention to his axioms. Different opinions have been expressed on PEANO: Most historians agree that he was a great mathematician whose contribution was vital to the foundations of modern mathematics. Some suspect him of having been an opportunist, at times even a plagiarist, who added little to the work done by his contemporaries.[1] One must bear in mind, however, that PEANO's work has been considered mainly in relation to that of contemporary mathematicians such as DEDEKIND or RUSSELL, or studied by historians having particular views on mathematics who judged him by the degree of his compliance with their own views.[2] The previous chapters attempted to consider PEANO in his own context, in the light of his own aims and the tradition he created or professed to belong to. We can now try to reassess the general appraisal of PEANO.

To understand the context of PEANO's work, one has to consider the evolution of the major mathematical trends of his day.[3] PEANO, as we have seen, belonged to the rigorist tradition whose origins were discussed at the beginning of this work. In the second half of the nineteenth century, this tradition led to growing inquiry into the foundations of mathematics: WEIERSTRASS, DEDEKIND and CANTOR are the best-known representatives of this trend. Yet though all

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[1] KENNEDY says that PEANO was a mathematical opportunist who concentrated on topics from which he got immediate results (KENNEDY, *Peano*, p. 175); This view is shared by GRATTAN-GUINNESS in "From Weierstrass to Russell," p. 14. HAO WANG, in "The Axiomatization of Arithmetic" (p. 149), says that "PEANO borrowed his axioms from DEDEKIND." RODRÍGUEZ-CONSUEGRA, in "Elementos logicistas en las obra de Peano y su Escuela," p. 221, lists a series of criticisms.

[2] A typical example is JOURDAIN's "The Development of the Theories of Mathematical Logic." In the second part (1912, p. 313), JOURDAIN finds three "great defects" in PEANO's system: "the absence of a thorough investigation of the principles of logic, the confusion of distinct ideas because of interesting and important analogies between them, and the continual use of definitions under hypothesis." JOURDAIN may be partially right, but his judgement is typical of a logicist mathematician and somewhat anachronistic, although it is true that PEANO made no attempt to revise his views, or at least express his opinions, after the "crisis of foundations." Another, more recent, example is CARL G. HEMPEL, "On the Nature of Mathematical Truth," in JAMES R. NEWMAN (ed.) *The World of Mathematics*, Vol. 3 (London: George Allen and Unwin, 1956), pp. 1619–1634. HEMPEL (pp. 1627–1628) credits FREGE and RUSSELL with the important result of having shown that "for the concepts so defined [*i.e.* "0," "natural number," and "successor,"], all PEANO postulates turn into true statements." HEMPEL disregards the fact that PEANO regarded these concepts as undefinable.

[3] For an outline of these trends and their historical roots see EVANDRO AGAZZI, "The Rise of the Foundational Research in Mathematics," *Synthese* **27** (1974): 7–26. See also BECKER, *Grundlagen der Mathematik*, Chapter 5; VOLKERT, *Die Krise der Anschauung*, Part 2.

foundationists were rigorists, each of them had his own aims, methods and philosophy, and three major schools of thought emerged at the beginning of the present century:

(i) The Logicist school, fostered mainly by RUSSELL and WHITEHEAD, claiming that pure mathematics is a branch of logic.

Two other schools, less easy to define, are:

(ii) The Formalist school, originated by HILBERT, claiming, in general, that pure mathematics consists merely of formal expressions or symbols which are manipulated or combined according to preassigned rules or agreements. The Formalist mathematicians searched for consistency and completeness in mathematics rather than the meaning of its expressions.[4]

(iii) The Intuitionist school, originated by L. E. J. BROUWER (1881–1966), holding that it is truth rather than consistency that matters in mathematics. This is an oversimplification, of course: WALTER VAN STIGT's recent and outstanding study of BROUWER's Intuitionism shows how complex Intuitionism is — like the character of its founder.[5] (The term is also somewhat misleading since Intuitionism is not concerned with the role of intuition in ordinary mathematical work, but rather with the nature of the foundations of mathematics.)

Several authors have already pointed out that, contrary to the general opinion, PEANO was not a logicist.[6] In the present chapter I shall try to show that PEANO's approach to mathematics contains elements from each of the above three schools, without, however, belonging specifically to any one of them. PEANO's contribution anticipated the above schools (though only RUSSELL openly admitted it), but it would be hard to identify it with any one of them in particular. PEANO shared the general metaphysical approach of the foundationists.

Peano and the Logicist Tradition

PEANO, as was noted in chapter 3, greatly impressed RUSSELL during the International Congress of Philosophy in 1900 in Paris. After the Congress, RUSSELL set out to learn PEANO's symbolism and wrote an article endeavoring to give a simple presentation of the logic of relations using this

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[4] I have relied on the definition given by DAVIS & HERSH, *The Mathematical Experience*, p. 413.

[5] See also DIRK VAN DALEN, "Brouwer: The Genesis of Intuitionism," *Dialectica* **32** (1978): 291–303. MAX BLACK, *The Nature of Mathematics*. These presentations do not, however, consider the historical development of Intuitionism.

[6] *e.g.* KENNEDY, "The Mathematical Philosophy of Giuseppe Peano;" GILLIES, in *Frege, Dedekind, and Peano*, Chap. 10; and FRANCISCO RODRÍGUEZ-CONSUEGRA, "Russell's Logicist Definitions of Numbers, 1893–1913: Chronology and Significance," *History and Philosophy of Logic* (1987) **8**: 141–169, p. 142.

symbolism (it is in this article that RUSSELL defined cardinal number as class of classes). The article was published in 1901 in PEANO's *Rivista di Matematica*, after which disagreements between the two mathematicians began to emerge.[7]

In the *Formulaire de Mathématique*, published in the same year, PEANO gave a general definition of "number of a class," Num$a$ (a concept that we have already treated in Chapter 7), also for infinite numbers. Under the title "Num Infn," he writes:[8]

$$f\,\mathrm{rcp} \quad \cdot 0 \quad a, b \varepsilon \mathrm{Cls} \cdot \supset : \mathrm{Num}a = \mathrm{Num}b \;. = .\; \exists\, (b\, f\, a)\,\mathrm{rcp}$$

(in PEANO's symbolism: "f" means "function,"[9] "rcp" means "reciprocal correspondence."[10]) PEANO explains:

> "Num$a$," means "the number (numerus) of a's." One calls it also "power (Mächtigkeit) of a," notably if the class a is infinite.

Of great importance is PEANO's remark that

> The definition $\cdot 0$ is expressed only by signs of logic. One can start arithmetic here: we shall define directly the signs > 0 N_0 $+$ $\times$ $\upharpoonleft$ [the last sign means elevated to the power[11]], without going through the primitive ideas of §. 20.

(The primitive ideas presented in §. 20 are those of zero, number, and successors. In other words, PEANO here seems to contradict what he had said previously, that these three cannot be defined.) PEANO goes on to comment that proposition $\cdot 0$ defines the quality "Num$a$ = Num$b$," that subsists if one can establish a reciprocal correspondence between a and b. And, most important, that this is a definition by abstraction (*cf.* Chapter 7 above): in other words, what PEANO seems to say is that zero, number and successor can be defined by abstraction.

But then he says:

> Given a class a, we can consider the class of classes:
>
> $$\mathrm{Cls} \cap x \ni [\exists\, (x\, f\, a)\mathrm{rcp}];$$
>
> the equality of this Cls of Cls, calculated on classes a and b, involves the equality Num a = Num b; but we cannot identify Num a with the Cls of all Cls considered, for *these objects have different properties.*

This last sentence is a step back to the previous claim that number cannot be defined; it naturally disappointed RUSSELL who, in his *Principles of Mathematics*,

7 RUSSELL, "Sur la logique des relations, avec des applications à la théorie de séries."
8 [110], §. 32 (p. 70).
9 *ibid.*, §. 10.
10 *ibid.*, §. 13.
11 *ibid.*, §. 25.

remarked that PEANO "does not tell us what these properties are, and for my part I am unable to discover them."[12]

Several authors suggest that PEANO was implicitly logicist,[13] and KENNEDY also points out that with the publication of WHITEHEAD and RUSSELL's *Principia Mathematica*, PEANO even softened his views and was more willing to concede to them the "class of classes" definition.[14] Yet the fact remains that PEANO was never a declared logicist, particularly not in the period in which he formulated his axioms. And, in fact, RUSSELL was careful enough not to consider him a logicist. In *The Principles of Mathematics* he only acknowledged PEANO's method as "a powerful instrument of mathematical investigation."[15] This description is very apt, since PEANO's system was undoubtedly powerful, though always remaining at the level of a mere instrument.

What then, was PEANO's contribution to mathematical thought, according to RUSSELL? His contribution was on several levels:[16]

(i) In metaphysics, PEANO helped to ban intuition from mathematics and led the way to strictly logical deduction, which RUSSELL believed to be the only true method in mathematics; in RUSSELL's own words:

> It seemed plain that mathematics consists of deductions, and yet the orthodox accounts of deduction were largely or wholly inapplicable to existing mathematics. Not only the Aristotelian syllogistic theory, but also the modern doctrines of Symbolic Logic, were either theoretically inadequate to mathematical reasoning, or at any rate required such artificial forms of statement that they could not be practically applied. In this fact lay the strength of the Kantian view, which asserted that mathematical reasoning is not strictly formal, but always uses intuitions, *i.e.* the *a priori* knowledge of space and time. Thanks to the progress of Symbolic Logic, especially as treated by Professor Peano, this part of the Kantian philosophy is now capable of a final and irrevocable refutation. By the help of ten principles of deduction and ten other premisses of

[12] §. 111. *Cf.* RODRÌGUEZ-CONSUEGRA, *The Mathematical Philosophy of Bertrand Russell*, p. 113.

[13] AIMONETTO, "Il concètto di numero naturale in Frege, Dedekind e Peano," p. 605. RODRÍGUEZ-CONSUEGRA, *The Mathematical Philosophy of Bertrand Russell*, p. 113. KENNEDY, too, remarks that "Significantly... Peano does identify the finite cardinal number with the natural numbers." KENNEDY, "Peano's Concept of Number," p. 402. To the question, what was PEANO's objection to the "class of classes?" KENNEDY answers that "Most probably it was the artificiality of this concept as opposed to what Peano saw as the 'natural' concept of number" (the same view is also expressed by AIMONETTO).

[14] *ibid.*, p. 403.

[15] RUSSELL, *The Principles of Mathematics* (2nd ed.), p. xvi.

[16] RUSSELL refers to the later writings of PEANO, mostly after 1897.

a general logical nature (*e.g.* "implication is a relation"), all mathematics can be strictly and formally deduced.[17]

(ii) In logic, PEANO's major contribution was to distinguish clearly between the relation of a term to a class of which it is a member (ε) and the relation of inclusion between classes.[18] In general, RUSSELL considered PEANO's formal treatment of classes as one of the best of its time (perhaps with the exception of FREGE).[19]

(iii) In mathematics, PEANO contributed in several fields, in particular the theory of numbers: Together with DEDEKIND and CANTOR he showed how to base all arithmetic and analysis upon series and properties of finite numbers.[20]

RUSSELL admits, however, that PEANO and his followers

> hold that the various branches of Mathematics have various indefinables, by means of which the remaining ideas of the said subjects are defined. I hold . . . that all Pure Mathematics (including Geometry and even rational Dynamics) contains only one set of indefinables, namely the fundamental logical concepts discussed in Part I.[21]

RUSSELL, to sum up, considered PEANO a mathematician who contributed considerably to facilitating the process of reducing mathematics to logic by showing that the entire theory of natural numbers could be derived from a certain number of primitive ideas and propositions in addition to those of pure logic.[22] In his later *Introduction to Mathematical Philosophy* (1919), he even says that PEANO's work "represents the last perfection of the 'arithmetization' of mathematics."[23]

Yet RUSSELL regarded PEANO's axioms as unsatisfactory since they did not characterize the natural numbers but only a more general concept, that of a progression of any objects whatsoever, a progression which has a first member, contains no repetition, and for each member of which there is an immediate successor. This was RUSSELL's major criticism of PEANO, expressed more explicitly in his *Introduction to Mathematical Philosophy*. Speaking of PEANO's three primitive ideas (*i.e.*, "number," "0," and "successor"), he complains:

> In Peano's system there is nothing to enable us to distinguish between these different interpretations of his primitive ideas. It is assumed that we know what is meant by "0," and that we shall not suppose that this symbol means 100 or Cleopatra's Needle or any of the other things that it might mean.

[17] RUSSELL, *The Principles of Mathematics*, §. 4.
[18] *ibid.*, Chap. 6, in particular §. 76–77.
[19] *ibid.*, §. 69.
[20] *ibid.*, §. 187.
[21] *ibid.*, §. 108.
[22] RUSSELL, *Introduction to Mathematical Philosophy*, p. 5.
[23] p. 7.

> This point, that "0" and "number" and "successor" cannot be defined by Peano's five axioms, but must be independently understood, is important. We want our numbers not merely to verify mathematical formulae, but to apply in the right way to common objects. We want to have ten fingers and two eyes and one nose. A system in which "1" meant 100, and "2" meant 101, and so on, might be all right for pure mathematics, but would not suit daily life.[24]

RUSSELL appeals to FREGE's definition of number as the completion of the process of "logicizing" mathematics.

So much for RUSSELL on PEANO. What was PEANO's attitude to Logicism?

Logicism did not appeal to PEANO. In his review of FREGE's *Grundgesetze der Arithmetik*, in the 1895 *Rivista di Matematica* [82], he dealt mainly with FREGE's ideography, and, as far as FREGE's Logicism was concerned, argued that FREGE was reasoning in a vicious circle.[25] He had the same attitude to RUSSELL's work, in which he praised, again, the use of ideography.[26]

For PEANO, logic remained a mere instrument, its main task being to make sure that the steps of a mathematical proof should follow each other without error. This was to be done with the help of an appropriate ideography.[27] He used logic extensively, but did not contend that mathematics could be reduced to logic.

PEANO was satisfied with a certain number of axioms and did not bother to consider their philosophical meaning. One of PEANO's pupils, LUDOVICO GEYMONAT, recalled that whenever he asked PEANO about RUSSELL's (philosophical) objection to PEANO's theory of numbers, he "preferred to take refuge in joking or evasive words, saying that these were philosophical questions

[24] *ibid.*, p. 9

[25] On the relations between PEANO and FREGE, see KENNEDY, *Peano*, Chapter 10.

[26] Upon receipt of RUSSELL's *The Principle of Mathematics*, PEANO wrote a short letter to RUSSELL, thanking him and saying: "I would very much like to talk at length about your book, which marks an epoch in the field of philosophy of mathematics, but I have to travel around Italy." See KENNEDY, "Nine Letters from Giuseppe Peano to Bertrand Russell," p. 207. As KENNEDY points out, PEANO's remark was high praise; it seemed however to derive more from PEANO's generosity than from his agreement with RUSSELL.

[27] This is argued, *inter alia*, by LEVI in his obituary of PEANO, "L'opera matematica di Giuseppe Peano," pp. 260–262. *Cf.* B. LEVI, "Intorno alle vedute di G. Peano circa la Logica matematica," pp. 66–67. LEVI belittled the role of logic in mathematics, and criticized PEANO for having even considered logic as an instrument of mathematics. LEVI said that mathematics needs much more, and logic can, at most, supply an ideography. On LEVI's criticism of PEANO see LOLLI, "I critici italiani di Peano: Beppo Levi e Federigo Enriques," in *Peano e i Fondamenti della Matematica*, pp. 51–71.

about which he was 'absolutely incompetent'."[28] PEANO repeated to GEYMONAT what he had already previously asserted, namely that there was no need to define number. "Number (positive integer) cannot be defined (seeing that the ideas of order, succession, aggregate, etc., are as complex as that of number.)"[29]

It is possible that PEANO was not surprised by the crisis which overtook the logicistic program after RUSSELL discovered a series of antinomies in logic and set theory.[30] RUSSELL and FREGE attempted to avoid these antinomies through further study of logic, an attempt which unfortunately complicated things even more. PEANO, for his part, made no attempt to rescue Logicism from this crisis.

PEANO's refusal to deal with the essence of number and his extensive use of symbols, leads one also to consider his relation with Formalism.

Peano and Hilbert's Formalism

A scheme to overcome the crisis, the so-called Formalistic program, was proposed by HILBERT. It is difficult to describe Formalism in a few words, since HILBERT never gave a straightforward definition of it; it was more or less formulated gradually and given different interpretations.[31]

The origins of HILBERT's Formalism can be found in his famous *Grundlagen der Geometrie* (*The Foundations of Geometry*, 1899), a small book attempting to

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[28] LUDOVICO GEYMONAT, "I fondamenti dell'aritmetica secondo Peano e le obiezioni 'filosofiche' di B. Russell," in TERRACINI (ed.) *In memoria di Giuseppe Peano*, pp. 51–69, on p. 56. The English quotation is from KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," p. 264.

[29] *ibid.*, p. 58; KENNEDY's translation, p. 264.

[30] It certainly did not surprize HENRI POINCARÉ, who, like PEANO, criticized logicism as tautological: see POINCARÉ, "La logique de l'infini," *Revue de Métaphysique et de Morale* **17** (1909): 461–482; "The Latest Efforts of the Logisticians," *The Monist* **22** (1912): 524–539.

[31] For the various stages of HILBERT's work in the foundations of mathematics see MICHELE ABRUSCI's introduction to HILBERT, *Ricerche sui fondamenti della matematica*; see also GREGORY H. MOORE, "The Emergence of First-Order Logic," in ASPRAY & KITCHER, *History and Philosophy of Modern Mathematics*: 95–135, pp. 104–108. For a comparison between RUSSELL & HILBERT see HENRI POINCARÉ, "The New Logics," *The Monist* **22** (1912): 243–256. For a presentation of HILBERT's approach in mathematics see PAUL BERNAYS, "Die Bedeutung Hilberts für die Philosophie der Mathematik," *Die Naturwissenschaften* **10** (1922): 93–99. For a detailed philosophical study of the formulation of HILBERT's program, see PECKHAUS, *Hilbertprogramm und Kritische Philosophie*. For an illustration of some successful aspects of HILBERT's program, see WILFRIED SIEG, "Relative Consistency and Accessible Domains," *Synthese* **84** (August 1990): 259–297.

present geometry in as clear and as logically perfect a manner as possible. In his introduction HILBERT writes:

> The following investigation is a new attempt to choose for geometry a *simple* and *complete* system of *independent* axioms and to deduce from these the most important geometrical theorems in such a manner as to bring out as clearly as possible the significance of the different groups of axioms and the scope of the conclusions to be derived from the individual axioms.[32]

And he begins his book by saying:

> Let us consider three distinct systems of things. The things composing the first system, we will call *points* and designate them by the letters A, B, C, . . . ; those of the second, we will call *straight lines* and designate them by the letters a, b, c, . . . ; and those of the third system, we will call *planes* and designate them by the Greek letters α, β, γ, . . . [33]

The key word in this passage is "things:" HILBERT thinks in terms of general abstract entities, rather than specific objects. This was the first step in a general attempt to show that the standard mathematical proof procedures were, after all, strong enough to suit all classical mathematics. In summer 1900, at the International Congress of Mathematicians in Paris, HILBERT read his famous paper on "Mathematical Problems," in which he presented 23 central problems which were to determine mathematical research.[34] On this occasion, HILBERT spoke of rigor and simplicity in words that are, at least partially, reminiscent of PEANO. He said:

> It remains to discuss briefly what general requirements may be justly laid down for the solution of a mathematical problem. I should say first of all, this: that it shall be possible to establish the correctness of the solution by means of a finite number of steps based upon a finite number of hypotheses which are implied in the statement of the problem and which must always be exactly formulated. This requirement of logical deduction by means of a finite number of processes is simply the requirement of rigor in reasoning. Indeed the requirement of rigor, which has become proverbial in mathematics, corresponds to a universal philosophical necessity of our understanding; . . .

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[32] *The Foundations of Geometry*, p. 1. On the origins of the *Grundlagen* see TOEPELL, *Über die Entstehung von David Hilberts "Grundlagen der Geometrie"*, and "On the Origins of Hilbert's 'Grundlagen der Geometrie'."

[33] *The Foundations of Geometry*, p. 3.

[34] HILBERT's lecture was translated into English by MARY WINSTON NEWSON: "The original appeared in the *Göttinger Nachrichten*, 1900, pp. 253–297, and in the *Archiv der Mathematik und Physik*, 3rd ser., vol. 1 (1901), pp. 44–63 and 213–237" (from p. 437 of "Mathematical Problems"). On this occasion, in an effort to shorten his talk, HILBERT presented only 10 problems. See REID, *Hilbert*, p. 81.

> Besides it is an error to believe that rigor in the proof is the enemy of simplicity. On the contrary we find it confirmed by numerous examples that the rigorous method is at the same time the simpler and the more easily comprehended. The very effort for rigor forces us to find out simpler methods of proof. It also frequently leads the way to methods which are more capable of development than the old methods of less rigor."[35]

The last paragraph, in particular, associating rigor and simplicity, is very similar to PEANO's trend of thought.

In 1901, HILBERT published a short article, "Über den Zahlbegriff" ("On the Concept of Number"), presenting his postulates for real numbers. KENNEDY complains that on this occasion, without naming PEANO, HILBERT labelled PEANO's development of real numbers the "genetic method", while reserving the label "axiomatic method" for his own presentation.[36] The axiomatic method, KENNEDY argues, was mainly PEANO's creation.

A further step in HILBERT's Formalistic program was taken at the Third International Congress of Mathematicians (Heidelberg 1904). HILBERT's paper, "On the Foundations of Logic and Arithmetic," was devoted to the consistency of arithmetic.[37] He criticized some of the contemporary views on arithmetic, mentioning, *inter alia*, FREGE, DEDEKIND, CANTOR, but again (surprisingly), ignoring PEANO. PEANO did not take part in this congress, but the fact that HILBERT did not mention him is still bizarre. HILBERT also criticized Logicism, without mentioning RUSSELL, in words again reminiscent of PEANO:

> Arithmetic is often considered to be a part of logic, and the traditional fundamental logical notions are usually presupposed when it is a question of establishing a foundation for arithmetic. If we observe attentively, however, we realize that in the traditional expositions of the laws of logic certain fundamental arithmetic notions are already used, for example, the notion of set and, to some extent, also that of number. Thus we find ourselves turning in a circle, and that is why a partly simultaneous development of the laws of logic and of arithmetic is required if paradoxes are to be avoided.[38]

The paper is not very clear, though in it HILBERT's overriding concern is for consistency in mathematics. In the years to follow he worked on his general program,[39] though a draft only appeared in 1922, in an important article,

[35] "Mathematical Problems," pp. 440–441.

[36] KENNEDY, "The Origins of Modern Axiomatics," p. 135.

[37] HEIJENOORT, *From Frege to Gödel*, pp. 129–138. The original German version, under the title "Über die Grundlagen der Logik und der Arithmetic," was published in *Verhandlungen des dritten internationalen Mathematiker-Kongresses*, edited by A. KRAZER (Leipzig: Teubner, 1905), pp. 174–185.

[38] HEIJENOORT, *ibid.*, p. 131.

[39] See, in particular, "Axiomatisches Denken," a paper delivered in 1917: HILBERT, *Gesammelte Abhandlungen*, 3: 146–156.

"Neubegründung der Mathematik. Erste Mitteilung" ("The New Foundation of Mathematics. First Article").[40] In this article HILBERT said that

> precisely through the development that I believe I can give to the axiomatic method, we shall be able to see how it leads us, through the principle of inference in mathematics to attain full clarity. As already said, we can of course never be sure of the consistency of our axioms beforehand, so long as we do not prove it.[41]

HILBERT's program consisted of two points:

> 1. All that hitherto has formed the actual mathematics will now be rigorously formalized so that the *actual mathematics*, or mathematics in a stricter sense, becomes the basis for provable formulas. The formulas of this basis will differ from the usual formulas only by the fact that in addition to mathematical symbols they will have the symbol →, the bracket [*Allzeichen*], and the symbols for statements that appear in between.[42]
>
> 2. In addition to this actual mathematics, a partly new mathematics, a metamathematics, comes into being to serve as a protection against the terror of superfluous restrictions and the danger of paradoxes.[43]

(I explain below what HILBERT meant by "superfluous restrictions".)

These points, which express the core of HILBERT's Formalism, were repeated and amplified in later articles.[44] FREUDENTHAL, in his article on HILBERT in the *Dictionary of Scientific Biography*, summarizes HILBERT's Formalism as follows: "To reduce mathematics to a finite game with an infinite but finitely defined treasure of formulas. This game must be consistent; it is the burden of metamathematics to prove that while playing this game, one can never hit the formula $0 \neq 0$. But if a vicious circle is to be avoided, metamathematics must restrict itself to counting beans."[45]

HILBERT's program underwent a variety of further developments in reaction to other contemporary views of mathematics, principally BROUWER's Intuitionist view. It was also developed by a number of mathematicians whose views

[40] *ibid.*, pp. 157–177.

[41] *ibid.*, p. 161.

[42] *ibid.*, p. 174.

[43] *ibid.*

[44] In "Die logischen Grundlagen der Mathematik," ("The Logical Foundations of Mathematics", 1923), for instance, HILBERT added to the sign of implication in the first point, the sign of negation: HILBERT, *Gesammelte Abhandlungen*, **3**: 178–191, p. 179.

[45] *Dictionary of Scientific Biography*, **6**: 393.

differed from those of HILBERT.[46] Here the story becomes complicated: BROUWER believed that mathematical objects could be said to exist only if they could be constructed, in a finite series of steps, starting from natural numbers. (In the early 1920s HILBERT's student HERMANN WEYL (1885–1955) took BROUWER's side.) Many of the standard proofs of mathematics came to be regarded by BROUWER and his followers as invalid. This alarmed HILBERT, who argued that the intuitionists would chop up and mangle science by throwing overboard everything which was troublesome, hence the "superfluous restrictions" mentioned in the quotation above.[47]

How is all this related to PEANO? The interaction between the two mathematicians was scarce: they did not correspond — at least, there are no letters from PEANO in HILBERT's *Nachlass* in Göttingen — and they seldom mention each other in print. In *Grundlagen der Geometrie*, for instance, the only Italian mathematician mentioned was PEANO's critic, GIUSEPPE VERONESE (1854–1917), despite PEANO's important work on the foundations of geometry. Only as late as 1928 did HILBERT acknowledge PEANO's contribution to the creation of a mathematical ideography, at the International Congress of Mathematicians held in Bologna. He said: "You see that an essential tool of my proof theory is the ideography. The classic among all ideographies, that of PEANO, is the one endowed with the most scrupulous precision and the widest scope."[48] (HILBERT may have made this remark only because PEANO was present.) Restricting his acknowledgement to ideography was quite correct and indicated the main relation between the two approaches to mathematical research. PEANO's emphasis was, indeed, on ideography, and he probably agreed with HILBERT's remark.

Nevertheless, the views of the two men had much more in common. Both PEANO and HILBERT sought to guarantee consistency; both had an axiomatic approach; both believed that the properties of objects could be studied by making a system of signs; both regarded numbers as having the simplest structural properties of objects, and both objected to the logicist view that mathematical concepts could be reduced to logical ones. As MICHELE ABRUSCI has recently discovered in HILBERT's papers, in his course of 1917 in Göttingen the latter quoted and praised PEANO's contribution to the axiomatic method.[49]

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[46] HERMANN WEYL, in his obituary of HILBERT, gave a (not very clear) presentation of the latter's formalism, evidently attempting to show that formalism is nearer to WEYL's intuitionism than to logicism; HERMANN WEYL, "David Hilbert and his Mathematical Work," in REID, *Hilbert*, pp. 245–283. See pp. 264–274. A more recent version of formalism is ABRAHAM ROBINSON's "Formalism 64," *Proceedings, International Congress for Logic, Methodology and Philosophy of Science, 1964* (Amsterdam: North Holland, 1965), pp. 228–246.

[47] REID, *Hilbert*, p. 155.

[48] HILBERT, "Probleme der Grundlegung der Mathematik," p. 137.

[49] ABRUSCI, "Peano e Hilbert."

BROUWER, too, considered PEANO a Formalist. He said:

> Because the usual spoken or written languages do not in the least satisfy the requirements of consistency demanded of this symbolic logic, formalists try to avoid the use of ordinary language in mathematics. How far this may be carried is shown by the modern Italian school of formalists, whose leader, Peano, published one of his most important discoveries concerning the existence of integrals of real differential equations in the *Mathematische Annalen* in the language of symbolic logic; the result was that it could only be read by a few of the initiated and that it did not become generally available until one of these had translated the article into German.[50]

BROUWER's view, that PEANO was a Formalist, is at least partially shared by a few historians of mathematics: according to JOHANNES TROPFKE, "Peano formalized Dedekind's axioms," and HELMUTH GERICKE says that "Peano formalized the foundations of Arithmetic."[51] Yet HILBERT's Formalism, which was later than PEANO's, became much more elaborate than the latter's axiomatic approach. HILBERT also criticized PEANO's neglect of the question of the consistency of his axioms and his excessive reliance on ordinary logic.[52]

PEANO, for his part, never spoke of metamathematics; for him (and here, perhaps, one may see some beginnings of Intuitionism) mathematics was more than a "game of symbols." In fact, in the first edition of the *Formulario*, PEANO drew up a series of rules laying down how the *Formulario* should be compiled. The second rule is: "The notations are somewhat arbitrary, but the *propositions are absolute truths, independent of the notations used.*"[53] I doubt whether HILBERT would have shared this view. CASSINA comments: "Peano, whether in Logic or in Mathematics, never worked with pure *symbolism*; *i.e.* he always required that the primitive symbols introduced represent *intuitive* ideas to be explained with ordinary language."[54]

My aim in this chapter was by no means to suggest, as BROUWER did, that PEANO belonged to the Formalist stream. However, despite the differences involved it would not be too far-fetched to say that PEANO's and HILBERT's

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[50] BROUWER, "Intuitionism and Formalism," p. 84 (in *Bulletin of the American Mathematical Society*). *Cf.* L. E. J. BROUWER, "Historical Background, Principles and Methods of Intuitionism," *South African Journal of Science*, **49** (August 1952): 139–146, p. 139. Both articles have been republished in BROUWER's *Collected Works*.

[51] TROPFKE, *Geschichte der Elementarmathematik* Vol. 1, p. 126. GERICKE, *Geschichte des Zahlbegriffs*, p. 130.

[52] ABRUSCI, "Peano e Hilbert," p. 194.

[53] [71] *Formulaire de mathématiques*, tome **1**, p. III. KENNEDY, *Peano*, p. 47. The italics are mine.

[54] CASSINA, "Su la Logica matematica di G. Peano," *Bollettino della Unione Matematica Italiana* (1933) **12**: 57–65, p. 59. Quotation from KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," p. 264.

views grew from the same root, or even to claim that HILBERT had not acknowledged PEANO appropriately. It is no more improbable to speak of PEANO as a precursor of HILBERT than to consider him a precursor of RUSSELL. As in the case of Logicism, one could say that PEANO was neither a formalist nor a non-formalist. (On this occasion the assertion is easier, because Formalism is not so sharply defined.) KENNEDY, at least, is right in asserting that facets attributed to HILBERT, such as simplicity or consistency, already existed in PEANO's axiomatic program. Yet, as KENNEDY himself remarks: "Peano was neither a logicist nor a formalist. He believed rather that mathematical ideas are ultimately derived from our experience of the material world."[55] Let us therefore consider whether PEANO's approach may also have had something in common with Intuitionism.

Peano — a Precursor of Modern Intuitionism?

Intuitionism, too, is not easily definable: like Formalism it varied with the time and from one mathematician to another.[56] Its modern form emerged relatively late compared with PEANO's major contribution: it was proposed by BROUWER about 1908.[57] But its roots, as BROUWER himself relates, are much older, and go back as far as KANT's philosophy of mathematics:

> In Kant we find an old form of intuitionism, now almost completely abandoned, in which time and space are taken to be forms of conception inherent in human reason. For Kant the axioms of arithmetic and geometry were synthetic a priori judgments, *i.e.* judgments independent of experience and not capable of analytical demonstration; and this explained their apodictic exactness in the world of experience as well as in abstracto.[58]

BROUWER explains why KANT's Intuitionism was completely abandoned:

> The most serious blow for the Kantian theory was the discovery of non-euclidian geometry, a consistent theory developed from a set of axioms differing from that of elementary geometry only in this respect that the parallel axiom was replaced by its negative. For this showed that the phenomena usually described in the language of elementary geometry may be described with equal exactness, though frequently less compactly in the language of non-euclidian geometry; hence, it is not

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[55] *Dictionary of Scientific Biography*, **10**: 443.

[56] See CARL J. POSY, "Brouwer's Constructivism," *Synthese* **27** (1974): 125–159.

[57] For a thorough study of BROUWER, his life and his philosophy, see VAN STIGT, *Brouwer's Intuitionism.*

[58] BROUWER, "Intuitionism and Formalism," p. 83.

> only impossible to hold that the space of our experience has the properties of elementary geometry but it has no significance to ask for *the* geometry which would be true for the space of our experience.[59]

However, Intuitionism was never completely abandoned. At the end of the nineteenth century several mathematicians complained that mathematics was becoming too abstract. LEOPOLD KRONECKER (1823–1891), for instance, objected to WEIERSTRASS' and CANTOR's work with the continuum, and argued that mathematics must be based on natural numbers only, since only these numbers can be intuitively grasped.[60] (He said: "God created the Natural numbers, man created the rest."[61]) POINCARÉ (1854–1912), the universalist mathematician and physicist, also criticized Logicism and Formalism and expressed Intuitionist views.[62] So did PEANO. In 1894 he wrote:

> In every science, after having analyzed the ideas, expressing the more complicated by means of the more simple, one finds a certain number that cannot be reduced among them, and that one can define no further.

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[59] *ibid.*, p. 85.

[60] For a biography of KRONECKER see E. T. BELL, "Men of Mathematics," (New York: Simon and Schuster, 1937), Chap. 25. For his views concerning the foundations of mathematics see FRANCESCO GAVA, "Dio e l'uomo nella matematica di Kronecker," *Historia Mathematica* **13** (1986), 255–276; HAROLD M. EDWARDS, "Kronecker's Views on the Foundations of Mathematics," in DAVID E. ROWE & JOHN MCCLEARY, *The History of Modern Mathematics. Volume I: Ideas and Their Reception. Proceedings of the Symposium on the History of Modern Mathematics* (Boston: Academic Press, 1989), pp. 67–77.

[61] This assertion by KRONECKER is often repeated in the literature of history of mathematics, but I found few precise references stating on what occasion KRONECKER made it. H. WEBER in his obituary of KRONECKER in *Jahresbericht der Deutschen Mathematiker-Vereinigung* **2** (1891–1892): 5–31, p. 19, says the assertion was made in a lecture in Berlin in 1886. In his "Über den Zahlbegriff" (1887), KRONECKER attempted to construct other types of numbers based on the natural numbers: *Werke*, **3**: 251–274. On KRONECKER and Intuitionism, see HAROLD EDWARDS, "Kronecker's Place in History," in ASPRAY & KITCHER (eds.), *History and Philosophy of Modern Mathematics*, pp. 139–144.

[62] For instance, in his article "La logique et l'intuition dans la science mathématique et dans l'enseignement" (1889), POINCARÉ said that contemporary mathematics had turned away from reality, and urged a return to intuition in teaching: *Oeuvres de Poincaré*, **11**: 129–133. In "De L'intuition et de la logique en mathématiques," POINCARÉ argued that both logic and intuition are necessary in mathematics, logic as a means of proof, and intuition as a means of discovery (see p. 126). In *Science et méthode* (Paris: Flammarion, 1908) he criticized HILBERT's and RUSSELL's approaches to mathematics. For POINCARÉ and foundationism see WARREN GOLDFARB, "Poincaré against the Logicists," in ASPRAY & KITCHER, *History and Philosophy of Modern Mathematics*, pp. 61–81. In a long article, "Les mathématiques et la logique," *Revue de Métaphysique et de Morale* **13** (1905): 815–835, **14** (1906): 17–34. POINCARÉ criticized RUSSELL, HILBERT, and PEANO.

> These are the *primitive ideas* of the science; it is necessary to acquire them through experience, or through induction; it is impossible to explain them by deduction.[63]

The key words in this statement are "experience" and "induction." By "induction," PEANO probably meant "abstraction;" in fact, in "The concept of number" he said: "These concepts [number, unity, successor of a number] cannot be obtained by deduction; it is necessary to obtain them by induction (abstraction)."[64] Unfortunately, "abstraction" is an ambiguous concept;[65] yet PEANO's assertion, despite its ambiguity, could very well point in the direction of Intuitionism.

The neo-intuitionist program was developed by BROUWER in the first decade of the present century. Its basis was presented in 1907 in his doctoral thesis, *On the Foundations of Mathematics*.[66] In the second chapter, under the title "Mathematics and Experience," he says:

> Proper to man is a faculty which accompanies all his interactions with nature, namely the faculty of taking a *mathematical view* of his life, of observing in the world repetitions of sequences of events, i.e. of causal systems in time. The basic phenomenon therein is the simple intuition of time, in which repetition is possible in the form: 'thing in time and again thing', as a consequence of which moments of life break up into sequences of things which differ qualitatively. These sequences thereupon concentrate in the intellect into mathematical sequences, not *sensed* but *observed*.[67]

BROUWER criticized PEANO: he said, *inter alia*, that PEANO's and DEDEKIND's systems suffered from the same weakness: they "edif[y] a logical system which is supported neither by an existence proof, nor by a consistency proof."[68] BROUWER remarked on DEDEKIND's work:

> Let us recall in this connection Dedekind's famous monograph 'Was sind und was sollen die Zahlen?', in which he aims at proving logically the arithmetic of whole numbers, starting from the most primitive notions. For this purpose he constructs a logical system (i.e. a mathematical

[63] "Notations de logique mathématique. Introduction au Formulaire de mathématiques" [66], in PEANO, *Opere Scelte*, **2**: 123–176, p. 173. English translation from KENNEDY, "The Mathematical Philosophy of Giuseppe Peano," p. 264.

[64] PEANO, *Opere Scelte*, **3**: 85. Translation from KENNEDY, *ibid.*

[65] *Cf.* KENNEDY, *ibid.*, p. 265.

[66] The original title was *Over de grondslagen der wiskunde*. An English translation, under the title *On the Foundations of Mathematics*, is presented in BROUWER's *Collected Works*, **1**: 13–101.

[67] *ibid.*, p. 53.

[68] *ibid.*, p. 91.

system of words), the axioms of which are the linguistic images of the connections between the basic notions (*whole and part*, *correspondence between elements*, *mapping of systems*, etc.) and which further is *finitely* constructed following the logical laws (thus without using complete induction, i.e. the mathematical intuition of 'and so on'). In order to have mathematical significance, this system ought to be completed by a mathematical existence proof. But in order to give that, we shall certainly be forced to use the intuition 'and so on', and then we see at once that we can obtain all the arithmetical theorems much more easily than by Dedekind's contrived system; accordingly Dedekind does not give the existence proof.[69]

Brouwer also criticized Russell and Hilbert. I will not go into the details of this criticism, which is irrelevant to the purpose of this chapter, but only point out that, as far as Peano is concerned, Brouwer may have not read him carefully: Peano did claim that primitive ideas are acquired through experience, which is very near to Brouwer's basic claim that

> In the system of definitions there are elements of mathematical construction which must remain irreducible and which therefore, when communicated, must be understood from a single word or symbol; these are the elements of construction which are immediately conceived in the basic intuition or intuition of the continuum. Notions such as *continuous*, *entity*, *once more*, *and so on* are irreducible.[70]

Moreover, three years later, in 1910, Peano said:

> Mathematical rigor is very simple. It is to affirm true things, and not to affirm things which we know not to be true. It is not to affirm all possible truths. Science, or truth, is infinite; we do not know more than a finite part, infinitesimal with respect to the whole. And from the science that we know, we must teach only that part which is most useful to the pupils.
>
> Therefore, to be rigorous it is not necessary to define all the entities which we consider. First of all, not everything can be defined. One cannot define the first entity, as Aristotle already remarked, one cannot define the definition, namely the sign $=$. The most modern studies reduce the entities belonging to the various parts of mathematics, namely logic, arithmetic and geometry, to half a score, which one does not know how to define.
>
> And even where one can define, it is not always useful to do so. Every definition expresses a truth, namely that one can establish an equality in which the first number is the defined entity, the second, or

[69] *ibid.*, p. 78.
[70] *ibid.*, p. 97.

defining member ("definiente"), is an expression composed of entities which have already been considered. Now, if this truth has a complicated form, or in any case is hard to present, it will belong to that order of truth which one should not teach in a certain school, which should better be omitted.[71]

These paragraphs may also be regarded as a summary of PEANO's approach to mathematics, namely that rigor is needed to present mathematics more clearly and to avoid mistakes.

Intuitionism, like Formalism, developed far beyond PEANO's foundationism; its development was influenced by its controversy with Formalism.[72] The Intuitionist mathematicians encountered considerable opposition because they showed that in many cases, standard proofs in mathematics are invalid.[73] Let me end this chapter by returning to BROUWER's comparison of Formalist and Intuitionist views by saying:

> During recent years they have reached agreement as to this, that the exact validity of mathematical laws as laws of nature is out of the question. The question where mathematical exactness does exist, is answered differently by the two sides; the intuitionist says: in the human intellect, the formalist says: on paper.[74]

Although I did not find any mention by PEANO of BROUWER's Intuitionism, he would no doubt have agreed that the validity of mathematical laws as laws of nature is beyond question.[75] But what would he have answered to the second question, concerning mathematical exactness? He might perhaps have said that it exists in the symbols by which it is presented, which are the bridge between human intellect and paper. One can certainly find seeds of Intuitionism in PEANO's work, just as one can find in it seeds of Formalism and Logicism.

Without belonging to any of the main Intuitionist streams, PEANO, as a foundationist who endeavored to establish the basis for the indubitability of mathematics, anticipated all of them.[76] It would be ahistorical to say that the difference between these three streams indicates a deficiency in PEANO's

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[71] PEANO, *Opere scelte*, 3: 275.

[72] For a presentation of Intuitionism see AREND HEYTING, "Intuitionistic Views on the Nature of Mathematics," *Synthese* **27** (1974): 79–91.

[73] An example is the "law of trichotomy," saying that every real number is either zero, positive, or negative, which BROUWER regarded as false.

[74] BROUWER, "Intuition and Formalism," p. 83.

[75] There were contacts between PEANO and BROUWER, but they occurred later and concerned BROUWER's quest for social reform and the possible role of PEANO's international language. See VAN STIGT, *Brouwer's Intuitionism*, pp. 68, 78.

[76] A similar view is held by CORRADO MANGIONE, "Peano e i fondamenti della matematica," in *Peano e i fondamenti della matematica*, pp. 21–34. MANGIONE emphasizes in particular the formalistic and intuitionistic elements in PEANO's work.

thought, at least in the period when he formulated his axioms. PEANO also insisted on his independence. Even so, it is still astonishing that even later on, PEANO kept totally aloof from controversies between the above-mentioned schools, and paid little attention to new studies on the foundations of mathematics. He apparently preferred to concentrate on his studies in linguistics with the aim of finding a language for mathematics and science, hoping that once rigor and clarity had been achieved, the basic problems would vanish by themselves. PEANO's solution, though it may be interesting and less rigid than the others I have quoted above, was naive and like all other foundationist solutions, it was an utter failure. As I will describe briefly in the Conclusion, it was precisely in the field where he had hoped for a successful accomplishment, namely in teaching, that PEANO became totally unintelligible.

Conclusion: The Failure of Peano's Foundationist Program

"The question for the ultimate foundations and the ultimate meaning of mathematics," said HERMANN WEYL in his obituary of HILBERT, "remains open; we do not know in which direction it will find its final solution nor even whether a final objective answer can be expected at all."[1] In fact none of the three major schools in foundationism achieved its aim — the indubitability of mathematics — and, to paraphrase PHILIP DAVIS and REUBEN HERSH, the story ended in mid-air some forty years ago, with WHITEHEAD and RUSSELL having abandoned Logicism, HILBERT's Formalism defeated by GÖDEL's theorem, and BROUWER left to preach Intuitionism in Amsterdam, disregarded by the rest of the mathematical world.[2] PEANO was no exception, though his failure was confined to the domain in which he concentrated much of his efforts, the teaching of mathematics.

Teaching was an important side of PEANO's work. It was through teaching, one must remember, that PEANO came to his axioms and to his general program of presenting mathematics in a rigorous, clear and flawless way. PEANO's endeavor, in the second part of his career, to apply his rigorous mathematical approach to teaching was also part of an effort to disseminate his views. His failure in the domain of teaching, therefore, is of central importance in an appraisal of his general program.

PEANO's attempts to apply his rigorous mathematical approach to teaching is amply demonstrated in many of his letters (scattered in various archives); for example, the previously mentioned letters to FELIX KLEIN, announcing the *Formulario* project. This chapter of PEANO's work, which includes the acquisition of a printing press and the founding of the *Rivista Matematica*, as a tool to spread his and his followers' ideas, would be a promising field for further study.[3]

Several mathematical schools in Italy were opposed to PEANO.[4] A resounding example of the existing tension was the failure of PEANO's outstanding pupil, CESARE BURALI-FORTI, to obtain a *Libera docenza* because of his insistence on vectorial methods in his thesis.[5] PEANO was more accepted abroad, although there, too, he had critics. History of mathematics has noted the work of several followers of PEANO, but the interaction between PEANO, his followers, and his opposers, both in Italy and abroad, has not yet been studied and would be a contribution to the sociology of science, complementary to the present study.[6]

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[1] WEYL, *Gesammelte Abhandlungen*, Vol. 4, p. 126.

[2] DAVIS & HERSH, *The Mathematical Experience*, p. 323.

[3] See KENNEDY, *Peano*, Chaps. 5 and 11.

[4] Opposition to PEANO was particularly strong in his own university, Turin, and, as KENNEDY notes, continued long after his death: KENNEDY, *Peano*, p. 172.

[5] *ibid.*, p. 85.

[6] For a study of the work of the PEANO school see, *inter alia*, BORGA-FREGUGLIA-PALLADINO, in *I contributi fondazionali della scuola di Peano*.

I mention here only PEANO's two main projects after the formulation of his axioms:

1. *The Formulario project* was an attempt to collect as many mathematical theorems as possible into a concise presentation using his symbols. PEANO hoped that his symbolism, which he believed to be the best and clearest, would be universally adopted by mathematics. He also believed that this ideography would foster rigor, avoid mistakes, and permit a better development of mathematics.

PEANO's symbolism is, in fact, quite simple to master, yet only some of his symbols were universally adopted, and his ideography did not, as he had hoped, become the language of mathematics. The *Formulario* remained a collection of mathematical formulae which today one would find in a library under the heading "history of mathematics," rather than "mathematics," which is enough to indicate PEANO's failure in this respect. Moreover, his ideography at times rendered the proofs of theorems more cumbersome than they had been without it, and with no guarantee against error.

2. *The Interlingua.* The above remarks about the *Formulario* can be repeated for PEANO's more general project, the creation of an artificial language which would facilitate communication between scientists. Though PEANO wrote a number of articles in this language, its fate was that of all artificial languages: it did not spread. Scientists, like people in general, prefer to communicate in existing languages, without the bother of studying a new, artificial language.

TERRACINI, mathematician and pupil of PEANO, testified in his autobiography to the failure of PEANO's didactic program:

> When I was a student, Peano used to give the calculus course for the students of the second year as well as the course of advanced analysis for more advanced students. Frankly, after so many years I could no longer say what the name of the advanced analysis course I heard from him was. On second thoughts, I would say that, on the contrary, it did not even have a name. Because — at least, as far as I can say on the basis of my memories after so many years — I think that the lessons ended up, just like those of calculus, in turning the pages of the *Formulario*, eventually pausing on some points.[7]

Indeed, PEANO's main preoccupation in his written work as well as in his teaching was to try to express everything in his system of notation and in the artificial language he had created, known, colloquially as "Peanian." In his teaching, not only did he exasperate his students with his notations, but it was more important to him that the students understood "Peanian" than mathematics. In a meeting devoted to PEANO held in October 1991 in Modena, I heard senior Italian mathematicians recall how his lectures degenerated into such a farce that the incredulous dean of the faculty came in disguise to listen to one

[7] TERRACINI, *Ricordi di un matematico*, pp. 40–41.

of them. To his distress, he found confirmation of the stories he had heard. But the university of Turin could not remove a mathematician of PEANO's stature, and PEANO therefore retained his teaching position, continuing to produce worthless presentations of mathematics. This was not the case however, at the Turin Military Academy, from which PEANO had to resign in 1901.[8]

These testimonies bear witness to the failure of PEANO's didactical program and, implicitly, of his heuristic approach. As LAKATOS said, rigorous proof-analyses deprive "mathematics of its beauty, present us with the hairsplitting pedantry of long, clumsy theorems filling dull thick books, and will eventually land us in vicious infinity."[9]

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[8] See KENNEDY, "Giuseppe Peano at the University of Turin," p. 706, and *Peano*, p. 100.

[9] LAKATOS, *Proofs and Refutations*, p. 54.

List of Publications of Giuseppe Peano

The following chronological list is taken from HUBERT KENNEDY's *Peano: Life and Works of Giuseppe Peano*, pp. 195–209. A few corrections have been made. KENNEDY's list is itself a complement to the list compiled by UGO CASSINA and published at the end of the three-volume collection of PEANO's *Opere scelte*. As KENNEDY says (*Peano*, p. 195), "Ugo Cassina used the consecutive numbering of Peano's publications which had already been used by Peano himself. He also distinguished between mathematical and philological publications, leaving only one publication outside these two categories (and outside the consecutive numbering.) Since the *Opere Scelte* will be the most available source, for most readers, of the original articles, I have kept the numbering of Cassina and have used a decimal numbering to insert new titles into his list. This new list was published in *Selected Works of Giuseppe Peano*; to it has been added only a few translations that have since come to my attention.

I have followed CASSINA's use of Roman numerals to distinguish monographs and volumes (of at least 64 pages), primes to denote translations and reprints, and the asterisk to signalize works pertaining to philology. Thus *160′–17′ is a reprint of the 160th work and the 17th work in philology. Titles of journals are those of *Union List of Serials*; abbreviations of journal titles are those of *Mathematical Reviews* (with the addition of *A.p.I.* and *R.d.M.*). In cases where the original article was untitled, I have given a brief descriptive phrase in English. This suffices to show that the article was untitled since none of Peano's publications was in English."

The following abbreviations of journal titles are used:

A.p.I. Academia pro Interlingua
Amer. J. Math. American Journal of Mathematics
Ann. mat. pura appl. Annali di matematica pura ed applicata
Atti Accad. naz. Lincei, Rend., Cl. sci. fis. mat. nat. Atti della Accademia Nazionale dei Lincei, Rendiconti, Classe di scienze fisiche, matematiche e naturali
Atti Accad. sci. Torino, Cl. sci. fis. mat. nat. Atti della Accademia delle scienze di Torino, Classe di scienze fisiche, matematiche e naturali
Boll. Un. Mat. Ital. Bollettino della Unione Matematica Italiana
Enseignement math. L'Enseignement mathématique
Giorn. mat. Battaglini Giornale di matematiche di Battaglini
Giorn. mat. finanz. Giornale di matematica finanziaria: Rivista tecnica del credito e della previdenza
Math. Ann. Mathematische Annalen
Mathesis Mathesis: Recueil mathématique à l'usage des écoles spéciales et des établissements d'instruction moyenne
Monatsh. Math. Monatshefte für Mathematik
Period. mat. Periodico di matematiche
R.d.M. Rivista di matematica (Revue de mathématiques)
Rend. Circ. mat. Palermo Rendiconti del Circolo matematico di Palermo
Scientia Scientia: International Review of Scientific Synthesis

Wiadom. mat. Roczniki Polskiego towarzystwa matematycznego, ser. II: Wiadomości matematyczne

Peano, Su no Gainen ni tsuite (Kyoritsu Publishing Company, 1969) has Japanese translations of [35] and [37] by T. UMEZAWA, with a commentary by K. ONO. *Selected Works of Giuseppe Peano*, H. C. KENNEDY editor-translator (University of Toronto Press, 1973), has English translations of items: 5, 9, 10, 16, 22, 23, 24, 29, 31, 50, 62, 90, 90.5, 91, 133′ (Additione), 143, 176, 193, and parts of 8, 11, 14.

1881

1 'Costruzione dei connessi (1, 2) e (2, 2)', *Atti Accad. sci. Torino* **16**, 497–503.

2 'Un teorema sulle forme multiple', *Atti Accad. sci. Torino* **17**, 73–9.

3 'Formazioni invariantive delle corrispondenze', *Giorn. mat. Battaglini* **20**, 79–100.

1882

4 'Sui sistemi di forme binarie di egual grado, e sistema completo di quante si vogliano cubiche', *Atti Accad. sci. Torino* **17**, 580–6.

1883

5 'Sull'integrabilità delle funzioni', *Atti Accad. sci. Torino* **18**, 439–46.

6 'Sulle funzioni interpolari', *Atti Accad. sci. Torino* **18**, 573–80.

1884

7 Two letters to the Editor, *Nouvelles annales de mathématiques* (3) **3**, 45–7, 252–6.

I, 8 Angelo Genocchi, *Calcolo differenziale e principii di calcolo integrale, pubblicato con aggiunte dal Dr. Giuseppe Peano* (Torino: Bocca).

I′, 8′ Angelo Genocchi, *Differentialrechnung und Grundzüge der Integralrechnung, herausgegeben von Giuseppe Peano*, trans. by G. Bohlmann and A. Schepp, with a preface by A. Mayer (Leipzig: Teubner, 1899).

Russian translations by N. S. Sineokov (Kiev, 1903) and K. A. Posse (St. Petersburg, 1922).

1886

9 'Sull'integrabilità delle equazioni differenziali del primo ordine', *Atti Accad. sci. Torino* **21**, 677–85.

1887

10 'Integrazione per serie delle equazioni differenziali lineari', *Atti Accad. sci. Torino* **22**, 437–46.

II, 11 *Applicazioni geometriche del calcolo infinitesimale* (Torino: Bocca).

1888

12 'Intégration par séries des équations différentielles linéaires', *Math. Ann.* **32**, 450–6 (English translation in [Birkhoff 1973]).

13 'Definizione geometrica delle funzioni ellittiche', *Giorn. mat. Battaglini* **26**, 255–6.

13′ 'Definição geometrica das funcções ellipticas', *Jornal de sciencias mathematicas e astronomicas* **9**, 24–5.

III, 14 *Calcolo geometrico secondo l'Ausdehnungslehre di H. Grassmann, preceduto dalle operazioni della logica deduttiva* (Torino: Bocca).

14.1 Communications to the Circolo (on a note of F. Giudice and on his response), *Rend. Circ. mat. Palermo* **2**, 94, 187–8.

15 'Teoremi su massini e minimi geometrici, e su normali a curve e superficie', *Rend. Circ. mat. Palermo* **2**, 189–92.

1889

16 *Arithmetices principia, nova methodo exposita* (Torino: Bocca).

17 'Sur les wronskiens', *Mathesis* **9**, 75–6, 110–12.

18 *I principii di geometria logicamente esposti* (Torino: Bocca).

19 'Une nouvelle forme du reste dans la formule de Taylor', *Mathesis* **9**, 182–3.

20 'Su d'una proposizione riferentesi ai determinanti jacobiani', *Giorn. mat. Battaglini* **27**, 226–8.

21 Angelo Genocchi, *Annuario R. Università di Torino* (1889–90), 195–202.

22 'Sur une formule d'approximation pour la rectification de l'ellipse', *Académie des sciences, Paris, Comptes-rendus hebdomadaires des séances* **109**, 960–1.

1890

23 'Sulla definizione dell'area d'una superficie', *Atti Accad. naz. Lincei, Rend., Cl. sci. fis. mat. nat.* (4) **6–I**, 54–7.

24 'Sur une courbe qui remplit toute une aire plane', *Math. Ann.* **36**, 157–60.

25 'Les propositions du V livre d'Euclide réduites en formules', *Mathesis* **10**, 73–5.

26 'Sur l'interversion des dérivations partielles', *Mathesis* **10**, 153–4.

27 'Démonstration de l'intégrabilité des équations différentielles ordinaires', *Math. Ann.* **37**, 182–228.

28 'Valori approssimati per l'area di un ellissoide', *Atti Accad. naz. Lincei, Rend., Cl. sci. fis. mat. nat.* (4) **6–II**, 317–21.

29 'Sopra alcune curve singolari', *Atti Accad. sci. Torino* **26**, 299–302.

1891

30 *Gli elementi di calcolo geometrico* (Torino: Candeletti).

30′ *Die Grundzüge des geometrischen Calculs*, trans. by A. Schepp (Leipzig: Teubner).

31 'Principii di logica matematica', *R.d.M.* **1**, 1–10.

31′ 'Principios de lógica matemática', *El progreso matemático* **2** (1892), 20–4, 49–53.

32 'Sommario dei libri VII, VIII e IX d'Euclide', *R.d.M.* **1**, 10–12.

33 Review of E. W. Hyde, *The Directional Calculus, based upon the methods of H. Grassmann*, *R.d.M.* **1**, 17–19.

34 Review of F. D'Arcais, *Corso di calcolo infinitesimale–Vol I*, *R.d.M.* **1**, 19–21.

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