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THE HISTORICAL AND PHILOSOPHICAL SIGNIFICANCE OF THE EMERGENCE OF POINT SET TOPOLOGY

A DISSERTATION SUBMITTED TO
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AND THE STUDY OF METHODS

BY

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PREFACE

From about 1980 to 1915 there emerged a branch of mathematics commonly called "point set topology." Having its roots in nineteenth century mathematical analysis, geometry, foundations, and set theory, point set topology summarized and unified many previous mathematical results and extended and generalized them. Point set topology also contributed, in its own right, several new, useful concepts to mathematics.

This dissertation, in its first chapter, briefly sketches the relevant historical mathematical developments that influenced the emergence of point set topology. In the second chapter other historical accounts of the emergence of point set topology are discussed and compared. Out of this discussion aspects of the historiography of mathematics are revealed as well as the general problems involved in the historiography of mathematics. A set of guidelines for an historiography of mathematics is suggested.

The third and last chapter of the dissertation discusses the philosophy of mathematics. The intention of this chapter is to appreciate the potential use that philosophy of mathematics may have to both interpreting

history of mathematics and constructing an histiography of mathematics.

This dissertation is thus concerned with the interplay of mathematics, its history, and its philosophy.

That this dissertation was ever completed was due in part to the patience and support given to me by members of the Committee on the Analysis of Ideas and Study of Methods at the University of Chicago. In particular, I owe thanks to Professor Charles Wegener and to Professor David Smigelskis, and to Professor William Tait of the Department of Philosophy, for their encouragement, conversations, and support.

Most of all, I owe a debt of gratitude to Profesor Saunders Mac Lane of the Department of Mathematics. His help and concern, valuable insights and criticism, and above all, his unfailing belief in this project and my ability to complete it, I will always remember. I hope I will act in the same manner to my students.

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CHAPTER I

A BRIEF HISTORY OF THE DEVELOPMENTS IN MATHEMATICS THAT INFLUENCED THE EMERGENCE OF POINT SET TOPOLOGY

Prior to the nineteenth century the intuitions, arguments, and proofs in the calculus and analysis were primarily geometrical and physical in nature. Proofs tended to depend on pictures, geometry, and the physical concepts of time and motion. It was during the nineteenth century that these geometrical and physical arguments were replaced by arithmetical or, what are now called, analytical arguments. Not only were the methods of proof and argument changing, but arithmetization itself led to advances in analysis.

The Arithmetization of Analysis

An early example of this changing form of proof is Bernhard Bolzano's proof of his version of the fundamental theorem of algebra--that every algebraic expression which is positive for some value and negative for some other value has a real root. Bolzano's proof, published in 1817, entitled Rein Analytischer Beweis der Lehrsatzes, dass zwischen zwei werthen, die ein entgegengesetztes Resultat gewahren, wenigsten eine reele Wurzel der Glechung liege[1]

[1](Prague, 1817). An English translation has

[Purely analytic proof of the theorem that between any two values which give results of opposite sign there lies one real root of the equation] comes eighteen years after Gauss' proof of the same theorem. Gauss' proof was a geometrical argument, Bolzano's proof was analytic. Part of Bolzano's proof depended on a sequence of nested intervals on the real line, whose right end-points formed a sequence converging within itself. Bolzano then argued that the sequence of end-points had a limit. However, this argument rests on an arithmetical construction of the real numbers, arithmetization lacking in 1817, causing Bolzano's proof to be incomplete. While Bolzano's proof is an early example of the development of analysis in the nineteenth century, it is difficult to discern any impact on the development of the foundation of analysis, since Bolzano's work appears only to have been appreciated nearly fifty years later.

A far more influential mathematician was Augustin-Louis Cauchy, who made major contributions to the arithmetization of the concepts of the calculus. Cauchy defined the limit concept in terms of arithmetical considerations in his Cour d'analyse algébrique of 1821.

> When the successive values attributed to a variable approaches indefinitely a fixed value so as to end by

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appeared in Historia Mathematica by S. B. Russ, "A Translation of Bolzano's Paper on the Intermediate Value Theorem," Historica Mathematica 7 (1980): 156-185.

> differing from it by as little as one wishes, this last is called the limit of all others. Thus, for example, an irrational number is the limit of diverse fractions which furnish closer approximations of it.[1]

His last phrase regards irrational numbers as the limits of sequences of rational numbers. Cauchy's limit is defined as the number to which the terms of the sequence approach in such a way so that the difference between the limit and the terms of the sequence can be made less than any given number. This means the existence of the irrational number depends, in the very definition of limit, upon the limit already existing.

This circular reasoning is a result of Cauchy, and others, assuming that if a sequence converges within itself--in what is now called Cauchy convergence, or the Cauchy criterion--then it must have a limit. Cauchy may have been aware of this circular reasoning since this definition of limit is missing from his 1823 Résumé des Leçons and his 1829 Leçons sur le calcul differentiel.

In the preface of the 1821 edition, Cauchy comments that to speak of continuity of functions one must understand the properties of "infinitesimals." "One says that a variable quantity becomes infinitely small when its numerical value

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[1]Augustin-Louis Cauchy, Cours d'analyse algébrique (Paris: de Burge, 1821). Page references are from Oeuvres completes, 4 vols. (Paris: Gauthier-Villars, 1887-1897), 3:19.

decreases indefinitely in such a way as to converge to the limit 0."[1] Cauchy then defines continuity of a function.

> Let f(x) be a function of the variable x, and suppose that, for each value of x intermediate between two given limits, this function constantly assumes a finite and unique value. If, beginning with a value of x contained between these limits, one assigns to the variable an infinitely small increment, i, the function itself will take on as an increment the difference f(x+i)-f(x) which will depend at the same time on the new variable i and on the value of x. This granted, the function f(x) will be, between the two limits assigned to the variable x, a continuous function of the variable, if for each value of x, intermediate between these two limits, the numerical value of the difference f(x+i)-f(x) decreases indefinitely with that of i. In other words, the function f(x) will remain continuous with respect to x between the given limits, if, between these limits, an infinitely small increment of the variable always produces an infinitely small increment of the function itself.[2]

Having established the notion of limit, infinitesimal and infinity, Cauchy was able to define the central concept of the calculus, the derivative. His definition was like that of Bolzano's. (Bolzano's work had again gone unnoticed.)

Let the function $y = f(x)$; the variable x given an increment $\Delta x = i$; and form the ratio

$$\frac{\Delta y}{\Delta x} = \frac{f(x + i) - f(x)}{i} .$$

The limit of this ratio, if it exists, as i approaches zero, Cauchy denoted by f'(x). From this definition of

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[1] Ibid.

[2] Ibid., p. 43. Cauchy's emphasis.

derivative, Cauchy could define the differential dy as $f'(x)dx$.[1]

With Cauchy the fundamental concepts of the calculus received a rigorous formulation. Cauchy is usually considered the creator of the modern version of the calculus. Using his notion of limit he developed the theory of continuity, the derivative, the differential, the definite integral, and infinite series.

Yet Cauchy's presentation had subtle omissions. One of these was the omission of a precise definition of the concept of number; a concept indispensible to the concept of limit and thus to the calculus.

To thoroughly and precisely formulate a mathematical concept, it is often required that the logically preceeding mathematical concepts be also thoroughly and precisely formulated. This "logical regression" of mathematical concepts often occurs in historical developments in mathematics. In the case with the calculus, the precisely formulated derivative requires a precise formulation of number. (And much later, the precise formulation of number required a precise formulation of set theory and logic.) Chronological developments in mathematics are often the reverse of their final, precise, mathematical formulations.

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[1]Ibid., 4:22-23.

The Arithmetization of the Real Numbers

While Cauchy may be credited with providing the calculus with its modern formulation, Karl Weierstrauss provided an arithmetical basis for the real numbers, and thus an arithmetical basis for analysis. One reason for Weierstrass' desire for an arithmetical basis of analysis was his construction in 1872 of a nowhere differentiable continuous function. (Such a function was known to Bolzano at least forty years earlier, but again, did not receive much attention.) Up to the 1870's it was assumed that a continuous function would have derivatives everywhere with the exception of a few points. This assumption was a result of a still too heavy dependence on geometrical or motion intuition of continuous functions and their derivatives.

To avoid the possible misleading impressions engendered by geometrical intuitions, Weierstrass established the calculus on the concept of number only. Basing the foundation of calculus on the real numbers required an arithmetical construction of the rationals, and then more importantly, and more difficultly, an arithmetical construction of the irrational numbers (another case of logical regression) independent of limit in the sense used by Cauchy. Weierstrass was thus lead to an arithmetical construction of the irrationals.[1] In his construction of the irrationals

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[1]Because Weierstrass did not publish his lectures

Weierstrass did not investigate the nature of the integers but began with the integers.

For Weierstrass, a whole number is the sum of some collection of units. A rational number, termed a "complex number" by Weierstrass, is a collection of units and an "aliquot part" of a unit, e_n, is defined as that number such that n successive additions of e_n equals a unit. For example, $\frac{1}{5}$ is an aliquot part of the unit 1, since five successive additions of $\frac{1}{5}$ equal a unit. Thus, the "complex number" 7.35 consists of the sum of 7 units, 3 one-tenths, and 5 one-hundredths.

Unlike Cauchy's system for irrational numbers, Weierstrass did not define irrational numbers as the limit of some sequence of rational numbers. Rather, an irrational number like $\sqrt{2}$ is considered the infinite sum of aliquot parts. That is, 1.41421 . . . is considered the sum of 1 unit, 4 tenths, 1 hundredths, 4 thousandths, . . . in any order chosen, with the crucial condition that the sum of

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on the arithmetization of the real numbers we must rely on the publications of his students, which, in turn, are not very accessible. See A. Pringsheim, Encyk. Math. Wiss. (1898-1904), I A 3:53-55; L. Pincherle, "Saggio di una introduzione all theoria delle prof. C. Weierstrass," Giornale di math. 18 (1880):178-254; or Otto Stolz, Vorlesungen uber allgemeine Arithmetic (Leipzig: n.p., 1885).

P. E. B. Jourdain has provided an historical and explanatory account of Weierstrass' theory in his article, "The Development of the Theory of Transfinite Numbers," Archive der Mathematik und Physik 14 (1908-9):303-307. See also Bertrand Russell's account in his Principles of Mathematics, (Cambridge: Cambridge University Press, 1903), p. 282.

any finite number of parts is always less than some rational number. Hence, the series 1.41421 . . . is always less than 1.415, no matter to how many terms 1.41421 . . . is taken. The series itself then is taken to be the irrational number, and not the limit of the sequence 1, 1.4, 1.41, 1.414, 1.4142, . . . as the irrational number.

At about the same time that Weierstrass' program became known through his students, Eduard Heine published "Die Elemente der Functionlehre" in Crelle's Journal.[1] Heine's paper contains an arithmetical construction of the reals, "Ueber Zahlen," and a section on the properties of continuous functions, "Ueber Functionen."

In his construction of the reals, Heine uses the term number series (Die Zahlenreihen) which he defines as

> . . . a series of numbers $a_1, a_2, \ldots, a_n, \ldots$ such that for any small η, there exists an n such that $a_n - a_{n+\nu} < \eta$ for all positive integers ν.[2]

The German word "Reihe" is used throughout the paper, and though its usual connotation is series, Heine undoubtably means sequence.

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[1]Eduard Heine, "Die Elemente der Functionlehre," Journal fur die reine und angewandte Mathematik 74 (1872): 172-188. The journal was edited by A. L. Crelle for many years and so acquired the abbreviated title, "Crelle's Journal."

[2]Ibid., p. 174.

Heine defines elementary series as those number series with the property that $a_n \to 0$ as $n \to \infty$ ("unter jede angebare Gross herabsinken"[1]).

Two number series $a_1, a_2, \ldots$ and $b_1, b_2, \ldots$ are equal if $a_1-b_1, a_2-b_2 \ldots$ is an elementary series.[2]

For every number series $\{a_i\}$ we identify with it a number symbol A. If $a_i = a$, for all i, then the symbol a is identified with the series $\{a_i\}$. Heine intends to blur the distinction between a and $\{a_i\}$. For example, if $a = \frac{1}{9}$, and $\{a_i\} = \{\frac{1}{9}, \frac{1}{9}, \frac{1}{9}, \ldots\}$ the rational number $a = \frac{1}{9}$ is equivalent to the number series $\{\frac{1}{9}, \frac{1}{9}, \frac{1}{9}, \ldots\}$. Thus the rationals can be recovered from the set of number series and the number series are equivalent to the rationals under the operations of arithmetic.

Continuing with the above example, the series $\{b_i\} = \{0.1, 0.11, 0.111 \ldots\}$ is equivalent to the series $a_i = \{\frac{1}{9}, \frac{1}{9}, \frac{1}{9}, \ldots\}$ since the series $\{a_i - b_i\} = \{\frac{1}{9} - .1, \frac{1}{9} - .11, \frac{1}{9} - .111, \ldots\}$ is an elementary series. Thus, $\frac{1}{9}$ is defined as the limit of $\{b_i\}$. Heine then shows that if the terms of the number series have the rational limit a, then the symbol A is also the symbol identified with the series.[3]

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[1] Ibid., p. 175.

[2] Ibid., p. 179. [3] Ibid.

$A > B$ is defined for A, identified with $\{a_i\}$, and B, identified with $\{b_i\}$, if there exists an N such that for all $n > N$, $\{a_n - b_n\}$ is bounded above zero. Similarly, for $A < B$. Thus, a sequence of number symbols $\{C_i\}$ is said to approach zero if, for every positive D there exists an n such that $|C_{n+\nu}| < D$ for all positive integers ν. If this situation holds for all number symbols D, it holds for all rational number symbols, d. And conversely (and this is the important part of Heine's construction) Heine shows in a proof by contradiction that if this property holds for all rational number symbols d, then it holds for all number symbols, D, rational or not.[1]

Heine extends the concept of limit to all number symbols, not just rationals, by defining A to be the limit of B if $|A - B_n| \to 0$, as $n \to \infty$.

With all these conditions on A and $\{a_i\}$ now established, Heine can now prove the theorem: "The symbol A is the limit of the terms a_i of the series to which it belongs." The proof requires that it can be shown that, for large enough n, $|A - a_n| < d$, for any rational d.

In his own words:[2]

2. Lehrsatz Das Zahlzeichen A is die Grenze a der Reihe, zu welcher es gehort.

---

[1] Ibid.

[2] Ibid.

> Beweis Man hat zeigen, dass $A - a_n$ unter jedes angebarre Zahlzeichen, also nur, dass es unter jede rationale Zahl d sinkt. Nun ist $A - a_n$ gleich
>
> $[a_1 - a_n, a_2 - a_n,$ etc., $a_n - a_n, a_{n+1} - a_n,$ etc.]
>
> oder $[a_{n+1} - a_n, a_{n+2} - a_n, \ldots]$
>
> Nimmt man n hinreichend gross, so bleiben in dem vorstehenden Ausdrucke die einzeln Glieder der Reihe under d, also liegt das Zahlzeichen under [d, d, d, etc.], h. d. unter d.

Heine has demonstrated the existence of the limit of rationals. Heine then remarks that the irrationals are formed from number series which are not rational, and shows that not all number series are rational.

Georg Cantor's 1883 paper Grundlagen einer allgemeinen Mannigfaltigkeitslehre is very similar to Heine's arithmetical construction of the reals.[1] The Grundlagen is a much more detailed treatment of the reals and grew out of an earlier paper of 1872, "Ueber die Ausdehnung eines Satzes aus der Theories des Trigonometrischen Reihen."

Cantor's construction of the integers and rationals is essentially the same as Heine's. Cantor used the word "Fundamentalreihe"--again the word "series" is used when

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[1]Georg Cantor, Grundlagen einer allgemeinen Mannigfaltigkeitslehre (Leipzig: B. G. Teubner, 1883). The Grundlagen is the fifth of six papers, of a series entitled "Ueber unendlichen lineare Punktmannigfaltigkeitslehre." This fifth paper was so important to Cantor that he had it published as a separate monograph. See also Georg Cantor, Gesammelte Abhandlungen, Ernst Zermelo, ed. (Berlin: Springer, 1932), pp. 165-208.

the intention is sequences--for any sequence of rationals (a_ν) in which[1]

> . . . after the choice of any arbitrary small rational number ϵ a finite number of members of the aggregate can be separated off, so that those remaining have pairwise a difference which in absolute terms is smaller than ϵ. Every such aggregate (a_ν) can also be characterized by the postulate,
>
> $$\lim_{\nu = \infty} (a_{\nu+\mu} - a) = 0, \text{ for any } \mu$$
>
> is a fundamental series.

Cantor then shows that if (a_ν) and (a'_ν) are two fundamental sequences, denoted by a and a', then $(a_\nu \pm a'_\nu)$ and $(a_\nu \cdot a'_\nu)$ are also fundamental sequences so that $a \pm a'$ and $a \cdot a$ are defined. For $a \neq 0$, $\frac{a'_\nu}{a_\nu}$ is a fundamental sequence allowing for $\frac{a'}{a}$ to be defined.

Like Heine, the rational numbers are included in the collection of all fundamental sequences if (a_ν) is equal to some rational number a, for all ν, (a_ν) is the rational number a.[2]

Cantor then proves that if (b_ν) is any series of real numbers, rational or irrational, and $\lim_{\nu = \infty} (b_{\nu+\mu} - b_\mu) = 0$, for arbitrary μ, then there is a unique real number b, determined by some fundamental series (a_ν) of rationals

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[1]Cantor, Gesammelte Abhandlungen, p. 186.

[2]Ibid., p. 187.

such that

$$\lim_{\nu = \infty} b_\nu = b$$

In Cantor's words:

> . . . Ist (b_ν) irgend eine Menge rational oder irrationaler Zahlen mit der Sechaffenheit, das $\text{Lim}\ (b_{\nu+\mu} - b_\mu) = 0$, (was auch μ sei), so gibt es eine durch eine Fundamentalreihe (a_ν) bestimmte Zahl b so dass
>
> $$\lim_{\nu = \infty} b_\nu = b$$
>
> Es zeight sich also, dass dieselben Zahlen b, welche auf Grund von Fundamentalreihen (a_ν) (ich nenne diese Fundamentalreihen von der ersten Ordnung) derart definiert sind, dass sie sich als Grenzen der an ausweisen, auf mannigfache Weisen auch als Grenzen von Reihen (b_ν) darstellbar sind, wo jedes b_ν durch eine Fundamental reihe erste Ordnung $(a_\mu^{(\nu)})$ (mit festern ν) definiert ist.
>
> Ich nenne daher eine solcher Menge (b_ν) wenn sie die Beschaffenheit hat, dass $\text{Lim}\ (b_{\nu+\mu} - b_\mu) = 0$ (bei beliebigen μ), eine Fundamentalreihe zweiter Ordnung.[1]

A pleasantly simple and clear explanation of this system by Cantor is found in Tobias Dantzig's Number, the Language of Science.[2]

Consider, for example, the following sequence of rational numbers which converge to the rational number 1.

$$\frac{1}{2}, \frac{3}{4}, \frac{7}{8}, \frac{15}{16}, \ldots, \frac{2^n - 1}{2^n}, \ldots$$

---

[1] Ibid., pp. 187-188.

[2] Tobias Dantzig, Number, the Language of Science (New York: The Macmillan Company, 1930), p. 148.

A set of sequences can be generated by the successive deletion of the leading term. The members of this new set are:

$$\frac{3}{4}, \frac{7}{8}, \frac{15}{16}, \frac{31}{32}, \frac{63}{64}, \ldots$$

$$\frac{7}{8}, \frac{15}{16}, \frac{31}{32}, \frac{63}{64}, \ldots$$

$$\frac{15}{16}, \frac{31}{32}, \frac{63}{64}, \ldots$$

Each sequence is "asymptotic to each other" in that term-for-term difference sequences, formed by any two of the sequences, converges to zero--as in $\lim_{\nu = \infty} (b_{\nu+\mu} - b_{\nu}) = 0$ above. The original sequence is then called "self-asymptotic."

The extension to irrational numbers lies in the fact that the self-asymptotic property is not confined to sequences having rational limits.

For example, the sequence 3, 3.1, 3.14, 3.141, 3.1415, 3.14159, . . . where the decimal expansion is non-periodic, is different from the rationals.

Hence, the set of sequences

3, 3.1, 3.14, 3.141, 3.1415, 3.14159, . . .

3.1, 3.14, 3.141, 3.1415, 3.14159, . . .

3.14, 3.141, 3.1415, 3.14159, . . .

is an asymptotic set of sequences. Thus, the original sequence converges to some non-rational number--because the original sequence was non-periodic and so fails to converge to a rational number.

This section will conclude with a brief description of Richard Dedekind's construction of the real numbers. In 1858, while lecturing on calculus, Dedekind became dissatisfied with the geometric evidence used in proofs for the real numbers. As he comments in his essay, "Continuity and Irrational Numbers:"

> . . . even the most rigorous expositions of the differential calculus do not base their proofs upon continuity but, with more or less conscious of the fact, they appeal either to geometric notions or those suggested by geometry, or depend upon theorems which are never established in purely arithmetic manner.[1]

Especially the theorem "that every magnitude which grows continually but not beyond all limits, must certainly approach a limiting value," he felt could be given a sound arithmetical proof, and at the same time, motivated Dedekind to explicate the concept of continuity.

Dedekind had received copies of both Heine's 1872 paper and Cantor's 1872 paper, and then submitted his essay for publication only when he felt his work agreed with the other two.

The now well-known "Dedekind cuts" are present in this 1872 paper. Dedekind saw the analogy between the three properties of rational numbers--transitivity, denseness, and division into two separate, ordered, classes by any one rational--and the real line. Then when an origin and a unit

---

[1]Richard Dedekind, Essays on the Theory of Numbers trans. by Wooster Beman (New York: Dover Publications), p. 2.

length are chosen, the analogy becomes a correspondence.

Yet the rationals have gaps between them, and the real line has no gaps.

> In what then does this continuity rest? Everything must depend on the answer to this question, and only through it shall we obtain a scientific basis for the investigation of all continuous domains . . . the problem is to indicate a precise characterization of continuity that can serve as the basis for valid deduction.[1]

Thus,

> I find the essence of continuity in the following principle.
> "If all the 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."[2]

This principle of continuity is not surprising. Dedekind's real contribution, though, is his realization that this had to be an axiom rather than a theorem.

The construction of the irrationals follows immediately from the definition of rational cuts. The irrationals are constructed by cuts such that the lower ordered set has no maximum element, and the upper ordered set has no minimum element. He also shows that such cuts exist. From then on when a cut is produced by no rational number, a number--irrational--is constructed, defined by this cut.

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[1] Ibid., p. 10.

[2] Ibid., p. 11.

Thus, by the end of his essay, he can prove the motivating theorem when he explains that it is equivalent to his principle of continuity.

Trigonometric Series and Uniform Convergence

Criticism of vague concepts, improvement of proofs, and changes in method were forces affecting the foundations of analysis and its arithmetization. Another area of analysis that ultimately influenced the development of point set topology were the developments in trigonometric series and uniform convergence of such series.

In 1870 Eduard Heine published "Ueber Trigonometrische Reihe" in which he explains the importance of uniform convergence with respect to Fourier series. Heine points out that the proof that

> a given function f(x) can be represented uniquely between $x = -\pi$ and $x = \pi$ by a trigonometric series of the form
>
> $$(\alpha) \quad f(x) = \frac{1}{2} a_0 + (a_1 \cos x + b_1 \sin x) + (a_2 \cos 2x + b_2 \sin 2x) + \ldots$$
>
> is based on the hidden assumption that f is uniformly convergent.[1]

The method of proof used up to then was to set the function equal to zero,

---

[1]Eduard Heine, "Ueber Trigonometrische Reihen," Crelle's Journal 71 (1870):353.

$$0 = \frac{1}{2} a_0 + \sum_{n=1}^{\infty} (a_n \cos nx + b_n \sin nx),$$

and then multiply both sides of the equation by cos nx and integrate termwise from $-\pi$ to π. It is then shown that $a_n = 0$ for all n. Similarly, multiply the original equation by sin nx, integrate termwise from $-\pi$ to π, and then $b_n = 0$ for all n. Since all the coefficients are 0, f can be represented uniquely at 0.

The hidden assumption is that the series is uniformly convergent and thus the series may be integrated term-by-term. Weierstrass had earlier proved that such integration was allowable on uniformly convergent series.

This invalid proof suggests that there may exist non-uniformly convergent trigonometric series representing a function, and this infinite series of functions may have coefficients which are not expressed as definite integrals.

The converse theorem is also not necessarily true. That is, if $f(x) = \sum_{n=1}^{\infty} f_n(x)$ is continuous on some interval, and is representable by a Fourier series, it cannot be assumed that the series is uniformly convergent.

Heine defined in his 1870 paper that a series is <u>uniformly convergent in general</u> if it is uniformly convergent in the remaining portions of an interval after some arbitrarily small neighborhoods of the points of discontinuity are removed. He then develops the following theorems:

I Theorem
The Fourier series

$$(\alpha) \quad f(x) = \frac{1}{2} a_0 + (a_1 \cos x + b_1 \sin x) + \ldots + (a_2 \cos 2x + b_2 \sin 2x) + \ldots$$

for a finite function f(x), which has a finite number of maximums and minimums . . . converges uniformly if f(x) is continuous from $-\pi$ to π (inclusive) and $f(-\pi) = f(\pi)$. In all other cases, it is only uniformly convergent in general.

II Theorem
A function, not necessarily finite, which is, in general, continuous, can be developed in at most one trigonometric series of the form (α) if the series subject to the condition that it be uniformly convergent in general, from $-\pi$ to π.

Or, equivalently:

III Theorem
If a trigonometric series of the form (α) is, from $-\pi$ to π, uniformly convergent in general, and in general represents zero . . . then all the coefficients are zero and the series represents zero throughout.[1]

Heine did not answer the question whether or not there exists nonuniformly convergent series representing functions, but he did demonstrate the importance of uniformity considerations.

I have mentioned Heine's 1870 paper because it attracted the interest of Cantor who, also in 1870, published "Ueber einen die Trigonometrischen Reihen betreffenden Lehrsatz" in Crelle's Journal. This was the first of Cantor's papers dealing with the uniqueness of trigonometric

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[1]Ibid., pp. 354-355.

representation of arbitrary functions. Sometimes called the Cantor-Lebesgue Theorem, Cantor proves in this paper that:

> If two infinite series $a_1, a_2, \ldots a_n, \ldots$ $b_1, b_2, \ldots$ have the property that the limit of
>
> $$a_n \sin nx + b_n \cos nx$$
>
> is zero with increasing n, for every value of x which lies in a given interval $(a < x < b)$, then a_n and b_n approach zero with increasing n.
>
> When this theorem is applied to the trigonometric series, it has the meaning that the series of the kind
>
> $$\frac{1}{2} b_0 + a_1 \sin x + b_1 \cos x + \ldots + a_n \sin nx + b_n \cos nx + \ldots$$
>
> can only converge for all values of x in a given interval $(a < x < b)$ if the coefficients a_n, b_n become infinitely small with increasing n.[1]

Two weeks after submitting this paper, Cantor submitted a paper in which he stated a uniqueness theorem, "Beweis, dass eine fur jeden reelen Wert von x durch eine trigonometrischen Reihe gegeben Function f(x) sich nur auf einzige Weise in dieser Form Darstellen last." These two papers were published consecutively in Crelle's Journal, pages 130-138, and 139-142 of the 1870 volume.

In this second paper he proves that:

> When a function f(x) of a real variable x is represented by a convergent trigonometric series,
>
> $$f(x) = \frac{1}{2} b_0 + (a_1 \sin x + b_1 \cos x) + \ldots + (a_n \sin nx + b_n \cos nx) + \ldots ,$$

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[1] Cantor, Gesammelte Abhandlungen, p. 70.

> for every value of x, then there does not exist (different) series of the same form which likewise converges for every x and represents the same function f(x).[1]

Cantor's method was to assume that there were two trigonometric series representing the same function f(x) and convergent to the same value for each x in the chosen interval. He then subtracted the two series and produced a representation of zero, again convergent for all values of x in the interval. That is,

$$\text{if } f(x) = \tfrac{1}{2}a_0 + a_1\cos x + b_1\sin x + a_2\cos 2x + b_2\sin 2x + \ldots$$

$$\text{and } f(x) = \tfrac{1}{2}a_0' + a_1'\cos x + b_1'\sin x + a_2'\cos 2x + b_2'\sin 2x + \ldots$$

with a_i not all equal to a_i', and b_i not all equal to b_i'

$$\text{then } 0 = (a_0 - a_0') + (a_1 - a_1')\cos x + (b_1 - b_1')\sin x + \ldots$$

Or, in other symbols, $0 = C_0 + C_1 + C_2 + \ldots + C_n + \ldots$

where $C_0 = \frac{1}{2}(a_0 - a_0')$ and $C_i = (a_i - a_i')$

for $i = 1, 2, 3, \ldots$

He then applied the Cantor-Lebesgue Theorem to the series $0 = \sum_{i=0}^{\infty} C_i$ with the consequence that $C_i = 0$, for all i. Thus he proves that the two expressions for f(x) must have the same coefficients, thus f is uniquely represented.

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[1] Ibid., p. 80.

Georg Cantor and the Beginnings of Set Theory

Early in 1871 Cantor again published in Crelle's Journal, "Notiz zu dem Aufsatze," a refinement of his previous paper. His work was being aided by Kronecker at the time, as Cantor added in a footnote:

> Ich verdanke sie einer gefalligen, mundlichen Mitteilung des Herrn Professor Kronecker.[1]

At this time Cantor was Privatdozent at Halle, and Heine was a senior faculty member who influenced Cantor to take up research in trigonometric series. Kronecker was in Berlin, and apparently, Cantor and Kronecker were on good terms then. When Cantor began to extend the uniqueness theorem to intervals in which the theorem held, with the exception of an infinite number of points, the relation between Cantor and Kronecker deteriorated. Soon Kronecker became one of Cantor's critics.

The 1871 "Notiz" was the first paper to consider the convergence over intervals with "exceptional sets." Cantor proved in this paper that the coefficients are still all zero even if the convergent series is not defined or discontinuous for only a finite set of points.

Cantor's next papers, "Uber trigonometrische Reihen," and "Uber die Ausdehung eines Satzes aus der Theorie der trigonometrische Reihen," published in

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[1]Ibid., p. 84.

Mathematische Annalen in 1871 and 1872, extended, as the latter title announces, the results of the uniqueness theorem to certain types of infinite sets of discontinuities. This paper was mentioned above (p. 11) regarding Cantor's construction of the irrationals, but the importance of this paper to set theory was that in it Cantor first distinguished different types of infinite sets.

It was published in Mathematische Annalen rather than Crelle's Journal. This represented a step down in prestige--the Annalen was relatively new (1872 was its fifth volume) and did not carry the prestige of the much older Crelle's Journal. It has been suggested that Cantor was not to have his 1872 paper published in Crelle's Journal because Kronecker found the discussion of infinite sets mathematically offensive.

In this 1872 paper, after arithmetically constructing the reals, Cantor establishes the correspondence between numbers and points on the line. More importantly, Cantor argues that this correspondence should only be considered an axiom. He noted that a given finite or infinite collection of numbers is termed a Wertmenge, whereas a finite or infinite collection of points is termed a Punktmenge. He then gives the following definition.

> . . . By a "limit point of a point set P" I mean a point of the line for which in any neighborhood of same, infinitely many points of P are found, whereby

> it can happen that the (limit) point itself also belongs to the set. By a "neighborhood of a point" is understood an interval which contains the point in its interior. Accordingly, it is easy to prove that a point set consisting of an infinite number of points always has at least one limit point.[1]

The last statement is the Bolzano-Weierstrass theorem: any infinite bounded point set must have at least one limit point. Thus, given a point set P and any point of the real line, the given point is or is not a limit point of the set P. The set of all limit points of a set P is denoted P' and called the first derived point set of P. If, for example, the point set P is the set of rational points on the real line, corresponding to the rational numbers, then P' is the real line, or linear continuum, corresponding to the set of real numbers.

If P', the derived point set of P, is an infinite set, then P' also has a derived set (invoking the Bolzano-Weierstrass theorem), P''. If P'' is infinite, then it has a derived set, P''', and so on. While P may be a subset of P' (as in the case of the rationals = P, and the reals = P'), the subsequent derivations are the reverse set inclusions.

$$P' \supseteq P'' \supseteq P''' \supseteq P'''' \supseteq \ldots \supseteq P^{(\nu)} \ldots$$

As Cantor comments, ". . . one finds through ν such transitions the concept of the νth derived point set $P^{(\nu)}$ of P."[2]

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[1] Ibid., p. 98. Cantor is discussing only closed and bounded sets.

[2] Ibid.

The case which became important for Cantor's uniqueness theorem was the one in which, after ν repetitions, the derived set $P^{(\nu)}$ consisted of only a finite number of points, thus making the next derivation the empty set. In other words, $P^{(\nu + 1)} = \emptyset$. Cantor then called the original set P a set of the νth kind. A point set of the νth kind, for finite ν, are the derived sets of the first species.[1]

Thus, the extension of the uniqueness theorem for infinite sets read:

> If an equation is of the form
>
> $$0 = C_0 + C_1 + C_2 + \ldots + C_n + \ldots$$
>
> where $C_0 = d_0$, $C_n = c_n \sin nx + d_n \cos nx$ for all values of x with the exception of those which correspond to the points of a given point set P on the νth kind in the interval $(0 \ldots 2\pi)$, where denotes any integer, then $d_0 = 0$, and $c_n = d_n = 0$.[2]

While this extension is for derived sets of the first species, i.e., where ν is finite, Cantor was aware of the possibilities of infinite derivations, $P^{(\infty)}$. And if $P^{(\infty)}$ is not finite, then there is $P^{(\infty + 1)}$, and so on. These are the derived sets of the second species.[3]

Cantor's 1872 paper was an important first step for transfinite set theory, however, the rest of Cantorian set

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[1] Ibid.

[2] Ibid., p. 99.

[3] In the Grundlagen Cantor changes his notation from ∞ to ω.

theory (finite and infinite) did not directly follow. Between 1872 and the first publication of his theory of linear point sets in 1879, Cantor's research involved the different magnitudes of infinity, the existence of correspondence between spaces of different dimensions, and the general concepts of continuity and dimension.

Cantor's work on the different magnitudes of infinity came as a result of his attempt to understand the features that distinguish the infinite set of rationals from the infinite set of reals. He did prove early in 1874 that the reals were nondenumerable in a paper entitled "Uber eine Eigenschaft des Inbegriffes aller reelen algebraischen Zahlen" in Crelle's Journal.[1] The proof was an application of the conclusion that the set of all algebraic numbers was countable. Using Liouville's theorem which states that given in any interval on the real line there were infinitely many transcendental numbers, and since the algebraic numbers were countable, reals must be uncountable. He did not use the now famous diagonalization argument--that method of proof did not appear until 1891.

In 1874, after having established the nondenumerability of the real numbers, Cantor mentioned to Dedekind in a letter the possibility of mapping the unit square onto

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[1]Cantor, Gesammelt Abhandlungen, pp. 115-118.

a line and conversely. At first neither Cantor nor Dedekind seriously considered considered the mapping, and it was not discussed again in their correspondence until 1877.

In 1877 Cantor developed his surprising correspondence between the unit interval and the unit square. In a series of letters with Dedekind about the correspondence and its proof, in which Dedekind found some errors that Cantor was able to repair, they both agreed that the important feature of his correspondence was that the mapping was discontinuous.[1] It did, however, raise the issue of the concept of dimension. (If planes and lines correspond, and correspond uniquely, through their respective co-ordinates then the previous concept of dimension based on co-ordinates must be inadequate.) A related question is the invariance of dimension under continuous correspondences. That is, if sets of different dimensions can not be continuously mapped, one onto the other, is it then possible that a continuous mapping will guarantee preservation of dimension?

During the next ten years a number of proofs were offered to demonstrate the invariance of dimension through consideration of continuity.[2] Jakob Luroth, Johannes

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[1]Cantor/Dedekind, Briefwechsel Cantor-Dedekind, eds. E. Noether and J. Cavailes (Paris: Herman, 1937), p. 34. In this letter Cantor exclaims, "I see it, but I don't believe it!"

[2]See Dale M. Johnson, "The Problem of the Invariance

Thomae, Eugene Netto, and Cantor all attempted to prove the invariance of dimension. Dale M. Johnson points out that these proofs were only partially successful. Johnson feels that the "primitive state of topology" made the proofs especially difficult.[1] The proofs used the methods of real analysis and geometry which were inadequate to solve such a subtle question.

Yet most mathematicians by 1890 felt that either Cantor's or Netto's proof was adequate, and the problem had been solved. Indeed, Arthur Schoenflies in his article "Mengenlehre" in the Encyklopadie claimed the problem had been solved.

That this problem was addressed at all was a result of Cantor's beginning work in set theory. It is his seminal work in set theory that one can point to a rare instance in the history of mathematics were a branch of mathematics is the product of only one man. Cantor published his most important research in set theory in the series of six papers entitled "Uber unendlichen linear Punktmannigfaltigkeitslehre" (mentioned above in note 1, p. 11) from 1879 to 1884. Much of the material is the beginnings of the theory of

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of Dimension in the Growth of Modern Topology," Archive for the History of Exact Science 20 (1979):97-188.

[1] Ibid., p. 146.

transfinite cardinals and ordinals. Cantor's purpose, though, was to study linear point sets and points in the n-dimensional arithmetic continuum (R^n) and their properties. As I have mentioned above (p. 23), the fundamental concept is that of limit point, "Grenzpunkt," or in latter terminology, "Haufungspunkt," accumulation point. A point p of a set is a limit point if every neighborhood, "Umgebung," of the point contains members of the set. Using the Bolzano-Weierstrass theorem that any infinite bounded point set possesses at least one limit point, Cantor developed the notion of derived set. In addition to derived set, Cantor developed other concepts, such as an everywhere dense set in a given interval--a set in which its first derived set contains the interval itself; an isolated point set--a set that contains none of its limit points; a nowhere dense set--a set that has no dense subset, etc.

Cantor was the first mathematician to provide a point set definition of the continuum in both lines and planes. In order to define a continuum, Cantor introduces a perfect set--a set is perfect if it is identical with its set of limit points.[1] He points out that a perfect set need not be dense in any interval, and in a footnote of his Grundlagen, he provides the famous example of a nowhere dense perfect set, the Cantor ternary set, the set

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[1]Cantor, Gesammelte Abhandlungen, p. 193.

of all reals of the form

$$r = \frac{c_1}{3} + \frac{c_2}{3^2} + \ldots \frac{c_n}{3^n} + \ldots$$

where the c_i can only take the values 0 and 2.[1]

Cantor then introduces the concept of connectedness in the following way: a set P is connected if for every pair of points p and p' of P and every $\epsilon > 0$, we can choose a finite number of points, $t_1, t_2, t_3, \ldots t_n$, in P such that the distances $pt_1, t_1t_2, t_2t_3, \ldots, t_np'$ are all less than ϵ. This is sometimes called well-chained. He then can define a continuum as a connected and perfect set.[2]

In the course of his work on the continuum, Cantor claimed that any derived set could be written as the union of perfect set and a set of the first species (one that is "reducible," in Cantor's words). This was a technical error discovered by the Swedish mathematician Ivar Bendixson who showed Cantor how to reformulate the decomposition of a derived set rigorously.[3] It was this reformulation that lead Cantor to define the notion of a closed set--a set

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[1]Ibid., p. 207.

[2]Ibid., p. 194.

[3]See Joseph Dauben's biography of Cantor, Georg Cantor: His Mathematics and Philosophy of the Infinite (Cambridge: Harvard University Press, 1979), pp. 113-114, for Dauben's discussion of the Cantor/Bendixson correspondence.

which contains all of its limit points. A closed set is compared to a set which is dense-in-itself--a set which is contained in its set of limit points or its first derived set. A perfect set--due to the help of Bendixson--is then both closed and dense-in-itself.

Most historians of mathematics believe that the theory of point sets had significant influence on the growth of real function theory. While there was controversy regarding the notions of infinite sets and the kinds of infinities, and occasional criticism of set theory because of the paradoxes and "pathological" counterexamples that were to later appear, the growth of mathematics was undoubtedly stimulated by Cantorian set theory.

The Uses of the New Mathematics of Set Theory

Camille Jordan was among the first to use Cantor's set theory ideas in geometry. A survey of his classic treatises on analysis reveal the effect of set theory and Weierstrassian rigor affecting analysis at that time. It is in his Cours d'Analyse de l'Ecole Polytechnique that the definition of the Jordan Curve and the Jordan Curve Theorem are found. Although, according to Raymond Wilder, the Jordan Curve, while known to many mathematicians, was not called the Jordan curve, then, as such. As Wilder explains:

> It is not difficult to surmise where Jordan got this notion [of defining a curve through two continuous

> functions on the unit interval]; it was common practice to define curves parametrically in the books on calculus. For instance, the parametric representation of the cycloid is given in Cauchy's famous calculus, published in 1823. Evidently, what Jordan did was to formulate a general concept which was already current. I don't know who first attached Jordan's name to the notion, as a matter of fact. The Polish topologistswere already doing so during the 'tweens of the present century, and most writers of the twenties assumed he was the first to formulate it explicitly. However, I have found no evidence that either Jordan's contemporaries or his immediate successors associated his name with the concept, which seems to be further evidence that the notion was generally current among analysts.[1]

It is in his Cours d'Analyse de l'Ecole Polytechnique that the definition of a Jordan curve and the Jordan Curve Theorem are found. The Cours is also, in the changes from the first edition to the second edition, a clear example of the changes in the standards of rigor from 1880 to 1900. And it is in Jordan's attempt to prove the curve theorem that these changes in rigor are so acute. The first edition, published in three volumes between 1882 and 1887, is a textbood for a "Cours d'Analyse de l'Ecole Polytechnique." It is not a research treatise, rather a book of methods in analysis. This intention of the volumes is to be rigorous, but contains almost no Weierstrassian methods of proof. Rather, arguments are mostly geometrically and

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[1]Raymond Wilder, "The Origin and Growth of Mathematical Concepts," Bulletin of the American Mathematical Society 59 (1953):432.

physically motivated. However, in the last volume of the first edition Jordan includes a "Note sur quelques points de la theorie des fonctions."[1] In this "Note" he offers revised proofs of the main theorem along the lines of Weierstrassian rigor. It is in this "Note" that the closed curve theorem first appeared.[2]

He first defines a curve as a set of points represented by the equations

$$x = f(t), \quad y = \phi(t)$$

where f and ϕ are functions of the parameter t.

If the functions are continuous, the curve is said to be continuous. (Si ces fonctions sont continues, la courbe sera dite continue.[3])

If $f(t_1) = f(t_2)$, and $\phi(t_1) = \phi(t_2)$ for $t_1 \neq t_2$ in the interval, then the curve is said to be closed. If, for certain distinct values of t within the period, the values of x are the same and the values of y are the same, then the curve has multiple points. Jordan only considers continuous closed curves without multiple points, i.e., simple closed curves, and states the theorem as

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[1]Camille Jordan, Cours d'Analyse de l'Ecole Polytechnique, 3 vols. (Paris: Gauthier-Villars, 1882-1887) 3:549-613.

[2]Ibid., p. 593.

[3]Ibid., p. 587.

> Every continuous curve C divides the plane into two regions, the one exterior, the other interior; the latter cannot be reduced to zero because it contains a circle of finite radius.[1]

Jordan's proof is based on polygons approximating the curve C from the inside and from the outside. He assumes the theorem for simple polygons; i.e., he assumes that a polygon without multiple points divides the plane into an interior and an exterior region.

First, Jordan constructs a simple polygon P' which approximates the curve C to any degree of accuracy. Then he offers a way to construct simple polygons S and S' from P' which are inside and outside of C, respectively. These approximate C and contain it within an annular region. From this an entire sequence of interior and exterior polygons can be constructed. These approximate C with increasing accuracy and corresponding pairs form smaller and smaller annular regions which squeeze down to C. Hence, the theorem follows according to Jordan.

The second edition of the Cours is radically changed from the first. Published between 1893 and 1896, it is much more a research treatise, the standards and style of the "Note" are incorporated into the main text of the first volume of the second edition. From 1883 on, Jordan had been at the College de France, where he succeeded Liouville,

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[1]Ibid., p. 593.

in a chair in mathematics. At the College he was able to do more research and the second edition reflects this emphasis.

Jordan's need for the closed curve theorem was a result of his proof of the Cauchy Integral Theorem of integrating a complex function along a closed, rectifiable curve.

> Let C be a closed continuous line without multiple points, not containing any critical points of the function f(z) on its interior. The integral of f(z) taken along any closed and rectifiable line K, within C, is zero.[1]

Thus, while previous mathematicians did not see the need to prove the obviously true statement that a simple closed curve divides the plane into a connected interior and connected exterior region, Jordan required a proof as a result of his analysis of the Cauchy Integral Theorem.

H. Guggenheimer, in his article, "The Jordan Curve Theorem and an Unpublished Manuscript by Max Dehn," discusses the history and the criticism of the theorem and its proofs.[2] The Jordan Curve Theorem is an example of an intuitively obvious theorem which was exceedingly difficult to prove rigorously. In 1905 Oswald Veblen commented that:

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[1] Ibid., p. 605.

[2] H. Guggenheimer, "The Jordan Curve Theorem and an Unpublished Manuscript by Max Dehn," Archive for the History of Exact Sciences 17 (1977):193-200.

> Jordan's explicit formulation of the fundamental theorem that a simple closed curve lying wholly in a plane decomposes the plane into an inside and an outside region is justly regarded as a most important step in the direction of a perfectly rigorous mathematics. This may be confidently asserted whether we believe that perfect rigor is attainable or not. His proof, however, is unsatisfactory to many mathematicians. It assumes the theorem without proof in the important special case of a simple polygon and of the argument from that point on, one must admit at least that all the details are not given.
>
> The work of Schoenflies, especially in formulating a converse theorem, has thrown much light on its relation to the theory of point sets and Analysis Situs in general, and elegant proofs under restrictive hypotheses have been given by Bliss and Ames. All these discussions make more or less use of the ideas of analysis, thus implying either an axiom to the effect that a plane is a doubly extended number-manifold or a set of congruence axioms. Either of these hypotheses imposes a restriction upon the formal generality of Analysis Situs as a science independent of the magnitude of the figures treated.[1]

As a first attempt, Jordan's proof is good, but what soon followed were many criticisms and new, improved proofs. As mentioned by Veblen, and discussed by Guggenheimer, Arthur Schoenflies was one of the first critics who offered a less complicated proof--one that also was not completely adequate. Yet Schoenflies saw wider implications of the Jordan Curve Theorem to point set topology. He believed that Jordan Curve Theorem could be extended to an n-dimensional generalization--the theorem that states that an (n-1)-dimensional continuous image of the sphere divides n-space

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[1]Oswald Veblen, "Theory of Plane Curves in Non-metrical Analysis Situs," Transactions of the American Mathematical Society 6 (1905):83. My emphasis.

into two connected disjoint regions. It was Brouwer who would later prove the n-dimensional Jordan separation theorem.

In 1890, only three years after Jordan's first definition of continuous curve Giuseppe Peano published his example of a space-filling curve, a curve which covers all points of a square. It completely upset the common intuitions of mathematicians of the day. A line supposedly had no "breadth" and yet Peano's construction clearly filled the square. Hilbert and E. H. Moore also constructed such curves.

In the short paper titled "Sur une courbe, qui remplit toute une aire plane," Peano defines his curve in an analytical way without any reference to geometry.[1] This demonstrates that by 1890 the arithmetization of analysis was standard practice. For the construction he considered x and y as continuous functions of a parameter t in the interval [0, 1]; as numbers between 0 and 1, x, y, and t are given ternary representations. So,

$t = 0.a_1a_2a_3 \ldots$ in ternary notation for points in [0, 1] which correspond with the ternary numbers

$$x = 0.b_1b_2b_3 \ldots$$

$$y = 0.c_1c_2c_3 \ldots$$

where (x, y) will represent points of the unit square.

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[1]Guiseppe Peano, "Sur une courbe, qui remplit toute une aire plane," Mathematische Annalen 36 (1890): 157-160.

Then Peano attains the correspondence by letting ka denote the 2-a digit, where a is one of the digits 0, 1, 2. Thus,

$$k0 = 2, \quad k1 = 2, \quad k2 = 0.$$

$k^n a$ is the result of applying the operation k n times to a, and it has the value a, if n is even, and it has the value ka if n is odd.

t is mapped to (x, y) by

$$b_1 = a_1 \qquad c_1 = k^{a_1}a_2$$

$$b_2 = k^{a_2}a_3 \qquad c_2 = k^{a_1+a_3}a_4$$

$$b_3 = k^{a_2+a_4}a_5 \qquad c_3 = k^{a_1+a_3+a_5}a_6$$

$$\vdots \qquad\qquad \vdots$$

$$b_n = k^{a_2+a_4+\ldots+a_{2n-2}}a_{2n-1} \qquad c_n = k^{a_1+a_3+\ldots+a_{2n-1}}a_{2n}$$

The mapping covers every point of the square and the correspondence is continuous by the way the ternary representation is constructed.

In 1900, E. H. Moore explained geometrically the Peano curve in the unit square.[1] (Figure 1)

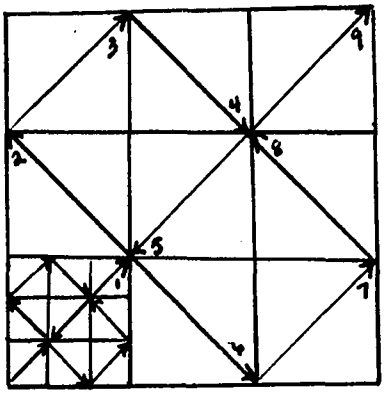


Figure 1

First we divide the unit square into 9 subsquares and draw a polygonal line (corresponding to the unit segment) through the square, as shown in Figure 1. Next we divide each smaller square and draw similar polygonal lines in each. If we continue in this manner, the limit curve is Peano's example, and the entire square is covered.

Peano's curve was another blow to the naive concept of dimension after Cantor's one-to-one correspondence between the unit interval and the unit square. Peano's curve was especially disconcerting since it was a continuous

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[1]Eliakim Hastings Moore, "On Certain Crinkly Curves," Transactions of the American Mathematical Society 1 (1900): 72-90.

correspondence. However, because of the ternary representation (and as can be seen by the curve above) certain points of the square are covered two and four times. Thus, Peano's curve is not a one-to-one correspondence.

Peano seemed to have opened a Pandora's box of strange examples and counter examples of curves and point sets which ran counter to one's usual geometrical intuitions. Set theory seemed to provide all sorts of opportunities for such examples. In 1903 William Osgood published an example of a Jordan Curve that itself had measurable area.[1] In 1910, L. E. J. Brouwer published an example of an indecomposable continua that divided the place into three regions and is a common bounder of all three.[2] William H. and Grace Chisholm Young published in 1906 the Theory of Sets of Points in which many of these "pathological" results are found.[3] In spite of being pathological, these results helped the development of point set topology.

Arthur Schoenflies was an early proponent of set theory and the potential value of its results. As mentioned

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[1]William Osgood, "A Jordan Curve of Positive Area," Transactions of the American Mathematical Society 4 (1903): 107-112.

[2]L. E. J. Brouwer, "Zur Analysis Situs," Mathematische Annalen 68 (1910): 422-434.

[3]William H. and Grace Chisholm Young, Theory of Sets of Points (New York: Chelsea Publishing Company, reprint 1972, originally published 1906).

earlier, he wrote an article entitled "Mengenlehr" for the Encyklopedia der mathematischen Wissenschaften in 1898. He also published a "Bericht" for the Deutschen Mathematiker-Vereinigung in 1900, Die Entwickelung der Lehre von den Punktmannigfaltigkeiten: Bericht Erstattet der Deutschen Mathematiker Vereinigung.[1] The Bericht was a semi-systematic, semi-historical document bringing the mathematical community up to date on set theory results. In 1904 he published the first of a three part article, "Beitrage zur Theorie der Punktmengen" in the Mathematische Annalen.[2]

Dale M. Johnson comments that the first Beitrage of 1904 opens with a good characterization of the status of point set topology as of the turn of the century. Schoenflies states that:

> As one of the most general problems of the theory of point sets we can point to the task of formulating and establishing the fundamental theory of analysis situs set-theoretically and setting forth the relationship which exist between the set-theoretico-geometric and the analytic modes of expressing these concepts and theorems. The paradoxical results, as they occur, e.g., in the one-to-one mapping of continua and the Peano curve, have completely destroyed the naive ideas of analysis situs. All the more we must demand that set theory provide a substitute and define

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[1]Arthur Schoenflies, Die Entwickelung der Lehre von den Punktmannigfaltigkeiten (Leipzig: B. G. Teubner, 1900) Jahresbericht der Deutscher Mathematiker-Vereinigung.

[2]Arthur Schoenflies, "Beitrage zur Theorie der Punktmengen," Mathematische Annalen I, 58 (1904): 195-234; II, 59 (1905): 129-160; III, 62 (1906): 256-328.

> the basic geometrical characteristic of analysis situs. Even if the much maligned intuition is no source of proof, it still seems to me that it is a goal of research to reconcile the content of geometrical definitions with the content of intuition--at least in the domain of analysis situs.[1]

Important to Schoenflies' work with the Jordan Curve Theorem and its converse is his notion of accessibility. The boundary of an open set is said to be accessible from the open set at a point P if, for every point A in the open set, there exists a simple continuous arc AP wholly in the open set. The example motivating this definition is the curve $y = \sin\frac{1}{x}$ between -1 and 1 on the x-axis (Figure 2). The points on the y-axis between -1 and 1 are not accessible from any open domain surrounding the curve.

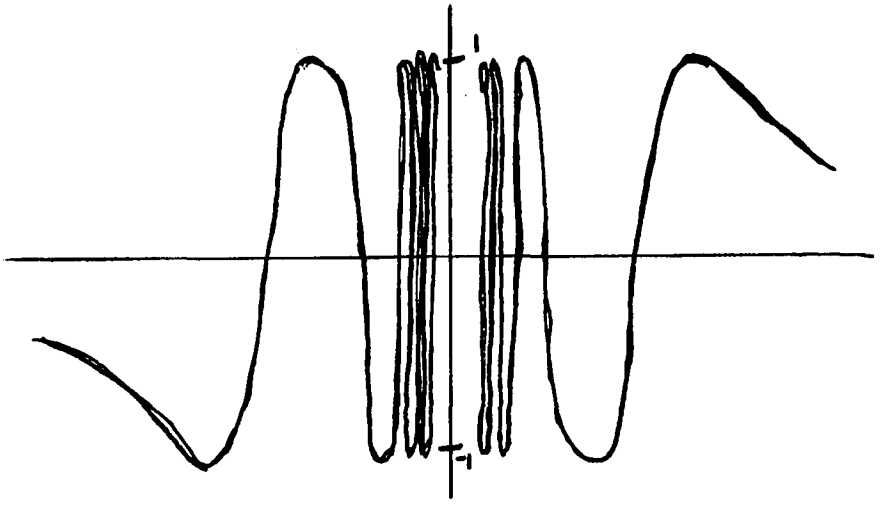


Figure 2

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[1]Johnson, "Invariance of Dimension," p. 175. Translation by Johnson.

However, Schoenflies recognized that all the points of a Jordan curve, i.e., the homeomorphic image of a circle, are accessible from both the interior domain and the exterior domain. "Domain" for Schoenflies and other mathematicians at this time usually meant open, connected set. This fact is the key to Schoenflies' converse to the Jordan Theorem. In Schoenflies' terminology, a closed curve is a bounded, nowhere dense, perfect, connected point set which divides the plane into two connected domains having the point set as their common boundary. A simple closed curve is a closed curve with all of its points accessible from each of the domains. Then the converse of the Jordan Curve Theorem states that every simple closed curve can be mapped in a one-to-one continuous manner onto the points of a circle.

It is clear that at this time Schoenflies, among others, was struggling to establish a sharp, clear distinction between a continuum and a curve. In 1923, R. L. Moore, in the Bulletin of the American Mathematical Society published a "Report on Continuous Curves from the Viewpoint of Analysis Situs." After defining simple closed curve in the usual parametric fashion he comments that:

> It is clear from the above definition that every continuous curve is a bounded continuum. That it is not, however, is very easy to determine directly from this definition whether or not even some of the simplest continua are continuous curves will, I think, be admitted if one recalls the interest aroused by Peano's discovery of the fact that a square plus its interior is such a curve and the proofs of this fact

> that were given by Peano, Hilbert, and E. H. Moore.
>
> In 1908, Schoenflies established a theorem which embodies a point set theoretic characterization (definition) of a continuous curve. That is, apart from minor matters of phraseology, etc., as follows: In order that a bounded continuum M should be a continuous curve, it is necessary and sufficient that (a) for every given positive number ϵ there are not more than a finite number of complementary domains of M of diameter greater than ϵ, (b) the boundary of every complementary domain of M is accessible at each of its points, from all sides, with respect to that domain.[1]

In 1908 Schoenflies collected his results from the Beitrage and the previous Bericht into the second part of his Bericht for the Deutsche-Mathematiker Vereinigung. In this second part of his Bericht he considered the converse of the Jordan Curve Theorem: if two simple connected bounded domains have the same boundary, then the boundary must be a simple closed curve. He generalized this result saying that therefore it was not possible for more than two domains to have a common boundary. A reasonable assumption only it happens not to be true. (Schoenflies had confused boundary with simple closed curve.) Not long after the publication of the second part of the 1908 Bericht, L. E. J. Brouwer published "Zur Analysis Situs" in the Mathematische Annalen (mentioned above). In this article Brouwer produced his now famous counterexample (and several others) of three domains having the same boundary--boundary being

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[1] R. L. Moore, "Report on Continuous Curves from the Viewpoint of Analysis Situs," Bulletin of the American Mathematical Society 29 (1923): 289-302.

called an indecomposable continuum. In other words, Schoenflies had not understood the sometimes subtle distinction between continuum and simple closed curve.

A popularization of this construction by Wada is reported by K. Yoneyama in the Tohoku Mathematical Journal of 1917.

> Suppose that there is a land surrounded by a sea, and that in this land there is a fresh lake. Also suppose that, from these lake and sea, canals are built to introduce the waters of them into land according to the following scheme.
>
> Let $E_1, E_2, \ldots, E_n, E_{n+1}, \ldots$ be a sequence of positive numbers, monotonously converging to zero, namely let
>
> $$E_1 > E_2 > \ldots > E_n > E_{n+1} > \ldots$$
>
> and
>
> $$\lim_{n \to \infty} E_n = 0$$
>
> On the first day, a canal is built from the lake, such that it does not meet the sea-water, and the shortest distance from any point on the shore of the sea to that of the lake and canal does not exceed E_1. The end point of this canal is denoted by L_1 (Figure 3).

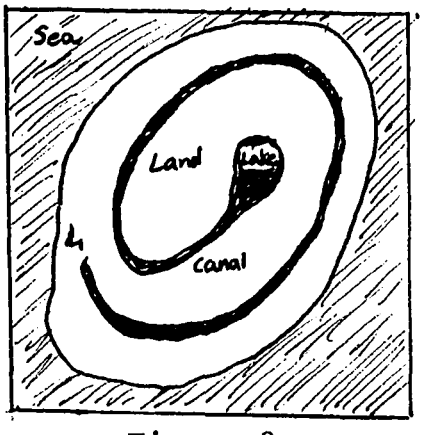


Figure 3

On the second day, a canal is built from the sea, never meeting the fresh water of the lake and canal constructed the day before, and the work is continued until the shortest distance from any point on the shore of the lake and canal filled with fresh water to that of the sea and canal filled with salt water does not exceed E_2. The end point of this canal is denoted by S_2 (Figure 4).

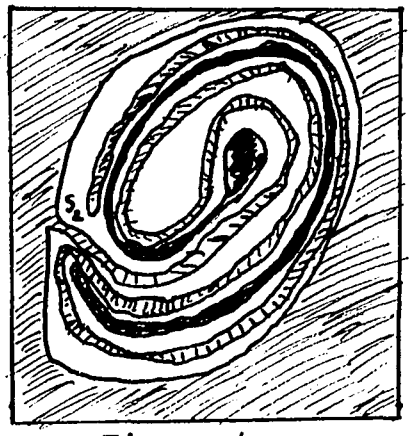


Figure 4

On the third day, the work is begun from L_1, never cutting the canals already built, and the work is continued until the shortest distance from any point on the shore of the sea and canal filled with salt water to that of the lake and canal filled with fresh water does not exceed E_3. The end point of this canal is denoted by L_2.

Now it is clear that we can continue the work day by day in the above way, by adequately narrowing the breadth of the canals, since the land is always semi-continuous at the end of the work of every day. If we preceed in this way indefinitely, we get at last an everywhere dense set of waters, fresh and salt, which never mingle together at any place. Now denote by M_L the shore of the lake and canal filled with fresh-water, and by M_S that of the sea and canal filled with salt water, and by M_P the set of limiting points of M_L and M_S, not contained in them. Then the sum of M_L, M_S, and M_P forms a continuous set, and any three points, each taken from the above different sets, form a system of three points, every two of which form a pair of principal points of the set. Thus the set is the required continuous one.

If we suppose that there are many lakes in the island, we may obtain by a similar method a

> continuous set consisting of many aggregates having the properties stated [above].
>
> Thus having proved the existence of a continuous set, which has a system of three points, every two of which form a pair of principal points, we give here the following definition . . .[1]

Nearly all of Schoenflies's theory of closed curves were shown to be incorrect through this series of ingenious counterexamples in Brouwer's "Zur Analysis Situs." Schoenflies responded to the criticism with corrections in the following article in Mathematische Annalen, "Bemerkung zu dem vorstehenden Aufsatz des Herrn L. E. J. Brouwer."[2] Brouwer's article is found on pages 422-434, Schoenflies' reply, pages 435-444.

Hans Freudenthal provides some interesting commentary regarding the correspondence.

> The two authors ordered and mailed combined reprints. Correspondence of Brouwer with Hilbert (14 May 1904, 24 June 1909, 26 July 1909) shows that some resistance by Shoenflies to Brouwer's criticism had first to be over come. Schoenflies was a brilliant lecturer (. . . Blumenthal called him the "lion" of a conference) but had difficulties in understanding Brouwer's subtlety and in competing with his awe inspiring rigor.
>
> At that time Schoenflies prepared the second edition of his 1900 Bericht [Published in 1913]. Thanks to Fricke's mediation, Schoenflies had accepted Brouwer's unofficial supervisorship. This turned out to be a

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[1] K. Yoneyama, "The Theory of Continuous Point Sets," Tohoku Mathematics Journal 12 (1917):43-158. Reprinted in L. E. J. Brouwer, Collected Works, H. Freudenthal, ed. (Boston: D. Reidel Publishing Company, 1976), 2:367-369.

[2] A. Schoenflies, "Bemerkung zu dem vorstehenden Aufsatz des Herrn L. E. J. Brouwer," Mathematische Annalen 68 (1919):435-444.

> hard job for both of them. In his correspondence with Hilbert (April through September, 1913) Brouwer complained about Schoenflies, and asked Hilbert to intervene; a picture card of 11 September 1913 bears the funny postscript of Mrs. Brouwer: "Wenn mein Mann vor Schoenflies verruckt wird, verdankt er es Ihnen." . . . Often Schoenflies misinterpreted Brouwer's remarks, which in fact were not easy to understand; sometimes he inserted them in the wrong place, or in a corrupted form, or as a second proof along with his own wrong proof . . .
>
> An isolated jotting, in Brouwer's hand, says in Dutch about A. Schoenflies 1913, "As a matter of fact all original non-trivial ideas in the Bericht have come from me," which is probably true. In the preface of A. Schoenflies 1913 Brouwer's "unselfish and efficient" cooperation is acknowledged. It appears from fragments that Brouwer would have liked Schoenflies to mention in the preface that his support was given although "Herrn B.s personliche Anshaungen uber die Grundlagen der Mengenlehr zu den leiten Prinzipien des Bericht vielfach in schroffen Gegensatz stehen."[1]

Inspite of Brouwer's (often devastating) criticism we must grant Schoenflies some credit for his attempts in this new field of mathematics. He attempted to use the new set-theoretic methods of analysis, striving for the highest standard of rigor for that period.

The Development of Abstract Spaces and Axiom Systems

It was the development of the notion of abstract spaces along with the establishment of axiom systems that ultimately clarified the confusion surrounding the emergence of various curves and continuua. And we find during this period, say, from 1890 to 1910, considerable activity

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[1]Brouwer, Collected Papers, 2:369.

regarding axioms for geometry, metric spaces, and abstract spaces in general, which eventually lead to the axioms of a general topological space.

An early and important paper regarding abstract spaces, metric spaces in particular, was Maurice Frechet's doctoral thesis at the University of Paris in 1906. Entitled "Sur Quelques Points du Calcul Fonctionnel," Frechet's motivating concern in this paper was to clarify the meaning and use of the concept of function.[1] Frechet explains:

> . . . we shall say that a functional operation U is defined on a set of elements E of arbitrary nature . . . properties of these operations constitutes the object of Functional Calculus.[2]
>
> determined value of U: U(A). The examination of the properties of these operations constitutes the object of Functional Calculus.[2]

Frechet notes that the functional calculus necessarily rests on a preliminary theory of set and that the generality of the results will depend upon the generality of this set theory. Consequently, his search is for those set theory results which are independent of the nature of the set elements. And his definition of limit follows this independent notion. He considers a sequence of arbitrary elements (L) which satisfy the two conditions: it is

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[1]Maurice Frechet, "Sur Quelques Points due Calcul Fonctionnel," Rendiconti del Circolo Mathematica di Palermo 22 (1906):1-74.

[2]Ibid., p. 1.

possible to determine if two elements of L are distinct, and it is possible to define the notion, limit of a set of elements of L, so that for an infinite set of elements $A_1, A_2, \ldots, A_n, \ldots$ of L, one can determine if there is or is not a limit element A.

> Dorénavant, nous limiterons donc à l'étude des ensembles tirés d'une classe (L) d'elements de nature quelconque mais satisfaisant aux conditions suivantes: on sait distingues si deux éléments de la classe (L) sont distincts ou non. De plus, on a pu donner une dèfinition de la limite d'une suit d'elements de las classe (L). Nous supposons donc qu'etant choisi au hasard une suit infine d'éléments (distincts ou non) de la classe (L) on puisse dire d'une façon certaine si cette suite $A_1, A_2, \ldots A_n, \ldots$ a ou non une limite A (d'ailleurs unique).[1]
>
> (I) If elements of the whole infinity, $A_1, A_2, \ldots, A_n, \ldots$ are all identical to the same element A, then the collection has the one limit A.
>
> (II) If an infinite collection $A_1, A_2, \ldots, A_n, \ldots$ has a limit A, then every collection of elements from the first collection, taken in the same order: $A_{n_1}, A_{n_2}, \ldots, A_{n_p}, \ldots$ has a limit A. I. e., an infinite subsequence has one limit which is also A.[2]

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[1] Ibid., pp. 5-6.

[2] Ibid.

An element of A of a class (L) is then defined as a limit element of E, where E is an arbitrary set of elements of class (L), when there exists an infinite collection of elements of E: $A_1, A_2, \ldots, A_n, \ldots$ which are distinct and tend towards A. Then the notions of derived set (ensemble derive), closed set (ensemble ferme), perfect set (parfait) are defined by analogy with point sets. An element of condensation (elemente de condesation) of a set E is a limit element of E which is also a limit element of each set obtained by removing denumerable set of elements from E.[1]

In section 9, "Les ensembles compact" Frechet defines a set as compact if it contains only a finite number of elements, or if all infinite sets of elements from the given set have at least one limit element.

> . . . Nous dirons qu'un ensemble est compact lorsqu'il ne comprend qu'un nombre fini d'èléments ou lorsque toute infinité de ses èléments donne lieu à au moins un èlément limite.[2]

A compact and closed set is then called "extremal."

It has been mentioned above (p. 24) in connection with Cantor's construction of derived sets that the νth derived set, $P^{(\nu)}$, contains the $\nu + 1$ derived set, $P^{(\nu)} \subset P^{(\nu + 1)}$, so that each derived set is closed. However, with Frechet's definition of limit element, this property does

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[1] Ibid.

[2] Ibid., p. 6.

not follow for abstract sets, and thus many of the theorems of point sets do not extend to Frechet's general theory of abstract sets.

As a result, Frechet considers a new class (V) of elements such that two elements can be distinguished as identical or not, and such that, to each pair of elements A and B of this class there exists a number, denoted (A, B), called the voisinage, i.e., the neighborhood of A and B such that (A, B) = (B, A) $\geq$ 0. (A, B) has two properties:

1° (A. B) = 0 if and only if A = B.

2° There exists a function f(ϵ) which tends to zero with ϵ [i.e., f(ϵ) $\rightarrow$ 0, as $\epsilon \rightarrow 0$.] such that if (A, B) $\leq \epsilon$ and (B, C) $\leq \epsilon$, then (A, C) $\leq$ f(ϵ) for all A, B, C in (V).

> Considérons une classe (V) d'éléments de nature quelconque, mais tels qu'on sache discerner si deux d'entre sont ou non indentiques et tels, de plus, qu'a deux quelconques d'entre eux A, B, on puisse faire correspondre un nombre (A, B) = (B, A) $\geq$ 0 qui jouit des deux proprietes suicantes: 1° La condition necessaire et suffisante pour que (A, B) soit nul est que A et B soient identifques. 2° Il existe une fonction positive bien déterminée f(ϵ) tendant vers zéro avec ϵ, telle que les inequalities (A, B) $\leq \epsilon$, (B, C) $\leq \epsilon$ entrainent (A, C) $\leq$ f(ϵ), quels que soient les éléments A, B, C. Autrement dit, il suffit que (A, B) et (B, C) soient petits pour qu'il en soit de même de (A, C). Nous apperlerons voisinage de A and de B le nombre (A, B).[1]

(V) is so defined that if E is in this subclass (V), and D is some subset of E, then D' (the derived set of D) will always be closed. Thus, every sequence in the class

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[1] Ibid., p. 18.

(V), satisfies the properties of class (L). His "voisinage" is defined to be a neighborhood of E for any pair of points A, B. I.e., A set E will be of class (V) if E has a neighborhood defined on it. From this Frechet defines convergence in a set E of class (V) in terms of neighborhoods--A_n converges to A if the neighborhoods (A_n, A) converge to zero. This provides Frechet with enough machinery to prove that the derived set of an arbitrary set of class (V) is closed. From this point on Frechet only deals with sets belonging to the class (V).

By page 21 of the thesis Frechet has defined spheroids of center A and radius ρ (sphéroide de centre A et rayon ρ) as the set of all B such that (A, B) $\leq \rho$. Then he proves

> Théoréme. Si un ensemble fermé F appartient à un ensemble compact E formé d'éléments d'une classe (V), on obtiendra F en supprimant de E les elements INTÉRIEURS à chacun des spheroides d'un certain ensembles denombrable de spheroides.[1]

If F is a closed subset of a compact set E, then E-F can be covered by at most a denumerable number of spheroids. Boundedness for a set of class (V) is also defined (ensemble limite):

> . . . un ensemble tiré d'une classe (V) tel que le voisinage de deux éléments quelconques de cet ensemble reste inférieur à un nombre fixe.[2]

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[1] Ibid., p. 21.

[2] Ibid., p. 22.

Every compact set is proved to be bounded.

> Théoréme. Tout ensemble compact formé d'èléments d'une class (V) est limité.[1]

And then Frechet proves the Heine-Borel property for extremal sets--sets that are closed and compact, viz., closed and bounded.

> Théoréme. Soit E un ensemble extrémal formé d'éléments d'une class (V). S'il existe une suite indéfinie G d'ensembles I_1, I_2, . . . telle que tout èlément de E soit interieur au sens éntroit a l'un au moins de ces ensembles I_n, on peut extraire de G un nombre fini de ces ensembles formant une famille H jouissant de la même propriété que G.[2]

If $G = \{I_1, I_2, \ldots\}$ is a set of subsets of an extremal set E such that for every element of A of E there is an n such that A is in interior of I_n, then there is a finite subset H of G which has the same property (i.e., it covers) as G. Notice that this theorem is phrased only in terms of denumerable sets covering E. The generalization to non-denumerable covering is usually credited to Lebesgue who first published his proof in 1904 in his Leçons sur l'Integration.

Frechet now generalizes more concepts from analysis. A sequence $\{A_n\}$ "satisfies" the conditions of a Cauchy sequence if for every $\epsilon > 0$, there is an n so that for all $p > 0$, $(A_n, A_{n+p}) < \epsilon$. A set of class (V) will be called

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[1] Ibid.

[2] Ibid.

normal if it is perfect, separable, and if every Cauchy sequence has a limit.

> Nous dirons alors qu'une classe (V) admet une généralisation du théoréme de Cauchy si toute suite d'èléments de cette classe, qui satisfait aux conditions de Cauchy, a un èlément limité (nécessairement unique).
>
> Nous appellerons ensuite classe séparable une classe qui puisse être considérée d'au moins une façon comme l'ensemble dérivé d'un ensemble dénombrable de ses propres èléments.[1]

For the rest of the chapter Frechet limits himself to "(V)-normal and separable" sets.

> . . . des classes (V) NORMALES, c'est-à-dire parfaites, séparables et admettant une généralisation du théoréme de Cauchy.[2]

Continuity of functions defined on (V)-normal sets is the next consideration. Frechet introduces the usual ϵ-δ definition of continuity for these functionals, and then shows that it is entirely equivalent to the previous sequential definition. He then defines uniform continuity of a functional U on a set E. U is uniformly continuous on E if $|U(A) - U(B)| < \epsilon$ for all pairs of A and B from E, whenever $(A, B) < \delta$, and δ is a function of ϵ. Thus he gains:

> Théoréme. Toute opération continue dans un ensemble EXTRÉMAL E formé d'èléments d'une classe (V) est uniformément continue dans E.[3]

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[1] Ibid., p. 23.

[2] Ibid., p. 24.

[3] Ibid., p. 29.

Page 30 of this thesis is where we find the definition of metric space as we now use it, and for which his thesis is so often cited. Section 49, entitled, "Introduction de l'écart" opens with the following remark:

> Lorsque nous apperlerons les résultats généraux de la PREMIÈRE PARTIE à des examples concrets, nous reconnaitrons d'abord que, dans chaque cas, on peut faire correspondre à tout couple d'éléments A, B un nombre (A, B) ≥ 0, que nous apperlerons l'écart des deux èléments et qui jouit des deux proprietes suivantes: (a) L'ecart (A, B) n'est nul que si A et B sont identiques. (b) Si, A, B, C sont trois èléments quelconques, on a toujours (A, B) ≤ (A, C) + (C, B).[1]

He points out that with the previous definition of neighborhood, by letting $f(\epsilon) = 2\epsilon$ we get the "triangle inequality." Any set which satisfies this new condition, as well as the first two conditions of neighborhoods will be of class (E) and thus have an écart defined on it. He needs this new machinery to get the "necessity" part of the following theorem. He had already proved the "sufficient" part of this theorem.

> Théoréme. La condition nécessaire et suffisante pour que toute opération continue dans un ensemble E d'èléments d'une classe (E), 1° soit bornée dans cet ensemble, 2° y atteigne sa limite supérieure, est que cet ensemble E soit extrémal.[2]

A necessary and sufficient condition that a set of E of class (E) be extremal is that every functional continuous on E be bounded there and assumes its least upper bound there.

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[1]Ibid., p. 30. [2]Ibid., p. 31.

The second half of Frechet's dissertation deals with the applications of his general theory of specific examples. An important example is the set of real functions of a real variable all continuous on the same interval I. Frechet takes limit to mean uniform limit and his ecart of f and g, (f, g) is defined by $(f, g) = \max_{x \in I} |f(x) - g(x)|$. Convergence in this norm implies uniform convergence.

Frechet seems to be the first mathematician to use the term "space" in an abstract sense. He uses the phrase "un point d'un espace (E_ω) a une infinite denombrable de dimensions." A point in a space of countably many dimensions.[1] This space is the set of all sequences of real numbers.

Frechet's paper, because of its generality, is remarkably "modern" in its approach. For that reason, too, many of his definitions remain today in use.

In Felix Hausdorff's Grundzuge we find references to Frechet's work in the very chapter where Hausdorf lists the axioms for a neighborhood space, so that Frechet certainly influenced the development of topology.

At about the same time Friedrich Riesz presented a paper at the 1908 International Congress of Mathematicians on point-set topology. Entitled "Continuity and Abstract

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[1] Ibid., p. 39.

Set Theory" ("Steiigkeit und Abstrakte Mengenlehre") Riesz started from the notion of condensation point of a subset.[1] Riesz's definition required that

> . . . each element, which is the condensation point of a subset M, is also the condensation point of each subset which contains the set M. The second postulate is that, if a subset is divided into two further subsets, each of its condensation points is also the condensation point of at least one of the additional subsets. Finally, we propose as a third postulate that a subset which consists of a single element, possesses no condensation point. By drawing on the second postulate, we herewith guarantee that only subsets possess condensation points.[2]

Riesz comments that these conditions are so general that one cannot develop much from them. Based upon an examination of mathematical continua he introduces a fourth condition which can be interpreted as a separation axiom.

> . . . each condensation point of a set is unequivocably determined by the totality of those subsets for which it is the condensation point.[3]

He then shows that the fourth axiom is an independent axiom through the following example. Let M be an infinite set, and specify that each point of M be a condensation

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[1]Friedrich Riesz, "Stetigkeit und Abstrakte Mengenlehre," Atti del IV Internazionale dei Matematici, 2 vols. (Rome: Salviucci, 1909), 2:18-24. During this period there was a distinction between limit points. Usually a "limit point" or "accumulation point" was a point which was a limit of a countable set of points, whereas a "condensation point" was the limit of an uncountable set of points.

[2]Ibid., p. 19.

[3]Ibid., p. 20.

point of every infinite subset of M. M satisfies the first three axioms but not the fourth. Reisz then compares this to Frechet's treatment since Frechet has limited his consideration to sets with the property that every condensation point of a subset be a limit element of a countable sequence contained in that subset.

Riesz, as a result of this paper, is usually credited as being the first to enunciate the concept of connected.

> . . . I now maintain that a new concept, namely that of connectedness (Verkettung), can be introduced; with it, all hitherto treated manifolds in which continuity properties matter, can now be described in relation to these. By that I mean the following. By means of an arbitrary principle let us determine for each pair of subsets of the manifold whether they are linked together or not. Again, we will begin by establishing only three fundamental postulates. First, we demand that whenever two sets are connected every pair of suprasets containing one of these sets is also connected. Second, when sets M_1 and M_2 are connected, and when set M_1 is split into two parts, at least one of these subsets shall be connected with M_2. Third, two sets which each consist of one element shall never be connected.[1]

Historically, the significance of Riesz's paper is that it is symptomatic of the search for the basic principles or concepts of point set topology by mathematicians at the turn of the century.

Foundations for Geometries

The work of Reisz, Frechet, Brouwer, Cantor and others during this period often dealt with fundamental

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[1]Ibid., p. 22.

concepts, theorems, or axioms closely related to the rise of point set topology. This kind of mathematical activity was concomitant with the activity of other mathematicians then investigating the foundations of various geometries.

The history of the development of foundations for geometries is intimately connected with the history of the development of non-Euclidean geometries during the latter part of the ninteenth century. These two histories have been extensively described, so I will present only a brief summary in order to place them in the context of the emergence of point set topology.

As a result of the study and criticism of Euclid's Elements it was understood by mathematicians that, among other things, the Fifth Postulate of Euclid's Elements--the Parallel Postulate--was independent of the other postulates, and could be altered to produce "non-Euclidean" geometries. These new non-Euclidean geometries were logically consistent, albeit sometimes counter-intuitive, in their results. The rise of non-Euclidean geometries caused mathematicians to re-examine the foundations of Euclidean geometry, to be especially careful of the foundations of the non-Euclidean geometries, and to re-consider the more general issue of the meaning of an axiom system.

Moritz Pasch was among the first mathematicians to contribute to this geometric research with his first edition

of Vorlesungen uber neurere Geometrie in 1882.[1] Pasch noted that Euclid's common notions, such as point and line, were really not, nor could not be, defined. Such definitions were circular. While everyone agreed to the essential idea conveyed by the terms "point," "line," "plane," etc., the terms would remain undefined. Once some set of undefined terms are agreed upon, then axioms are established which state the properties of the undefined terms and relations among them. With considerable depth of understanding and foresight, Pasch points out that while the motivation for the axioms may come from physical situations, the importance of an axiom system lies in its independence from the real, physical world.

> . . . if geometry is to become a genuine deductive science, it is essential that the way in which inferences are made should be altogether independent of the meaning of the geometrical concepts, and also of the diagrams; all that need be considered are the relationships between the geometrical concepts asserted by the propositions and definitions. In the course of deduction it is both advisable and useful to bear in mind the meaning of the geometrical concepts used, but this is in no way essential; in fact it is precisely when this becomes necessary that a gap occurs in the deduction and (when it is not possible to supply the deficiency by modifying the reasoning) we are forced to admit the inadequacy of the propositions invoked as the means of proof.[2]

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[1]Moritz Pasch, Vorlesungen uber neurere Geometrie, 2nd ed., revised by Max Dehn (Berlin: Springer, 1926).

[2]Pasch, 2nd ed., p. 90. Translated and quoted by Morris Kline in his Mathematical Thought from Ancient to Modern Times (New York: Oxford University Press, 1972), pp. 1008-1009.

Kleine goes on to point out the amount of activity in the foundations of gemoetry, especially in projective geometry, at this time. In 1889, Peano published The Principles of Geometry (I Principii di geometria), Federigo Enriques published Lectures on Projective Geometry in 1898 (Lezioni di geometria proiettiva), N. Whitehead published The Axioms of Projective Geometry in 1906, and in 1910 Oswald Veblen and John W. Young published the two volume Projective Geometry which has become a classic from this period.

Probably the most famous geometry book to come out of this period is David Hilbert's Grundlagen der Geometrie, first published in 1899, but revised and published in many editions.[1] Similar to Pasch's view, Hilbert lists undefined terms of "point," "line," "plane," "between," "congruent," and "line" explaining that "The precise and mathematically complete description of these relations [among the undefined terms] follows from the axioms of geometry."[2]

These undefined terms need not have any objective, physical reality. As Hilbert once said in a famous anecdote, the terms "point," "line," "plane," could be replaced by the terms "table," "chair," "beer mug" and the geometry would still be mathematically valid.

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[1]David Hilbert, Foundations of Geometry, 2nd ed. Trans. from the 10th German ed. (LaSalle, Illinois: Open Court, 1971).

[2]Ibid., p. 3.

Hilbert then lists five groups of Axioms

I	Axioms of Incidence	(eight axioms)
II	Axioms of Order	(four axioms)
III	Axioms of Congruence	(five axioms)
IV	Axiom of Parallels	
V	Axioms of Continuity	(two axioms)

With these axioms Hilbert proved some of the basic theorems of Euclidean geometry. The rest of Euclidean geometry from these axioms was completed by other mathematicians.

Of greater interest to Hilbert was the notion of the consistency of the axioms of geometry (and therefore of Euclidean gemoetry in its entirety). Since the undefined terms could be arbitrary, and possibly unrelated to any real, physical world interpretation, the question of the consistency of the resulting geometry is crucial. Especially in the case of non-Euclidean geometries where results are often not easily represented pictorially, consistency is important. It was also shown at this time that the consistency of non-Euclidean geometry rested on the consistency of Euclidean geometry.

Poincare, in 1898 in the Monist, commented on the consistency problem saying that we could believe in the consistency of an axiomatically founded structure if we could give it an arithmetic interpretation.

> In following up all the consequences of the different geometrical axioms, are we never led to

> contradictions? The axioms are not analytical judgments a priori; they are conventions. Is it certain that all conventions are compatible?
>
> These conventions, it is true, have all been suggested to us by experiences, but by crude experiences. We discover that certain laws are approximately verified, and we decompose the observed phenomenon conventionally into two others: a pure geometrical phenomenon, which obeys these laws; and a very minute disturbing phenomenon.
>
> Is it certain that this decomposition is always permissible? It is certain that these laws are approximately compatible, for experience shows that they are all approximately realized at one and the same time in nature. But is it certain that they would be compatible if they were absolutely rigorous?
>
> For us the question is no longer doubtful. Analytical geometry has been securely established, and all the axioms have been introduced into the equations which serve as its point of departure; we could not have written these equations if the axioms had been contradictory. Now that the equations are written, they can be combined in all possible manners; analysis is the guarantee that contradictions shall not be introduced.[1]

In his Grundlagen, Hilbert provides an arithmetical framework for the axioms of geometry in Chapter II, "The Consistence and the Mutual Independence of the Axioms."

> Consider the field Ω of all algebraic numbers that arise from the number 1 and the applications of a finite number of times of the four arithmetic operations of addition, subtractions, multiplications, division and the fifth operation, $|\sqrt{1 + \omega^2}|$, where ω denotes a number that results from these five operations.
>
> Consider a pair of numbers (x, y) from the field Ω as a point and the ratios (u : v : w) of any three numbers from Ω as a line provided u, v are not both zero. Furthermore, let the existence of the equation
>
> $$ux + vy + w = 0$$

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[1] H. Poincare, "On the Foundations of Geometry," The Monist 9 (October, 1898):38-39.

> mean that the point (x, y) lies on the line (u : v : w). Thereby, as is easy to see Axioms I, 1-3 and IV are immediately satisfied. The numbers of the fiels Ω are real. Bearing in mind that these can be ordered by their magnitudes it is easy to find interpretations for these points under which all axioms of order are also valid.[1]

By means of algebraic expressions for translation and rotation in analytic geometry, congruence is interpreted algebraically (i.e., two figures are congruent if one can be transformed onto the other by translation, reflection about the x-axis, and rotation).

After every concept has been interpreted arithmetically, and it is shown that the axioms are satisfied by the interpretation, Hilbert's argument is that the theorems must also apply to the interpretation because they are logical consequences of the axioms. If there were a contradiction in Euclidean geometry, then the same would hold of the algebraic interpretation of geometry, since the former is based upon the latter. The algebraic interpretation of geometry is, in turn, dependent on arithmetic. Thus, if arithmetic is consistent, so is the algebraic interpretation of geometry, and therefore, so is plane synthetic geometry.

We have encountered another case of "logical regressions" (mentioned above on page 5). In Hilbert's work the consistency of geometry ultimately depends on the consistency of the arithmetic of the integers. And as

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[1]Hilbert, p. 29.

we shall see, the consistency of arithmetic begs questions regarding the nature of numbers, the number line, and the continuum. Recall that similar issues--the arithmetization of the real numbers and uniform continuity of a function on a closed bounded interval--also gave ways to investigations into the nature of numbers and the real number line.

During this period of investigations on the foundations of geometries mathematicians were also considering different axioms for the real line. Papers like "Definition in Terms of Order Alone in the Linear Continuum and in Well-Ordered Sets" by Oswald Veblen in the Transactions of the A.M.S. in 1905, Russell's Principles of Mathematics of 1903 which contains chapters on real numbers, continuity, the philosophy of the continuum, and geometries, Young and Young's The Theory of the Sets of Points of 1906, and "On a Set of Postulates which Suffices to Define a Number-Plane" by R. L. Moore in the Transactions of 1915 all attest to the considerable interest to the issues of the real line and its axiomization.

A part of this research often included consideration of the Heine-Borel Theorem. Sometimes referred to as the Heine-Borel principle, the theorem essentially states that

> If a collection of open intervals on the real line covers a closed interval on the real line, then a finite subcollection also covers the interval.

"To cover a closed interval by a collection of intervals" means for every x in the closed interval, x is in the interior of some interval of the collection. (The interior of an interval is the interval minus the boundary.)

The above is "essentially" the Heine-Borel Theorem since during this time the Heine-Borel was stated and proved many times in different forms. It appears in different forms because of its usefulness in real function theory and in the geometry of the real line and plane. For example, in 1904 Veblen published a short paper, "The Heine-Borel Theorem" in which he shows that:

> . . . the Heine-Borel theorem is equivalent, as a continuity axiom, to the Dedekind cut proposition . . . the equivalence of the closure of a set with what may be called the H-B property is shown to apply not only to the continuum but to any set of numbers. This result applies in manifold of any number of dimensions as well as to linear sets.[1]

Veblen's statement of the theorem is

> If every number of an interval AB belongs to at least one segment of a set of segments $[\sigma]$, then there exists a finite subset of $\sigma_1, \sigma_2, \ldots, \sigma_n$ such that every number of AB belongs to at least one of the segments $\sigma_1, \sigma_2, \ldots \sigma_n$.

"Segment" for Veblen is an open interval, and "interval" for Veblen is a closed interval. After proving the theorem,

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[1]Oswald Veblen, "The Heine-Borel Theorem," Bulletin of the American Mathematical Association 10 (1904):436.

[2]Ibid.

Veblen shows that it is equivalent to the Dedekind cut proposition

> Besides being of great convenience in the routine of limiting processes, the generalized form of the Heine-Borel theorem is equally fundamental with the Dedekind cut proposition as an axiom of continuity for the real number system. The Dedekind state is as follows:
>
> If all the numbers of an interval AB fall into two classes [M] and [N], such that every M is greater than every N, then there exists a number 0 such that $M \geq 0 \geq N$ for every M and every N.[1]

Veblen's paper is only one of many papers at this time (like for instance N. J. Lennes' "Note on the Heine-Borel Theorem") using or generalizing the Heine-Borel Theorem.

The title and proof of this theorem have a curious history. Every historical reference I have seen regarding this theorem refers the reader to Eduard Heine's 1872 paper "Die Elemente der Functionlehre" in Crelle's Journal. For example, Veblen commented that the theorem is "essentially involved in Heine's proof of the theorem of uniform continuity, but has first been stated in explicit form by Borel."[2]

Young and Young on page 41 of their Theory of Sets of Points refer to Heine's 1872 paper.[3] And even more

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[1] Ibid., p. 437.

[2] Ibid.

[3] W. H. Young and Grace Chisholm Young, The Theory of Sets of Points, 2nd ed. Originally published in 1906. (Bronx: Chelsea Publishing Company, 1972), p. 41.

modern histories, like Morris Klein's[1] and Carl Boyer's[2] refer to Heine's 1872 paper.

Looking at Heine's 1872 paper we find that the first part deals with his version of the arithmetical construction of the reals, (see pp. 8-11 above), the second part is about functions and continuity of functions. The very last page, Theorem 6 is the theorem

> If f(x) is continuous on the closed interval (a, b), then it is also uniformily continuous.[3]

Heine's proof is essentially of ϵ-δ form. However, nowhere in the proof is there any kind of "Heine-Borel covering intervals" method. The Heine-Borel Theorem is nowhere contained in Heine's proof of uniform continuity as claimed by so many authors. (I have supplied a photo copy of the theorem and proof as evidence, see Figure 5.) It is true that the theorem of uniform continuity of a bounded function on a closed interval can be used to prove the Heine-Borel theorem, and conversely. Consequently, Heine's theorem and proof may be thought of as equivalent to the Heine-Borel Theorem. However, Heine never stated nor proved the Heine-Borel Theorem.

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[1] Kline, p. 953.

[2] C. Boyer, A History of Mathematics (New York: John Wiley & Sons, Inc., 1968), p. 665.

[3] Heine, p. 188. See page 70 below.

3. *Folgerung.* Wenn eine (für alle einzelnen Werthe) von $x=a$ bis $x=b$ continuirliche Function nicht überall gleiche Werthe besitzt, so erreicht sie für einen bestimmten Werth von x ein Maximum und ebenso ein Minimum.

5. *Lehrsatz.* Wenn die von $x=a$ bis $x=b$ (für alle einzelnen Werthe) continuirliche Function $f(x)$ für jeden einzelnen Werth, der zwischen a und einer rationalen oder irrationalen Zahl X liegt, wo $a<X<b$, wie nahe man auch X kommt, nicht positiv, über X hinaus aber positiv wird, so ist $f(X)=0$.

Beweis. Es sei x_1, x_2, etc. eine Zahlenreihe für X, deren Glieder sämmtlich unter X liegen sollen. Dann wird

$$f(X) = [f(x_1), f(x_2), \text{etc.}]$$

nicht positiv; negativ kann es wegen der Continuität von $f(x)$ unmöglich sein, weil es dann einen bestimmt angebbaren, von Null verschiedenen negativen Werth besässe, während $f(x)$ selbst für den kleinsten Werth, der x grösser als X macht, nach der Voraussetzung, positiv ist. Es bleibt daher nur übrig, dass $f(X)$ Null ist.

6. *Lehrsatz.* Eine von $x=a$ bis $x=b$ (für alle einzelnen Werthe) continuirliche Function $f(x)$ ist auch gleichmässig continuirlich. (B, §. 3, Def. 1).

Beweis. Bezeichnet 3ε eine beliebige Grösse, so existirt eine solche Zahl, dass von $x=a$ bis zu ihr hin $f(x)-f(a)$ absolut $\leqq 3\varepsilon$ ist. Ein Werth, der dies leistet, ist der grösste und macht zugleich $f(x)-f(a)-3\varepsilon=0$. (B, §. 3, Lehrs. 5). Dieser Werth sei x_1. In ähnlicher Art findet man eine Zahl x_2 als die grösste, welche bewirkt, dass von $x=x_1$ bis $x=x_2$ immer $f(x)-f(x_1)\leqq 3\varepsilon$ bleibt. So fährt man fort; kommt man nach einer endlichen Anzahl n von Operationen zu $x_n=b$ oder findet, dass $f(x)-f(x_{n-1})$, von $x=x_{n-1}$ bis $x=b$, noch nicht 3ε überschreitet, so ist der Satz bewiesen.

Es bliebe noch der Fall übrig, dass kein n existirt, dass also die Grössen x_1, x_2, etc. eine unendliche Reihe von wachsenden Grössen bilden, die unter b liegen. Diese Reihe wäre dann eine Zahlenreihe, deren Zahlzeichen X sei; hervorzuheben ist ihre Eigenschaft, nach der für jedes n die Gleichung besteht: $f(x_{n+1})-f(x_n)=3\varepsilon$. Nun sei η_0 von der Beschaffenheit, dass $f(X)$ sich von $f(X-\eta)$ um weniger als ε unterscheidet, so lange $\eta<\eta_0$. Zwischen die Zahlen $X-\eta_0$ und X mögen von der obigen Zahlenreihe x_n, x_{n+1}, etc. fallen, so dass (B, §. 2, Folg. 1) $f(x_{n+1})-f(x_n)$ kleiner als 2ε wäre, während es andererseits 3ε sein müsste. Die zu Grunde liegende Annahme ist daher unmöglich, und die Function eine gleichmässig continuirliche.

Halle, im October 1871.

Figure 5

The title "Heine-Borel," comes from Schoenflies and his famous (st least in this dissertation) Bericht. On pages 51 and 119 of the 1900 edition of the Bericht we find references to Heine and a statement of the Heine-Borel theorem for countable covering, with the title Heine-Borel

> . . . Dieser Satz kann ubrigens auch als eine unmittelbare Folge des S. 51 erwahnten Heine-Borel'schen Theorems betrachtet werden.[1]

By the 1913 edition of his report he simply used the title Borel Theorem.[2] The title, "Heine-Borel," has remained. Borel's name is added since he was the first to explicitly state the theorem, but for only a countable covering. The extension to a non-countable covering is credited to either Lebesgue who published his proof in 1904, or to Young who published his proof in 1902. Due to national chauvinism, the French often refer to the theorem as the Borel-Lebesgue Theorem, e.g., Bourbaki, General Topology.

Lebesgue did point out that the importance of the Theorem is not in its proof, which is relatively easy, but in perceiving its importance and stating it as a distinct theorem.

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[1]Schoenflies, Bericht (1900), pp. 51, 119.

[2]Schoenflies, Bericht (1913), pp. 234-235.

The Heine-Borel Theorem was used a great deal as a tool in the early developments of Analysis Situs, as it was then called. Besides theorems relating the real line, Heine-Borel was used to prove the Jordan Curve Theorem. The two theorems were then considered closely related. In fact during this period, depending on the paper, the Jordan Curve Theorem or the Heine-Borel Theorem was called the theorem of Analysis Situs. For an example of their connection, see N. J. Lennes, in his paper, "Curves in Non-Metrical Analysis Situs with an Application in the Calculus of Variations."[1] Lennes proves the Jordan Curve Theorem, after considerable preliminaries regarding polygons, using the Heine-Borel Theorem.

From foundations of geometry and axiom systems, we "logically regress" to the foundations of mathematics. Historically, the early investigations into the foundations of mathematics were occurring at the same time as the emergence of point set topology. More than just concurrently developing, the research into the foundations of mathematics affected the manner in which the form of point-set topology was to emerge. That is, more than content, the form and method of point set topology was influenced by developments in the foundations of mathematics.

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[1]N. J. Lennes, "Curves in Non-Metrical Analysis Situs with an Application in the Calculus of Variations," American Journal of Mathematics 33 (1911):287-326. Though published in 1911 it was read before the Chicago Section of the American Mathematical Society in 1905.

Several mathematical activities during the late ninteenth century created interest in the foundations of mathematics. One was the discovery of contradictions or paradoxes in set theory. In his correspondence with Dedekind, Cantor asked whether the set of all cardinal numbers is itself a set, because if it is, it would have a cardinal number larger than any other cardinal. He thought he answered this in the negative by distinguishing between consistent and inconsistent sets.[1] Earlier, Cesare Burali-Forti argued that the sequence of all ordinal numbers, which is well-ordered, should have the greatest of all ordinal numbers as its ordinal number. Then this largest ordinal is greater than all the ordinal numbers, etc.[2]

Several more paradoxes were soon discovered. Bertrand Russell came upon his paradox when considering the set of sets which are not elements of themselves. In June 1902 he wrote to Gottlob Frege communicating his new found paradox.

> . . . There is just one point where I have encountered a difficulty. You state that a [predicate function too, can act as the indeterminate element [i.e., a function can be an argument of

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[1]Cantor, Gesammelte Abhandlungen, pp. 445-448.

[2]Cesare Burali-Forti, "Una questione sui numeri transfiniti," Rendiconti del Circolo mathematico di Palermo 11 (1897):154-164, 260. See note below, p. 74.

> itself]. This I formerly believed, but now this view seems doubtful to me because of the following contradiction. Let w be the predicate: to be a predicate that cannot be predicated of itself. Can w be predicated of itself? From each answer the opposite follows. Therefore we must conclude that w is not a predicate. Likewise there is no class (as a totality) of those classes which, each taken as a totality, do not belong to themselves. From this I conclude that under certain circumstances a definable collection does not form a totality.[1]

Russell shortly afterwards published this set paradox in his chapter, "The Contradiction," in his Principles of Mathematics in 1903. The significance of Russell's letter to Frege should not be underestimated. Frege had recently finished the second volume of his Grundgesetze der Arithmetic, in which he believes he had formally, precisely, and rigorously established the foundations of arithmetic. It has been suggested that he was even proof-reading the printer's galleys of his book when Russell's letter arrived. The letter contained the paradox which imperilled an important basis of Frege's work. Considering Frege's disappointment, Frege's reply to Russell's letter is an example of the highest intelectual integrity.

> Your discovery of the contradiction caused me the greatest surprise and, I would also say, consternation, since it has shaken the basis on which

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[1] In Jean van Heijenoort, ed., From Frege to Godel: A Source Book in Mathematical Logic, 1879-1931 (Cambridge: Harvard University Press, 1977):124-125. Burali-Forti's paper is also found in this collection, pages 104-112.

> I intended to build arithmetic . . . I must reflect further on the matter. It is all the more serious since, not only the foundations of my arithmetic but also the sole possible foundations of arithmetic, seem to vanish.[1]

Not long after Russell's paradox, Jules Richard published his paradox involving the description of a number using fewer than a specified number of words.[2]

Most of the paradoxes occur as a result of some self-referential description, or impredicative definition. Russell's paradox involves a set built upon sets, Richard's paradox involves a number described by numbers. Russell and Whitehead pointed out that the object being defined in these paradoxes is defined in terms of a set of objects that contains the object being defined. Their response to these impredicate definitions was their theory of types. A set of type 0 contains only elements. A set of type 1 contains only elements of type 0, etc. Russell's paradox is avoided since sets of different types are involved, invalidating the paradox.

The paradoxes, like the "pathological" curves, forced mathematicians to re-examine the foundations of set theory, as the pathological curves forced mathematicians to re-examine the foundations of curves and continua.

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[1]Ibid., p. 128.

[2]Ibid., pp. 143-144.

Ernst Zermelo believed that the solution to the problem of the paradoxes of set theory resided in the restriction of the concept of set. Following Cantor's distinction between consistent and inconsistent sets, Zermelo argued in his paper, "Untersuchungen uber die Grundlagen der Mengenlehre" that sets that were "too big" would be excluded.[1] Zermelo introduced a "selection axiom" (Aussonderungs axiom) in which a defining property P and a set M determine a set. A set is formed of all those elements which possess the property P only if P already implies a relation of the form $x \in M$. Thus Russell's paradox is avoided since the property P = "not a member of itself" does not already define a relation of the form $x \in M$ (where M is the set of sets that are not members of themselves).

However, paradoxes of the type of Richard's paradox still exist unless the notion of "property" was restricted. Zermelo's set theory axiomatization was improved upon by Abraham Fraenkel,[2] and subsequent axiom systems were proposed by von Neumann,[3] Godel and Bernays, and others.

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[1]Ernst Zermelo, "Untersuchen uber die Grundlagen der Mengenlehre," Mathematische Annalen 65 (1908):261-281.

[2]Abraham Fraenkel, "Zu den Grundlagen der Cantor-Zermeloschen Mengenlehre," Mathematische Annalen 86 (1922): 230-237.

[3]John von Neumann, "Eine Axiomatizierung der Mengenlehre," Crelle's Journal 154 (1925):219-240. See also van Heijenoort, pp. 34-56.

Zermelo's axiomatization is sufficient for constructing a set theory needed for most classical analysis. However, like the consistency of arithmetic, the consistency of set theory now became an issue.

In addition, another method or device, the axiom of choice, became a debated issue in set theory at this time. For a long time the axiom of choice had been used implicitly in proofs in analysis and set theory. Following a suggestion by Erhard Schmidt, Zermelo explicitly used the axiom of choice in his 1904 paper "Proof that every set can be well-ordered."[1] van Heijenoort includes the following comments:

> Zermelo's very short paper is a part of a letter to Hilbert dated "Munden in Hannover, 24 September 1904. Cantor had conjectured that "every well-defined set can be brought into the form of a well-ordered set" and had seen in that a "fundamental law of thought of great consequence, particularly noteworthy for its universal validity."
>
> [Zermelo's] proof remains relatively simple because it makes use of a powerful new mathematical tool, the axiom of choice. What Zermelo proves is, in fact, that the axiom of choice implies the well-ordering theorem.
>
> The proof provoked sharp reactions among mathematicians, so sharp that a few years later Zermelo had to discuss them at length, after presenting a second version of the proof.[2]

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[1]The proof is translated and reproduced in van Heijenoort (pp. 139-141).

[2]van Heijenoort, p. 139. The second proof and discussion are also included, pages 183-215.

Closely related to the work on the axiomization of set theory then was work on the foundations of mathematical logic. I have mentioned Gottlob Frege above regarding his work in the foundations of arithmetic, however, the real goal of Frege's work was a precise and thorough construction of mathematical logic so that arithmetic (and thus mathematics) would be constructed as an extension of logic. Frege's Begriffsschrift, a formula language, modeled upon that of arithmetic, for pure thought essentially establishes the major symbols and methods of mathematical logic. Following the Begriffsschrift of 1879, he published Die Grundlagen der Arithmetic in 1884, and the Grundgesetze der Arithmetic in 1893, volume 1; 1903, volume 2.

Frege's work and view of mathematics resulted in what is usually called a "school" of philosophy regarding the foundations of mathematics. The "logistic school" led by Frege sought to clarify and precisely establish logic and then build mathematics from logic. As noted, Frege's plan received a decisive setback from the existence of paradoxes. Bertrand Russell and Alfred North Whitehead continued the logistic school with their publication of the three volume Principia Mathematica. The Principia, containing the Theory of Types, was originally intended by Russell to be a continuation of his Principles, but it

became a separate treatise. David Hilbert, already concerned about the consistency of arithmetic, began to formulate his own systematic foundation for mathematics--later called the "formalist school." Still another group, led by L. E. J. Brouwer, challenged many of the basic tenets of formal logic and methods of analysis instituting another school, the "intuitionists." These schools of thought regarding the foundations of mathematics are only tangentially related to the contents of the point-set topology. However, they do represent a change in view about mathematics which established a milieu favorable to the growth of point set topology.

By 1910 set theory had emerged as a separate (and respectable) discipline, and the general concept of an axiom system was regarded as important. An excellent example of this "modern" viewpoint is Hermann Weyl's Die Idee der Riemannschen Flache.[1] Weyl's work is a text based on lectures he gave at Gottingen in 1911-1912 on Riemann surfaces. The book covers the theory of complex variables, analytic functions, Riemann surface, and complex functions on Riemann surfaces. In the style of modern writing Weyl defines "surfaces" from an abstract, axiomatic point of view.

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[1]Hermann Weyl, Die Idee der Riemannschen Flache, 2nd ed. (New York: Chelsea Publishing Company, 1947). First published in 1913.

> The concept of the "two-dimensional manifold" ["zweidimensionalen Mannigfaltigkeit"] should not, for us, be tied to the idea of a point space, but given a more abstract meaning. When, generally, any totality of things ["eine Gesamtheit von Dingen"--a phrase used by Cantor] (taking the role of points) is given and, as a result of the definitions concerning them, a continuous connection exists, similar to that which exists between the points of the plane, then we speak of a two-dimensional manifold. All continuity concepts can be referred back to that of neighborhood, so an explanation of a two-dimension manifold will require both:
>
> 1. A statement of those particular things which can be considered as "points" of the manifold.
> 2. An explanation of the concept of "neighborhood" ("Umbebung").[1]

Weyl then gives a definition of a two-dimensional manifold

> Given an totality of things, which we call "Points of the Manifold F." To each point p of the manifold F are certain sets of points of the manifold defined as neighborhoods of p over F. Each neighborhood U_0 of a point p_0 must contain P_0 and allows a reversibly unique image on the interior points of a usual Euclidean circle K_0 (where p_0 passes over the midpoint of the circle) with the following stated properties:
>
> 1. If p is a point of U_0 and U is a neighborhood of p over F consisting of only points of U_0, then (under each mapping into K_0) the image of U contains the image point p in the interior; in other words, a circle k can be described about the image point p' of p so that each point of k is the image of a point of U.
> 2. If K is a circle with midpoint p, entirely contained in K_0, then there exists a neighborhood U of p over F whose image is entirely in K.[2]

Weyl's description of a two-dimensional manifold is also a topological space.

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[1] Ibid., p. 17.

[2] Ibid., pp. 17-18.

Felix Hausdorff and the Grundzuge der Mengenlehre

Point set topology fully emerges with the publication of Felix Hausdorff's Grundzuge der Mengenlehre in 1914.[1] In the introduction, Hausdorff disavows the book as a report or treatise; he claims the Grundzuge is a textbook for students. Yet, since it is so well organized, so thorough and so modern (and because it contains no exercises or problems for the students to work on) it really is more of a treatise on set theory and topology as it was known in 1914.

The book is very comprehensive, beginning with Cantorian (naive) set theory and ending with special functions and special topological spaces. The book is very modern. Many of the definitions and approaches used in the book are still in use today.

I cannot determine what the reception was to this book in Europe. In the Jahresbuch of 1914 there is a short one page review of the book in which two-thirds of the review is just a reprint of Hausdorff's own introduction, and the last third is just a list of the contents.

Apparently the book remained fairly unknown in this country since the first American review that I found was

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[1] Felix Hausdorff, Grundzuge der Mengenlehre (New York: Chelsea Publishing Company, 1978). First edition published in Berlin in 1914.

published in 1920. (This may have been a result of World War I). The review was written by Henry Blumberg in the Bulletin of the A.M.S. The review is very favorable and makes the curious comment that

> . . . If there are still mathematicians who hold the theory of aggregates under general suspicion, and are reluctant to grant it full recognition as a rigorous, mathematical discipline, they will find it hard to retain their doubts under fire of the logic of Hausdorff's treatise.[1]

This may be a kind of description of the state of knowledge of set-theory and topology in the general mathematical community in the United States around 1920.

The first six chapters of the ten chapters of this book deal with set theory. Chapter I deals with the "sum" and "section" of sets; i.e., the union and intersection of sets. Hausdorff also introduces the notion of duality, his "principle of duality." This principle states that equations involving sets remain valid if each set is replaced by its complement and the symbols for union and intersection are interchanged (essentially deMorgan's laws); and also that the subset relations remain valid after the same changes as in the above as long as the inclusions are reversed. Also found in Chapter I are the topics of rings, fields, sequences of sets, and of real

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[1]Henry Blumberg, "Hausdorff's Grundzuge der Mengenlehre," Bulletin of the American Mathematical Society 27 (1920):116.

numbers, and the points of divergence of a sequence of functions.

Set is defined in the manner of Cantor rather than in the manner of Zermelo of which Hausdorff is aware. He prefers the naive set theory view of Cantor for its heuristic and pedagogical value.

Chapter II deals with functions and their laws of combinations and operations.

Chapter III is about cardinal numbers. Hausdorff's definition of a cardinal number is in terms of correspondence:

> . . . we associate uniquely with a system of sets A a system of things "a"--of indifferent nature--in such a way that the same things correspond to two sets if and only if the sets are equivalent.[1]

These symbols or "things" we call cardinal numbers. The remainder of Chapter III is concerned with cardinalities of various infinite sets.

Chapter IV is about ordered sets. In this chapter Hausdorff distinguished sets in a manner similar to Dedekind cuts. For example, the subset M of the ordered set A is said to be "coinitial" with A if no element of A exists preceding every element of M. Similarly with "cofinal." M is "dense in A" if for every pair $a < b$ of elements of A there exists a pair of elements $m < n$ of M such that $a \leq m < n \leq b$.

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[1]Hausdorff, p. 47.

The fifth chapter is devoted to well-ordered (wohlgeordnete) sets and ordinal numbers. The treatment is similar to the chapter on cardinal numbers in that there is a discussion of finite induction, powers and products. In this chapter Hausdorff proves, in the fashion of Zermelo that every set can be well-ordered.

Chapter VI deals with partially ordered sets, the distinctions related to coinitial and cofinal elements of sets, and the classification of ordered sets.

Chapter VII begins with the statement:

> In der Anwendung auf die Punktmengen des Raumes, in der Klarung und Verscharfung der geometrischen Grundbegriffe hat die Mengenlehre ihre schonsten Triumph gefiert, die selbst von denjenigen zugestanden werden, die sich der abstrakten Mengenlehre gegenuber skeptisch verhalten.[1]
>
> Set theory has its highest triumph in its application to point sets in space and in its clarification and refinement of fundamental concepts of geometry. This triumph is conceded even by those who may be skeptical about abstract set theory.

He then reviews the preceding chapters and the various uses of set theory and the structure of the sets used in these chapters. He comes then to his justification of a topological structure of sets.

> Now a theory of spatial point sets would naturally have, in virtue of the numerous accompanying properties, a very special character, and if we wish to confine ourselves from the outset to this single case, we should be obliged to develop one theory for linear point sets, etc. Experience

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[1] Ibid., p. 209.

> has shown that we may avoid this pleonasm and set up a more general theory comprehending not only the cases just mentioned but also sets (in particular, Riemann surfaces, spaces of a finite or an infinite number of dimensions, sets of curves, and sets of functions). And, indeed, this gain in generality is associated not with increased complication, but on the contrary, with a considerable simplification, in that we utilize--at least for the leading features--only few and simple assumptions (axioms). Finally, we secure ourselves in this logical-deductive way against the errors into which our so-called intuition may lead us; this alleged source of knowledge--the heuristical value of which, of course, no one will impunge--has, as it happens, shown itself so frequently insufficient and unreliable in the more subtle parts of the theory of aggregates, that only after careful examination may we have faith in its apparent testimony.[1]

After a brief discussion of the axioms of a metric space, Hausdorff lists the axioms of a (Hausdorff) neighborhood space, which he calls a topological space.

The "Umgebungsaxiome:"[2]

> Untereinem topolgischen Raum verstehen wir eine Menge E, worin den Elementen (Punkten)x gewisse Teilmengen U_x zugeordnet sind, die wir Umgebungen von x nennen, und zwar nach Massgaben der folgenden
>
> Umbebungsaxiome:
>
> (A) Jedem Punkt x entspricht mindestens eine Umgebung U_x; jede Umgebung U_x enthalt den Punkt x.
>
> (B) Sind U_x, V_x zwei Umbebungen desselben Punktes x, so gibt est eine Umgebung W_x, die Teilmenge von beiden ist ($W_x \subset U_x \cap V_x$).
>
> (C) Liegt der Punkt y on U_x, so gibt es eine Umgebung U_y, die Teilmenge von U_x ist ($U_y \subset U_x$).
>
> (D) Fur zwei verschiedene Punkte x, y gibt es zwei Umgebungen U_x, U_y ohne gemeinsamen Punkt ($U_x \cap U_y = \emptyset$).

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[1] Ibid., pp. 210-211. [2] Ibid., p. 213.

He points out that for the Euclidean plane, the neighborhoods of any point P may be taken as the circles have P as the center. He defines a space satisfying the four neighborhood postulates as a "topological space." "In diesem Kapitel setzen wir also nur die Umgebungsaxiome, d.h. einen topologischen Raum voraus."[1] This is the object that we now call T_2 (Hausdorff) space.

There are several examples given of topological spaces. Besides the usual Euclidean spaces, certain spaces in which the distance from point to point is measured in different metrics, spaces of countably infinite numbers of dimensions, and functioning spaces.

An "inner" or "interior" point x of a set A is one possessing a neighborhood U_x lying entirely in A. A point of A that is not one of its inner points is a "brink" point (Randpunkt)--different from a boundary point. The inner points of the complement of a set A are called the outer points of A. Hausdorff then determines the boundary points through the following: Let A be a set in the space E and let B be the complement of A, then

$$E = A \cup B$$

The inner points of A are denoted A_i, the brink points of A are denoted A_r. Similarly with the complement B, we have B_i and B_r.

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[1] Ibid., p. 214.

Thus,

$$A = A_i \cup A_r$$

$$B = B_i \cup B_r$$

so, $$E = A_i \cup A_r \cup B_i \cup B_r$$

The boundary of A (Grenze) (which is also the boundary of B) is

$$A_g = A_r \cup B_r = B_g$$

A region (Gebiet) is a set consisting of only inner points. He defines the null set as a region, the whole space E as a region, and every neighborhood is a region--an "immediate consequence of axioms (A) and (C)."[1] His regions we now call open sets, and we thus find the completed definition of a topological space in his following statement:

> Die Summe beliebig vieler und der Durchschnitt enlich vieler Gebiet is wieder ein Gebiet.[2]

He then discusses sets A_α, A_β, A_γ.

> x is an α-point of A if every neighborhood U_x of x contains at least one point of A (which may be x itself)
>
> x is a β-point if every neighborhood of x contains an infinite number of points of A
>
> x is a γ-point if every neighborhood of x_3 contains a nondenumerable set of points of A.[3]

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[1] Ibid., p. 215. [2] Ibid., p. 216.

[3] Ibid., p. 220.

A_α, A_β, and A_γ are the respective sets of such points. A_β is Cantor's derived set of A. The three sets are obviously closed. Hausdorff points out that arbitrary intersections and finite unions of closed sets are still closed. A set is dense-in-itself if $A \subset A_\beta$ or if $A_\alpha = A_\beta$; and a set is perfect if $A = A_\beta$. The union of any member of dense-in-itself sets is again dense-in-itself. The largest subset of A that is dense-in-itself is called the "kernel" of A.

An infinite set without "accumulation points" (Haufungspunkt--what we call limit points) is said to be divergent. A set without divergent subsets is called compact, after Frechet, Hausdorff remarks. Then he states Cantor's intersection theorem

> (Durchschnittssatz von Cantor) Eine absteigende Folge $A_1 \supset A_2 \supset \ldots$ kompakter, abgeschlossener, von Null verschiedener Mengen hat einen von Null verschiedenen Durschschnitt.[1]
>
> (A decreasing sequence of compact, closed, non-empty sets has a non-empty intersection.)

He then proves the Heine-Borel theorem as a special case by showing that a compact, closed set contained in the union of a sequence of open sets is contained in the union of the a finite number of these open sets.

> (Satz von Borel) Ist eine kompakte abgeschossene Menge in der Summe eine Folge von Gebeiten enthalten, so ist sie bereits in einer Summe von endlich Gebieten dieser Folge enthalten.[2]

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[1] Ibid., p. 230.

[2] Ibid., p. 231.

And he proves the converse

> (Umkehrung des Borelschen Satzes). Wenn fur jedes System von Gebieten, in deren Summe die Menge A enthalden is, A bereits in einer Summe von endlich vieven Gebieten dieses Systems enthalten is, so ist A kompact und aggeschlossen.[1]

Hausdorff introduces the notion of "relativity," or in modern terminology "inherited topology." A is said to be (relatively) closed in a set M if it is the intersection of M and a closed set. A region is a "relative region of M" if it is the intersection of M and a region.[2]

On page 244 we find the modern definition of connected sets.

> Eine von Null verschiedene Menge A heisst zusammenhangend, wenn sie sich nicht in zwei fremde, von Null verschiedene, in A abgeschlossene Teilmengen spalten lasst.
>
> (A non-empty set M is said to be connected if it is not expressible as the union of two non-empty sets that are closed in M and have no points in common.)

A component of a connected set is then defined.[3]

> . . . Unter einer Komponente von A verstehen wir eine grosste zusammenhangende Teilmenge von A, d.h. eine solche, die in keiner anderen enthalten ist.
>
> (. . . By a component of A we understand it to be the largest connected subset of A, i.e., a connected subset contained in no other such subsets.)

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[1] Ibid., p. 232.

[2] Ibid., p. 240.

[3] Ibid., p. 245.

Hausdorff proves some theorems on connectivity of sets, defines dense sets, and concludes the chapter with a discussion of the topology of the real line. He essentially summarizes the work of Cantor, Pasch, Weierstrass, and others regarding sets of points on the real line and the arithmetical construction of the reals.

As one can see by this brief overview of Hausdorff's seventh chapter, most of the fundamental concepts of point set topology, as they are used today, can be found in this chapter.

Chapter VIII is about special topological spaces; one of the results being that Hausdorff extends the Heine-Borel Theorem to more general situations.[1] In this long chapter he also discusses metric spaces, Borel sets, and concludes it with a discussion of polygons, their interiors and exteriors, and finally a proof of Jordan Curve Theorem in the manner of Brouwer's proof.

Chapter IX deals with functions. He again provides the modern topological definition of continuous function with:

> Definition. Die Funktion $y = f(x)$ heisst im Punkte a stetig, wenn zu jeder Umgebung V_b des Punktes $b = f(a)$ eine Umgebung U_a des Punktes a existiert, deren Bild V_b liegt: $F(U_a) \subset V_b$.
>
> (Definition: The function $y = f(x)$ is continuous at a point a, if to every neighborhood V_b of the

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[1] Ibid., p. 272.

> point b = f(a), there is a neighborhood U_a of the point a in which the image of V_b lies: $F(U_a) \subset V_b$.[1]

In this chapter he proves many of the well-known theorems dealing with continuous functions. For example, that the image of a compact set under a continuous function is compact, and that a continuous function is uniformly continuous over a compact domain.[2] In the manner of Brouwer he proves the invariance of domain and the invariance of dimension in his section entitled "Curves and Dimension-number." The chapter ends with discussion of sets of functions, function spaces, and series of functions.

Chapter X is a discussion of measures and integration.

From today's viewpoint, nearly seventy years after the publication of this book, we can easily appreciate the wide range and thoroughness of Hausdorff's treatment. In 1913 the book must have been considered encyclopedic in detail and comprehensiveness.

From an historical point of view Hausdorff's Grunzuge is an unusual document in that it mathematically retraces so much of the historical development that lead to the emergence of point set topology. Continuous

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[1] Ibid., p. 359.

[2] Ibid., p. 364, p. 367.

functions and curves, arithmetization of the reals, cardinal and ordinal numbers, set theory, metric and more generalized spaces, axiomization, etc., are all found in his book. That Hausdorff was thoroughly conversant with investigations of Cantor, Schoenflies, Hilbert, Brouwer, Veblen, Frechet, etc., can be seen by his notes to each chapter. He cites and uses all the relevant papers (certainly all the papers discussed in this brief historical sketch) on the topics dealt with in each chapter.

One of the more curious aspects of this book is the amount of new, previously unpublished material that is found in it. That is, I have found no previously published journal articles or papers dealing with the axioms of point set topology. Prior to 1913 he published, among others, the following:

"Eine gewisse Art von geordneten Mengen," Koniglich Sachsische Gesellschaft der Wissenschaften, Mathematisch-Physikalische Klasse-Sitzungsberichte 53 (1901):460-475.

"Der Potenzbegeiff in der Mengenlehre," Jahresbericht der Deutschen Mathematikerverienigung 13 (1904):569-571.

"Intersuchengen uber Ordnungstypen," K.S.-S., 58 (1906):106-169; 59 (1907):84-159.

"Uber dichte Ordnungstypen I," J.D.M.V. 16 (1907): 541-546.

"Grundzuge einer Theorie der geordneten Mengen," Mathematische Annalen 65 (1908):435-505.

These articles are only related to the axioms and concepts of point-set topology, the major new results are found in the Grundzuge.

In 1967, the Jahresbericht der Deutschen Mathematiker Vereinigung published a memorial to Hausdorff (he and his wife committed suicide in 1942, rather than be deported to a Nazi concentration camp) consisting of remembrances and bibliographical material.[1] We learn in these articles that Hausdorff first began in astronomy, changed to probability and geometry, then to sets and order types, and then topology.

G. G. Lorentz writes:

> Nach 1909 beginnt die fruchtbarst Periode in Hausdorffs Leben. Erschreibt sein klassisches Buch, Grundzuge der Mengenlehr. Die hervorragended Darstellungskunst, Die Bescheidenheit, die elegante Kurze and Klarheit brachten ein Meisterstuck hervor, welches niemals seitdem ubertroffen und nur selten angenahert wurde. In den Kapiteln uber metrische und topologische Raume eroffnet das Buch eine neues Gebeit. Schon 1906 beachtete Frechet die Bedeutung der kompackten Mengen und studierte metrische und vollstandige Raume. Frechet behandelte auch Raume mit einer Metrik ρ, welche die Ungleichung
>
> $$\rho(x, z) \leq f(\rho(x, y), \rho(y, z))$$
>
> anstelle der Dreiecksungleichung erfullte (f war hierbei einigen Einschrankungen unterworken). Aber die Definition topologischer Raume mittels des Umgebungsbegriffes ist Hausdorffs Grosstat. Zum

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[1] Jahresbericht der Deutschen Mathematiker Vereinigung, 69 (1912):54-75.

erstenmal gibt Hausdorffs Buch eine einfache und vollstandige Theorie der topologischen und metrischen Raume. Es ist nun schwierig klarzustellen, wieviel Begriffe und Eigenschaften, die inzwischen zum festen Bestand wurden, zum erstenman in diesem Buch erscheinen.

(The most fertile period of Hausdorff's life begins after 1909. He writes his classic Grundzuge der Mengenlehre. Its superlative presentation, its modesty, its elegant brevity and clarity produced a masterpiece never surpassed since and approached but rarely. In the chapters on metric and topological spaces the book opens up a new field. Already in 1906, Frechet took into consideration the significance of compact sets and studied metric and complete spaces. Frechet also treated spaces with a metric ρ, which satisfied the inequality

$$\rho(x, z) \leq f(\rho(x, y), \rho(y, z))$$

in place of the triangle inequality (f was here subjected to some restrictions). However, the definition of topological spaces by means of the neighborhood concept is Hausdorff's greatest feat. For the first time, Hausdorff's book presents a simple and complete theory of topological and metric spaces. It would be difficult to point out how many concepts and properties which have since become permanently fixed, appeared for the first time in this book.)[1]

M. Katetov, writing in the Dictionary of Scientific Biography, agrees with Lorentz' assessment.

Hausdorff's main work was in topology and set theory. Various definitions of topological spaces and related concepts had been given, mainly by Maurice Frechet, before Hausdorff's Grundzuge der Mengenlehre appeared. The interrelations of these different approaches had not been completely recognized; and no clear way had been known to effect a gradual transition from very general spaces to those similar to spaces actually occurring in analysis and geometry.

In the Grundzuge, Hausdorff took a decisive step step in this direction. His broad approach, his aesthetic feeling, and his sense of balance may have played a

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[1]Ibid., pp. 55-56.

> substantial part. He succeeded in creating a theory of topological and metric spaces into which the previous results fitted well, and he enriched it with many new notions and theorems. From the modern point of view, the Grundzuge contained, in addition to other special topics, the beginnings of the theories of topological and metric spaces, which are now included in all textbooks on the subject. In the Grundzuge, these theories were laid down in such a way that a strong impetus was provided for their further development. Thus, Hausdorff can rightly be considered the founder of general topology and of the general theory of metric spaces.
>
> The Grundzuge is a very rare case in mathematical literature: The foundations of a new discipline are laid without the support of any previously published comprehensive work.[1]

Our brief historical sketch of the emergence of point set topology ends and is summarized by one mathematician and one book. The emergence of this subject entailed both new ideas and concepts and new approaches and methods in analysis and geometry. New topics such as sets and set theory, continua and curves, arithmetization of the real numbers, special theorems such as the Jordan Curve Theorem and the Heine-Borel Theorem, metric spaces, etc., all contributed conceptually to the development of topology. Concomitantly new methods in mathematics were evolving; such as the arithmetization of analysis, new standards of rigor found in Weierstrassian and Cantorian methods, the method and use of the infinite in a rigorous and precise

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[1] M. Katetov, "F. Hausdorff," Dictionary of Scientific Biography, C. Gillispie, ed. (New York: Charles Scribner's Sons, 1972) 6:176-177.

manner, and the new orientation to the foundations of mathematics and axiomization. These new methodological techniques contributed to both the content and the form in which point set topology would emerge.

That all of these topics and methods are to be summarized and utilized in Hausdorff's book is a very unusual incident in the history of mathematics. The significance of the history of the emergence of point set topology to the philosophy and historiography of mathematics are the subjects of the next chapters.

CHAPTER II

HISTORY OF MATHEMATICS AND HISTORIOGRAPHY OF MATHEMATICS

Chapter I was a brief sketch of the historical developments in mathematics leading to the emergence of point set topology. In this second chapter I shall discuss other histories of point set topology, the general concept of historical writing about mathematics, and the historiography of mathematics, i.e., how one goes about writing the history of mathematics.

The View of Point Set Topology at the Time of Its Emergence

When reviewing other historical accounts of topology one finds various titles associated with it. Topology has been associated with "Mengenlehre," "Analysis Situs," "Topologie," and has even been considered a part of philosophy of mathematics.

The earliest title is "Topologie" given by Benedict Listing who published Vorstudien zur Topologie in 1847. Listing was a student of Gauss' in 1834 and became professor of physics at Gottingen. He intended to title his book, Geometrie der Lage (Geometry of Position), possibly

suggested by Gauss' Latin term "geometria Situa." But Geometrie der Lage had already been used as a title by Karl von Staudt for his book on projective geometry. Poincare is credited with initiating the Latin terminology, "Analysis Situs," in 1895. Of course Poincare's "analysis situs" was algebraic topology--or "combinatorial topology" as it was soon called--and not point set topology. L. E. J. Brouwer's early work in topology was certainly algebraic topology so that his article title, "Zur Analysis Situs" (1910), is apposite. In 1922 Oswald Veblen popularized the title in the United States with the publication of his Analysis Situs (again dealing with algebraic topology) for the American Mathematical Society Colloquium Publication Series.

Point set topology, set theory, and philosophy of mathematics were topics found within the same classification in the late nineteenth and early twentiety centuries. This is reflected in the section titles of the Jahrbuch uber die Fortschritte der Mathematik--the major mathematical reviewing journal of the 19th century. In the 1898 edition there was no section of set theory or point set topology. Section I, "History and Philosophy," had within it, Chapter 2, "Philosophy and Pedagogy." In this "Philosophy and Pedagogy" chapter we find a review of Schonflies

"Mengenlehre" for the Encyklopaedia. Under Section B of this 1898 edition we find Section 8, "Elementary and Synthetic Geometry," with the sub-heading, "Kontinuitats-betrachtungen (Analysis situs, Topologie)." Most of the articles reviewed are about geometries, projective geometries, and combinatorial geometry. As the years continue there is still no set theory chapter, but set theory and point set papers are cross-indexed under both "Philosophy" and "Analysis Situs."

By 1905, Chapter 2 of Section I has been changed to "Philosophy, Set Theory and Pedagogy." In that changed section we find the Young and Young's book reviewed and E. H. Hobson's The Theory of Functions of a Real Variable reviewed under set theory. Hausdorff's (1914) use of the word "topologie" is clearly intended for set-theoretic considerations; recall Hausdorff's book is titled Grundzuge der Mengenlehre. By 1923 when Zorretti and Rosenthal wrote their article on point set topology, "Die Punktmengen," for the Encyklopaedie der Mathematischen Wissenschaften[1] set theory and point set topology were separate disciplines.

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[1]L. Zorretti and A. Rosenthal, "Die Punktmengen," Encyklopaedia der Mathematischen Wissenschaften, II C 92 (Leipzig, Teubner, 1924).

Since point set topology is a relatively recent development in the history of mathematics there are few histories of the subject. The late ninteenth century was a productive period of historical writings in mathematics. For example, we find Felix Klein's Vorlesungen uber die Entwicklung der Mathematik im 19. Jahrhundert (1920), left incomplete due to his death W. W. Ball's, A Short Account of the History of Mathematics (1888) Moritz Cantor's Vorlesungen uber Geschichte der Mathematik (1892-1904, 4 volumes), and Florian Cajori's, A History of Mathematics (1st ed., 1893, 2nd ed., 1919). As the contemporary historian of mathematics, Ivor Grattan-Guinness has commented:

> . . . the turn of the century was a bright period in research in the history of mathematics, with many distinguished mathematicians contributing major historical studies to Biblioteca Mathematica and other journals, to the Encyklopaedie der mathematischen Wissenschaften and in the form of books. This kind of activity was especially evident in Germany, where men such as H. Burkhardt, F. Engel, A. Pringsheim, and P. Stackel stand out among many others.[1]

Since these man were writing history during the late nineteenth century, point set topology was then not history, but current, emerging mathematics, so very little history regarding set theory or topology can be found in these works. Occasionally a presidential address, a lecture or paper

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[1]Ivor Grattan-Guinness, Dear Russell-Dear Jourdain (New York: Columbia University Press, 1977), p. 5.

regarding the "state of the art" or recent progress in mathematics will have references regarding set theory and topology, and provides insights.

For example, William H. Young was a great proponent of set theory in the early twentieth century. With his wife, Grace Chisholm, they published many, many papers on set theory along with their 1906 Theory of Sets of Points. In 1924, as President of the London Mathematical Society he gave his presidential address, "The Progress of Mathematical Analysis in the Twentieth Century."[1]

Young's paper is essentially a defense of the usefulness of set theory. He opens his address with an attack on the "non-believers."

> In all times perhaps, but more especially in recent times, the opposition to progress in abstract science has been made by those who were so possessed by an end to be aimed at, that they had no inclination and had often lost the capacity to study the means by which this end could be secured. The contentions are indeed not infrequently based on insufficient knowledge of the history and literature of their own special branch of research and may be summed up in the proposition that all scaffolding is futile, because no scaffolding is to appear on the finished edifice. But their objections are also due--and this is where they are most forcibly met--to ignorance of the use to which the newer concepts and methods are being put.

After a few more rhetorical flourishes he continues with:

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[1]W. H. Young, "The Progress of Mathematical Analysis in the Twentieth Century," Proceedings of the London Mathematical Society 24 (1925):421-433.

> Apart from the isolated and more tangible progress in technique, in mathematics as in physical science, the gradual getting down to the bed-rock of thought is characteristic of modern advance. Thus, in physics we have arising the molecular theories, about which all the most fruitful results may be expected to centre; and, in mathematics, the Theory of Sets, the acknowledgement and singling out of which for study may be said to form the characteristic great and notable progress in mathematics in this first quarter of a century.[1]

He then discusses the contributions of Cantor and Lebesgue, and notes the resistance to Cantor's ideas. Of this resistance to the new ideas of set theory Young believes that:

> The reluctance with which the Theory of Sets was allowed to take its individual place in the scheme of mathematics was but a symptom of the same spirit which, in the domain of scientific studies generally, is dictating the demand to relegate mathematics to the rank of the handmaid of the other sciences (Hilfswissenschaft).[2]

The remainder of the address describes instances where set theory was proved to be an invaluable tool in analysis. On the one hand Young is trying to argue that this new field is more than just a foundational tool, but a field of study in its own right, and yet, on the other hand, to influence the audience favorably toward set theory he provides examples of applications of its results.

In one sense Young's address reflects the newness of set theory in that its role and use in mathematics has

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[1] Ibid., pp. 421-422.

[2] Ibid., p. 425.

not yet been determined by the mathematical community. By 1924 the journal Fundamenta Mathematicae, which devoted many of its articles to set theory and point set topology, was well established, and "Mengenlehre" was a separate section in the Jahrbuch uber die Forschritte der Mathematik, yet set theory (and thus point set topology) had not become an established discipline. Mentioned above regarding the reception of Hausdorff's book, Henry Blumberg commented in a review of the book in 1920

> . . . If there are still mathematicians who hold the theory of aggregates under general suspicion, and are reluctant to grant it full recognition as a rigorous mathematical discipline, they will find it hard to retain their doubts under fire of the logic of Hausdorff's treatise.[1]

Other Histories of Point Set Topology

One of the earliest histories that includes a history of set theory and topology is Florian Cajori's A History of Mathematics.[2] The last chapter of his history, "The Nineteenth and Twentieth Centuries," is subdivided into the topics of an introduction, synthetic geometry, analytic geometry, algebra, analysis, theory of functions, theory of numbers, and applied mathematics. Within each topic

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[1] Blumberg, p. 116.

[2] Florian Cajori, A History of Mathematics, 2nd ed. (London: Macmillan & Co., 1919).

is discussed the work of various mathematicians contributing to the topic. Inspite of how abbreviated much of his discussion is Cajori does capture most of the ideas and spirit of the historical developments of the topics. Under the topic heading of analytic geometry we find the subtopics of Analysis Situs, Intrinsic Co-ordinates, Definition of a Curve, Fundamental Postulates, and Geometric Models (pp. 323-328). The Analysis situs discussion consists of the famous Konigsberg bridge problem and the four color problem; intrinsic co-ordinates describes the historical alternatives to rectangular or polar co-ordinates when dealing with spaces of different curvature; definition of a curve relates the problems involved with a proper definition of a continuous curve; and fundamental postulates briefly describes the history of the postulates of geometry. Later in the book (pp. 396-407) we find a relatively long discussion of the historical development of the "Theories of Irrationals and Theory of Aggregates" under the Analysis heading. Cajori's summary of the arithmetization of analysis is quite good. Regarding set theory he comments that:

> The theory of aggregates (Mengenlehre, théorie des ensembles, theory of sets) owes its development to the endeavor to clarify the concepts of independent variable and function. Formerly the notion of an independent variable rested on the naive concept of geometric continuum. Now the independent variable

> is restricted to some aggregate of values or points selected out of the continuum. The term function was destined to receive various definitions.[1]

He discusses the work of Cantor, Peano, Zermelo, Richard, Russell, Poincare, and L. E. J. Brouwer with regard to set theory and the paradoxes. He includes a short, but adequate, discussion of the limit concept, derived sets, etc., as being important parts of the "theory of point sets." For Cajori the intellectual culmination of the theory of point sets is with measure and integration.

> The theory of point sets led in 1902 to a generalization of G. F. B. Riemann's definition of a definite integral by Henri Lebesgue of Paris. E. B. Van Vleck saw the need of this change: [Bulletin of the American Mathematical Society 23 (1919):6-7] 'This (Riemann's integral) admits a finite number of discontinuities but an infinite number only under certain narrow restrictions . . . In 1902 Lebesgue with great penetration framed a new integral which is identical with the integral of Riemann when the latter is applicable but is immeasurably more comprehensive . . . This new integral of Lebesgue is proving itself a wonderful tool. I might compare it with a modern Krupp gun, so easily does it penetrate barriers which before were impregnable.'[2]

With Cajori we see again that the then contemporary view of set theory and the "theory of point sets" was that it evolved as a result of certain problems in analysis, particularly the irrationals, arithmetization problems, and functional analysis. As the field matured it became a useful tool for other areas such as measure and integration.

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[1] Ibid., p. 400.

[2] Ibid., pp. 405-406.

Cajori's first edition was published in 1913, the second, "revised and enlarged," in 1919 I have used. There is no reference to Hausdorff although Moore, Young and Young, and, in passing, Schoenflies are mentioned. I am not criticizing Cajori for not including Hausdorff in his history--after all his book is a history of all of mathematics in only 480 pages--but wish to point out that Cajori's omission of point set topology as a distinct field, or even as a developing one, is symptomatic of the uncertain position of point set topology in the early teens of this century.

Other historical writings contemporary with Cajori do not contain material on set theory or point set theory. Moritz Cantor's four volume Vorlesungen uber Geschichte der Mathematick (1920) ends with the beginning of the nineteenth century, this is also true of W. W. Ball's Short Account of the History of Mathematics (1888). Surprisingly Felix Klein's Vorlesungen uber die Entwicklung der Mathematik im 19 Jahrhundert (1926) also contains virtually no material regarding set theory or point set topology. Published after Klein's death in 1925, the work was edited by Richard Courant and Otto Neugebauer. The lectures are contained in two volumes. Volume I has chapters on Gauss, the French schools of mathematics in the nineteenth century, Crelle's Journal and pure mathematics in Germany, the development of algebraic geometry, applied mathematics,

complex functions, and group theory. Volume II contains chapters in invariant theory, special relativity, and quadratic differential forms and Riemann surfaces. These chapters reflect Klein's mathematical interests throughout his life, so that his history is more a personal history than a comprehensive history of nineteenth century mathematics--of which he was not an insignificant part. Indeed, one of Klein's important roles was that of one of the editors of the Mathematische Annalen in which many of Cantor's first papers on set theory and others were published. Klein also prevailed upon the editors of the Encyklopaedie to include the article on Mengenlehre by Schoenflies. Thus, Klein was certainly aware of the development of set theory and point set topology in the late nineteenth century, but chose not to include it in his historical survey.

As mentioned above, Zoretti and Rosenthal wrote a systematic, but not historical, article for the Encyklopaedie (1924). Their article is based on Borel's article for the French edition of the Encklopaedie published in 1912.[1]

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[1]The history of the writing and publication of the Encklopaedie in its various parts, editions, and languages is, for me, a tangled web of starts, stops, changes, and revisions which I have never been able to fully understand. Begun in the late nineteenth century in Germany, portions were later rewritten and then published in France, afterwards, re-rewritten and re-published in Germany. The numbering system of the volumes has completely baffled me.

The article begins with the fundamental properties of point sets, continues to "the structure of closed sets," then, the "correspondence between sets of different dimensions," "the cardinality of point sets," and then to "applications of set theory." Similar to Cajori, one learns that the major justification of the usefulness of set theory comes with its applications--namely to measure and integration. Citations and references are very good in the article. While not historical in account, one could use the references and citations for historical research (which I can recommend from personal experience).

More recent historical studies that contain material relevant to the emergence of point set topology are Carl Boyer's The History of Calculus and Its Conceptual Development (1949) and his A History of Mathematics (1968), Morris Kline's Mathematical Thought from Ancient to Modern Times (1972), Bourbaki's Elements d'histoire des mathematiques (1960), E. T. Bell's Men of Mathematics (1937) and Development of Mathematics (1940), and Jerome Manheim's The Genesis of Point Set Topology (1964). There have been numerous other histories of mathematics written during this century such as J. F. Schott's A History of Mathematics (1958), David Eugene Smith's two volume History of Mathematics (1923-1925), Howard Eves' An Introduction to

the History of Mathematics (1964), and D. J. Struik's A Concise History of Mathematics (1967), but these latter books have no account of either set theory or point set topology, or are so brief as to be inconsequential. Jerome Manheim's The Genesis of Point Set Topology would appear to be a history that would preclude my historical sketch of Chapter I. While I found Manheim's book a useful reference work at times, for reasons discussed below, I eventually disagreed with his approach, some choice of topics, and view of the emergence of point set topology, and thus did write my own historical sketch.

Carl Boyer's The History of Calculus and Its Conceptual Development is an excellent historical work in its range, scope, detail and care.[1] Boyer began his project in the 1930's and published the first edition in 1939, and a second edition in 1949. Realizing the difficulties with writing a history of so potentially large a topic Boyer comments in the preface to the first edition

> . . . This is not a history of the calculus in all its aspects, but a suggestive outline of the development of the basic concepts, and as such should be of service both to students of mathematics and to scholars in the field of the history of thought. The aim throughout has therefore been to secure clarity of exposition, rather than to present a confusingly elaborate all-inclusiveness of detail to display a

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[1]Carl Boyer, The History of Calculus and Its Conceptual Development (New York: Dover Publishing Company, 1949), reprint.

> meticulously precise erudition. This has necessitated a judicious selection and presentation of such material as would preserve the continuity of thought, but it is hoped that historical accuracy and perspective have not thereby been sacrificed.[1]

His book is not a minutely detailed history of the calculus; that has been left to others. Rather, Boyer conveys the "mathematical and scientific milieu" before, during, and after the decisive contributions of Newton and Leibniz.

The last three chapters, "The Period of Indecision," "The Rigorous Formulation," and "Conclusion" were of special interest to me in that he discusses the rigorization of the calculus and the arithmetization of analysis. These chapters discuss the way mathematicians reconciled their physical, geometrical, and intuitive sense of the concepts of the calculus with the newly emerging demands of rigor and arithmetization.

As he summarizes in the Introduction:

> . . . We shall find that the history of the calculus affords an unusually striking example of the slow formation of mathematical concepts by the emancipation from all sense data of ideas born of our primary insuitions. The derivative and the integral are, in the last analysis, synthetically defined in terms of ordinal considerations [i.e., the arithmetization of analysis] and not of those of continuous quantity and variability. They are, nevertheless, the results of attempts to schematize our sense impressions of these last notions. This explains why the calculus, in the early stages of its development, was bound up

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[1] Ibid., p. ii.

> with the concepts of geometry or motion, and with explanations of indivisibles and the infinitely small; for these ideas are suggested by naive intuition and experience of continuity.[1]

Chapter VI, "The Period of Indecision," discussed the reactions and criticisms of the concepts and methods of the newly invented calculus. Bishop Berkeley's criticism and the various responses to his criticism are well documented. Indeed, the majority of the chapter discusses the other justifications, explications, and proofs of the concepts and methods of the calculus.

Ultimately most of the arguments and disagreements regarding the calculus turned on the various opposing views or interpretations of the notions of limit and continuity. The concept of limit carried with it the attendent problems of infinity, infinitesimals, and transfinite arithmetic, as it was understood in the seventeenth and eighteenth centuries. Continuity was so strongly connected with intuitions about time and motion that the idea that mathematical continuity might be different from physical continuity was naturally resisted.

Yet it was the rigorous arithmetization of these concepts--limit and continuity--that eventually lead to the calculus becoming mathematically "acceptable."

Boyer describes the conceptual change this way.

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[1]Ibid., p. 11.

> It is interesting, in looking back, to see that the methods most adverse to the introduction into mathematics of the notions of infinity and continuity were precisely those which made this introduction possible. The method of limits, which at that time appeared to lead to neither infinity nor the law of continuity, was to furnish the logical basis for these, . . . [1]

In other words the early practitioners of the calculus sought to avoid notions such as division by zero through the use of infinitesimals or other methods. Yet it was those very methods which led inescapably to the concepts of limits and continuity. Thus, once mathematicians understood that the calculus, and other parts of analysis, such as Taylor series, required the use of limits and continuity, their new task became the arithmetization (and thus rigorization) of those concepts in particular, and eventually of analysis in general.

It is from this very perspective that Boyer writes in his chapter, "The Rigorous Formulation," about Bolzano, Cauchy, Weierstrass, Heine, Dedekind, and others. In the language of Thomas Kuhn and his Structure of Scientific Revolutions, these later mathematicians performed the important "mopping up" task of providing the calculus and analysis with a sound foundation. The "revolution" brought on by Newton and Leibniz had occurred, Bolzano, Cauchy, and others "cleaned up" the calculus--preserving the important conceptual contributions of the calculus, but

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[1] Ibid., p. 267.

re-formulating its foundation on mathematically acceptable (i.e., mathematically rigorous--in the sense used then) principles.

Boyer's description of the emergence of the calculus and the response to it reveals that there was both a growth in the content of mathematics, with the advent of the calculus and then arithmetization of analysis, and a change in the methods of mathematics, with the change from the geometrization to the arithmetization of the calculus and analysis

> The centering of attention on the procedures rather than on the bases of mathematics was emphasized in the eighteenth century by an extraordinary success in applying the calculus to scientific and mathematical problems. A more critical attitude was inaugurated in the nineteenth century by persistent efforts to find a satisfactory foundation for the conceptions involved in this new analysis of the infinite. Mathematical rigor was revived, and it was discovered that Euclid's postulates are not categorical synthetic judgments, as Kant maintained, but simply assumptions. Such premises, it was found, may be so freely and arbitrarily chosen that--subject to the condition that they be mutually compatible--they may be allowed to contradict the apparent evidence of the senses. Toward the close of the century, as a result of the arithmetizing tendency in mathematical analysis, it was further discovered that the concept of infinity, transcending all intuition and analysis, could be introduced into mathematics without impairing the logical consistency of the subject.[1]

With more attention to the "procedures" came a "more critical attitude." Indeed, the late nineteenth century has been referred to as commencing the "age of criticism."

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[1]Ibid., p. 2. My emphasis.

His concluding chapter effectively summarizes the intellectual developments that contributed to the emergence of calculus

> . . . the fact of such progressive achievement [i.e., the achievement of calculus] is not to be interpreted as the unfolding of a well-conceived plan. Throughout the advance of science and mathematics, elements have constantly been discarded as well as added. For this reason no investigator is able to foresee the direction which the elaboration of his views will take. Only in retrospect can one trace the path along which such development has proceeded. Although in retracing this thread of thought one can readily recognize the notions from which the ultimate concepts have sprung, the former are not in general to be identified with the latter. Each is to be considered in the light of the mathematical and scientific milieu of the period in which it appeared.[1]

He then discusses those forces which retarded the development of the calculus, besides those of the over-dependency to physical, intuitive prejudices. He suggests that

> In all probability, however, the chief obstacle in the way of the development of the concepts of the calculus was a misunderstanding as to the nature of mathematics.[2]

The "misunderstood" view is that "mathematics is a branch either of empirical science or of transcendental philosophy".[3] Instead, according to Boyer,

> Mathematics is neither a description of nature nor an explanation of its operation; it is not concerned with physical motion or with the metaphysical generation of quantities. It is merely the symbolic logic

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[1] Ibid., p. 301.

[2] Ibid., p. 303. My emphasis.

[3] Ibid.

> of possible relations, [He cites, here, Morris Cohen's Reason and Nature] and as such is concerned with neither approximate nor absolute truth, but only with hypothetical truth. That is, mathematics determines what conclusions will follow logically from given premises. The conjunction of mathematics and philosophy, or of mathematics and science, is frequently of great service in suggesting new problems and points of view.[1]

Recall, the first edition of Boyer's book was published in 1939, and the second, from which I am quoting, in 1949. His view of the "nature of mathematics" is consonant with the prevailing philosophical positions at that time. I shall discuss below how I believe Boyer's philosophical position affected his historical view and writings.

Regardless of his philosophical position regarding the nature of mathematics, Boyer's treatment of the history of the calculus is clearly a developmental, thematic interpretation. He did not focus on detail, rather he chose to describe the mathematical and scientific milieu out of which the calculus emerged. The only other thematic or developmental treatment of mathematics related to history of point set topology is found in E. T. Bell's The Development of Mathematics.[2] Due to Bell's rather dramatic and exaggerated writing style one is, at first, wary of the

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[1] Ibid., p. 308. My emphasis.

[2] E. T. Bell, The Development of Mathematics (New York: McGraw-Hill Book Company, 1940).

scholarly value of his history. And Bell has been criticized for some serious errors.[1] Hence, we must be skeptical of Bell's descriptions of personalities and motivations of mathematicians (especially regarding Jews), but, I believe, his descriptions of the history and mathematics are accurate and historically sound.

The twentieth chapter of his Development of Mathematics is entitled "Invariance," and Bell uses the theme of invariance (or invariants) in mathematics to discuss algebraic invariants, Lie groups, which he calls "transformation groups," Klein's Erlanger Program, and geometrical and spatial invariants. The last section of this chapter, "Intrinsic spacial [sic] invariance," begins with the comment in which he believes that

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[1]For example, Joseph Dauben, writing in his biography of Georg Cantor (pp. 11-12), laments that it was the fault of E. T. Bell for popularizing

> . . . the portrait of Cantor as a man whose problems and insecurities stemmed from Freudian antagonisms with his father and whose relationship with his arch-rival Leopold Kronecker was exacerbated because both men were Jewish. In fact, Cantor was not Jewish. He was born and baptized a Lutheran and was a devout Christian during his entire life.

Dauben refers the reader to Bell's Men of Mathematics (1945) where Bell writes (pp. 562-563)

> . . . The aggressive clannishness of Jews has often been remarked, sometimes as an argument against employing them in academic work, but it has not been so generally observed that there is no more academic hatred than that of one Jew for another when they

> With the extraordinary rapid development of topology, or analysis situs, in the twentieth century, the concept of invariance penetrated more deeply than ever before in both geometry and analysis. Experts in topology claim that their methods render complicated situations in analysis spacially intuitive; and to judge by the results they obtain with apparent ease in some of the most intricate problems of modern dynamics, they must be right.[1]

Bell characterizes topology as being

> . . . concerned with certain qualitative properties of a space as already defined, namely as a set of objects with a definite system of properties. The objects are usually called points; but the crude intuitive notion of a point is not the only one to which the topology of a general or abstract space is applicable. Of the properties assignable to a space, that of a neighborhood is central here.
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> Topology is the study of those properties of spaces what are invariant under homeomorphic transformations. (Here he cites Solomon Lefschetz, Topology, 1930.)[2]

He then briefly discusses the history of "combinatorial" topology and then explains that there are

> . . . two main directions, the combinatorial and the Cantorian, or analytic, which topology followed in the twentieth century. The famous theorem stated next [Jordan Curve] suggests Cantor's point sets rather than the cells described above:[3]

> disagree on purely scientific matters or when one is jealous or afraid of another . . . When two intellectual Jews fall out they disagree all over, throw reserve to the dogs, and do everything in their power to cut one anothers' throats or stab one another in the back.

This is Bell discussing the Cantor/Kronecker disagreements.

[1] Bell, p. 424.

[2] Ibid., p. 425.

[3] Ibid., p. 430.

So in addition to Poincare's contribution to combinatorial topology

> The second major impulse to topology of the period before Poincare's work of 1895 was Cantor's theory (1879-84) of point sets (Mengenlehre). With the encyclopaedic report (1908) of A. Schoenflies (1853-, German) on Cantor's theory and its development, point sets were firmly established as the groundwork of analysis. By 1920 Mengenlehre had become so popular, especially with the Polish school, that a bulky new periodical, Fundamenta mathematicae, was founded in Warsaw to celebrate the recently acquired freedom of Poland--lost nineteen years to the anti-Cantorians--and to develop Cantor's theory in all of its ramifications. These included much topology,[1] mostly from the analytic and abstract points of view.

He then discusses the work of Frechet, Brouwer ("that the dimensionality of an n-dimensional number-manifold is a topologic invariant"), Betti, Alexander, Lefschetz, and R. L. Moore.

Bell is convinced that modern topology is one of the most modern of modern mathematics.

> Topology has been given [in this chapter] what may seem more than its just allotment of space in a general account for a reason of the very first importance. Many feel that topology had added a new dimension to mathematical thought. The classical quantitative, metric, and arithmetic approaches appear to be incapable of leading to anything either valuable or comprehensive in the qualitative aspects of mathematics. Certainly topologic thinking is unlike any other in mathematics, and it requires either a type of mind distinct from the ordinarily good mathematical intelligence, or a training radically different from that fostered by conservativism, if it is to be applied effectively.[2]

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[1] Ibid., p. 432.

[2] Ibid., p. 434.

> . . . One thing seems certain: to think topologically, the thinker must be young. The cradle with its enchained teething rings may be a little too early; but the education of a prospective topologist should not in any case be deferred beyond the third year.[1]

This last quotation says more about Bell's awe of modern topology than his views of education.

Actually he closes the chapter with a few, rather prophetic words.

> It seems not altogether fantastic to imagine that a few centuries hence the qualitative habit of thought will have superseded the quantitative in the growing parts of mathematics. Certain indications in science, and many in mathematics, point to the analysis of structure as the mathematics of the future. Stated roughly, it is not things that matter, but the relations between them; and if topology with its spacial visualizations of intricate relations between abstract 'objects' has made possible a rudimentary but still difficult analysis of relations, it may be the germ of mathematics of the future. In any event, topology is so unlike previous mathematics that it hints of a major transition.[2]

Considering the importance of general topology in the twentieth century it is surprising how few general histories of mathematics contain any significant discussion about the subject. For example, Howard Eves' An Introduction to the History of Mathematics contains only passing reference to point set topology. Two of the more famous general surveys, Carl Boyer's 1968, A History of

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[1] Ibid., p. 425.

[2] Ibid., p. 435.

Mathematics[1] and Morris Kline's 1972 Mathematical Thought from Ancient to Modern Times[2] are exceptions and contain chapters, or portions of chapters, on the history of topology.

In Boyer's A History of Mathematics we find a full chapter on "The Arithmetization of Analysis." Similar in style and content to the chapters I have mentioned in his history of calculus, Boyer traces the development of analysis from the beginnings of Fourier series, to the Bolzano-Weierstrass theorem, to the arithmetization of the real numbers, and down to Cantor and his work. The last chapter of his book, "Aspects of the Twentieth Century," contains a section on point set topology, immediately following a brief section of history of measure and integration. For Boyer, as with Bell, topology is a phenomena of the twentieth century. "The new theories of integration were closely allied with another pronounced characteristic of the twentieth century, the rapid growth of point set topology," is the way Boyer opens his discussion of topology.[3]

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[1]Carl Boyer, A History of Mathematics (New York: John Wiley & Sons, 1968).

[2]Morris Kline, Mathematical Thought from Ancient to Modern Times (New York: Oxford University Press, 1972).

[3]Boyer, A History of Mathematics, p. 665.

Boyer describes Frechet's work, and the influence of Hilbert, Poincare, Fredholm (with regard to integral equations and functional analysis), and Cantor on the development of topology. Characteristic of Boyer, he considers the abstract concepts of topology as symbols of the mathematical milieu of the twentieth century.

> . . . Probably no aspect of twentieth-century mathematics stands out more clearly than does the ever greater degree of generalization and abstraction. From the time of Hilbert and Frechet the notions of abstract set and abstract space have been fundamental in research.[1]

He ends his section with a discussion of Hausdorff's Grundzuge, pointing out the important contribution of Hausdorff's axioms. Underscoring his view of the abstractions and generalizations afforded by topology he comments:

> If any one book marks the emergence of point set topology as a separate discipline, it is Hausdorff's Grundzuge. It is interesting to note that although it was the arithmetization of analysis that began the train of thought that lead from Cantor to Hausdorff, in the end the concept of number is thoroughly submerged under a far more general point of view. Moreover, although the word "point" is used in the title, the new subject has as little to do with the points of ordinary geometry as with the numbers of common arithmetic. Topology has emerged in the twentieth century as a subject that unifies almost the whole of mathematics, somewhat as philosophy seeks to coordinate all knowledge. Because of its primitiveness, topology lies at the basis of a very large part of mathematics, providing it with an unexpected cohesiveness.[2]

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[1] Ibid., p. 667.

[2] Ibid., p. 669.

Boyer's use of the word "primitiveness" undoubtedly means that topology's concepts are profoundly basic or fundamental so that the concepts tend to pervade many aspects of modern mathematics--a view with which I would agree.

Morris Kline, in his Mathematical Thought from Ancient to Modern Times, devotes a full chapter to topology, "The Beginnings of Topology." Kline begins this chapter with the interesting historical and conceptual summary under the heading, "The Nature of Topology."

> A number of developments of the nineteenth century crystallized in a new branch of geometry, now called topology but long known as analysis situs. To put it loosely for the moment, topology is concerned with those properties of geometric figures that remain invariant when the figures are bent, stretched, shrunk, or deformed in any way that does not create new points or fuse existing points . . .
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> Topology has had numerous and varied origins. As with most branches of mathematics, many steps were made which only later were recognized as belonging to or capable of being subsumed under one new subject . . .[1]

Somewhat different from Boyer, Kline views the history of topology as an outgrowth of several trends of the nineteenth century rather than a symbol of the twentieth. Indeed, sprinkled throughout his discussion are references for the reader back to previous sections of his history--such as those dealing with Cantor, Jordan, Borel, Lebesgue, and Frechet. He then describes Hausdorff's work and book, the Grundzuge, with a discussion of the axioms and other

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[1]Kline, p. 1158.

results of Hausdorff. Beyond Hausdorff he discusses the work of Urysohn, Menger, and Poincare on metric spaces, abstract spaces, and dimension theory. The balance of the chapter is a brief history of algebraic topology, the fixed point theorems of topology, and homotopy theory.

We see the differences in historical attitudes between Boyer and Kline. Kline, known for his interest in the nineteenth century and the applications of mathematics, describes point set topology in terms of a culmination of several technical developments of nineteenth century mathematics. Boyer, who tends to prefer viewing mathematical developments in larger historical settings, describes point set topology as more of an expression of the character of the new twentieth century movement towards greater abstraction and generalization.

In either case, both authors leave the distinct impression that the emergence of point set topology was almost to be expected, that it "naturally" occurred either as an inescapable result of nineteenth century mathematical ancestors or of twentieth century tendencies.

While it is true that the work of various mathematicians in different fields was embraced by this new approach and theory of topology, none of the participants at the time ever described their work as a part of a

larger, more general, setting. Certainly part of Hausdorff's genius was appreciating the very general setting of topology in which so much of analysis and geometry could be placed, but a more accurate characterization of the emergence of point set topology prior to Hausdorff was that much of the "facts" of point set topology "were in the air" with no one, except perhaps Hausdorff, appreciating their interconnections.

A good example of a belief in the inescapable, inevitable historical development of mathematics is found in Bourbaki's "Historical Notes" at the ends of various chapters of their books. These notes have been collected into a volume, Elements d'histoire des mathematiques.[1] In their General Topology is a "Historical Note" after Chapter I on point set topology.[2]

After commenting that the "ideas of limit and continuity go back to antiquity," and a complete history of limit and continuity would require the study of Greek philosophy as well as Greek mathematics, Bourbaki believes that:

> It is Riemann who should be considered as the creator of topology, as of so many other branches of

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[1]Nicolas Bourbaki, Elements d'histoire des mathematiques (Paris: Hermann, 1969).

[2]Nicolas Bourbaki, General Topology (Paris: Hermann, 1964), pp. 162-168.

> modern mathematics. He was the first to attempt to formulate the notion of a topological space; he conceived the idea of an autonomous theory of such spaces; he defined invariants (the "Betti numbers") which were to play a pre-eminent part in the later development of topology; and he was the first to apply topology to analysis (periods of abelian integrals).[1]

Bourbaki then comments on the mathematical milieu during the nineteenth century favorable to the development of topology

> . . . In the first place, the desire to put mathematics on a firm basis, which was the cause of so many important researches throughout the nineteenth century and up to the present day, had led to a correct understanding of the notions of a convergent series and a sequence of numbers tending to a limit (Cauchy, Abel) and to the notion of a continuous function (Bolzano, Cauchy). On the other hand, the geometrical representation (by points of a plane) of the complex numbers . . . constituted an advance of the same order as the adoption, in our century, of the language of geometry in the study of Hilbert space, and contained the germ of the possibility of a geometrical representation of every object capable of continuous variation.[2]

Bourbaki's mission, in their books, was to present mathematics they considered thoroughly understood and worked-out in its most modern, abstract setting. The modern, abstract setting is, for Bourbaki, the "correct" understanding of mathematics. Presumably, then, notions about convergent series and sequences of numbers tending to a limit prior to the "correct" understanding were not a part

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[1] Ibid., p. 162.

[2] Ibid. My emphasis.

of mathematics, only a part of its history. Bourbaki's modern viewpoint can be detrimental to their historical references. For example, they say that

> Before a general theory of topological spaces, such as Riemann had envisaged, could be developed, it was necessary that the theory of real numbers, of sets of numbers, of sets of points on a line, in a plane and in space should be more systematically investigated than they had been in Riemann's time.[1]

Through the use of the passive voice, Bourbaki suggests that a kind of consciously planned research program on the part of some one or group of mathematicians were adopted to produce general topology. It is true that much of the investigations about the real line, real numbers, etc., were conducted prior to systematic treatment by Hausdorff, but major results in general topology--the Heine-Borel theorem, Jordan Curve theorem, the notion of compactness,--were also known before Hausdorff's book. Each contributing idea to the emergence of point set topology had its own history and development; they all eventually created the stage for the emergence of point set topology. Yet there was certainly no plan, no a priori understanding of what were the important and necessary preconditions or pre-concepts before "a general theory of topological spaces, such as Riemann had envisaged, could be developed."

Bourbaki commits the error in historical writing of "rational reconstruction." Instead of using the actual

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[1]Ibid., p. 164.

history as their guide, they use the modern understanding and viewpoint as their guide in writing history. They wish to "rationally reconstruct" the history of mathematics along the lines of its formal or intellectual construction. Bourbaki tends to write history as they would have liked it to have occurred rather than how it actually occurred.

To temper my criticism somewhat let me remind the reader that Bourbaki was not attempting to write complete, thorough mathematics history in their "Historical Notes." Probably the intent was to give the graduate student who was learning from their texts some appreciation of the setting and historical developments of the particular topic discussed. Yet, Bourbaki contributes to the belief in the inescapable inevitability of certain mathematical concepts occurring when they did in the history of mathematics. This "march of history" suggests, if anything, an inherent problem of professional mathematicians attempting to write mathematics history.

The only substantial monograph exclusively devoted to the history of point set topology is Jerome Manheim's The Genesis of Point Set Topology.[1]

Manheim's monograph is a wide ranging study of the historical developments creating the "genesis of point

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[1]Jerome Manheim, The Genesis of Point Set Topology (New York: Macmillan Company, 1964).

set topology." Beginning his book with a brief discussion of the history of the calculus, Manheim believes the calculus "forced upon philosophically oriented mathematicians the necessity to consider seriously the notion of nearness."[1]

> . . . The vague and inconsistent notions of limit which accompanied the expositions of Newton and Leibniz created difficulties which they and their followers were unable to resolve. For the next two centuries the attention of the leading mathematicians was directed towards this problem of nearness; its ultimate satisfactory solution provided both a basis and a motivation for the development of point set topology.[2]

Thus, after his discussion of the "difficulties associated with the appearance of calculus," Chapter II describes the "early attempts to make analysis rigorous." For Manheim, the beginnings of research into point sets came with the development of Fourier series. He describes the various equations analyzing the vibrating string, their historical significance, and their generalizations which led to Dirichlet's and Riemann's study of Fourier sets. As he summarizes:

> Investigations by Dirichlet established sufficient conditions for a Fourier series to converge to the function from which it was derived. Incidental to these researches he reevaluated the notion of a function, insisting upon the generality which characterizes the current conception. The idea that a function can be based upon the more primitive notion of set had its inception in Dirichlet's work.

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[1] Ibid., p. 1.

[2] Ibid.

> Riemann attempted to find necessary and sufficient conditions for representation of a function by its Fourier series. Although he failed in this task (the problem is yet unsolved), the byproducts of his investigations had profound effects on the development of analysis. These researches were the first to recognize different types of infinite point sets, a property developed into a unified theory by Georg Cantor some years later. In fact Cantor's interest in set theory stemmed from his researches on trignonometric series.[1]

Chapter IV is an excellent treatment of the history of the arithmetization of analysis. Starting with Bolzano's proof of his form of the fundamental theory of algebra, Manheim then discusses Bolzano's continuous but not-differentiable function, Riemann's continuous but not-differentiable function, Weierstrass's "pathological" function, also continuous everywhere but nowhere differentiable, and ends with the arithmetical construction of the real numbers. He deals with all the major mathematicians and important papers regarding three topics in a clear and thorough manner. Indeed, as I wrote my historical sketch I often referred to and compared my work with Manheim's work in those areas. For Manheim all the unusual functions and all the difficulties of obtaining a true arithmetical construction on the reals revealed the tension resulting from the mathematicians' intuitive or pictorial sense of their mathematics and their formal mathematical results. Not only was naive intuition not necessarily a valid source of ideas,

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[1]Ibid., p. 64.

occasionally it actually misled mathematicians.

> The disquieting thesis that pictorial representation did not have a greater generality than analytic construction was neither rapidly nor happily accepted when presented by Fourier. The suggestion that the inclusion actually went the other way so disturbed the equilibrium of the structure of mathematics (and mathematicians) that more than four decades elapsed from discovery (Bolzano, circa 1830) to print (du Bois-Reymond's exposition of Weierstrass' function, 1874). . . . This was not coincidental. It was noted in the summary to Chapter I that sensory contact provided a sense of comfort. In particular it is the conviction, conscious or not, that the world that is contacted through the senses is totally consistent (while there is no such conviction about the conceptual world) that causes the reluctance to abandon sensory truth-referents. . . . Overcoming the reliance on sensory perception was a necessary step in the evolution to topology.[1]

But Manheim becomes a bit carried away in his view of the importance of the abstract non-sensory character of mathematics.

> . . . Indeed, arithmetization [of analysis] demonstrated the thesis that mathematics has an existence independent of the perceptual world and that, insofar as mathematicians, qua mathematics, is concerned, its intersection with reality is totally irrelevant. This concept, a major characteristic of modern mathematics, was indispensable to the development of the notion of a topological space.[2]

Manheim commits an error similar to Bourbaki. From the modern viewpoint, topology (and mathematics, for that matter) is viewed as a subject independent of the real world. Yet, it is not clear that such a view was a requirement for the development, the emergence, of point set topology.

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[1] Ibid., p. 94.

[2] Ibid.

Indeed, we may be able to argue the opposite--because topology did emerge when it did, it created the modern viewpoint of mathematics. I believe that topology, as a subject matter, and the abstractness and independence of mathematics, as a viewpoint, were evolving together, each assisting the development of the other.

The last two historical chapters of his monograph are entitled "The Development of Point Set Topology" and "The Emergence of Point Set Topology." By "development" he means those mathematical preconditions prior to the full "emergence" of point set topology. In particular, Manheim discusses in the development chapter the importance of uniform convergence to Fourier series leading to Georg Cantor's work on trigonometric series which, in turn, lead to his work on point sets. And it is the Cantorian theory of point sets that set the stage for the "emergence" of point set topology.

Manheim opens his last chapter with the following characterization of point set topology.

> The principle domain of point set topology is the axiomatic study of nearness; it is essentially a study of geometry in the small. Considerations of uniformity, compactness, and connectedness are basic. Since well-behaved spaces are Euclidean in the small, a major impact of point set topology has been on the pathological spaces, spaces which are locally non-Euclidean.[1]

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[1]Ibid., p. 111.

This introduction is curious because most of his last chapter deals with the conceptual setting of the late nineteenth century conducive to the emergence of point set topology. He really devotes little of the chapter to the mathematics aiding the emergence. That is to say, Manheim discusses Riemann's Habilitationschrift, Klein's Erlanger Program, some of the early extensions of set theory, and Frechet's and Riesz's contributions, but all in the context of the new conceptual setting of abstractness and generalization. He really does not discuss "the axiomatic study of nearness . . . geometry in the small." In other words, Manheim's last historical chapter, while very important, is somewhat confused as to a distinction between the directly responsible mathematical contributions to the emergence of point set topology, and the indirect mathematical conceptual contributions setting the stage, establishing the mathematical milieu, for the emergence of point set topology.

> Frechet's study of abstract spaces brought into existence very general geometries, geometries which did not necessarily conform to Klein's method of classification. A space, in this new view, was a set of objects called points and a set of relations among these points; geometry became simply the theory of such a space. Frigyes Riesz and Hermann Weyl both sought to overcome the limitations inherent in Frechet's dependence upon the limit concept.
>
> The studies of Frechet, Riesz, and Weyl all advanced the subject to abstract spaces. It was, however, Hausdorff who saw how to give these spaces both the generality and precision necessary to establish point set topology as a separate discipline.

> This achievement was possible because Hausdorff united a recognition of the utility of choosing the neighborhood concept as fundamental with a set of axioms that was both general enough to handle abstract spaces and sufficiently restrictive to serve as a basis for development of a fruitful set of theorems.[1]

Excellent summary of the contributions of Frechet, Riesz, Weyl, and Hausdorff.

Manheim ends his monograph with the concluding chapter, "From Newton to Hausdorff." In this chapter Manheim briefly summarizes the historical antecedents of point set topology, a "summary of investigations important to the development of point set topology" as he terms it. He believes that the roots of point set topology can be found in the work of the eighteenth century mathematicians on the calculus, "The works of the early analysts are viewed as the point of departure for systematizing problems which led to the development of point set topology."[2] From "early analysts," Manheim's summary chapter, and indeed the whole monograph, essentially traces the history of analysis relevant to point set topology in the eighteenth and nineteenth centuries. As I mentioned above, occasionally Manheim comments on the mathematical milieu, such as

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[1]Ibid., p. 132.

[2]Ibid., p. 133.

> The movement towards abstraction and generalization in mathematics is a measure of progress towards the notion of a topological space. In this respect a first debt is owed to Euler and other formalists, men who sought to work beyond the confines of geometry.[1]

Or,

> The [arithmetization] programs which began as an attempt to rescue analysis from the onslaught of pathological functions led to a new discipline, point set topology, a subject ideally suited to the study of such functions.[2]

Or,

> Considerable attention has been given to the role of geometry in analysis with respect to functional representation. The changing notion of function indicated the need to arithmetize analysis while the latter suggested the utility of the axiomatic method in analytic developments. Topology, which has emerged as the result of these movements, generalized the notion of a space. Axiomatization finally ended the conflict between geometry and analysis by obscuring the boundaries between the two disciplines. One of the fundamental measures of progress in mathematics is unification. Point set topology represents a major achievement in this direction.[3]

From the point of view of "mathematical milieu" we see that Manheim views the emergence of point set topology as representing a "movement towards abstraction and generalization," as part of "programs . . . to rescue analysis," or as a "major achievement" in the "progress" towards "unification." For Manheim, unification, or the

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[1] Ibid., p. 133.

[2] Ibid., p. 137.

[3] Ibid., p. 143.

> . . . drive toward unity in mathematics suggests that, if all of mathematics is ever structured in a coherent manner, the pictorial representation of the structure may be that of an inverted pyramid. The development will begin with an intuitive notion, such as that of set, together with a few axioms. . . . Historically, however, mathematics does not proceed in this fashion. . . . the historical development of mathematics appears to be characterized by a reaching out towards both more complex and more elemental notions from starting points that are determined by man's contacts with the physical world.[1]

Manheim is thus conscious of the difference between history and rational reconstruction. Yet, he does not clearly separate the changing mathematical milieu from the actual mathematical developments. "Generalization," "abstraction," "obscuring the boundaries between the two disciplines of geometry and analysis," and "the drive toward unity," are important features of the mathematical milieu which fostered the growth of the specific, mathematical results necessary for the emergence of point set topology. In turn, though, those specific results, whether it be the contributions to set theory by Cantor, the contributions to abstract spaces by Frechet, or foundational considerations by Hilbert all contributed to the new, emerging mathematical milieu of the twentieth century. The combined effect of the specific mathematical results and the mathematical milieu allowed for the emergence of point set topology.

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[1]Ibid.

His very last comment is very interesting and provocative:

> the historical development of mathematics appears to be characterized by a reaching out towards both more complex and more elemental notions from starting points that are determined by man's contacts with the physical world.

It is a pity that he did not pursue the implied ideas further. Thinking of historical developments as both progress into more complex or more advanced concepts, and, at the same time, progress into the foundations or basic concepts could be a very useful tool in historical research.

What the Histories Reveal About Themselves

Reviewing the various histories of point set topology we see that they reveal aspects of each historian's implicit view of the methods of writing mathematics history. Florian Cajori tends to write history from the perspective of "famous mathematicians"; i.e., the history of mathematics is the history of what the particular famous mathematicians contributed to mathematics. Carl Boyer, on the other hand, views mathematicians immersed in both a conceptual and mathematical milieu, and that the history of mathematics is the history of mathematicians resolving differences and problems regarding their use and perception of mathematics. An extreme version of this view

is found with Morris Klein who appears to interpret modern developments primarily from roots in the nineteenth century. E. T. Bell and Bourbaki take the most modern viewpoints; Bourbaki tends to interpret history of mathematics as the history of mathematical developments leading to the modern, "correct" viewpoint. E. T. Bell considers topology as the epitome of modern mathematics, and thus its history is the history and characterization of modern mathematics. Bourbaki and Cajori tended to focus on famous mathematicians; Boyer and Bell tended to focus on important themes. Finally, Manheim probably reaches the closest balance between thematic influences like abstraction, generalization, etc., and the specific accomplishments of the mathematicians involved.

None of the historians examined possible influences of the universities, the training and professional life of the mathematicians, the existing and emerging mathematical societies and journals, etc., on the development of point set topology.

While the above historians may differ on their points of view there are common characteristics among them as historians. The most common characteristic found in all of these historians is the historical view that the history of mathematics is the history, or (mostly) chronological description, of mathematical results. That

is, the history of mathematics is not the history of mathematicians and their discoveries or inventions; rather the history of mathematics is the history of the important ideas in mathematics, and along with the ideas, the mathematicians associated with them. Another way of elucidating, through contrast, this "normal" mathematics history is to point out that the history of mathematics is almost never written as a part of the growth of science, Western civilization, etc. Mathematics history stands alone. Historians of mathematics are implying that the growth of mathematics is independent of the men, the institutions, and the culture in which mathematics progress is described.

Indeed, if there is ever any discussion of mathematics and the culture in which it is found, the discussion is usually from the point of view of how mathematical developments influenced the course of history, influenced the scientific culture. Notable examples of mathematics influencing culture and history are Salomon Bochner's The Role of Mathematics in the Rise of Science,[1] Alfred North Whitehead's Science and the Modern World,[2] and Morris Kline's Mathematics in Western Culture.[3]

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[1]Salomon Bocher, The Role of Mathematics in the Rise of Science (Princeton: Princeton University Press), 1966.

[2]Alfred North Whitehead, Science and the Modern World (New York: The Free Press, 1969).

[3]Morris Kline, Mathematics in Western Culture (New

Bocher describes his book thusly.

> This is, throughout, a book about mathematics, even if in various contexts it is seemingly concerned with other matters. It is a book about the uniqueness of mathematics as a force of our intellectuality, and about the mystique of its creativity, about the growing efficacy of mathematics, its widening importance, and its continuing spread.[1]
>
> The essays of this collection are all concerned with the role of mathematics in the rise and unfolding of Western intellectuality, with the sources and manifestations of the clarity and the mystique of mathematics, and with its ubiquity, universality, and indispensability.[2]

And hence the bulk of his book is a defense of the thesis that mathematics has played a crucial role in history, especially in the history of science. My purpose in citing Bochner is not to present his case and then judge it; rather, my point is that Bochner mostly describes the influence that mathematics had on the culture surrounding it, and less so describes how the culture may have influenced the development of mathematics. In the case of Greek mathematics he shows more balance between the culture influencing mathematics, and vice versa. However, for the most part, it is a description of the influence of mathematics on Western scientific culture.

York: Oxford University Press, 1953).

[1] Bochner, p. v.

[2] Ibid., p. 5.

In his Science and the Modern World, Whitehead includes a chapter entitled "Mathematics as an Element in the History of Thought." For Whitehead, the "science of Pure Mathematics, in its modern developments, may claim to be the most original creation of the human spirit." Indeed,

> The originality of mathematics consists in the fact that in mathematical science connections between things are exhibited which, apart from the agency of human reason, are extremely unobvious. Thus the ideas, now in the minds of contemporary mathematicians, lie very remote from any notions which can be immediately derived by perception through the senses; unless indeed it be perception stimulated and guided by antecedent mathematical knowledge.[1]

Thus this chapter is an explanation of how and why mathematics is so "original" and "remote from any . . . senses." As part of his explanation Whitehead wishes to place "mathematics as an element in the history of thought."

> . . . Even now there is a wavering grasp of the true position of mathematics as an element on the history of thought. I will not go so far as to say that to construct a history of thought without profound study of the mathematical ideas of successive epochs is like omitting Hamlet from the play which is named for him. That would be claiming too much. But it is certainly analogous to cutting out the part of Ophelia. This simile is singularly exact. For Ophelia is quite essential to the play, she is very charming--and a little mad. Let us grant that the pursuit of mathematics is a divine madness of the human spirit, a refuge from the goading urgency of contingent happenings.[2]

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[1] Whitehead, p. 19.

[2] Ibid., p. 20.

Whitehead's imagery is intriguing. Yet he is ultimately disappointing since in his discussion of the nature of mathematics he seems to not get much further than imagery. My immediate point with Whitehead is another example of one who sees the unique and important position of mathematics in the history of science or Western culture. A third example is Morris Kline's Mathematics in Western Culture which, again, is an historical work which takes the perspective of mathematics' contribution and influence in Western culture.

Thus we see that if there are historical considerations regarding the relations between mathematics and science or culture, they tend to be considerations of mathematics historical infleunces on science or culture, and not the reverse.

The second major characteristic found in the historical writings about point set topology, and, for that matter, in most mathematics history is the sense that the history of mathematics, like the important ideas of mathematics, is fixed, timeless. There is virtually no consideration of changing history, of differing interpretations of mathematics history, or of re-interpretations of mathematics history. It is the singular absence of explicit, stated, historical methods or viewpoints in mathematics history that, in my opinion, truly distinguishes it from other historical writings.

Aspects of the Historiography of Mathematics

The methods or theories of writing history--historiography--are critical issues debated in nearly all historical disciplines. I shall restrict myself to the history of science, but one can easily see from the writings of R. G. Collingwood, The Idea of History, E. H. Carr, What is History?, Bendedetto Croce, Theory and History of Historiography, and Patrick Gardiner (ed.) Theories of History that historiography is not a simple, clear, and agreed-upon subject. Within the history of science, we find, among others, the rather famous Thomas Kuhn's The Structure of Scientific Revolutions, and his (I believe) more valuable, The Essential Tension, Joseph Agassi's Towards an Historiography of Science, Karl Popper's The Poverty of Historicism, Maurice Finocchiaro, History of Science as Explanation, and most recently Larry Laudan's Progress and Its Problems. These are only a few of the many titles discussing history of science, historiography of science, and related philosophical issues. At the risk of belaboring a point, there is virtually no discussion in mathematics history regarding the historiography of mathematicics.

I have found a few isolated cases of direct discussion on the matter of historiography of mathematics, but mostly we must infer from mathematics historicans'

writings what they believe to be the "proper" history of mathematics, or we can try, with much care, to extend the theories regarding historiography of science to mathematics history.

Let us consider the few mathematics historiographical discussions I have located. One of the oldest, and most curious, pieces is a long-out-of-print monograph by George Sarton, The Study of the History of Mathematics.[1] First published in 1936 it was reissued in 1957 by Dover Press. The book is difficult to find, and since it is entirely about history of mathematics, I shall quote from it at length. His monograph contains three sections, "The Study of the History of Mathematics," "Note on the Study of the History of Modern Mathematics," and a fairly extensive Bibliography, including a good list of biographies of "modern" (ca. 1800-1935) mathematicians.

In his opening comments he discusses the reasons for separate courses in the history of science and the history of mathematics, although he is not happy with the separation.

> It is a pity that this [separation] should be so, for the history of mathematics should really be the history of culture. Take the mathematical developments out of the history of science, and you suppress the skeleton which supported and kept together all the rest. Mathematics gives to science its innermost unity and cohesion, which can never be entirely

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[1]George Sarton, The Study of the History of Mathematics (Cambridge: Harvard University Press, 1936).

> replaced with props and buttresses or with roundabout connections, no matter how many of these may be introduced.[1]

Like the others I have mentioned he tends to see mostly the influence of mathematics on science, although he tempers that somewhat.

> . . . [the historian of mathematics] should seize every occasion to indicate the relationships between mathematics and other sciences, and to insist that these relationships have always been reciprocal: mathematical problems being often the result of physical needs, while mathematical elaboration gave physics, and, gradually, other sciences, not only means of discovery of almost miraculous potency, but also perfect models of analysis and synthesis.[2]

or as he remarks later,

> There is no doubt that mathematical discoveries are conditioned by outside events of every kind, political, economic, scientific, military, and even by the incessant demands of the arts of peace and war. Mathematics never did develop in a political or economic vacuum. However, we think that those events were only some of the factors among others, factors the power of which might vary from time to time. It might be almost decisive in one case, and ineffectual in another.[3]

He soon comments about the complicated interconnectedness of most mathematical ideas and their history as a partial explanation of the challenge of writing mathematics history.

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[1] Ibid., p. 4.

[2] Ibid., p. 5.

[3] Ibid., pp. 15-16.

> . . . Whenever the historian tries to relate the history of a single group of ideas he is obliged to abstract one pattern from a network of endless complexity, and such an abstraction, however interesting it may be, is always arbitrary to a degree.
>
> The study of special branches of mathematics or of special mathematical ideas is very useful, for it helps one to understand those particular ideas more deeply, but it should not be allowed to confuse our historical perspective. The historian must try to keep in mind the chronological succession not of this or that idea, abstracted from the rest, but of the main ideas, all of them in their mutual relationships and in their diverse connections with the rest of life.[1]

Sarton has often described the history of science as a "secret history." It is a secret history in that few people know (or presumably, care) that the history of science is an ongoing discipline, and that results in the study of the history of science have negligible effect on society. Wherein Sarton waxes rather eloquent as to the importance of this "secret history."

> . . . For societies, even as for individuals, one must make a sharp distinction between the things which are the most urgent and those which are most important. These things are not by any means the same. The most urgent necessity is to live, to remain alive, that is, to eat, sleep, to be happy, to procreate children, and obtain security for one's family. That means physiology, business, and sport, and often enough war. However, the most important things are not to satisfy one's physiological needs, but to increase the cultural heritage which has been bequeathed to us. The urgent things are obvious enough, and men's efforts to obtain them fill the whole historical picture; one can hardly see anything else. Yet all the time some men pursue in the darkness, secretly, the fulfillment of their intellectual desires and of humanity's highest purpose.[2]

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[1] Ibid., p. 7 [2] Ibid.

Inspite of its elitist tinge, the comment is very moving.

Now if the history of science is a secret history, then, as Sarton says, "the history of mathematics is doubly secret, a secret within a secret," (I suppose that makes the historiography of mathematics "trebly" secret), "for the growth of mathematics is unknown not only to the general public, but even to scientific workers."[1]

Yet, for Sarton, once one knows the history of mathematics one is witness to some of man's greatest accomplishments.

> The history of mathematics is exhilarating, because it unfolds before us the vision of an endless series of victories of the human mind, victories without counterbalancing failures, that is, without dishonorable and humiliating ones, and without atrocities. At the same time it helps to dispel pessimism. However great the victories may be, the seasoned historian expects still more and greater ones. Has it not always been so?[2]

Well, quite frankly, no. It has not always been so. Sarton is writing in 1935, and in other passages it is clear he is aware of Russell, Whitehead, and Frege, among others. The paradoxes of set theory, and maybe even Godel's proof, were results probably known to Sarton. His exuberance about the "endless victories . . . victories without counterbalancing failures" is somewhat unfounded by 1935. Later he provides some balance, but not much.

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[1]Ibid., pp. 8-9.

[2]Ibid., p. 10.

> As to the pure mathematician, even he should not be too easily satisfied with the latest synthesis. To begin with, that synthesis may be incomplete. Some elements which were not deemed essential for it be have other values, they may prove to be essential for other structures, or the one from which they were eliminated may not be as final as it seems. Indeed, no theory is ever final. A new discovery, a new point of view may cause its abandonment and its supercedure by another, and the facts neglected in one shuffling may be considered invaluable in another. Every synthesis implies sacrifices; it is not merely a simplification but also and unavoidably a betrayal of reality, a distortion of the truth, and the mathematician who takes the trouble of considering the origin and evolution of ideas, as well as their final shaped, will improve his understanding of them and enrich his mind.[1]

Ultimately for Sarton the history of mathematics is the history of unpredictable results. We shall never really be able to thoroughly account for the developments in mathematics; and furthermore, we can never predict the future of mathematics. Mathematics, and its history, is too "capricious."

> . . . There are many facts which one cannot account for in a general way, and this applies to mathematical inventions as well as to any other details of human behavior. Many mathematical developments are capricious in the extreme . . . [Galois] called our attention to the great irregularity and disorder of our mathematical knowledge. An orderly development would only be possible for a godlike mathematician knowing in advance all the possible mathematics.
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> Here is really the crux of the matter. Mathematicians and other scientists, however great they may be, do not know the future. Their genius may enable them to project their purpose ahead of them; it is as if they had a special lamp, unavailable to lesser men, illuminating their path; but even in the most

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[1] Ibid., p. 21.

> favorable cases the lamp sends only a very small cone of light into the infinite darkness . . .
> The capriciousness of mathematical development cannot be emphasized too much.[1]

The second major section of Sarton's book deals with the study of the history of modern mathematics. In a rather circuitous manner, Sarton says that writing the history of modern is technical and difficult. He actually cautions against premature historical writing; that the passage of time provides a more rational perspective on mathematical developments.

> . . . the professional historian of mathematics is generally unable to see the details of modern mathematics, and especially of the twentiety-century mathematics, in their proper perspective. He is always in danger of not seeing the wood for the trees, and of appreciating incorrectly the relative weight of different facts and theories.[2]

He then offers a few suggestions regarding research in the history of mathematics; suggestions such as the historian should be especially careful when dating mathematical discoveries (comparing first <u>enunciation</u> with first <u>publication</u> of some discovery, and skeptically considering all publication dates on journals and papers as evidence of priority of discovery); such as the historian should always consider work incomplete (new revelations and information can always appear); and in general, no less care in historical research is required

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[1]Ibid., pp. 17-18. [2]Ibid., p. 31.

as in pure mathematical research (double standards are not permissible).

These are good, general, common sense suggestions that any historian (or researcher) would follow when investigating mathematics or its history. In other words, Sarton offers no specific method or approach to researching and writing mathematics history. The title of the monograph, The Study of the History of Mathematics, is sufficiently ambiguous, implying either introductory remarks to students about to study the history of mathematics, or comments to mathematicians or historians about to undertake mathematics history research.

Thus for either group Sarton provides intellectual exhortation and encouragement before embarking on the study of the history of mathematics.

> The main reason for studying the history of mathematics, or the history of any science, is purely humanistic. Being men, we are interested in other men, and especially in such men as have helped fulfill our highest destiny.[1]

Or,

> The main duty of the historian of mathematicians, as well as his fondest privilege, is to explain the humanity of mathematics, to illustrate its greatness, beauty, and dignity, and to describe how the incessant efforts and the accumulated genius of many generations have built up that magnificant moment, the object of our most legitimate pride as men, and of our wonder, humility, and thankfulness as individuals. The study of the history of mathematics

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[1]Ibid., p. 22.

> will not make better mathematicians but gentler ones, it will enrich their minds, mellow their hearts, and bring out their finer qualities.[1]

Despite his professed admiration for the study of the history of mathematics Sarton offers few instructions as to how to go about the study of the history of mathematics. At the beginning of this monograph Sarton refers the reader to his Study of the History of Science, with an Introductory Bibliography where the reader will find remarks there which "apply equally well to the history of mathematics." The Study of the History of Science was published a year earlier (1935), written in the same general, narrative fashion. The Study of the History of Mathematics' predecessor is also quite limited in real, tangible instructions regarding the actually researching and writing of history. Yet, at the end of the Study of the History of Science is a bibliography containing names of books dealing with "historical methods." In The Study of the History of Mathematics no such books are listed, reinforcing my opinion that Sarton and most others rarely considered the historiography of mathematics. This lack of consideration of the historiography of mathematics, I suspect, is a result of historians implicit belief that the evidence and interpretation of mathematical results is a clear, simple, and straightforward matter. History

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[1]Ibid., p. 28.

of mathematics is the history of mathematical discoveries and inventions (the words tend to be used interchangeably and thus conflated). These discoveries or inventions are accumulated eternal truths; the latest truth neatly falling upon the foundations of previous truths. And these mathematical truths have an existence independent of the mathematicians involved. It is as though the mathematicians act as a kind of midwife or catalyst in the process of the development of mathematics; if mathematician A had not discovered Theorem X, then eventually mathematician B would have discovered Theorem X (or a closely related theorem), because Theorem X is crucial to the field in which it is found. Consequently famous mathematicians are famous for discovering (or inventing) the "important" or "far reaching" mathematical truths. Famous mathematicians are famous for the depth and breadth of their important mathematical results, and thus biographies of famous mathematicians are valuable for the history of mathematics.[1]

Since the history of mathematics is so "obvious," it makes sense that there has been so little consideration

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[1]Related to the interest in biographies of famous mathematicians is an interest in the manner or means in which these individuals made such discoveries. Sometimes referred to as the "psychology of invention in the mathematical field," there have been studies of the methods of invention or discovery to possibly determine and generalize mathematical creativity and insight. See Hadamard in the bibliography, for example.

of the historiography of mathematics. Since historiography of mathematics is not a fashionable academic field, what does exist regarding historiography is scallered about in the literature, some of it in obscure or unexpected journals.

For example, we shall consider some of the comments of Kenneth O. May. Trained as a mathematician in the 1940's, but always interested in the history of mathematics, he became a professional historian of mathematics and contributed much, especially organizationally, to the field. He was one of the founders, and first editor, of Historia Mathematica, the only American journal devoted exclusively to mathematics history. He also established a center for history of mathematics at the University of Toronto and in 1972 published Bibliography and Research Manual of the History of Mathematics.[1] The manual is a large (over 800 pages) bibliography of books, articles, monographs, and histories classified by biography, mathematical topics, "epimathematical" topics, historical periods, and information retrieval. The last classification deals with special libraries, collections, information systems, etc.

Within the last classification is the subheading, "Historiography." Thomas Kuhn's Structure of Scientific

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[1]Kenneth May, Bibliography and Research Manual of

Revolutions is found there along with Sarton's books which I have discussed among others. Few of the citations are modern historiographical studies, most were written in the early part of this century, and most are very general discussions, e.g., mistakes committed in historical writings, documenting scientific literature. Indeed, one listed reference May recommends is Jasque Barzun's The Modern Researcher, which is very good for researching and writing history in general, but has few specifics for mathematics.

Consequently, May's Research Manual is not a great bibliographic source of information about the historiography of mathematics. May does, though, include a short chapter containing comments of an historiographical nature, although he does not term them as such. Under Part I of the book are three chapters; "Information Retrieval," "Personal Information Storage," and "Historical Analysis and Writing." Despite the computer-oriented language, the chapters are not about computer information storage and recall, but about libraries, catalogues, primary and secondary sources, and a system of note cards. In essence, the first two chapters are about the library/legwork of research. Chapter 3, "Historical Analysis and Writing," is a short

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the History of Mathematics (Toronto: University of Toronto Press, 1972).

chapter of do's and dont's when writing the history of mathematics.

As regards "Approaches to History" he says,

> History has far more facets than can be described here. One can focus on the development of ideas, biography, periods, civilizations, countries, cities, institutions, terms, symbols, topical fields, education, communication, bibliography, sociology of mathematics, and so on. No one approach can give a complete picture. Each one and every combination has value. But a choice will help to guide information collection as well as analysis and writing.[1]

Unfortunately the above statement is so general and obvious that it lacks any usefulness. Later he comments that

> To give rules for the production of history is as difficult as to tell someone how to discover a theorem or construct a new proof. It is easier to suggest some things to avoid.[2]

So, among the "things to avoid" are:

"Credulity." Do not uncritically accept statements about the history of mathematics.

> The historical and mathematical literature contains many errors. Some result from repeating baseless traditions. Some are merely careless. Others arise from ignorance, misunderstandings, prejudice, or chauvinism. They appear in all sources, even the most respectable and seemingly primary . . .
>
> It is therefore incumbent on the historian to examine everything critically. He should not believe anything automatically, but he should record what he finds and record also the source of information.[3]

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[1] Ibid., p. 28.

[2] Ibid., p. 29.

[3] Ibid.

Good point, yet often difficult to do. When writing my brief historical sketch of the developments in mathematics leading to the emergence of point set topology, I found such an instance for erroneous "fact." Many historians have commented that Heine, in his 1872 paper, "Die Elemente der Functionlehre," proves the Heine-Borel Theorem. When we examine the paper the theorem is not found. Heine proved an equivalent theorem, but the "Heine-Borel process" of choosing a finite subcollection of covering intervals from an infinite collection is nowhere to be found in Heine's paper.

Next, May warns of "Prioritits." By this he means,

> . . . Questions of priority [of discovery] are worthy of historical consideration only when they involve historical issues, not merely efforts to dispense justice. Moreover, the question of who did something "first" usually turns out to be meaningless because of unremovable ambiguities in the definition of "something" and what is meant by "doing" it . . . Priority chasing for its own sake should be avoided.[1]

Historians of mathematics also confuse priority in the senses that a mathematician may have been the first to prove or know something (e.g., Bolzano and his nowhere differential continuous curve) whereas another may have known that same idea or proof later (e.g., Weierstrass) and was responsible for the mathematical public knowing it. An excellent example of this interest in priority related

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[1]Ibid.

to point set topology is the paper by H. Guggenheimer, "The Jordan Curve Theorem and an Unpublished Manuscript by Max Dehn."[1] Guggenheimer provides an excellent summary of the history of the major proofs of the Jordan Curve Theorem, the eventual purpose of which was to show that Max Dehn had the earliest, modern-style proof, yet it was unpublished.

As Guggenheimer remarks

> . . . It is a mystery why Dehn never published the paper, which was ahead of its time. In addition to a proof of the Jordan Curve Theorem, the manuscript contains a proof of the Shoenflies Theorem (7 years before Shoenflies' discovery) which is elementary and very simple; the idea of the proof is very close to one given by R. H. Bing in 1960.[2]

Apparently, for Guggenheimer, if a tree falls in the mathematics history forest with no one to hear the sound, then it still made a sound, and thus should be noted.

"The Solomon Syndrome."

> Closely related to the illusion that historians are dispensers of priority in the idea that they should rank mathematics and judge who was "right" and "wrong" by their own standards. This may be good fun, but it is not very illuminating. It tells little about what happened and why. The historian's tasks are description and explanation, not judging and moralizing.[3]

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[1] See above.

[2] Guggenheimer, p. 198.

[3] May, p. 29.

While I certainly agree that we cannot judge historical figures by contemporary standards, I cannot agree that the historian's tasks are simply "description and explanation." That is, to decide what, how, and why something should be "described and explained" implies viewpoints and choices upon the part of the historian of mathematics. To include, to leave out, to explain at length, to explain briefly, etc., are decisions the historian must make when writing history. How these decisions are made is part of the study of the historiography of mathematics.

"Unhistorical analysis."

> One of the weaknesses of scientists who take up history is the tendency to judge historical events timelessly or, more often, as though they were occurring today. . . .
>
> Of course, it is important to see how modern ideas originated and developed, but a full understanding of this development requires considering things as they were at the time and in terms of conditions of that time. In particular it must be remembered that words mean different things at different times. Failure to take this into account can lead to ludicrous errors.[1]

A very good point, but an extremely difficult rule to follow. Ideally an historian should transform himself back to the mathemetical and intellectual situation he is describing. Ideally, the historian's description should be from the point of view, and the point of ignorance, of the mathematicians at the time. That is, knowing what

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[1]Ibid., p. 30.

we know now about mathematics, it is very difficult to put ourselves in the state of ignorance, the not knowing of certain results and implications, of the mathematicians and their work we are investigating.

Joseph Agassi, in his Towards an Historiography of Science,[1] discusses this difficulty in the sections entitled, "The Advantage of Avoiding Being Wise after the Event," and "The Difficulty of Avoiding being Wise after the Event." This is a number from the journal, History and Theory.[2] In summary,

> The Advantage of Avoiding of Being Wise After the Event is that this allows us to see the world with the eyes of those who participated in the event, and thus to explain it.
>
> The Difficulty of Avoiding Being Wise After the Event arises from having suppressed the reasonable errors which the event has corrected.[3]

In other words, "Being Wise after the Event" means that we must pick and choose theorems and mathematicians we consider important in our historical presentation. As Agassi puts it

> There is much to be said for trimming the edges of the history of science. We may appreciate false starts but find it unimportant to dwell on them at

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[1]Joseph Agassi, Towards an Historiography of Science (Middletown: Wesleyan University Press, 1963).

[2]Studies in the Philosophy of History, Beiheft 2.

[3]Agassi, p. viii.

> length because, important as they may have been, they led nowhere.[1]

On the other hand, "Being Wise after the Event" tends to cause historians to exclude errors, mistakes, or insignificant theorems or mathematicians from their writings.

> In most cases it is not only easy to be wise after the event; we can hardly avoid being so. Undoubtedly, the oversight of certain facts and ideas was not due to technical difficulties alone [during some historical period] but to others of a different character. And if we wish to avoid being wise after the event we must find out what these were. The difficulties, I suggest, are usually the scientific theories accepted at the time. We tend to be wise after the event because we tend to forget how the world looked before the event took place, because we tend to forget the scientific errors which the event corrected.[1]

The worst difficulty with being wise after the event is not appreciating the errors in mathematics history. Most importantly, there are distinctions among errors.

> Modern European languages as yet possess no verbal distinction between a valueless or a silly or a negligent or an irresponsible error, and one committed responsibily and judiciously. Since, according to the misleading accepted usage of these words, all errors are faults and all who err are wrong it is time that phrases like "valuable error," "stimulating error," "intelligent error," "clever mistake," be used and single words be coined to convey shades of meaning. In law courts the difference between responsible errors and errors due to negligence is well known since the law often treats them differently. But ordinary language is so powerful . . . that it reinforces the confusion between different kinds of errors. By using "he erred" and "he was wrong" as synonyms, we often condemn people and imply that one ought to avoid errors successfully. The phrase "he had no idea

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[1]Ibid., p. 53.

> that" ambiguously covers both the case of a person who asked the right question but (understandably) gave it the wrong answer, and the case of a person who avoided the same error by (foolishly) failing to ask the right question.[1]

I would contend that most of Shoenflies' work in the history of point set topology is not discussed in standard histories because he erred, and because historians view the errors as foolish errors. From the point of view of Schoenflies, trying to extend and generalize the concepts involved with the Jordan Curve Theorem, his error is symptomatic of both the difficulty and subtly of correctly generalizing and abstracting results in mathematics (clearly, an important motivation among mathematicians during the nineteenth century) and the potentially misleading results of naive intuition in the realm of set theoretic results. Schoenflies' error was both understandable, given the context of the period, and stimulating, in that L. E. J. Brouwer was moved to correct the errors, and, as a result, fashioned new ideas in topology.

We have passed somewhat from Kenneth May's prohibition when writing mathematics, but May's last few comments are closely related to the above discussion.

May also warns the historian against "Speculative History."

> It is often necessary, and quite legitimate, to supply missing information by deduction from

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[1] Ibid., p. 54.

> established facts, or even by plausible inference, if it is labelled as such. But there is a style of "history" that consists in simply fabricating an account of how something might have happened (for example, how counting might have first been invented) and then coming to regard this as fact.[1]

When writing history there is a great desire to provide a "seamless web" of description, explanation, and narrative. Missing information or gaps of knowledge can ruin the idealized historical account so that the temptation to unnecessarily or extravagantly speculate is strong.

"The Matthew Syndrome."

The problem of disproportionate amounts of writing given over to famous mathematicians and their work.

> The importance, ability, and achievements of well-known figures are exaggerated due to the fact that their papers and publications are more likely to be preserved, they are more often cited, and interest in them results in publications that created an audience and triggers further work. The result is a distorted picture of the history of mathematics as the work of a few "stars." The historian should be on guard against this "halo effect" and strive to give a balanced picture that includes "minor" as well as "major" personalities.[2]

Lastly, and certainly not the least prohibition that May warns is "Plagiarism." He lists several variations on the theme of theft and fraud in historical research and writing. In the end his point is simply that one must be honest, forthright and give proper credit regarding

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[1] May, p. 30.

[2] Ibid.

sources of all information and ideas when writing history. Indeed, one of the marks of real scholarship is the acknowledgement of the help of others. However, a difficult problem which May does not touch on is how to distinguish when information for an idea came from a direct, identifiable source, and when it is common knowledge. For example, does it require elaborate citations to say that Cauchy formulated the calculus in modern terms, or is it now considered common knowledge? Presumably, the answer to such a question would depend on the context of the discussion of Cauchy. The point is that May is certainly correct to remind historians to cite the sources of information, but excessive citations and references can pedantic and detract from narrative.

Another historian of mathematics, Ivor Grattan-Guinness, in his "Notes on some Methodological Problems in the History of Science," addresses the problem about the historiography of mathematics differently from May, and should be included in this discussion.

While he uses the phrase, "history of science," he is a professional historian of mathematics and his discussion is applicable to the history of mathematics.

> The history of science has increased significantly in quality as well as quantity in recent years. One feature, and perhaps a consequence, of this growth of interest has been a developing awareness among some historians of methodological and

> philosophical problems underlying their work. What are we doing in the history of science? Can we discuss historical method at all, and if so how? This is the kind of question that is asked. By contrast, however, other historians regard such discussions as fruitless; after the lengthy arguments have finished, we are still left with the old cliches of 'using one's judgement' and 'allowing for the special circumstances of the case.'[1]

Not wanting to be "left with the old cliches," Grattan-Guinness offers his article as a way of encouraging more discussion on the matter of the historiography of science.

> . . . The notes below certainly do not constitute a comprehensive philosophy; they are merely tentative suggestions concerning some methodological problems that seem to me to be significant for the historian of science.[2]

A major portion of the paper then discusses Karl Popper's theories about science and the philosophy of science. And from my reading and understanding of Popper I believe that Grattan-Guinness' interpretation to be quite accurate.

For Popper, our knowledge of science is characterized by its tentative and conjectural nature.[3] Scientists develop theories which are, at best, tentative and conjectural regarding the explanation and prediction about

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[1]Ivor Grattan-Guinness, "Notes on Some Methodological Problems in the History of Science," RETE 1 (1972): 1.

[2]Ibid.

[3]Ibid., p. 2.

observable events. The scientists test theories with an unending series of increasingly severe tests. The more severe the test the conjectured theory can pass, the more credible the theory through corroboration as a result of passing the test.

> The tests of our theories may be either logical or empirical. If prediction of a theory is not fulfilled, then the theory is refuted and its falsity has been demonstrated. However, if the prediction is successful, then the theory is corroborated. But the word, "corroboration," is not to be taken as synonymous with, "verification," for this latter word has a connotation of "making more certain," or "establishing securely." In Popper's view, the idea that a theory can be made more certain is mistaken, for any theory, however well previously corroborated, may fail its next test.[1]

Often described as a "fallibilist" philosophy of science, Popper emphasizes fallible aspect of scientific theories; they simply can be wrong. We can never know for certain, forever, that a scientific theory will always hold true. Grattan-Guinness believes Popper's views to be beneficial for the historian of science.

> . . . if one reads the great works of science armed with ideas about tentative knowledge, critical discussion, bold tests, imaginative theories, then one will soon receive a nasty shock. Yes, the theories are there, the problems are stated, after a fashion; but what are all those paragraphs allegedly defining exact conceptions, or indulging

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[1]Ibid., p. 3. A seldom discussed contagious aspect of Popper's writing is his excessive use of underscored (italicized) words, sentences, or whole paragraphs. Grattan-Guinness seems to have been infected.

> in investigations of apparently fruitless and unimportant features of a theory? The answer is simple but important: our predecessors were rarely fallibilists. ["And neither are we!" says Grattan-Guinness in a footnote.] In fact, the history of thought, scientific and otherwise, is saturated throughout with justificationist methodologies of all kinds.[1]

Grattan-Guinness' purpose in summarizing Popper's theory is to compare Popper's theory to what Grattan-Guinness calls, "justificationist" or "verificationist" methodologies in the history of science. A justificationist methodology views science and its history as a series of statements, each in turn, justifies the preceding statement. "Professor X did this because Professor Y did that because . . . " is the form of a justificationist history. Similarly a verificationist methodology treats history as an explanation, or verification, of what we know now.

He collects both methodologies under the rubric of "unveilism."

> Today, as we work on the history of science, science itself is at a certain state of development. Among its mass of knowledge we hold certain results as important, certain techniques as influential, and so on, and our task as historians is to show how this situation came about--to unveil it, emerging from the past. Thus we describe the past in terms of modern, or at least later ideas, because by this means we shall clarify the form of the ideas that were actually developed at that time.[2]

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[1] Ibid., p. 7.

[2] Ibid., p. 8.

He criticizes this approach for several reasons.

> It is clear that unveilism is a form of historical verificationism; for the aim is to show how the knowledge that we know emerged in the form in which we know it, thus verifying our own version of it. Unveilism is also connected with historical determinism, for, since our knowledge emerged for us in the way that we wanted it to, then it could not have emerged in any other way . . .
>
> As opposed to unveilism, I would suggest that the prime interpretative task of historians is not to resolve muddles by means of posterior knowledge, but to reconstruct them in terms of current ignorance.[1]

The theme of "reconstruction of state of ignorance" of the past is a popular theory in the historiography of science. Certainly not an easy or clear goal to attain, but a theme that does reoccur. As part of the theme of reconstruction, one must be aware of "changing priorities within knowledge." By this Grattan-Guinness means:

> Like the refutation of theories, the change of priorities is also a product of critical discussion, but it takes a rather different form. For it is not necessarily the case here that theories became modified through criticism or refutations, but that they are reinterpreted in the light of new knowledge and become assigned--in a basically intact form--to fresh positions in the general structure of knowledge, more, or less, important than before.[2]

In other words, what was important then, what was of high priority then must be given importance or high priority in our historical reconstruction.

> . . . what was then held as important may seem to us quaint or uninteresting. Yet we must examine

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[1]Ibid., p. 9. [2]Ibid., pp. 9-10.

> these old "blind alleys" carefully, however much we regard them as failures; if our predecessors found them significant, then we must also.[1]

Lastly, Grattan-Guinness reminds the reader to think of historical developments in science as a "double growth of our knowledge." New developments in science can contribute both to the extension of existing knowledge and to a deeper understanding of the foundations of that knowledge.

> All knowledge has foundations, which are themselves tentative. Thus we dig down to them through the mud of our knowledge, rather than lay them down in concrete and construct our knowledge from them. Therefore it follows that we have a double growth of knowledge--one growth that digs deeper into new (but tentative) foundations following the failure of their predecessors, and the other that pushes upwards into ever more detailed technicalities. Quantitatively speaking, most of our knowledge is of this latter type, but foundational questions always hold a prominent part in the development of the theories which they found. . . . foundational changes may lead to complicated reconstructions, both in the foundations and also in the technical knowledge "above;" even the knowledge which claims these new foundations will change, if several previously disparate theories, each with its own foundations, are unified under a new theory.[2]

Regarding the history of point set topology, his last comments above about "previous disparate theories, each with its own foundations" being given a unified base theory is very apt.

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[1] Ibid., p. 10.

[2] Ibid., p. 11.

Another historian of mathematics, Clifford Truesdell, has also written, albeit somewhat indirectly and in his own inimitable, cantankerous style, about the historiography of mathematics and science. Truesdell is the author of Rational Thermodynamics, Six Lectures on Modern Natural Philosophy and Essays on the History of Mechanics, among others. He has edited several volumes of the works of Leonhard Euler, and is a world authority on him. He is also the editor of The Archives for the History of the Exact Sciences, a source of excellent monographs on the history of mathematics, wherein Truesdell demands and maintains the highest standards of historical, scientific scholarship. Because of his important role in and contributions to the profession of the history of mathematics, I feel incumbent to include his comments.

Truesdell's article, "The Scholar: A Species Threatened by Professions," is the text of an address delivered at a banquet of the History of Science Society and the Society for the History of Technology in 1972.[1]

Like other historians I have mentioned Truesdell stresses the importance of the historian understanding the setting which he is investigating.

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[1]Clifford Truesdell, "The Scholar: A Species Threatened by Professions," Critical Inquiry 2 (Summer 1976): 631-648.

> The workshop of the scholar in the history of science is the periods in which his authors lived. He should know those periods' ways on life and belief and education, both the common and the eccentric: their political histories; their variety in aspects; their social and economic structures; their architectures, literatures, and arts. He should feel at home in houses of those times, sit easily in their chairs, both figurative and wooden, and discern what was then mostly admired or rejected in painting and sculpture and decoration. He should have read not only the books that carried the intellectual products of his period, but also those that were then the fare of young minds as they were taught, such books having been commonly of an earlier time. The student who does not command, as a minimum, the main episodes of Holy Scripture, classic mythology, and the corpus of golden Latin is glaucomatose in the modes of thought of Western men educated before 1900.[1]

Having this "background" knowledge the historian of science then needs the "tools of science:" "languages, logic, and the faculty of criticism."[2] Indeed, one aspect of the study of the history of science which is not emphasized enough is the need for the historian to know (several) foreign languages well. Mathematics being so international, contributions in the development of mathematics can come from many countries. It is also crucial to always consider the changing meanings of words--especially technical terms--being always careful to not inject the modern meanings of technical terms in historical words.

By logic Truesdell means:

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[1]Ibid., p. 631.

[2]Ibid., p. 632.

> . . . our scholar will not fall into the trap of confusing symbolism with mathematics. For example, he will recognize some passages of FARADAY's work and nearly all the difficult parts of GIBB's as being mathematically strict though not written in symbols, and he will equally easily perceive that some long strings of symbols and equations published by LAPLACE and FOURIER range from the unconvincing to the ridiculous and hence are not mathematics at all.[1]

And by the faculty of criticism Truesdell means:

> The most precious tool of scholarship is the faculty of criticism. A historian of any kind must select, even if only by defining his subject of study. . . . Our scholar must select what is science; within science he must select what is permanent; within what is permanent he must select what is important; within what is important he must select what is true. Of course, what is untrue, unimportant, or transitory has played its part and cannot be overlooked; all this must be known, and some needs mention, but the louts and loons ought not be let shout FAUSTUS or TAMBURLAINE off the stage. Neither may our scholar fall into hero-worship; although the historian today is little likely to accept the Golden Legends which the tribes of professional scientists teach their apprentices, . . .[2]

In brief, Truesdell wants the historian of science to be learned, careful, critical, and, what I would call, "humanistic." That is, the historian of science sees the development of science in larger terms than specialized disciplines with specialized problems. The history of science is a "humanistic" study in that the history of science is an example of some of man's greatest intellectual endeavors. To study the history of science narrowly is to lose the full richness of man's achievement.

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[1]Ibid., p. 634. [2]Ibid., p. 638.

> . . . History of science should be taught to laymen, not merely imparted as a trade skill of fledgling historians-of-science. If the results of research are both important in themselves and decently presented--two conditions which usually fail to be met--they can be learned and taught by any qualified scientist and even by some ordinary historians. Here MORRIS KLINE, himself the author of one of the few general histories of mathematics that is much more than a collection of elementary odds and ends or a capricious selection of special topics, has set the example for years by the doctorands in mathematical education he has guided. These men have written theses which follow the development of concrete mathematics and free of isms and philosophic or sociological or historical slants, they are lucidly written so that any college teacher of mathematics can read them with profit and pleasure, for himself and for his students forever after.[1]

So as Truesdell pounds the podium, exhorting scholars to remain scholars and not degenerate into professionals,[2] he is arguing for "the greater good" that the study of the history of science contributes to our society.

His viewpoint, from an historiographical sense, is very similar to Sarton's. And while it was not his intent in this speech to list specifics regarding the historiography of mathematics, we can glean those mentioned above.

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[1]Ibid., p. 647.

[2]Ibid. He adds:

> If the history of science should be primarily the concern of teachers, necessarily unionized in law as well as spirit, what will happen to the few scholars, the very few, who can do old fashioned research? If professionalism of the fields labeled "learned"

A case of listing specifics, though, is found in Michael Crowe's article in Historia Mathematica, "Ten 'Laws' Concerning Patterns of Change in the History of Mathematics." Crowe believes it is time for discussion on the historiography of mathematics.

> . . . the present state of the historiography of mathematics differs little (except in quality) from what it was nearly a century ago when M. Cantor published the first volume of his Vorlesungen, . . . The situation may, however, be changing. The late Imre Lakatos' "Proofs and Refutations" and Raymond Wilder's Evolution of Mathematical Concepts are examples of works that may pave the way to a new historiography of mathematics.[1]

Crowe never really explains his notion of "old" or "current," in contrast with the "new," historiography of mathematics, but he has been sufficiently impressed by the recent works of Agassi, Kunh, Popper, and others in the "new" historiography of science to believe that mathematics needs a "new" historiography.

Thus, his paper is intended

> . . . "to stimulate discussion on the historiography of mathematics by asserting ten 'laws' concerning change in mathematics, which touch on issues that

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> continues to grow at its present pace, the great scholar, the man who while standing upon the same ground as his fellows is taller and sees above their heads, may soon find himself proscribed.

[1]Michael Crowe, "Ten 'Laws' Concerning Patterns of Change in the History of Mathematics," Historia Mathematica 2 (1975):161-162.

will have to be considered if a new historiography of mathematics is to develop.[1]

The ten "laws" are:

1. New mathematical concepts frequently come forth not at the bidding, but against the efforts, at times strenuous efforts of the mathematicians who create them.

2. Many new mathematical concepts, even though logically acceptable, meet forceful resistance after their appearance and achieve acceptance only after an extended period of time.

3. Although the demands of logic, consistency, and rigor have at times urged the rejection of some concepts now accepted, the usefulness of these concepts has repeatedly forced mathematicians to accept and to tolerate them, even in the face of strong feelings of discomfort.

4. The rigor that permeates the textbook presentations of many areas of mathematics was frequently a late acquisition in the historical development of those areas and was frequently forced upon, rather than actively sought by, the pioneers in those fields.

5. The 'knowledge' possessed by mathematicians concerning mathematics at any point in time is multilayered. A 'metaphysics' of mathematics, frequently invisible to the mathematician yet expressed in his writings and teaching in ways more subtle than simple declarative sentences, has existed and can be uncovered in historical research or becomes apparent in mathematical controversy.

6. The fame of the creator of a new mathematical concept has a powerful, almost controlling, role in the acceptance of that mathematical concept, at least if the new concept breaks with tradition.

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[1]Ibid., p. 162.

7. New mathematical creations frequently arise within, and depend in the mind of their creators upon, contexts far larger than the preserved content of these creations; yet these contexts, for all their original importance, may impede or even prohibit the acceptance of the creations until they are removed by the mathematical community.

8. Multiple independent discoveries of mathematical concepts are the rule, not the exception.

9. Mathematicians have always possessed a vast repertoire of techniques for dissolving or avoiding the problems produced by apparent logical contradictions and thereby preventing crises in mathematics.

10. Revolutions never occur in mathematics.[1]

Each of the laws was followed by a brief discussion and example of what Crowe intended the law to summarize. For example, with law 5, Crowe cites the initial resistance to the acceptance of the imaginary numbers because they ran counter to the "metaphysical" understandings of mathematicians as to what numbers should or should not be. Law 7 refers to cases for example in the history of mathematics where a mathematician's philosophical interest motivated, and then sometimes interfered with, the mathematical creation. He cites Hamilton and Grassmann whose creations

> . . . arrived in philosophic wrappings which at first obscured the view of the mathematical community and then were unceremoniously discarded. Yet these wrappings were a necessary condition in the minds of Hamilton and Grassmann for their own acceptance of the gifts of their fertile

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[1]Ibid., pp. 162-165.

> imaginations, and were scarcely seen by them as distinguishable from the gifts themselves.[1]

Law 10 means that Crowe believes existing mathematics is never "overthrown" through the creation of some new mathematics. Old mathematics may be discarded, through lack of use, need, or scope. Old mathematics is certainly improved upon, but it is never "overthrown" in the sense that what was once considered mathematics is now no longer considered mathematics. In other words, while modern formulations of old mathematics may appear unrecognizable compared to the older mathematics, the "essence," or "kernel," of the mathematical concept involved still remains in the modern formulation.

This last law points to the interesting (albeit prosaic) philosophical question of "What is mathematics?". Mathematics is certainly not false mathematics. Yet, since standards of mathematical rigor and understanding have changed through history, "acceptable" mathematics certainly has changed. Thus, "acceptable" mathematics usually means "acceptable by current standards, practice, and knowledge." Older mathematics that does not satisfy current standards, practice, or knowledge is something less than "real" mathematics. Older mathematics may have historical, educational, or heuristic value, but it is not "real" mathematics. In

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[1]Ibid., p. 164.

summary, depending on how one answers the question, "What is mathematics?", determines one's response to Crowe's belief that "Revolutions never occur in mathematics." Revolutions need not be violent nor quick, and so if one believes that what was once mathematics is no longer mathematics, then possibly a revolution has occurred.

Crowe's list of laws is certainly intriguing. At the very least, it underscores the historical view that mathematics history is not simply a chronological record of theorems and mathematicians; that the history of mathematics is a rich, human history. Crowe's list brings attention to the interplay among mathematicians, the international "invisible college" of mathematics, and to both the private and public philosophies regarding mathematics.

Crowe's list also implicitly provides a crucial distinction between historiography--the methods and tools of the historian--and interpretation of history. I would argue that Crowe's list is more a set of guidelines for <u>interpreting</u> historical developments in mathematics, rather than a set of recommendations for the historiography of mathematics. In other words, Crowe's list does not provide the historian with any real techniques, approaches, or instructions for the investigation or writing of mathematics history. Crowe's list certainly does provide the historian a series of checks to interpret or evaluate other

historians or review his own. This is not to suggest that Crowe's work is not useful (at the very least, he has "stimulated discussion" from me), nor to suggest that the distinction between historiography and interpretation is great--they are both important interrelated aspects of the historian's work.

As far as I can judge Crowe's paper has not stimulated much discussion. About a year later, Herbert Mehrtens published "T. S. Kuhn's Theories and Mathematics: A Discussion Paper on the 'New Historiography' of Mathematics" in Historia Mathematica.[1] Mehrtens, one of the few sociologists of mathematics, discusses the applicability of Kuhn's work to the history of mathematics and responded to Crowe's paper. I believe that Kuhn's Structure of Scientific Revolutions has little applicability (and this is not a criticism of Kuhn) to the history of mathematics,[2] and Mehrtens tends to support this position in his paper.

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[1]Herbert Mehrtens, "T. S. Kuhn's Theories and Mathematics: A Discussion Paper on the 'New Historiography' of Mathematics," Historia Mathematica 3 (1976):297-320.

[2]One easy way to appreciate its inapplicability is that there are absolutely no references in Kuhn's book to the history of mathematics. For a while, discussion, interpretation, and criticism of Kuhn's book was an active cottage industry in the philosophy and history of science. Kuhn's book seemed to be the one you'd love to hate. Much more applicable to history of mathematics is his Essential Tension (Chicago: University of Chicago Press, 1977).

Since Kuhn's book is beyond the scope of this dissertation I shall quote Mehrtens only on an important distinction between science and mathematics affecting their respective histories and philosophies. One of the major distinctions between mathematics and science is the nature of their subject matters or concepts. In a note at the end of the article, Mehrtens attributes to H. J. Bos the view of

> . . . the intrinsic difference between science and mathematics: science uses symbolic generalizations of something, mathematics studies symbolic generalizations themselves--they are the concepts.[1]

Mehrtens' is the only paper I have found discussing Crowe's laws. And as I have mentioned before, these types of papers, in general, are few and scattered about in the literature. This lack of discussion (lack of interest?) reinforces my belief that historians in general, and historians of mathematics in particular, do not consider the historiography of mathematics a pressing issue, despite (at the risk of belaboring a point) the considerable academic activity in the historiography of science.

This is not to suggest that good mathematics history cannot be written without explicitly stated

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[1]Mehrtens, p. 319. The nature of the concepts or subject matter of mathematics begs the question of the mathematician's (and historian's) ontological commitment to the concepts of mathematics. This is discussed below in Chapter III.

historical rules and methods. The history of mathematics is an active scholarly field with many distinguished members and distinguished works, three works I shall now discuss.

In particular, Thomas Hawkins' Lebesgue's Theory of Integration,[1] Joseph Dauben's biography Georg Cantor,[2] and Judith V. Grabiner's The Origins of Cauchy's Rigorous Calculus[3] are three examples of thorough, well-written, and informative mathematics history. In addition to their exceptional scholarship, I have chosen to discuss these books because they deal with mathematicians and mathematics related to point set topology, and because each is an example of a particular genre of mathematics history.

Hawkins' intentions in his book are best summarized by him in his preface:

> This book grew out of my dual interest in the mathematics of integration theory and in the nature of its historical evolution. It is well known that modern theories of measure and integration have their roots in the work of the French mathematician Henri Lebesgue. Although a relatively small amount of space is devoted to actually discussing Lebesgue's work, it represents the focal point of the entire book. My objection has been to place Lebesgue's

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[1]Thomas Hawkins, Lebesgue's Theory of Integration, 2nd ed. (New York: Chelsea Publishing Company, 1975).

[2]Joseph Dauben, Georg Cantor: His Mathematics' and the Philosophy of the Infinite (Cambridge: Harvard University Press, 1979).

[3]Judith Grabiner, The Origins of Cauchy's Rigorous Calculus (Cambridge: The Massachusetts Institute of Technology Press, 1981).

> early work on integration theory (1901-10) within its proper historical context by relating it, on the one hand, to the developments during the nineteenth century that motivated it and gave it significance and, on the other hand, to the contributions made in this field by his contemporaries. It is my hope that this volume will engender an appreciation for Lebesgue's genius balanced by an appreciation for the manner in which his works builds upon that of many other mathematicians.[1]

Hawkins' view of the development of mathematics is one of an increasing accumulation of mathematical concepts and theorems. Lebesgue's accomplishments can only be thoroughly understood through previous mathematical developments and by the "problems" that the Lebesgue theory "solved."

> . . . pre-Lebesgue contributions paved the way for a sophisticated theory of integration. Specifically, they provided Lebesgue with both (1) a fully developed measure-theoretic point of view and (2) a number of theoretical 'problems' that had been discovered within the context of Riemann's definition of the integral (although they were not seriously regarded as problematic at the time). To appreciate what Lebesgue did and why he did it, his work must be regarded within the context of the historical developments that produced (1) and (2) without distorting the historical perspective, since they overlap considerably, and this approach was therefore not attempted.[2]

Hawkins is aware of the interrelation between developing and established mathematics--"a fully developed measure-theoretic point of view"--and the "the 'theoretical problems'" or questions created by the established mathematics. Indeed, one aspect of the interrelation is that

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[1] Hawkins, p. v.

[2] Ibid., p. ix.

each stimulates the other; problems stimulate the establishment of new mathematics to solve the problems, and new mathematics in turn often creates new problems to be solved.

Thus, Hawkins traces the mathematical developments that lead up to Lebesgue's theory of integration by beginning with a history of Riemann's theory of integration (Chapter 1). Once Riemann's theory was established, Hawkins then discusses the development of Riemann's ideas (Chapter 2) and, for example, discusses trigonometric series and term-by-term integration. Of particular interest to me was his discussion of Heine. Hawkins ability to blend biography, history, and explanation of the relevant mathematics is clearly shown in his discussion of Heine.

> Outside the circle of Weierstrass's students, the significance of uniform convergence for the term-by-term integration of series was not generally recognized until Heinrich Heine (1821-81) called attention to it dramatically in a paper [1870] on trigonometric series.[1] Heine, who was a professor of mathematics at the University of Halle, probably became familiar with Weierstrass' ideas through Georg Cantor (1845-1918), who had come to Halle after receiving his doctorate at Berlin in 1867 . . . . . Heine observed, as to whether the coefficients in the representation of a function f in the form
>
> $$(1) \quad f(x) = 1/2\, a_0 + \sum_{n=1}^{\infty} a_n \cos nx + b_n \sin nx$$

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[1] Hawkins refers to him as Heinrich Heine, while others, and from what I can tell, Heine himself, used Eduard. His full name was Heinrich Eduard Heine.

> are necessarily unique . . .
> . . . By considering the difference of two possibly distinct representations of f of the form (1) Heine reduced the problem to the following: If, for all x except those forming a set P
>
> $$(2) \quad 1/2\, a_0 + \sum_{n=1}^{\infty} a_n \cos nx + b_n \sin x = 0,$$
>
> then are the coefficients a_n and b_n all equal to zero? He was able to show that the answer is Yes when the convergence in (2) is uniform in general with respect to the set P, which is thus taken to be finite.[1]

Hawkins then mentions the influence Heine had on Cantor.

> Cantor was induced by Heine's research to study the uniqueness problem, and he set himself the task of removing the hypothesis of uniform convergence in general. This he accomplished in a series of papers of 1870-71.[2] Having thus completely removed the assumption of uniform convergence in general and thereby the reason for restricting P to be finite, Cantor [1872] next turned to the question of uniqueness when P is an infinite set.[3]

Consequently, Hawkins aptly shows how Cantor's work in set theory was originally motivated by his investigation into trigonometric series.

Chapter 3, "Set Theory and the Theory of Integration," is a discussion of the important, necessary contributions to measure and integration by set theory. Chapter 4, "The End of the Century: A Period of Transition," continues

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[1] Hawkins, pp. 22-23.

[2] See pages 17-21 above.

[3] Hawkins, p. 24.

with the development of measure and integration and includes a section on Schoenflies' Bericht and his contributions to measure theory. As Hawkins explains:

> Towards the end of the nineteenth century, the German mathematician Arthur Schoenflies (1853-1928) was commissioned by the Deutsche Mathematiker-Vereinigung to prepare a report on curves and point sets. A preliminary report was presented in September of 1898 and eventually grew into a two-volume work on the theory of sets and transfinite numbers. Over half of this work is devoted to applications to the theory of functions of a real variable, including the theory of integration. Although it represents the work of one man, it probably accurately reflects the attitudes prevalent at the turn of the century--when Lebesgue's own research was getting underway--towards Borel's theory of measure and towards the 'problems' that had arisen within Riemann's theory of integration.[1]

Hawkins, characterization and evaluation of Schoenflies' Bericht is appropriate and accurate. It is another example of Hawkins' thoroughness of detail in the recording of mathematical developments. Along with the mathematical developments, Hawkins provides useful biographical information and academic connections among mathematicians. The reader can appreciate both the mathematical developments and the mathematical community--the interactions, the discussions and criticisms, which affected the mathematical developments.

In short, Hawkins account is a professional technical history of mathematics in that he concentrates on the

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[1]Ibid., p. 106.

mathematics, the mathematicians, and the mathematical centers of Europe. Certainly any student or instructor of analysis would gain deeper appreciation of analysis through learning its history by reading Hawkins.

A more personal account in the history of mathematics is found in Joseph Dauben's Georg Cantor: His Mathematics and Philosophy of the Infinite. Considering the central importance of set theory in modern mathematics, it is surprising that the only previous historical accounts of Cantor's life and work are Herbert Meschkowski's 1967 Probleme des Unendlichen: Werk and Leben Geor Cantor[1] and Ivor Grattan-Guinness' long article "Towards a Biography of Georg Cantor."[2] Dauben's introduction to his biography clearly indicates his appreciation of Cantor and the need for such a book.

> Georg Cantor (1845-1918), the creator of transfinite set theory, is one of the most imaginative and controversial figures in the history of mathematics. Toward the end of the nineteenth century his study of continuity and the infinite eventually forced him to depart radically from standard interpretations and use of infinity in mathematics. Because his views were unorthodox, they stimulated liverly debate and at times vigorous denunciation. Leopold Kronecker considered Cantor a scientific charalatan, a renegade, a "corrupter of youth," but Bertrand Russell described

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[1]Herbert Meschkowski, Probleme des Unendlichen: Werk and Leben Geor Cantor (Braunschweig: Viewweg, 1967).

[2]Ivor Grattan-Guinness, "Towards a Biography of Georg Cantor," Annals of Science 27 (1971):345-391.

> him as one of the greatest intellects of the nineteenth century. David Hilbert believed Cantor had created a new paradise for mathematicians, though others, notably Henri Poincare, thought set theory and Cantor's transfinite numbers represented a grave mathematical malady, a perverse pathological illness that would some day be cured. Both in his own time and in the years since, Cantor's name has signified both controversy and schism. Ultimately, transfinite set theory has served to divide mathematicians into distant camps determined largely by their irreconcilable views of the nature of mathematics in general and of the status of the infinite in particular.[1]

All the ingredients for historical drama are included in the man, the mathematics, and the results of and reactions to his work. And Dauben's biography is a good one. He traces the early developments in analysis which Cantor learned as a student, the problems and questions that lead Cantor to his work in set theory, and all the mathematical (and philosophical and religious) work Cantor accomplished. At the same time Dauben blends in essential information regarding Cantor's personal life, professional associates and friends, and his place in the European mathematical community. Ultimately, Dauben is interested in the more general intellectual history of which Cantor and set theory are important symbols.

> . . . For anyone concerned with intellectual history, in fact, the development of Cantorian set theory may be regarded as a microcosm in which the nature of the creation and development of a significant new idea of science may be studied.[2]

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[1]Dauben, p. 3.

[2]Ibid., p. 4.

Hence Dauben considers his book more as intellectual history than biography

> It is well to remember that this book is neither the biography of a man, nor even the history of a single idea, although it does focus on the mathematics of Georg Cantor and specifically upon the background, emergence, and development of his theory of sets and transfinite numbers. Perhaps the best way to describe what I have attempted is to say that this book represents a study of the pulse, metabolism, even in part the psychodynamics of an intellectual process: the emergence of a new mathematical theory.[1]

Despite his disclaimer about not being biography, and my not really knowing the meaning of the "pulse, metabolism," or "psychodynamics of the intellectual process,"[2] I believe Dauben's book is excellent intellectual history and biography.

For example, Dauben discusses the relationship between Cantor and Richard Dedekind.

> . . . in the same year (1872) that Cantor's theory of real numbers was outlined in his paper on trigonometric series, Richard Dedekind published a short memoir: Stetigkeit und die Irrationalzahlen. Cantor had mailed a copy of his own paper of 1872 to Dedekind, which Dedekind acknowledged in his preface. In return, Dedekind forwarded his publication to Cantor. In carefully reading Dedekind's short monograph, Cantor agreed heartily with its results, though he was never completely convinced of the naturalness of Dedekind's approach, nor of its easy application in analysis. Nevertheless, their exchange of ideas, both by letter and through discussion during vacations spent together in the Harz Mountains, Dedekind was to have an important

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[1] Ibid.

[2] We must indulge our scientific authors occasional lapses of metaphysical extravagance.

> influence upon Cantor's thinking during one of its most creative periods. Indeed, for a time Dedekind assumed the roles of mentor, counsel, and friend.[1]

Comments like the above as well as describing and analyzing all of Cantor's mathematical, philosophical, and religious writings create a very thorough scientific biography. Dauben also describes the considerable interactions Cantor had with the European mathematical community; ranging from disagreements with Kronecker, Mittag-Leffler and others to Cantor's sole principal work in establishing the Deutsche Mathematiker-Vereinigung. Besides enumerating the mathematical and social contributions of Cantor, Dauben also provides important historical explanations and distinctions which make for an altogether unusually informative history.[2]

Structurally, Judith Grabiner's The Origins of Cauchy's Rigorous Calculus lies between Dauben's biography and Hawkins' technical professional history. Like Dauben, Grabiner provides much information about the personal and

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[1] Ibid. , pp. 47-48.

[2] For example, Dauben describes the important distinction made then by Cantor between different connotations of "real numbers." For Cantor, Dauben explains, it was

> . . . essential to differentiate between the reelen Zahlen, real numbers as opposed to the complex numbers in a formal mathematical sense, and realen Zahlen, real, actual numbers which enjoyed more than a purely formal existence. In fact, he insisted that his transfinite numbers were real in the same sense that the finite whole numbers might be regarded as real. (Ibid., p. 126).

professional life of Augustin-Louis Cauchy, although her work is not intended as strict biography. And like Hawkins, she offers a good explanation of the developments in the calculus and analysis that led to Cauchy's insightful and original work, although she did not intend to write a strictly technical history. In her book her viewpoint is somewhat more general in that she attempts (rather successfully, in my opinion) to capture the mathematical and intellectual milieu out of which Cauchy's work came.

> In looking for the eighteenth-century origins of Cauchy's foundations of the calculus, I shall demonstrate that they grew, not principally out of that period's discussion of foundations but from other, quite different parts of its work. The men who created the major results of eighteenth-century analysis--Newton, Maclaurin, Euler, d'Alembert, Lagrange--unwittingly also developed many of the ideas and methods which later were used to make it rigorous. The work of these men is especially noteworthy. Lehonhard Euler, whose work historians sometimes take to illustrate the lack of rigor in eighteenth-century calculus, nevertheless developed many techniques that Cauchy would later adapt. Even more important was Joseph-Louis Lagrange. Lagrange was the crucial transitional figure between the eighteenth- and nineteenth-century points of vies, in terms of both technique and attitude.[1]

It is Grabiner's interest in <u>rigor</u> that gives her book its distinctive viewpoint. Grabiner is writing a thematic mathematics history--her theme being the role and significance of rigor in the change of mathematical "technique and attitude" from the eighteenth century to the nineteenth century.

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[1]Grabiner, pp. 3-4.

Grabiner is not just interested in Cauchy's version of the calculus, but in the "origins of Cauchy's rigorous calculus," to use her book's title. And her notion of rigor is historical, she is writing about nineteenth century rigor.

> When a nineteenth-century mathematician spoke about rigor in analysis, or in any subject, he had several general things in mind. First, every concept of the subject had to be explicitly defined in terms of concepts whose nature was held to be already known . . . Following Weierstrass, in analysis this meant that every statement about equality between limits was translatable, by well-defined rules, into an algebraic statement involving inequalities. Second, theorems had to be proved, with every step in the proof justified by a previously proved theorem, by a definition, or by an explicitly stated axiom. This meant in particular that the derivation of a result by manipulating symbols was not a proof of the result; nor did drawing a diagram prove statements about continuous curves. Third, the definitions chosen, and the theorems proved, had to be sufficiently broad to support the entire structure of valid results belonging to the subject. The calculus was a well-developed subject, with a known body of results. To make the calculus rigorous, then, all previous valid results would have to be derived from the rigorous foundations.[1]

Consequently, Grabiner begins her book with a discussion of nineteenth century rigor (essentially the arithmetization of analysis) and Cauchy's formulation of the calculus. In the next two chapters, she then discusses the eighteenth century developments to show how they set the stage for Cauchy's rigorous calculus.

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[1] Ibid., p. 5.

> . . . Cauchy synthesized previous work and built a firm foundation so well as to obscure the attempts that preceded him. Just as Euclid's Elements were so successful that they drove much earlier work into obscurity; just as the Newton-Leibniz calculus made it unnecessary to read much earlier works on areas and tangents; so Cauchy's Cours d'analyse and Calcul infinitesimal made obsolete many of the earlier treatments of limits, convergence, continuity, derivites, and integrals.[1]

Grabiner believes a "revolution in calculus"[2] has occurred since Cauchy's work has "made obsolete many of the earlier treatments." It is also in her view a revolution in rigor. As I have commented above (p. 175), if one believes that what was once mathematics is no longer mathematics (in Grabiner's terms, "made obsolete"), then indeed a revolution has occurred.

The "hero" of the next two chapters on the eighteenth century is Lagrange because of his contributions to the calculus, especially the use of the algebra of inequalities.

> . . . it is fortunate that eighteenth-century mathematicians did not concentrate on foundations to the exclusion of other questions. Through their successful concern with particular problems, these analysts developed the mathematical substance for which Cauchy could provide the rigorous basis; and while they were solving problems, they also evolved many of the concepts and techniques which Cauchy needed. This combination of techniques and results with Lagrange's program for reducing the calculus to algebra provided the necessary conditions for Cauchy's achievement.[3]

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[1] Ibid., p. 15.

[2] The title of the first chapter is "Cauchy and the Nineteenth-Century Revolution in Calculus." (My emphasis.)

[3] Ibid., p. 46.

The remaining three chapters are a very careful and complete series of explanations on the origins of Cauchy's conceptualization of limit, continuity, convergence, derivative, and definite integral. With effective, interesting details she discusses both the precursors work and the important, unique contributions of Cauchy.

A good example of her interesting attention to detail is her discussion of the epsilon notation.

> In considering the epsilon-based concepts off limits, continuity, and convergence, it should be remarked that the Greek letter ε used by modern mathematicians--a notation that Cauchy originated and applied in several of his proofs--probably comes from the correspondence between 'epsilon' and the initial letter of the French word erreur. In later work on probability theory, Cauchy in fact used the letter epsilon to stand for error. The epsilon in a modern proof may be regarded as an inheritance from the days when inequalities belonged in approximations. The epsilon notation is a reminder that, paradoxically, the development of approximations and estimates of error brought forth many of the techniques necessary for the exact and rigorous proofs about the concepts of the calculus. Eighteenth-century mathematics were never more exact than when they were being approximate. And it was algebra that paved the way for the recognition of the link between approximation and rigor.[1]

It is examples like the above of Grabiner's attention to detail and to the more general considerations surrounding Cauchy's work that makes her book so interesting and valuable. Hers is a third kind of historical study: a

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[1]Ibid., p. 76.

thematic study in which the "epimathematical" theme of rigor acts as setting and direction for her book.[1]

Towards an Historiography of Mathematics

Having discussed histories of mathematics relevant to the history of point set topology and what they reveal about themselves, and having discussed historiographical studies relevant to the history of mathematics, such as they exist, it is now useful to glean from these discussions those ideas and lessons which contribute to an historiography of mathematics.

But, before outlining such an historiography of mathematics, it is very important to stress how easy it is to criticize other historical or historiographical writings, and how very difficult it is to research and write the history of mathematics.

The difficulty lies in several areas. First, when investigating any topic in the history of mathematics one is impressed by both the plethora and scarcity of materials

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[1] "Epimathematical historical topics" is a term coined by May in his Handbook referring to the external and related aspects of the history of mathematics. Under this heading we find "rigor" (pp. 578-79), and we are also referred to "proof" (p. 574). Curiously, Grabiner never mentions Raymond Wilder's historical article, "Relativity of Standards of Mathematical Rigor," Dictionary of the History of Ideas, ed. by Philip Weiner (New York: Charles Scribner's Sons, 1973), pp. 170-177. Wilder's article only touches on Cauchy, but it certainly discusses rigor from an historical viewpoint.

available. This may seem contradictory but the researcher can be easily overwhelmed by the many, many papers, letters, journals, books, and documents that are potential sources of information for the particular topic at hand. It is very easy to get lost in the old world of papers and books which may contain the appropriate material, but which are most often either dead-ends or distractions. Consequently after determining all the material which is <u>not</u> appropriate for the historical topic the investigator can be lead to believe that there is little evidence available for his historical research. Finding the few valuable and appropriate needles in the huge haystack of documents available makes for very difficult work. When we read some historian's work we are only reading the needles (the particularly shiny ones) he has found and can easily forget, as we criticize those documents or his commentary, the work required to produce them.

Second, while it is important for the historian to transfer himself back to the time of the mathematicians he is studying, back to the state of ignorance of the mathematicians, this is a very difficult goal. An historian of mathematics today has been mathematically trained in the modern style, approach, and topics. Many of the historical documents will contain notations, words, symbols, or references with which the historian will be

unfamiliar. Even when he finds the appropriate documents, thoroughly understanding them, in their own terms, is a difficult task in itself.

Third, understanding an historical document means understanding the larger context in which it occurs. The reasons the mathematician wrote the paper, the previous papers he had written, other relevant papers other mathematicians have written, the setting in which the paper was written, the response to the paper, etc., are all aspects of the larger context in which the paper, or any historical document, must be evaluated. (This can easily lead us back to our first difficulty.)

Fourth, documents and mathematicians do not exist in isolation. Similar to the third difficulty, there may be crucial external influences affecting either the mathematician or the document. The cultural, education, social, institutional, etc., factors must be considered for the potential influence one or more may have had on the paper or mathematician.

Writing mathematics history is not a simple, straightforward task of collecting the names, papers and dates and producing some smooth seamless narrative. From personal experience in writing my brief sketch on the history of point set topology (and I use the words

"brief sketch" intentionally, since I am well aware of it being an incomplete history of the subject) I have gained a great respect for the difficulties and of the considerable accomplishment of writing good history of mathematics.

Fifth, in the mathematical community I have found there is relatively little appreciation of or support for quality mathematics history or the difficulty of producing it. This is not meant as a criticism of mathematicians or their sense of history. By their very training mathematicians are not taught to think historically. Good mathematics, the kind to strive for, is usually the most current mathematics. Mathematicians must expend most of their time and energies with comprehending the new and the important; the history of mathematics plays almost no role in their professional lives. Since there is also a tacit equation of older mathematics with "less difficult" or "less sophisticated" mathematics, the conventional wisdom is that investigating the old is probably easier than understanding the new. Consequently, investigating and writing mathematics history is considered a less challenging task than mathematics research. And since mathematics history is ancillary to their professional lives, the place of mathematics history for research mathematicians is only in its edification.

The point of all this is thus by the nature of mathematicians and mathematics departments, there is litle, if any, consideration of the training of historians of mathematics. Standard history departments--which tend to emphasize politicial, social, cultural, American, and European history--seldom have positions for or training of historians of science in general, let alone historians of mathematics in particular.

In summary, my present view of the discipline of the history of mathematics is that it is one in which the scholarly difficulties are general, not acknowledged, its departmental position uncertain, and the training of new historians of mathematics rare. However I do believe when developing an historiography of mathematics, there are three primary areas an historian should consider: tools and knowledge, historical attitude and questions, and intent and purpose of mathematics history.

Tools and Knowledge

Mastery of the Relevant Mathematics

At the very least the history of mathematics is a technical history, so the historian must be equipped with an understanding of the relevant mathematics. The relevant mathematics means not just the mathematics directly related to the historical topic, but the historically

related topics, which may be mathematically indirectly related, and which may have contributed to the development of the topic.

Awareness of Changing Language, Terminology, and Style

As mathematics develops the language, symbols, and terminology change (another difficulty for the student who has learned the most current, modern terminology). Indeed, an index of development in mathematics is the change in terminology. For example, the word "content" meant "measure"--either inner or outer measure--in earlier works in measure and integration. There used to be two notions of the word "compact:" sequentially compact and bi-compact.

Mathematical style has changed. The modern style of presentation of axioms, definitions, theorems, proofs, corollaries, proofs, etc., is relatively recent, along with the predominant use of symbols. As recent as 1900 many of the papers and books in mathematics were much more discursive. Proofs tended to be more in words than in symbols--they were not necessarily less rigorous as a result, but their written appearance could be misleading. For example in Judith Grabiner's book on Cauchy she takes great pains to show that Cauchy's written proofs were just as rigorous as a symbolic epsilon-delta proof later used for the calculus.

Mastery of Foreign Languages

This important tool of the historian of mathematics is probably not stressed enough. At the very least the historian of mathematics must know German and French. Latin is important for historical studies from the seventeenth century and before--recall Newton's Principia was originally published in Latin, not English. And obviously Greek is required for studies of older periods.

It is important to be able to read and translate foreign languages rather than be dependent on translations (they may not even exist), by translators unfamiliar with mathematical technicalities. For example Joseph Dauben points out about a translation of Cantor's Grundlagen: that the translator did not distinguish between Zahlen and Anzahlen, translating both as "number" without making distinctions when Cantor did.

The three tools I have mentioned so far could be summarized by the requirement that the historian of mathematics must be a meticulous scholar of detail. The correct technical mathematics, the correct senses of the terminology, the correct translation (in some cases this may not be possible--the French word "ensembles" probably did not mean set in the modern sense, probably a grouping, assemblage, or aggregate would be better terms, if at all) are all essential for valuable mathematics history.

Knowledge of the External Historical Factors

A historian of mathematics must be aware of the relevant university centers (along with their organization and structure, e.g., the French and German university systems were very different), the relevant journals, scientific and mathematical associations, education and training of mathematicians, and biography. It is important to know under which mathematician a mathematician studied. It can be valuable to know who was editing a particular journal at one time, or why a mathematician chose to publish a long article as a separate monograph rather than in a journal. Up to the nineteenth century much of mathematics was motivated by problems in physics so that an historian must have some knowledge of the history of physics for research in such periods. In other words, mathematics does not develop in isolation. While it is less affected by external factors than other fields, mathematics and mathematicians exist in scientific, cultural, and institutional milieu. Mathematics history without such considerations is incomplete.

Knowledge of the Larger External History

Occasionally the politics, social culture, religion, or philosophy of a period can influence the development of

mathematics. Understanding Newton's religious beliefs, and understanding his work with alchemy,[1] provides a better history and understanding of the Principia. Joseph Dauben discusses at length Cantor's Catholicism and his correspondence with various members of the Papal hierarchy concerning his work to give a much fuller history of Cantor's mathematical contributions. In short, the historian of mathematics must be aware of the larger historical setting in which the mathematics and mathematician are found.

Historical Attitude and Questions

The above list of tools and knowledge are the working requirements of the historian of mathematics. With them the historian must then, as Truesdell was quoted above (p. 170), select and interpret material important for his historical study. Yet the selection and interpretation process is dependent on certain historiographical attitudes, certain questions the historian is always asking himself as he researches and writes.

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[1]See, for example, Robert Schofield's Mechanism and Matherialism: British Natural Philosophy in an Age of Reason (Princeton: Princeton University Press, 1970).

The Difference Between Logical, Mathematical Order and Historical Order

Since the first exposure an historian usually has of a topic in mathematics is in a mathematics course, the understanding he has of the topic is from the modern viewpoint. It is almost always the case, though, that the historical development of the topic is nowhere near the same order as the logical, mathematical order. Indeed, it seems to usually be the reverse. The important key concepts, definitions, theorems are often the last to be really appreciated historically. Mathematically and logically the key concepts, etc., are placed first in exposition or teaching. It has been said that the history of mathematics is a characature of its logical, mathematical arrangement. In the case of mathematics history, ontogeny does not recapitulate philogeny; nor should it. To know what should come first, second, and so on, in the precise, logical, mathematical order usually means knowing what comes last. Historically it was seldom the case that a mathematician knew beyond a few steps where the mathematical topic would lead. Consequently, to be able to know in advance the precise wording of definitions, axioms, or theorems, in order to gain the greatest utility, elegance, depth and force, is usually impossible.

An important view of mathematics history is that it is a history of clarifications, generalizations, improvements, abstractions, and sharpening of concepts, definitions, axioms, theorems, and proofs.

The Difference Between the Published Documents and Private Work

Most mathematicians go through several drafts of their papers, proofs, ideas, etc., before committing them to publication. Like the situation described above, what the mathematician finally produces for the public, the published work, reveals little as to how and why the mathematician did what he did. Occasionally the author will add some explanations about the source of his ideas or aids, however, especially in more modern publications, the work is almost completely disembodied from its creator. When possible the historian should know the reasons or sources of mathematical ideas, theorems, and proofs.

A particularly good description of this attitude is given by Peggy Marchi in an essay discussing Imre Lakatos.

> . . . Lakatos would say, if you want to know why a particular discovery was made, ask first, what problem the mathematician was trying to solve. Ask second what were the solutions of his predecessors, if he had any, or what were his own previous unsuccessful solutions. Ask third how these unsuccessful solutions failed. Ask fourth what qualities a successful solution must have. Of course, none of this information can tell you

> how the successful solutions to the problem was discovered. But according to Lakatos, great discoveries do not come out of vacuums. They come from people trying to solve problems, problems that they and perhaps others have previously failed to solve. Simple as this observation is, it humanizes mathematics greatly.[1]

Another method of learning about the source of a paper or idea is to find, if possible, the private papers--letters, notebooks, drafts, notes, etc.--that may provide insight into the creative enterprise of producing new mathematics.

After all, the development of mathematics occurs through the activities of mathematicians. As it has been said, the most important existence criteria in mathematics is the existence of mathematicians.

Mathematicians, in all ages I suspect, are reticient to discuss publicly (certainly not publish) their founderings and false proofs and wrong leads. For mathematicians, mathematics is finished mathematics, correct, complete mathematics. All the preceding hunches, guesses, tests and trials are not the real mathematics so records of such activity are not valuable. Professor Saunder MacLane once said that the mathematician's most valuable tool was

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[1]Peggy Marchi, "Mathematics as a Critical Enterprise," in Essays in Memory of Imre Lakatos, R. S. Cohen, ed.(Boston: D. Reidel Publishing Company, 1976), p. 385.

his wastebasket. He went on to say that most mathematicians used their filing cabinets instead. True. Things that are just wrong, misleading, or useless can only contribute to the clutter; yet, the historian can't (or at least shouldn't) help thinking that in some of those papers in the wastebasket are the seeds of the final, polished papers. Hans Freundenthal captured the spirit of producing mathematics and publishing quite well in this comment:

> This is the way we write our mathematical papers. We conceal the train of thought which led us to the result. We would not even know how to express this; if we did, it would be like exhibiting dirty linen in public. As mathematicians we are used to objectivate, which in itself is a good habit. The way we got a particular idea is nobody's business. It is true that we are writing a mathematical paper and not our "confessions."[1]

With this tendency among mathematicians to not reveal the sources or thinking processes it becomes especially important for the historian to always consider the difference between the published document and the private activity. When possible, information about the latter should provide a much richer history of the former.

Determining the Roles of Mathematicians

We all know the famous mathematicians and their contributions. But, as in the case with Grabiner's

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[1]Hans Freudenthal, Mathematics as an Educational Task (Dordrecht, Holland: D. Reidel Publishing Company,

history of Cauchy's contributions, we must be aware of the not-so-famous mathematicians and their contributions. Some mathematicians set the stage through either the beginning or partial theorems or ideas, from which the famous mathematician solves the important or deep problem or theorem. In other cases the not-so-famous provides a sort of mopping-up function by proving theorems which are the result of some famous mathematician. Or as in the case of Schoenflies and Brouwer, it was Schoenflies' errors which lead Brouwer to some of his valuable contributions.

Determining the Role or Significance of the Establishment of Theorems, Axioms, Separate Disciplines, etc.

The historian must perform the historical task of deciding when a particular theorem, set of axioms, or topic became established as a separate entity. Theorems may come out of papers in which they were only a lemma or even an aside. The Heine-Borel Theorem, in the case of Heine, was really a theorem about continuous functions over a closed and bounded domain. The Jordan Curve Theorem came at the end of a large volume of analysis, embedded in a discussion of complex-valued functions. Both theorems were eventually recognized as being important unto themselves.

1973), p. 103. Freudenthal is also discussed in Chapter III.

I have yet to be able to pin down when point set topology was considered a separate established discipline. As I have mentioned in Chapter I, Hausdorff reveals little about his work on point set topology prior to the publication of his Grundzuge, yet it is generally accepted that the publication of the Grundzuge established point set topology as a separate discipline.

In some cases the historian may only be left with the conclusion that mathematicians at the time knew about the subject; that the "invisible college" of mathematicians had the subject "in the air," and one, like Hausdorff, recorded it for publication.

It is probably safe to say that graduate students producing dissertations in a new field, courses being taught in the new field, new textbooks on the subject, inclusion in the indexing journals are all signs of the establishment of a separate discipline.

With hindsight we can probably look back on certain papers or mathematicians being the seminal factors of a new discipline. I suspect that at the time the mathematical community at large did not sense the impending separate discipline.

Intent and Purpose of Mathematics History

Steps towards an historiography of mathematics would be incomplete without some discussion of the various reasons why mathematics history is written.

Technical, Professional History

Much mathematics history is written to provide an historical account of the technical, professional results gained by mathematicians over the course of history. This type of history is a recording of the facts of the theorems, proofs, definitions, axioms, etc., and the relevant mathematicians related to such work.

Illuminating or Appreciating Present Mathematics

Students and mathematicians often find a greater appreciation of present mathematics when seen in the light of mathematics history. There is a tendency to use mathematics history to justify what we now know by seeing current mathematics as the improvement and culmination of the history of mathematics. Certainly much of Bourbaki's approach to history is from the modern viewpoint. What is considered important now thus has an important history. I suspect that is why so relatively little has been written about the history of point set topology. Presently,

point set topology is not considered an important mathematical subject. The concepts of point set topology are essential, but the subject itself is not a source of currently important research problems. Thus, so little historical study of it.

Understanding the History of Science

Many developments in the history of science required the tools of mathematics in their developments. This is much of Bochner's point in his book Mathematics and the Rise of Science. Certainly Morris Kline believes that much of mathematics and its history is the history of the solution of physical problems through mathematics. Up until the nineteenth century much of mathematics could be characterized this way. Mathematicians were usually allied with natural scientists or natural philosophers so that mathematics, nature, and philosophy of nature were all part of the same study.

General Intellectual History

In some areas of the history of mathematics this has been especially illuminating. Much of Greek mathematics and its history is intimately connected with Hellenic intellectual history. For example, A. Szabo has written on this in his article, "The Transformation of

Mathematics into Deductive Science and the Beginnings of its Foundations on Definitions and Axioms."[1] Or Otto Neugebauer's The Exact Sciences in Antiquity is another example of understanding some of the intellectual history of the Egyptians and Babylonians through their mathematics.

I have mentioned A. N. Whitehead's comments about mathematics in the history of thought, and other philosophers have occasionally treated mathematics and its history as part of the general intellectual milieu of a period.

Understanding the Philosophy or Nature of Mathematics

The study of the history of mathematics inevitably leads one to ideas or conclusions about its nature, structure, or philosophy. How it develops suggests aspects of why it develops and suggests explanations of its unique nature. History of mathematics then becomes history-as-explanation; mathematics has this history because mathematics has this particular nature. One becomes inclined to think that mathematics had to develop in such a way because it has the particular nature.

Thus, establishing a historiography of mathematics or an historical approach to mathematics begs nascent philosophical questions about the nature of mathematics,

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[1] Scripta Mathematica, 27 (1964), Part I:24-48A, Part II: 113-139.

the philosophy of mathematics, and the relations between the history and the philosophy of mathematics. These subjects are discussed in the next chapter.

CHAPTER III

PHILOSOPHY OF MATHEMATICS AND ITS RELATIONS TO THE HISTORY OF MATHEMATICS

Having discussed the specific history of point set topology and the more general problem of historical method in mathematics history, it is now necessary to consider those aspects of the nature and philosophy of mathematics that pertain to its history and historiography. We shall also consider the potential contributions of the history of mathematics to various aspects of the philosophy of mathematics. It is the interrelations between its history and its philosophy that ultimately reveals the place of mathematics in human thought.

The Different Senses of the Philosophy of Mathematics

> There are some philosophers who are capable of discoursing after dinner on various results in modern science and their philosophical significance, but manage to preserve a basic opacity while creating an illusion of understanding and not committing factual mistakes.
>
> --Hao Wang

Under the rubric, "philosophy of mathematics," are found several different topics describing or explicating

the structure, the objects, the foundations, the nature, the epistemology or ontology, the logic, the truth or certainty, or the deductive rigor involved in mathematics.

One reason for so many senses of the philosophy of mathematics is that many scholars feel competent to discuss the philosophy of mathematics or science--one only needs to wax reflective, imagine some idealized form of mathematics (the ultimate "formal" science), and the commentaries follow. Thus, my inclusion of Hao Wang's aphorism. More important, Wang, and only a few others, appreciate the distinctions among the various senses of the philosophy of mathematics. In his wide ranging From Mathematics to Philosophy, Wang comments on the variety of "topics in the philosophy of mathematics."

> The achievements of Frege have led gradually to a formal and rigid trend not only in the philosophy of mathematics but also, notably through the work of earlier Wittgenstein and Carnap, in general philosophy. On the mathematical side, the influences of Cantor and Hilbert culminated in the work of Godel. He is the central figure in the discipline of mathematical logic, which has more recently evolved into a technical branch of mathematics. Gradually the inadequacies of mathematical logic as the basic tool for the philosophy of mathematics and for general philosophy have come to be felt. On the one hand, as mathematical logic develops into a substantial subject, the appeal of bold simplicity both inside the subject and as projected on mathematics and on knowledge is lost. On the other hand, the disregard by mathematical logic of actual mathematical activity and practice is seen to yield a one-sided approach to the philosophy of mathematics and knowledge in general.

> A partial list of overlapping basic issues commonly discussed in the philosophy of mathematics is the following.
>
> 1. The nature of pure logic and its place in human knowledge.
> 2. The characterization of mathematical concepts.
> 3. The place of intuition and formalization in mathematics.
> 4. The relation between logic and mathematics.
> 5. The nature of mathematics and its relation to concepts such as necessity, analyticity, certainty, the a priori, and self evidence.
> 6. The place of mathematics in human knowledge.
> 7. Mathematical activity and practice.[1]

Wang's background is in pure mathematics and logic (he was also a friend and kind of personal secretary to Kurt Godel, so much of the interest in this book lies in his comments, letters, and records of conversations with Godel), hence he is well qualified to discuss aspects of the nature of mathematics.

Another mathematician and philosopher, Willem Kuyk, has written about foundations and the history of foundations in his Complementarity in Mathematics. He has developed a list similar to Wang's.

> The history of the foundations of mathematics, the present author maintains, can be viewed as an interrelated set of attempts to unite a variety of aspects of the (classical) mathematical activity. Included are the following:
>
> (1) the role of 'logic,' or 'naive logic,' as a spontaneous innate ability of man to form

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[1]Hao Wang, From Mathematics to Philosophy (London: Routledge and Kegan Paul, 1974), pp. 30-31.

concepts of things, and an unending variety of things, concepts of concepts of things, etc.;

(2) the role of language, as a communicative tool, bearing information;

(3) the role of (formal) logic, as an historically unfolding set of (more or less formalized) rules of thought, made with the specific purpose of ordering and organizing, coherently and deductively, the concepts belonging to specific domains of thought (such as mathematics, physics, etc.);

(4) the role of constructive thinking, in the sense of building new entities from given entities in such a manner that the properties of the new entities can be derived from the properties of the old entities, without using a terminology which (pre)supposes the existence of a 'Platonic' world of all properties of some sort;

(5) the role of intuition, as an immediate grasping of evidence regarding thinking, i.e., a way of coming to knowledge, independent of deduction;

(6) the evaluation of the question whether in mathematics there is a 'first knowledge' as opposed to 'derived knowledge,' or 'second(ary) knowledge' (e.g., is the number concept primary or is the concept of a 'set' basic to all of mathematics, as Cantorists assume? What is the nature of our knowledge about numbers?);

(7) an account of the fact that along deductive paths the application of purely mathematical methods often leads to (approximate but) true knowledge about the world, i.e., that basic mathematical properties of number and the knowledge of topology and analysis may find application in non-mathematical domains (e.g., is it 'accidental' that the group concept is applicable in physics, or are mathematical structures to be viewed as part of a much wider thought pattern, including thought in the (natural) sciences?).[1]

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[1] Willem Kuyk, Complementarity in Mathematics (Boston: D. Reidel Publishing Company, 1977), pp. 130-131.

In comparison, we see that Wang's list tends to emphasize the place of mathematics in human knowledge; the study of the philosophy of mathematics as a part of the history of ideas. Kuyk's list tends to emphasize the activity of mathematicians as part of the philosophy of mathematics with considerations of the place of individuals' intuition, constructive thinking, personal knowledge. Both of them counterbalance the tendency to over-emphasize foundations and axioms as the most important senses of the philosophy of mathematics. Forming an amalgam from these two lists we see that there are six general senses of the philosophy of mathematics.

1. foundations and axioms
   This sense is concerned with the "fundamental" concepts of set, number, etc., the concepts of deduction, rigor, presentation and justification, as well as foundations and axiom systems.

2. characterization of the nature of mathematics
   Ranging from Russell's famous statement that "pure mathematics is the class of all propositions of the form 'p implies q,'" to Bourbaki's view that the nature of mathematics is found in its structures, the characterization of mathematics attempts to capture the unique nature of mathematics.

3. the role and use of mathematics in other fields
   This sense of the philosophy of mathematics pertains to contributions, applications, and uses of mathematics to other disciplines and possible explanations as to why it has had such a significant effect on other fields.

4. mathematical activity
   This sense of the philosophy of mathematics is concerned with the concepts of mathematical

thinking, insight, and creativity, with mathematical discovery and invention, and with the form of research carried on by mathematicians.

5. truth, certainty, the a priori, and necessity
   This sense of the philosophy of mathematics is concerned with truth and certainty in mathematics, the a priori in mathematics, and the necessity of conclusions in mathematics.

6. ontology and epistemology in mathematics
   This last sense of the philosophy of mathematics is concerned with the ontological commitment to the abstract concepts of mathematics by working mathematicians. It is also concerned with how they come to know, understand, and believe these abstract concepts.

The remainder of this section will be a detailed, although not to be considered definitive, discussion of each of these senses of the philosophy of mathematics. I am doing this for several reasons.

First, it is my belief that much of the indeterminate conclusions and unproductive discussions regarding the philosophy of mathematics are a result of confusing and conflating the various senses of philosophy of mathematics. Examples abound in the discussions of philosophy of mathematics. Let me consider two.

Haskell Curry in his short essay, "Remarks on the Definition and Nature of Mathematics," gives a brief account of the formalist philosophical position on mathematics.

> The principal thesis is that mathematics may be conceived as an objective science which is independent

> of any except the most rudimentary philosophical assumptions. It is a body of propositions dealing with a certain subject matter; and these propositions are true insofar as they correspond with the facts. The position take is a species of formalism, which may be called empirical formalism.[1]

This is well and good. Curry is giving a short, precise (but limited) characterization of the nature of mathematics (the second sense I list on page 215). This follows with a subheading, "The Problem of Mathematical Truth."

THE PROBLEM OF MATHEMATICAL TRUTH

> There are three principal types of opinion as to the subject matter of mathematics, viz. realism, idealism, and formalism. We shall consider here the realist and intuitionist views, leaving formalism for the next section.[2]

Now the confusion begins. Curry began with a characterization of mathematics, then he ostensibly switches to "mathematical truth," which is another sense of the philosophy of mathematics. Yet under the heading, "The Problem of Mathematical Truth," he switches again to realism, idealism, and formalism. In my opinion, realism, idealism and formalism pertain more to the senses of ontology and epistemology, than to the problem of mathematical truth.

Later in the essay he returns to formalism with the subheading, "The Formalist Definition of Mathematics,"

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[1]Haskell Curry, "Remarks on the Definition and Nature of Mathematics," in Hilary Putnam and Paul Benacerraf, eds., Philosophy of Mathematics: Selected Readings (Englewood Cliffs: Prentice-Hall Publishing Co., 1964), p. 152.

[2]Ibid.

with the eventual definition of mathematics[1] that "mathematics is the science of formal systems." This "definition" reveals the potentially restrictive and unproductive nature of definitions of mathematics. More to the point though is that within this subheading, "The Formalist Definition of Mathematics," Curry actually implies his view of mathematical truth:

> . . . we start with a list of elementary propositions, called axioms which are true by definition,[2] and then give rules of procedures by means of which further elementary theorems are derived.[3]

Curry's solution to "The Problem of Mathematical Truth" is that mathematics is true by definition. This is certainly a part of the formalist sense of the philosophy of mathematics. Curry should have included his solution under the heading "The Problem of Mathematical Truth" rather than under the heading "The Formalist Definition of Mathematics."

Let me add that within the term "formalism" is to be found senses of ontology and truth in mathematics, and

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[1]Giving mathematics a short, neat definition seems to have a long tradition in the philosophy of mathematics. From Aristotle and Plato down to Russell and Pierce there has been an uncontrollable urge upon the part of philosophers to succinctly and completely capture the essence and nature of mathematics in some pithy definition. Asking for a short, neat definition of (say) art would be considered naive, yet the tradition to define mathematics remains strong.

[2]My emphasis. The others are Curry's.

[3]Curry, p. 153.

a sense of the foundations of mathematics. In discussion of formalism the foundational, ontological, and truth senses are often conflated. This is not surprising since Hilbert, Church, Curry, and others use formalism to explain or explicate several senses of the philosophy of mathematics. The difficulty arises when the senses are not clearly demarcated and thus conflated.

Curry's short essay is just one of many examples of confusion and conflation of the different senses of the philosophy of mathematics. A more recent example of this confusing of senses is found in Nicolas Goodman's paper, "Mathematics as an Objective Science."[1] Goodman writes from the point of view of a working mathematician who (rather arrogantly) takes to task various philosophical positions on mathematics with regard to "objectivity in mathematics."

> . . . I shall examine four views which, though they do not exhaust the current range of opinion in the philosophy of mathematics, are nevertheless sufficiently representative to raise what seems to me to be the main issues of about the objectivity of mathematics. I shall argue that each of these views arises from an oversimplification of what happens when we do mathematics.[2]

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[1]Nicolas Goodman, "Mathematics as an Objective Science," American Mathematical Monthly 86 (1979):540-551.

[2]Ibid., p. 540. Notice already that Goodman is conflating the senses of "objectivity of mathematics" and "mathematical activity of mathematicians."

Goodman introduces his argument with an explanation of his term "surfacism." All it means is that philosophical positions that do not take into account the real activity of working mathematicians can only develop a superficial or "surface" impression of mathematics. Thus the philosophical positions have thinly sublime surface quality with no real depth.[1] For example,

> . . . Belief in the solid inside of a material object, [the surfacist philosopher] asserts, is a remnant of belief in the immortal soul, which was the "solid" inside of a human being. As a matter of fact, he argues, the usual account is incomprehensible. Who can visualize a material object except by visualizing its surface? Who, when visualizing a material object, can visualize anything in addition to its surface?
>
> It seems to me that the views about the nature of mathematics that I wish to discuss are forms, more or less disguised, of surfacisim.[2]

Goodman's view of the philosophy of mathematics, which he equates with the phrase "mathematics as an objective science," is that

> . . . a philosophy of mathematics is closely analogous to a view about the nature of material objects. Its main function should be to facilitate the ongoing social process of doing mathematics. It follows that a serious philosophy of mathematics must satisfy the principle of objectivity. [I.e., Anything which is practically real should be taken as objectively real (p. 541)]. That is, it must not deny objective

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[1]The implication being that only mathematicans are capable of philosophical observations and judgements. Just imagine if all art or literature critics quit their profession because they were not also artists and novelists.

[2]Goodman, p. 541.

> reality to any aspects of mathematical activity which have practical reality.[1]

Aside from his own rather naive philosophical position, "the principle of objectivity," Goodman again confuses the activity of mathematicians with the objectivity of mathematics, an aspect of the nature of mathematics.

He then preceeds to describe the philosophical positions of formalism, intuitionism, logicism, and platonism. In each case he uses a rhetorical sleigh of hand by first naming mathematicians and philosophers associated with each position, but then never actually citing passages from their cited works. For example, consider his view of formalism:

> To make this statement more concrete, let us imagine asking a formalist what he takes to be the content of the fundamental theorem of arithmetic. If he is really a strict formalist, he must reply that, standing alone, it has not content at all. The theorem is, after all, just a string of symbols. What makes us feel that it has content is only that it plays a definite role in certain activities we engage in . . . [2]

Goodman rejects the formalist position because it does not, according to him in the above example, take into account the mental, intuitive activity of the mathematician as he deals with the string of symbols. The mental, intuitive activity he terms "intuitive constructions."

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[1] Ibid., p. 542. My emphasis.

[2] Ibid.

> Now my argument may be summarized as follows. Intuitive constructions are practically real. They are vital to the practice of mathematics. It is the essence of formalism that it denies their objective reality. Therefore, by the principle of objectivity, formalism cannot be an adequate philosophy of mathematics.[1]

More confusion. His description of the fundamental theorem of arithmetic for the formalist is about the ontological commitment the formalist takes regarding the fundamental theorem of arithmetic. In fact, Goodman unwittingly admits that when he refers to the "content" of the fundamental theorem of arithmetic. The "content" of mathematics is an ontological issue. The activity, the intuition, the mental processes of mathematics are entirely other issues. Formalism does not "deny their objective reality," formalism is simply not concerned with "intuitive constructions" when considering the "content" of a mathematical theorem.

As I have said, Goodman uses a kind of sleigh of hand. He never actually quotes the papers he refers to. With the subject of formalism he mentions Hilbert's paper, "On the Infinite."[2] Let me inject a few quotations from that paper to supplement Goodman's characterization of formalism.

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[1] Ibid., p. 544.

[2] David Hilbert, "On the Infinite," in Philosophy of Mathematics: Selected Readings, Hilary Putnam and Paul Benacerraf, eds. (Englewood Cliffs: Prentice-Hall Publishing Company, 1965), pp. 134-151.

> . . . the infinite occupies a justified place in our thinking, that it plays the role of an indispensible concept.[1]
>
> . . . The subject matter of mathematics is, in accordance with this theory, the concrete symbols themselves whose structure is immediately clear and recognizable.[2]
>
> Let us remember that we are mathematicians and that as mathematicians we have often been in precarious situations from which we have been rescued by the ingenious method of ideal elements.[3]

Hilbert was quite well aware of the part played by mathematicians in the construction of mathematics. What makes his paper superior to Goodman's is his awareness of the distinctions of the different senses of the philosophy of mathematics.

In my opinion, Goodman makes two major errors. First, as I believe I have demonstrated, he confuses the various senses of the philosophy of mathematics. Among others, he confuses "mathematical content" with "mathematical activity." Second, he dismisses the various philosophical positions because they do not take into account his "objective reality." True, in general, the positions (as he presents them) do not take into account "objective reality." But Goodman is asking them to consider one sense of the philosophy of mathematics while they were considering others.

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[1] Ibid., p. 137.

[2] Ibid., p. 142. My emphasis.

[3] Ibid., p. 145.

Since they did not consider his particular philosophical interest in mathematics, they are useless to him, and therefore he dismisses them. It is one thing to dismiss a philosophical position because it is incorrect or weak, it is another (inappropriate) thing to dismiss a position because it is of no use to one's particular position. Goodman dismisses the other positions because they are of no use to his position. This, I believe, is his second major error.

It is an easy and often pointless game to pick apart philosophical positions that confuse or conflate the various senses of the philosophy of mathematics. Of serious value is to separate, explicate, and recover the useful portions of the various philosophical positions. This is especially difficult because mathematics suggests so many philosophical senses through its results, its applications, its complexity, rigor, truth, etc.

This is difficult but very important for understanding the history and historiography of mathematics. This is very important for at least two major reasons. First, working mathematicians probably carry with them some (more or less) implicit philosophy of mathematics regarding its nature, structure, ontology, etc. For example, consider Cantor's views about the infinite and his philosophy of mathematics. Cantor was more explicit, as Dauben's

biography reveals, than most mathematicians regarding his philosophy of mathematics. Equipped with a critical understanding of the different senses of the philosophy of mathematics the historian or philosopher is better able to interpret the comments and views of mathematicians about their work.

Second, historians also carry an implicit philosophy of mathematics in their understanding of its nature, structure, etc. Recall the discussion in Chapter II about the implicit views on the nature of mathematics of historians as revealed by their histories (see the section entitled "What the Histories Reveal about Themselves"). Equipped with a critical understanding of the different sense of the philosophy of mathematics historians may be more reflective, self-critical, and clear about their aims, procedures, results, and interpretations of their historical investigations. It can be suggested that this lack of consideration of the philosophy of mathematics may be one reason why there is little writing in the historiography of mathematics.

Third, one is in a better position to critically evaluate and appreciate other's historical writings about mathematics equipped with an understanding of the different senses of the philosophy of mathematics.

Thus, I shall now discuss these different senses of the philosophy of mathematics. The primary goal is to separate, explicate, and clarify the various senses. It is not my intent to criticize, analyze, compare, or contrast them--although that may occur indirectly. Once these senses have been clearly demarcated and understood, then their potential use and implications to the history and historiography of mathematics may become clear.

Foundations and Axioms

From about 1980 to 1930 there was considerable activity in establishing foundations of mathematics as well as axiom systems. Three major "schools" or viewpoints emerged, the logicist, the intuitionist, and the formalist foundations of mathematics.

The logicist school of Frege, Russell, and Whitehead believed that mathematics could be derived from logical principles. Or, that mathematics could be built upon a foundation of logic. Using concepts such as class, class-membership, reducibility, infinity, choice, and type, the logicists in the persons of Russell and Whitehead attempted to demonstrate, through the Principia Mathematica, that all of mathematics could be derived from logic. As is now well known, the logicist program became extraordinarily complicated and cumbersome with the eventual ramified

type theory and other "improvements" as a means of saving and completing the program. Moreover, the logicist program required, in addition to the axioms of logic, an axiom of infinity, and the axiom of choice. The additional axioms are not logical axioms, but mathematical axioms. Thus, the logicist program could not be strictly constructed on logical axioms alone.

One of the aims of the logicist program was the guarantee, through a suitable axiom system, of truth and certainty in mathematics. Recall, one of the motivating forces for Frege and Russell were the paradoxes of set theory. The paradoxes seemed to call into question the possibility that the truths of mathematics were certain, indubitable truths. After all, if mathematics was to be based on set theory, and within set theory there were paradoxes or contradictions, how could one have certainty about the truths of the rest of mathematics which was built from set theory? Hence, the Russell-Whitehead logicist program.

David Hilbert, in his essay "On the Infinite," describes the situation very well:

> Let us admit that the situation in which we presently find ourselves with respect to the paradoxes is in the long run intolerable. Just think: in mathematics, this paragon of reliability and truth, the very notions and inferences, as everyone learns, teaches, and uses them, lead to absurdities. And

> where else would reliability and truth be found if even mathematical thinking fails?
>
> But there is a completely satisfactory way of escaping the paradoxes without committing treasons against our science.[1]

Hilbert's way of "escaping the paradoxes without committing treasons" was to take a different approach. Basically,--and this is the major distinction of the formalist school from the logicist school--his approach was to abandon the search for truth and certainly and to attempt to guarantee the consistency of mathematics. That is, questions of truth and certainty are no longer in the realm of the foundations of mathematics, rather the question of "formal axiomatics" proved to be consistent must be answered.

In brief, Hilbert believed that all true arithmetical propositions could be formally proved (the completeness of the system). And he believes that for any proposition P: P and its negation, not-P, could be proved (the consistency of the system). And lastly, his aim was to produce a meta-mathematics to prove the completeness and consistency of the formal system. An "external mathematics" capable of proving the consistency and completeness of arithmetic. If the basic arithmetic of mathematics could be shown to be consistent, and complete, then this was a sufficient guarantee that the mathematics derived from these axioms

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[1]Hilbert, in Putnam and Benacerraf, p. 141.

of arithmetic was, if not true and certain, at least consistent and complete.

Hilbert proposes that it be shown that all mathematical statements can be reduced to formulas. Hilbert included statements about elements he termed "ideal elements," like the ideal element of infinity. Since mathematics now has no meaning or ontological content the ontological distinction between (say) the number five and the concept of infinity disappears. The distinction between real, evident elements of mathematics and ideal elements no longer exists. What remains to be shown is not that some formula is true or false, but that the whole system of formulas is consistent.

L. E. J. Brouwer had yet another approach to the foundations of mathematics. He felt that the basic ideas of mathematics were derived from our perception of time, "the apriority of time."

> . . . This neo-intuitionism considers the falling apart of moments of life into qualitatively different parts, to be reunited only while remaining separated by time, as the fundamental phenomenon of the human intellect, passing by abstracting from its emotional content into the fundamental phenomenon of mathematical thinking, the intuition of the bare two-oneness. This intuition of two-oneness, the basal institution of mathematics, creates not only the numbers one and two, but also all finite ordinal numbers, inasmuch as one of the elements of the two-oneness may be thought of as a new two-oneness, which process may be

> repeated indefinitely; this gives rise still further to the smallest infinite ordinal number ω.[1]

Brouwer insisted that it was not sufficient to prove the existence of some mathematical object. Existence for Brouwer means constructible existence. To say a mathematical object exists means that a method of constructing the object in a finite number of steps exists. This latter requirement is often called Constructivism, or the constructivist aspect of Brouwer's intuitionism.

In one sense, the distinctions and disagreements among the various schools of the foundations turn on their differences regarding the notion of infinity.[2] Roughly speaking, the logicists postulated the existence of an infinity, Hilbert included infinity as one of the "ideal elements" of his system, and Brouwer accepted only the notion of potential infinity.

All these systems organize or justify existing mathematics. When they were created their purpose was not to produce new, previously unknown, mathematics, rather, the aim was to justify, rigorize, or present in a defendable form the existing mathematics. If, once the

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[1] L. E. J. Brouwer, "Intuitionism and Formalism," in Putnam and Benacerraf, p. 69. My emphasis.

[2] See, for example, Allan Calder's article "Constructive Mathematics," in Scientific American 241 (October, 1979): 146-171, for a discussion of this point.

foundationals system has been erected, new mathematics is created based upon the system, that is well and good, but that is not the intended, motivating purpose of the systems.

With Frege and Russell it was truth, certainty, and rigor, with Hilbert is was consistency and completeness, and with Brouwer it was acceptability of, in all three cases, existing mathematics and methods, first and foremost.

Moreover, implicitly all three programs believe that the structure of mathematics and its foundation begin with clear, indubitably true concepts, and the rest of mathematics flows from these indubitably true concepts. Coined as the "Euclidean programme" by Imre Lakatos and used in a number of his essays,[1] Lakatos distinguishes the form of structure of mathematics from the form of the structure of the natural sciences. Natural science follows an "Empiricist programme" in which scientists begin with observations and experiments of the natural world with the intention of deducing basic statements--empirical terms--about the real world. For example, one observes the

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[1]Imre Lakatos, "Infinite regress and foundations of mathematics," and "A renaissance of empiricism in the recent philosophy of mathematics," among others, found in his collected Philosophical Papers, John Worrall and Gregory Currie, eds. (Cambridge: Cambridge University Press, 1978), 2:3-23, 24-42.

falling of objects to the ground to determine basic statements about gravity, or basic equations about falling bodies. The end result in an "Empiricist programme" is the basic statements about the natural world. In contrast, mathematics has the form of an "Euclidean programme" in which the "basic statements" begin the program.

> I call a deductive system a 'Euclidean theory' if the propositions at the top (axioms) consist of perfectly well-known terms (primitive terms), and if there are infallible truth-value injections at the top of the truth-value. True, which flows downwards through the deductive channels of truth-transmission (proofs) and inundates the whole system. (If the truth-value at the top was False, there would be no current of truth-value in the system.) Since the Euclidean programme implies that all knowledge can be deduced from a finite set of trivially true propositions consisting only of terms with a trivial meaning-load, I shall call it also the Programme of Trivilization of Knowledge.[1]

Lakatos was known for his sarcasm so the term "trivilization of knowledge" was intentional. Yet, his point is good.

> . . . Mathematics has been trivialized, derived from indubitable, trivial axioms in which only absolutely clear trivial terms figure, and from which truth pours down in clear channels. Concepts like 'continuity,' 'limit,' etc., were defined in terms of concepts like 'natural number,' 'class,' 'and,' 'or,' etc. The 'arithmetization of mathematics' was a most wonderful Euclidean achievement.[2]

The foundational schools are then just variations of the "Euclidean programme." Consequently Lakatos' understanding

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[1] Ibid., p. 4.

[2] Ibid., p. 10.

of the history and motivation behind the various schools is that:

> No Euclidean theory, however, can ever stand up to the skeptical criticism. And the most incisive skeptical arguments against mathematical dogmatism came from the self-tormenting doubts of the dogmatists themselves: 'Have we really reached the primitive terms? Have we really reached the axioms? Are our truth-channels really safe?' These questions played a decisive role in Frege's and Russell's great enterprise to go back to still more fundamental first principles, beyond the Peano axioms of arithmetic.[1]

Indeed, one of the major differences between the intuitionists and the classical mathematicians is the acceptability of one of the "truth-channels." Intuitionists do not accept the Law of the Excluded Middle as a "truth-channel" device in mathematics. Results gained by the Law of the Excluded Middle are not, for the intuitionists, considered acceptable.

While the various foundational schools disagree as to the basic indubitably true, trivial statements and occassionally as to the truth-channels, they all implicitly agree that, at least, that is where the foundations of mathematics begin.

Characterization of the Nature of Mathematics

For many philosophers the "essence" or "essential nature" of mathematics is found in its axioms or

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[1]Ibid., p. 14.

foundational system. For example, Max Black's The Nature of Mathematics is a long discussion of the various foundational schools, with an occasional nod to early Wittgenstein and F. P. Ramsey. Black explains his view of the nature of mathematics and reasons for examining the foundations.

> . . . For mathematics is the study of all structures whose form can be expressed in symbols, it is the grammar of all symbolic systems and, as such, its methods are peculiarly appropriate to the investigation of its own internal structure. But the structure of mathematics, though implicit in its theorem, is not clearly shown and tends to be confused even by those who are most familiar with it. It is the philosopher's task to exhibit the inherent structure and to invent a suitable symbolism for its expression. Elimination of unnecessary postulates and the explicit exhibition of the structure of mathematics prevents confusion of purpose within the science and adds to the aesthetic satisfaction of contemplating it.[1]

But for Black this all means examining the foundations--the implied belief being that understanding the foundations means understanding the theorems, the branches of mathematics and the essential nature of it as a whole.

Rather, I believe that to begin to describe or explicate the nature of mathematics one must attend to its presentation, its permanence, its abstractness and generalization, its applicability to the natural world,[2] its unity and its form and structure.

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[1]Max Black, The Nature of Mathematics (Paterson: Littlefield, Adams & Co., 1959), p. 4.

[2]Eugene Wigner memorialized this idea with his article entitled, "The Unreasonable Effectiveness of

The Presentation of Mathematics

While Russell's view that "pure mathematics is the class of all propositions of the form 'p implies q'"[1] may appear two narrow a view, his point is well taken. Not only in its foundation, but in its general presentation, after allowing for terms, axioms and definitions, mathematics can be presented as this special language of statements, p, q, r, s, . . . in which one follows, by rules of inference such as modus pollens, after another. No other science can be arranged quite like mathematics. Thus, mathematics is sometimes characterized as a "formal science," as opposed to the "natural sciences."

The Permanence of Mathematics

Not only is its presentation unique but its permanence is a singular aspect of mathematics. While the history of mathematics is replete with false statements, theorems, and proofs, and while mathematicians have been simply wrong, and their errors have gone unnoticed for (sometimes) long periods of time, that which appears to be true, appears to be true forever. It seems unlikely that the Pythagorean Theorem will ever be shown to be false.

Mathematics in the Natural Sciences," Communications on Pure and Applied Mathematics 13 (1960):1-15.

[1]Russell, Principles of Mathematics, p. 3.

The permanence of its truths is tied to two things: first, the application of the truths, and second, the generality of the truths. For example, the permanently true statement, "5 + 7 = 12" must be applied to specific situations. It is true that five stones added to a quantity of seven stones produces a total quantity of twelve stones. But, five quarts of water mixed with seven quarts of alcohol will not produce twelve quarts of mixture. Thus, while the equation "5 + 7 = 12" must be applied to only appropriate situations, there are many, many situations to which it may be applied. The generality of the truth, "5 + 7 = 12," means that it, in fact, is <u>not required</u> to apply to anything. One could call it a truth internal to mathematics. After constructing (or postulating) the whole numbers and addition, "5 + 7 = 12" becomes one of those p, q, r,'s . . . Russell refers to. At that point the equation, formally and technically, is not about anything. It is the absence of real, natural world connotations, its abstract nature (mathematics for mathematics sake, if you wish) that adds to the generality and permanence of mathematical truths.

<u>The Unity of Mathematics</u>

Another aspect of the unique nature of mathematics is its unity. As we saw in the historical chapters of

this essay, a number of mathematicians and historians perceived a change in the history of mathematics in that topics like topology tended to unify and reveal interconnections among previously considered diverse parts of mathematics.

This view of the unity of mathematics is an especially important part of the Bourbaki view of mathematics. According to Bourbaki, seemingly disparate mathematical theories may have analogous axiomatic structures. One aspect, then, of the philosophy of mathematics is to locate the common underlying structures of mathematics.

With Bourbaki all known structures have been reduced to combinations and variations of three fundamental "mother structures:"

1. Algebraic structures. Structures like groups and rings which require a law of composition.
2. Order structures. Structures defined by a transitive, binary relation between elements.
3. Topological structures. Structures which formulate in the abstract concepts such as neighborhood, open set, space, etc.[1]

A mark of mathematical genius, for Bourbaki, is the recognition of the underlying unifying structure of

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[1]Nicolas Bourbaki, "The Architecture of Mathematics," in Great Currents of Mathematical Thought, F. Le Lionnais, ed. (New York: Dover Publications, 1971).

seemingly unrelated mathematical objects or theorems. This is a very modern philosophical viewpoint of mathematics. And its source is more in mathematics than in philosophy. The twentieth century history of mathematics is very much a history of abstract structures being applied to existing mathematics and studied in their own right. Thus, it makes sense that Bourbaki's philosophical overview of the important unity of mathematics be in terms of these structures.

The Role and Use of Mathematics in Other Fields

The great range of applications of mathematical structures to the real, physical world is another aspect of mathematics' nature. As mentioned above, Eugene Wigner captured the spirit of the great, effective, and often surprising usefulness of mathematics to science in his title, "The Unreasonable Effectiveness of Mathematics in the Natural Science." As he explains

> . . . the enormous usefulness of mathematics in the natural sciences is something bordering on the mysterious and that there is no rational explanation for it. Second, it is just this uncanny usefulness of mathematical concepts that raises the question of the uniqueness of our physical theories.[1]

Wigner's paper is then composed of three sections: a description of mathematics, a description of physics, and

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[1]Wigner, p. 2.

the role of mathematics in physical theories. His description of mathematics seems to reflect his physicist view of mathematics: mathematics is primarily an internal subject matter in which rigor, proof, and elegance play substantial parts. From his viewpoint as a kind of formalist, Wigner believes that:

> Most more advanced mathematical concepts, such as complex numbers, algebras, linear operators, Borel sets--and this list could be continued almost indefinitely--were so devised that they are apt subjects on which the mathematician can demonstrate his ingenuity and sense of formal beauty.[1]

His description of physics is that it is the systematic study of and search for "regularities" and "invariance" in the physical world. The regularities and invariances are summarized in the "laws of nature." He reminds the reader that the laws of nature have a conditional, conjectural quality about them.

> . . . the laws of nature are all conditional statements and they relate only to a very small part of our knowledge of the world . . . conditional statements cannot be entirely precise: that the conditional statements are probability laws which enable us only to place intelligent bets on future properties of the inanimate world, based on the knowledge of the present state.[2]

The balance of the paper is a series of examples of the unusually effective (and sometimes surprising)

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[1]Ibid., p. 3.

[2]Ibid., p. 6.

usefulness of mathematics in the laws of nature.

His paper thus is mostly an unnecessary defense of an idea which nearly everyone agrees: mathematics has many important applications to the real world.

> . . . The miracle of the appropriateness of the language of mathematics for the formulation of the laws of physics is a wonderful gift which we neither understand nor deserve. We should be grateful for it and hope that it will remain valid in future research and that it will extend, for better or for worse, to our pleasure even though perhaps also to our bafflement, to wide branches of learning.[1]

His "marriage" to mathematics because of its uncanny usefulness in physics is almost an act of faith. What properties about mathematics or what it is about mathematics which makes it always so potentially valuable to the laws of nature are never really discussed by Wigner.

Felix Browder attempts to answer the question of why mathematics is so effective in his paper, "Does Pure Mathematics Have a Relation to the Sciences."[2] He quite agrees with Wigner that mathematics has uses and relations to the sciences, but he is neither awed nor surprised by the fact. He characterizes the usefulness of mathematics to the other sciences thusly:

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[1]Ibid., p. 13.

[2]Felix Browder, "Does Pure Mathematics Have a Relation to the Sciences," American Scientist 64 (1976): 542-549.

1. The potential usefulness of a mathematical concept or technique in the advance of scientific understanding has very little to do with what one can foresee before the concept or technique appeared.

2. Such usefulness has very little to do with the pure or applied character of the motivation underlying the creation of the technique or concept, or with its degree of logical or formal abstraction.

3. Concepts or techniques are useful only if they can eventually be put in a form that is simple and relatively easy to use in a variety of contexts. Putting them in such a form is not always an obvious task in terms of their original appearance, and often requires innovative originality of a very high order.

4. We do not know what will be useful (or even essential) until it has been used. We cannot rely upon the concepts and techniques that have been applied in the past without ruling out the possibility of significant innovation.

Browder then produces a series of very impressive examples of the uses and applications of mathematics in the natural sciences. After which, he discusses some of the contemporary problems in the philosophy of mathematics; particularly the various foundational schools. His tentative conclusion regarding the reason why mathematics is so effective and useful follows:

> The answer I seem to be producing for the question listed earlier may seem perverse to some. Mathematics in its own distinct way is an objective science dealing with certain basic intuitive theorems of human experience and practice by a continually more ingenious apparatus of technical devices and simplifying concepts. Because of its origin and its nature, mathematics is not unreasonably effective in the physical sciences;

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[1]Ibid., p. 543.

> it is simply (though surprisingly) effective. With a certain amount of ingenuity and insight (amounting perhaps to genious), someone will probably even make it reasonably effective in the biological sciences.[1]

Abstraction, Generalization, and Formalization

The concepts of abstraction, generalization, and formalization are probably the major distinguishing characteristics of modern mathematics. Recall from Chapter II how historians often discussed point set topology as an especially good example of "modern" mathematics. Point set topology was the synedoche of modern mathematics because it was abstract, and it generalized and unified previous results, and in its abstract and generalized manner it could formalize and arrange much mathematics.

Repeating Bell's version of the future of mathematics (p. 119 above).

> It seems not altogether fantastic to imagine that a few centuries hence the qualitative habit of thought will have superceded the quantitative in the growing parts of mathematics. Certain indications in science, and many in mathematics, point to the analysis of structure as the mathematics of the future. Stated roughly, it is not things that matter, but the relations between them; and if topology, with its special visualizations of intricate relations between abstract 'objects' has made possible a rudimentary but still difficult analysis of relations, it may be the germ of mathematics of the future. In any event, topology is so unlike previous mathematics that it hints of a major transition.

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[1] Ibid., p. 548.

In this same book, Bell quotes a lecture of E. H. Moore given in 1906. Moore comments that we lay down a fundamental principle of generalization by abstraction:

> The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features . . .
>
> From the linear continuum with its infinite variety of functions and corresponding singularities G. Cantor developed his theory of classes of points (Punktmengenlehre) with the notions: limit-point derived class, closed class, perfect class, etc., and his theory of classes in general (allgemeine Mengenlehre) with the notions: cardinal number, ordinal number, order-type, etc. These theories of Cantor are permeating Modern Mathematics . . .
>
> We notice a more general theory dating from the year 1906. Recognizing the fundamental role played by the notion of limit-element (number, point, function curve, etc.) in various special doctrines M. Frechet has given, with extensive applications, an abstract generalization of a considerable part of Cantor's theory of classes of points and the theory of continuous functions on classes of points. Frechet considers a general class P of elements p with the notion limit defined for sequences of elements. The nature of the elements p is not specified (that is, the elements are completely general or abstract): the notion limit is not explicitly defined; it is postulated as defined subject to specified conditions. For particular applications explicit definitions satisfying the conditions are given.[1]

The process of abstraction in mathematics is the stripping away of the extraneous, nonessential details of a concept or object and revealing the common core, the central, or elemental concepts or structure found in

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[1] Bell, Development of Mathematics, pp. 502-503. It is unclear from the quotation whether the underscored words and passages are Bell's or Moore's emphasis.

seemingly diverse or dissimilar mathematical objects. Recall how Hausdorff (p. 85 above) commented that Riemann surfaces, spaces of curves, spaces of functions, metric spaces, spaces of points were all specific cases of the more general, abstract object--topological space. The axioms of point set topology revealed the core similarity of these different spaces.

Another example of the process of abstraction in the history of point set topology is the Heine-Borel Theorem. The basic concept or idea of the theorem was at the core of various theorems in topology and analysis so that it was eventually stated and proved as a separate theorem, rather than a lemma, of point set topology.

In another sense of the word, "abstract" objects of mathematics are abstract in the sense that they do not necessarily have any reference to the real, sensory world. As Moore said above, the "nature of the elements" is not specified--the elements are completely general or abstract.

Objects of mathematics can also be general. Their generality is that they can be applied to a variety of mathematical settings. As with the case of the Heine-Borel Theorem, it was in its generality, it was found in many different settings, that it then became regarded as important and central to analysis.

What began as a theorem about point sets on the line was then generalized to sets in higher dimensions and different spaces. T. H. Hildebrand, in his paper, "The Borel Theorem and Its Generalizations," listed several versions of the Heine-Borel Theorem in different settings, and then discusses the many applications (the generality of the theorem) in diverse parts of analysis.[1]

Another example of generalization in the history of point set topology is the case of the Jordan Curve Theorem. After being stated and proved for the plane, there were attempts to generalize the theorem to higher dimension Euclidean spaces.

Both concepts, generalization and abstraction, almost form working strategies for the contemporary mathematician. Mathematicians often investigate the degree to which their theorems, terms or definitions are sufficiently and usefully general or abstract. "Have I generalized this idea far enough?", "What are the possible generalizations of this theorem?" are questions a mathematician might ask of his work. Similarly, mathematicians, in an almost inductive manner, will survey various theorems or concepts, looking for hidden central ideas, in an attempt to abstract the core concepts or theorem found among them.

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[1]T. H. Hildebrandt, "The Borel Theorem and its Generalizations," Bulletin of the American Mathematical Society 27 (1926):423-474.

The third key feature after abstraction and generalization is formalization. Once the concepts, objects, or theorems of a subject have been abstracted and generalized, once they have been thoroughly understood and "distilled," the process of formalization of the concepts, objects and theorems can occur. The formalization of mathematics is the process of arranging the concepts, objects, and theorems in a tightly organized, meticulously arranged, and precisely and rigorously ordered presentation. This is what is sometimes called the "formal language" of mathematics. The "formal language" is a deductive and systematic arrangement of the axioms, terms, definitions, theorems and proofs which is rigorous and precise. It is rigorous and precise in the sense that there are no unstated or implicit assumptions, there are no vague concepts or terms, and that each new statement is some consequence or result of previous statements. This presentation of the results allows for the close scrutiny and checking of the results. The presentation thus provides a kind of guarantee (through the checking) of the results.

This highly abstract, general, and formal presentation of results in modern mathematics is one of its hallmarks. Indeed, it is a signal to the working mathematician that his concepts or theorems may not be

sufficiently abstract, rigorous, or precise when he cannot present his results in an efficient, formalized presentation.

Mathematical Activity

Mathematics is the product of mathematicians. A common stereotype of the working mathematician is a lonely recluse working in a chalk-dust covered office with a large blackboard containing indecipherable words, symbols, and formulas. The mathematician is usually preoccupied, absent-minded, and given to occasional outbursts when an insight flashes in his mind.

Like most stereotypes, they reveal more about the person who carries the stereotype than the actual object of the stereotype. Since stereotypes are, however, charactures of the genuine article, they usually contain a small grain of truth within them. In the case of the lonely, eccentric mathematician, the small grain of truth is that a mathematician does not need a laboratory or the real world in which to carry on research. It is also true that the objects of the research are very obscure, abstract, and difficult to explain to the laymen. Even within the profession of mathematics it is rare to find a large number of mathematicians working on sufficiently similar topics that they all can truly understand, and truly appreciate, the other's research.

Yet the stereotype has suggested a bias when it comes to explaining how the mathematician works, the nature of individual mathematical activity. The emphasis in explaining mathematical activity lies in mathematical insight. The mathematician pondering a problem or proof has an insight which solves the problem or completes the proof. The proof is written out in formal form, and then the mathematician turns to the next theorem and its proof. The "insight theory" of creativity and discovery is a strongly held view. It is found in books ranging from Jacques Hadamard's The Psychology of Invention in the Mathematical Field to Martin Gardner's Aha! Insight. The former is a psychological study of how mathematician's perceive relations, theorems and proofs (with the famous example of Henri Poincare boarding a bus and suddenly perceiving the solution to a problem),[1] and the latter is a collection of puzzles whose solution or answer is surprising or has a slight twist.[2]

The point is that this emphasis on insight obscures the fact that the working mathematician is in constant

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[1]Jacques Hadamard, The Psychology of Invention in the Mathematical Field (New York: Dover, 1945), p. 13.

[2]Martin Gardner, Aha! Insight (San Francisco: W. H. Freeman, 1978).

contact with other mathematicians through personal contacts, correspondences, or journals. The mathematician does not work in isolation. Before theorems or proofs are published in journals, they are refereed, but even before that the mathematician usually circulates his results to other appropriate mathematicians. He usually seeks their evaluation, criticism, and corrections of his ideas.

Yet the typical philosophical view of this activity is given, for example, by Quine:

> . . . the characteristic mathematical situation; the mathematician hits upon his proof by unregimented insight and good fortune, but afterwards other mathematicians can check his proof.[1]

Imre Lakato's Proofs and Refutations is a long supplement to this view of mathematics progressing by inspiration and insight and followed by formal proof. Lakatos' wants to explicate the middle ground between "unregimented insight" and "formal proof presentation." He believes that

> between the rationalism of a machine and the irrationalism of blind guessing [neither of which] hold for live mathematics: an investigation of informal mathematics will yield a rich situational

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[1]Williard V. O. Quine, Mathematical Logic, rev. ed. (Cambridge: Harvard University Press, 1951), p. 87. Quoted in Lakatos, see note below.

> logic which is neither mechanical nor irrational, but which cannot be recognized and still less, stimulated, by formalist philosophy.[1]

Proofs and Refutations is a mathematical variance of Karl Popper's Conjectures and Refutations. What Popper described for the philosophy of science, Lakatos is aiming for mathematics. His purpose is

> . . . to elaborate the point that informal, quasi-empirical, mathematics does not grow through a monotonous increase of the number of indubitably established theorems but through the incessant improvement of guesses by speculation and cricitism, by the logic of proofs and refutations.[2]

The style of Lakatos' book is to create a scenario of a group of students and a teacher discussing the Euler polyhedra formula ($V + F - E = 2$) and its proof. Their discussion about the various proofs, counterexamples, reformulation of terms and definitions, and the theorem itself is a long, wide-ranging (but thoroughly engaging) discussion of the philodophy of "informal" mathematics.

For example, the distinction is made between a "local counterexample" and a "global counterexample." As in:

> TEACHER: . . . let me introduce the following terminology. I shall call a 'local counter-example' an example which refutes a lemma

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[1]Imre Lakatos, Proofs and Refutations (Cambridge: Cambridge University Press, 1976), p. 4. Quine's quotation found in footnote 1 of same page.

[2]Ibid., p. 5.

> (without necessarily refuting the main conjecture), and I shall call a 'global counterexample' an example which refutes the main conjecture itself. Thus, your counterexample is local but not global. A local, but not global, counterexample is a criticism of the proof, but not of the conjecture.[1]

Lakatos shows that in the face of a "global counterexample" the mathematician will either reduce the domain of applicability of the theorem. (As in the statement, "For all x, with the exception of . . . , if . . . then . . . ") or will actually bar the particular counterexample--Lakatos called this "monster-barring." (As in the statement, "the following theorem is true with the exception of the following case . . . ") He points out that in the face of "local counterexamples" the mathematicians will search for faulty or hidden lemmas or assumptions.

This "informal mathematics" he calls "proof analysis." The proposed proof is subjected to questions, possible counterexamples, and general analysis. He believes that as with rigorous proofs there can be rigorous proof-analysis. And he sees all of this in an historical setting. Indeed, the footnotes contain references to the mathematicians and their work which pertains to the fictional discussion among the students and teacher.

> The dialogue form should reflect the dialectic of the story: it is meant to contain a sort of rationally reconstructed or 'distilled' history.

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[1] Ibid., pp. 10-11.

> The real history will chime in in the footnotes, most of which are to be taken, therefore, as an organic part of the essay.[1]

Lakatos sees an historical evolution of proof and proof-analysis, which he believes is crucial in the understanding of mathematical activity. Because much of his argument turns on these distinctions, I will quote it at length.

> In this section I have tried to show how the emergence of mathematical criticism has been the driving force in the search for the 'foundations' of mathematics.
>
> The distinction that we made between proof and proof-analysis and the corresponding distinction between the rigour of proof and the rigour of proof-analysis seems to be crucial. About 1800 the rigour of proof (crystal-clear thought experiment or construction) was contrasted with muddled argument and inductive generalization . . . It was also thought that one proves what one has set out to prove. It did not occur to anybody that the verbal articulation of thought-experiment involved any real difficulty . . .
>
> In the early nineteenth century the flood of counterexamples brought confusion. Since proofs were crystal-clear, refutations had to be miraculous freaks, to be completely segregated from the indubitable proofs. Cauchy's revolution of rigour rested on the heuristic innovation that the mathematician should not stop at the proof: he should go on and find out what he has proved by enumerating

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[1] Ibid., p. 5. Lakatos was a student of Popper's, which may account for his excessive use of italics. Lakatos' dies in 1974 unexpectedly of a brain tumor. His personal life was as colorful as his writings. Hungarian by birth, born Imre Lipschitz in 1922, he was a member of the anti-Nazi resistance, but eventually had to flee Hungary during the uprising in 1956. During the war he changed his name from the obviously Jewish Lipschitz to the safer Monar. After the war he was reunited with a set of his shirts monogrammed "I.L." Faced with the problem of shirts in short supply after the war, but now a communist, he changed his name again, this time to the more working-class Lakatos.

> the exceptions, or rather stating a safe domain where the proof is valid. But neither Cauchy--nor Abel--saw any connection between the two problems. It never occurred to them that if they discover an exception, they should have another look at the proof . . .
>
> The nineteenth-century union of logic and mathematics had two main sources: Non-Euclidean geometry and the Weierstrassian revolution of rigour. They brought about the integration of proof (thought-experiment) and refutations and started to develop proof-analysis, gradually introducing deductive patterns in the proof-thought experiment. What we called the 'method of proof and refutations' was their heuristic innovation: it united logic and mathematics for the first time. Weierstrassian rigor triumphed over its reactionary monster-barring and lemma-hiding opponents who used slogans like 'the dullness of rigor,' 'artificiality versus beauty,' etc. The rigor of proof-analysis superseded the rigor of proof: but most mathematicians put up with its pendantry only so long as it promised them complete certainty.[1]

In historical perspective, this is how Lakatos sees the activity of mathematicians in producing mathematics. One of the lasting values of Lakatos' work is his alternative to the commonly held view that mathematical insight is the major explanation of the development and growth of mathematics. As Peggy Marchi explains:

> . . . Mathematicians often like to pretend that there is something which differentiates great mathematicians from ordinary mortals. That thing is mathematical insight. I would certainly grant that some mathematicians are hugely gifted and perhaps even this gift is so great as to be different in kind from the intelligence which most of us possess. Be that as it may, ordinary mortals sometimes want to understand how a certain mathematician came to make a certain discovery. According to the insight theory, it is impossible

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[1]Ibid., pp. 55-56.

> to understand this, unless an ordinary mortal is satisfied with the explanation that a great mathematician had a good insight and thus was able to make that particular discovery. Lakatos' overview of mathematics as trial and error cannot explain the entire psychological process of discovery, but it can shed light on it, light which is not shed by the insight theory.[1]

Paradoxically Lakatos' ideas have remained essentially uninvestigated and unchallenged. Proofs and Refutations originally appeared in 1963-64 in parts in four successive issues of the British Journal of the Philosophy of Science, and the subsequent issues had no commentary on the articles. This is partly, I would imagine, because the ideas and writing were so fresh and original, but also because of the welter of details, asides, extra references, and diverse cases that makes Lakatos' work such a jumble. It is difficult to comment on a jumble. His strong points and brilliance show through, but it is very difficult to move from the large overview of his work to the small details of the picture.

At the risk of belaboring a point, the value of Lakatos' analysis is that he provides a critical framework

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[1]Marchi, "Mathematics as a Critical Enterprise," p. 386.

for the understanding of mathematical activity. Too often the products of mathematicians' activities are considered synonymous with the activity by philosophers like Black and Quine. Lakatos deserves credit for emphasizing the distinction and attempting to characterize both aspects as parts of the philosophy of mathematics.

Truth, Certainty, and Necessity

Another of the distinguishing features of mathematics is the sense that the truth and certainty of mathematics is one of its special virtues. As Lakatos' pointed out, the last bastion against the epistemological skeptics who believe there is no certainty or certain truth in knowledge is the bastion of mathematics. As Hilbert rhetorically asks (p. 228 above)

> And where else would reliability and truth by found if even mathematical thinking fails?

Considering the importance of truth to mathematics it is interesting how relatively few papers have been written on mathematical truth. Paul Benacerraf in the Journal of Philosophy considered the issue in his paper "Mathematical Truth."[1] Actually much of the paper is devoted to the

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[1]Paul Benacerraf, "Mathematical Truth," Journal of Philosophy 70 (1973):661-679. Hilary Putnam has also written, "What is Mathematical Truth," in his Mathematics, Matter and Method (Cambridge: Cambridge University Press, 1975). His style is so chaotic and sophomorically cute that I can neither make sense of his writing nor really wish to comment on the article.

distinctions between "mathematical truth" and "nonmathematical truths."

> . . . For, as I will suggest, accounts of truth that treat mathematical and nonmathematical discourse in relevantly similar ways do so at the cost of leaving it unintelligible how we can have any mathematical knowledge whatsoever; whereas those which attribute to mathematical propositions the kinds of truth conditions we can clearly know to obtain, do so at the expense of failing to connect these conditions with any analysis of the sentences which shows how the assigned conditions are conditions of their truth.[1]

After discussing various semantical theories of truth Benacerraf comes to two conditions for the concept of mathematical truth.

> The first component of such an over-all view is more directly concerned with the concept of truth. For present purposes we can state it as the requirement that there be an over-all theory of truth in terms of which it can be certified that the account of mathematical truth is indeed an account of mathematical truth . . .
>
> Another way of putting this first requirement is to demand that any theory of mathematical truth be in conformity with a general theory of truth--a theory of truth theories, if you like--which certifies that the property of sentences that the account calls "truth" is indeed truth . . .
>
> My second condition on an over-all view presupposes that we have mathematical knowledge and that such knowledge is no less knowledge for being mathematical. Since our knowledge is of truths, or can be so construed, an account of mathematical truth, to be acceptable, must be consistent with the possibility of having mathematical knowledge; the conditions of the truth of mathematical propositions cannot make it impossible for us to know that they are satisfied.[2]

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[1] Ibid., p. 662.

[2] Ibid., p. 666.

For Benacerraf mathematical truth is inextricably tied to epistemological considerations. We know mathematical truths because we know mathematics--and mathematics is only true mathematics.

He would like to agree with Godel's account of mathematical knowledge that Godel offers in his "What Is Cantor's Continuum Problem?": According to Godel,

> . . . the objects of transfinite set theory, . . . clearly do not belong to the physical world and even their indirection connection with physical experience is very loose.
>
> But despite their remoteness from sense experience, we do have something like a perception also of the objects of set theory, as is seen from the fact that the axioms force themselves upon us as being true. I don't see any reason why we should have less confidence in this kind of perception, i.e., in mathematical intuition, than in sense perception, which induces us to build up physical theories and to expect that future sense perceptions will agree with them and, moreover, to believe that a question not decidable now has meaning and may be decided in the future.[1]

But Benacerraf is troubled by this view.

> . . . What troubles me is that without an account of <u>how</u> the axioms "force themselves upon us as being true," the analogy with sense perception and physical science is without much content. For what is missing is <u>precisely</u> what my second principle demands: an account of the link between our cognitive faculties and the objects known.[2]

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[1] Godel, "What Is Cantor's Continuum Problem?" in Putnam and Benacerraf, p. 271.

[2] Benacerraf, p. 667.

Thus, for Benacerraf the problem of mathematical truth reduces to problems of mathematical knowledge and a general theory of truth. The two issues--mathematical knowledge and a general theory of truth--must be reconciled with each other.

One reason I have quoted Benacerraf at length is to reveal how little relevance such discussion has to the working mathematician. The conclusions of the technical, philosophical debates regarding mathematical truth, mathematical knowledge, and truth in general will have no effect on the working mathematician. Consequently, there are two senses of mathematical truth in the philosophy of mathematics. One sense is found in the work of Benacerraf and others. To a certain extent these philosophers are using mathematics as a model or an object of discussion to gain insights into more general epistemological and truth issues. The other sense is the notion of truth that is implied (or used, if at all) by the working mathematician.

For the working mathematician, if pressed as to why mathematics is true, he will usually refer to the basic, fundamental axioms of mathematics. These axioms are true either by definition (as in the case with Curry, p. 218 above) or by convention. Then the structure or

method of mathematical deductions and proofs "guarantees" the continuing truth of the system--as Lakatos calls them, the "truth channels." This is the method of "logical regression." There is, of course, the potential problem of an infinite logical regression? In other words, what determines when to stop the logical regression. How does one know for certain that the logical regression has arrived at the most basic, most fundamental and true objects, terms or axioms? Curiously, one response by mathematicians to the issue of certain and true foundations is that the mathematics built up from those foundations is true. Consider Herman Weyl's comment when discussing Hilbert programs of consistency:

> . . . No Hilbert will be able to assure us of consistency forever; we must be content if a simple axiomatic system of mathematics has met the test of our elaborate mathematical experiments so far.[1]

Or consider JohnVon Neumann's comment that

> After all, classical mathematics, even though one could never be again absolutely certain of its reliability . . . stood on at least as sound a foundation as, for example, the existence of the electron. Hence, if one is willing to accept the sciences, one might as well accept the classical system of mathematics.[2]

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[1]Herman Weyl, Philosophy of Mathematics and Natural Science (New York: Antheneum, 1963), p. 235. Originally published by Princeton University Press in 1949. My emphasis.

[2]John Von Neumann, "The Mathematician," in

Despite such reservations the working mathematician is usually content to believe that the axioms using the "primitive concepts" of (say) "set," "and," "or," etc., are so basic as to be true beyond question. Their very existence in our mathematical vocabulary establishes their truths.

It is my opinion that these beliefs held by mathematicians implies another belief held by mathematicians: it is the belief that there is only one mathematics. Mathematicians cannot image another mathematics. It may have developed differently, different areas of mathematics may be considered of different significance or importance, but the essential core of mathematics would be the same. Partly this belief stems from the belief in historical inevitability of certain mathematical results.

For example, the Heine-Borel Theorem is found in so many applications in analysis that it is difficult to imagine analysis not progressing without the Heine-Borel Theorem. If Borel had not stated and proved the theorem in 1895 it is believed that some other mathematician would have noticed its central importance, noticed it as being a separate theorem, and stated and proved it. It was an

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The Works of the Mind, R. B. Heywood, ed. (Chicago: University of Chicago Press, 1974), pp. 188-189.

historical inevitability that the Heine-Borel theorem was stated and proved in the late nineteenth century. It is the belief in some core, some essential mathematics, and the belief in the historical inevitability in the development of those core, essential ideas of mathematics that reflect upon each other in the mind of the working mathematician.

It is in a similar sense that the working mathematician believes in the necessity of mathematical results. It is because of the accepted "truth-channels" through which the truths (theorems) of mathematics "flow" that the results of mathematics are "necessary" results. Consider Pierce's definition that mathematics is the science of necessary conclusions, there, again, is a kind of inevitable, inescapable quality of mathematics, "the necessity of mathematical results," that is almost independent of the working mathematician.

From the perspective of mathematical platonism (see below), the truths and objects of mathematics reside in some timeless realm. The necessity of mathematics thus entails that the role of the mathematician is to discover these necessary truths and objects of mathematics.

Ontology and Epistemology in Mathematics

Probably the most difficult problem in the philosophy of mathematics is the status and explanation

of the ontology and epistemology of mathematics. Mathematics has a kind of hard, objective reality which will not change or conform to the whims of the mathematician. It is what it is, not what the frustrated mathematician would like it to be. Indeed, the comment that "good mathematics is discovered, bad mathematics is invented" points to the view that the mathematician cannot invent the mathematics he would like, rather he must discover the real mathematics that ultimately exists.

The philosophical schools do, to varying degrees, account for the ontology of mathematics. Platonism probably has the most articulated position regarding ontology. For example, Paul Bernays, in his article, "On Platonism in Mathematics," argues that

> . . . Hilbert states the axiom: Given any two points, there exists a straight line on which both are situated. "Exists" refers here to existence in the system of straight lines.
>
> This example shows already that the tendency of which we are speaking consists in viewing the objects as cut off from all links with the reflecting subject.
>
> Since this tendency asserted itself especially in the philosophy of Plato, allow me to call it "platonism."
>
> The value of the platonistically inspired mathematical conceptions is that they furnish models of abstract imagination. These stand out by their simplicity and logical strength. They form representations which extrapolate from certain regions of experience and institution.[1]

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[1]Paul Bernays, "On Platonism in Mathematics," in Putnam and Benacerraf, p. 275.

Thus, for the platonist mathematical objects are treated not only as if their existence is independent of the mathematician's cognitive operations, but also as if the facts about mathematical objects neither depended on the relation to the mathematician's mind, nor on their being verified or tested. Notice that for the platonist the ontology and epistemology of mathematics are thus very closely related. Recall Godel's comment above (p. 257) that he believed in some kind of human sense organ capable of perceiving the abstract concepts of mathematics. According to Hao Wang

> . . . Godel conjectures that some physical organ is necessary to make the handling of abstract impressions (as opposed to sense impressions) possible, because we have some weakness in the handling of abstract impressions which is remedied by viewing them in comparison with or on the occasion of sense impressions. Such a sense organ must be closely related to the neural center for language.[1]

Mark Steiner, in his Mathematical Knowledge, agrees to a certain extent with Godel's position.

> . . . I have tried to show only how a human faculty of acquiring mathematical knowledge might exist without running afoul of some standard objections. I conclude with some facts that point to the actual existence of such a faculty.
>
> First, there is the extraordinary agreement among mathematicians of all cultures on the basic truths of mathematics . . . Whatever explains this

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[1]Wang, From Mathematics to Philosophy, p. 85.

> agreement will very likely also show that the agreement is based upon knowledge. It may be that this agreement is brought about by the relationship of mathematical truth to the evidence of physical sensation; but the way I see mathematical intuition, as beginning with sensory data, that relationship need not dishearten the Platonist . . .
>
> Second, there is evidence that the great mathematicians have been able to convince themselves of the truth of various mathematical propositions without knowing their proof . . . The "mathematical," esthetic, quality of [for example, Ramanujan's] conjectures showed that more than mere guesswork was operating, as did his improbably high percentage of success . . .
>
> Finally, we should not ignore the utterances of mathematicians themselves. We may beware of introspection, but it would be foolish to ignore it. Mathematicians do claim intuition for themselves: they say that intuition grows with familiarity with the subject matter much as our sense perception improves through familiarity with a terrain.[1]

We contrast this platonist position with the constructivist position of the strict institutionists. For the institutionists the basal concepts of the world and mathematics determine their perception. Our knowledge of the world, according to the intuitionists, insures our knowledge of mathematics.

And we can contrast both of the above positions with Hilbert's formalist view. As mentioned above (p. 223) according to Hilbert.

> The subject matter of mathematics is, in accordance with this theory, the concrete symbols

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[1]Mark Steiner, Mathematical Knowledge (Ithaca: Cornell University Press, 1975), pp. 135-137.

> themselves whose structure is immediately clear and recognizable.

Recall, Hilbert believed that with actual mathematical statements reduced to empty formulas and finite mathematical reasoning immediately clear and evident, then mathematics would be free of all presuppositions.

All of these positions must be set against the implicit philosophy of the working mathematician. His position is a kind of "internalist" philosophy. The meaning, content, and understandings mathematical objects carry are only relative, internal, to the body of mathematics in which they are found. When asked about his research or its significance, the working mathematician will describe it in terms of some field, in terms of some problems in that field, in terms of previous attempts to solve the problem,etc. The ontology and epistemology of mathematics for the working mathematicians is almost all relative to the already established internal body of knowledge.

What the philosophers and mathematicians have in common is their understanding that mathematics is the study of abstract objects, their interrelations with other abstract objects, and the abstract problems and questions they generate. How one epistemologically accounts for the knowledge and understanding of abstract, non-physical

objects is essentially the manner of knowing and accounting for mathematics.

The Implications of History of Mathematics to the Philosophy of Mathematics

> Under the present dominance of formalism, one is tempted to paraphrase Kant: the history of mathematics, lacking the guidance of philosophy, has become blind, while the philosophy of mathematics, turning its back on the most intriguing phenomena in the history of mathematics, has become empty.
>
> --I. Lakatos

The last two sections of this dissertation are a discussion on how to avoid blind history or empty philosophy. I quite agree with Lakatos that a philosophy of mathematics can be empty without considerations of the history of mathematics (the subject of this section), and that the history and historiography of mathematics are enriched by considerations of the appropriate aspects of its philosophy (the subject of the next, and last, section).

It is easy to find examples of philosophical treatments of mathematics which indeed tend to be empty by their lack of considerations of the history of mathematics. Max Blacks' The Nature of Mathematics is almost devoid of any historical considerations of mathematics, and the results show.

For example, Black discusses Dedekind's construction of the real numbers. He compares it with Eudoxus'

construction of irrational numbers and the problem of incommensurables in Greek mathematics. He then suggests that Dedekind was resolving a philosophical problem of incommensurables.

> It is at this stage that Dedekind produces his abstract definition of pure number. Suppose the rational numbers are divided in any way into two classes, L, R, say, such that
> (i) all the members of L are less than all the numbers of R;
> (ii) L contains at least one rational number;
> (iii) all the rational numbers belong either to L or to R.[1]

And so on. Black later comments that

> It is interesting to see that Dedekind himself appears to have been well aware of the provisional nature of his definition.[2]

He then quotes Dedekind on the importance of the continuity of the straight line.

Black never mentions the actual mathematical problem that Dedekind was trying to solve; establishing a rigorous foundation for the calculus through a rigorous arithmetization of the real numbers. Dedekind's research was occurring in the late 1860's and early 1870's, at the same time Weierstrassian rigor was gaining acceptance by the mathematical community. Dedekind was aware of both Heine's and

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[1]Black, The Nature of Mathematics, pp. 94-95.

[2]Ibid., p. 95.

Cantor's construction of the reals, and before publishing his account made certain that his construction was not in conflict with the others. Besides, Dedekind's choice of examining the real number line was in response to the many arguments in the calculus then that depended on geometrical intuitions.

All three mathematicians were concerned with establishing a rigorous arithmetical foundation for some part of the existing mathematics at that time. Black's omission of this historical background leaves the uninformed reader with the impression that Dedekind's interests were primarily axiomatic or philosophical.

In contrast to Black's treatment of Dedekind, we can consider Kurt Godel's discussion of Platonism. Recall that the essay is entitled, "What is Cantor's Continuum Problem?" and that it begins with a discussion of the mathematical issues, in an historical light, in Cantor's work on set theory. After having provided a mathematical and historical setting of the problem, Godel then discusses the philosophical implications of these mathematical issues. The full significance of Godel's discussion is better appreciated through the inclusion of the mathematical and historical setting of the philosophical problem.

At the very least, then, philosophical considerations of mathematics must take into account the historical period of the mathematics being discussed. Almost all historical developments in mathematics began as attempts to solve problems--either of a physical world kind or of problems within mathematics. Before discussing the philosophical implications of significance of some part of mathematics, it is valuable to consider the problems that part of the mathematics was attempting to solve. It is also then important to know the previous attempts to solve the problem, the reasons for the failures and successes--in other words, to appreciate its history.

It is philosophically significant that point set topology was able to unify, generalize, and abstract many, seemingly diverse topics in mathematics. This aspect of point set topology in itself is characteristic of the nature of mathematics; a problem for the philosophy of mathematics. The historical roots of point set topology--the roots in geometry, trigonometric series, general analysis, foundations, etc.--greatly enhance this appreciating of the axioms of point set topology to unify, generalize, and abstract its historical roots.

It should also be remembered by the philosopher of mathematics that the history of mathematics is replete

with errors, false theorems, incorrect proofs, and concepts or ideas that do not stand the test of time. The history, the development, of mathematics is not a steady accumulation of eternal truths. Mathematics is much more organic, changing, in its history than the conventional view. According to Lakatos, mathematics is very conjectural. Before a theorem is proved it is a conjecture, but for Lakatos, it still remains, to a certain extent, a conjecture even after being proved. The proof undergoes proof-analysis, criticism, etc. Consequently, for the philosophy of mathematics, mathematics may not be so distinct--as the formal science--from the other natural sciences.

To put this another way, the history of mathematics is often a history of disagreements (sometimes severe disagreements) among mathematicians as to correctness, truth, applicability, or potential of mathematical results. Even accepting a platonic realm of idealized mathematics, the history of mathematics shows that mathematicians often disagree if and when a true valuable object from that idealized realm has been discovered. Philosophically this calls into question the notion of absolute, indubitable truths of mathematics. Or, put differently, if the philosopher believes there to be one mathematics only, then he will not find support from the history of mathematics as to agreement among mathematicians as to what one mathematics may be.

The chronological development of mathematics is usually nowhere near the logical, systematic and rigorous presentation of finished mathematics. It is only after a field in mathematics has a number of results, when it is beginning to "take form" that it becomes apparent as to what the basic, fundamental concepts of the field and its logical, appropriate organization may be. Consider the historical example of point set topology. Many of the important results and concepts were established before the enunciation of the axioms and fundamental definitions by Hausdorff. The Jordan Curve Theorem, the Heine-Borel Theorem, definitions of closed sets, limit points, metric spaces, etc., were all established before the existence of the axiom.

Hans Freudenthal catches this idea in the Preface of his Mathematics as an Educational Task.

> Like preludes, prefaces are usually composed last. Putting them in the front of the book is a feeble reflection of what, in the style of mathematics treatises and textbooks, I usually call the didactical inversion: to be fit to be print, the way to the result should be the inverse of the order in which it was found; in particular the key definitions, which were the finishing touch to the structure, are put at the front. For many years I have contrasted the didactical inversion with the thought-experiment. It is true that you should not communicate your mathematics to other people in the way it occurred to you, but rather as it could have occurred to you if you had known that what you know now, and as it would occur to

the student if his learning process is being guided.[1]

Thus, when discussing mathematics one must distinguish mathematics in its historical development from mathematics in its completed (usually modern) form. They are two very distinct forms. Too often the philosopher of mathematics does not make the distinction explicit, or worse, considers "finished" mathematics as the only kind of mathematics, as (say) in the case of the Bourbaki philosophical position.

The more important and more subtle lesson from the history of mathematics bearing on the philosophy of mathematics is that philosophical terms and concepts dealing with mathematics also have a history. As I mentioned above (p. 192), for example, the philosophical concept of standards of rigor has, in itself, a history in the history of mathematics. The standards of rigor of the Greeks, as Wilder points out, were different than the standards of rigor for Weierstrass, Frege, and Cantor. Or, if the philosopher wishes to discuss the ontology of mathematics, then historical considerations must be taken into account. Historically, it was not that long ago that negative integers were considered "fictious"

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[1]Hans Frendenthal, Mathematics as an Educational Task (Dordrecht: D. Reidel Publishing Company, 1973), p. v.

numbers, really not a part of the mathematical realm. Complex numbers were not accepted until a geometric representation gave a kind of credence to their usefulness and existence. Nineteenth century mathematicians often considered two kinds of infinity: potential infinity and the actual infinity. All of these examples point to the different ontological commitments of mathematicians depending on the concept and period.

If the philosophy of mathematics is to have any explanatory, critical, or evaluative power in mathematics or the history of mathematics, then it must consider the history of its own concepts within the history of mathematics. Indeed, historically, some philosophical positions pertaining to mathematics had considerable influence in the development of mathematics. Consider for example, that Bishop Berkeley's criticism of the calculus were not on mathematical grounds, rather, philosophically certain parts of the calculus were unacceptable. Or consider Kronecker's criticism of Cantor's transfinite set theory. His mathematics was not criticized, it was the philosophically untenable position, according to Kronecker, of the forms of the infinite.

It is only rather recently in mathematics history in which the philosophy of mathematics has separated

from mathematics and lost its critical, evaluative function. In other words, today's working mathematicians are not concerned with what professional philosophers of mathematics think of mathematics.

Richard Rorty expresses well this change in the influence of philosophy:

> In the nineteenth century, the notion of philosophy as a foundational discipline which "grounds" knowledge-claims was consolidated in the writings of the neo-Kantians. . . "Philosophy" became, for the intellectuals, a substitute for religion. It was the area of culture where one touched bottom, where one found the vocabulary and the convictions which permitted one to explain and justify one's activity as an intellectual, and thus to discover the significance of one's life.
>
> At the beginning of our century, this claim was reaffirmed by philosophers (notably Russell and Husserl) who were concerned to keep philosophy "rigorous" and "scientific." But there was a note of desperation in their voices, for by this time the triumph of the secular over the claims of religion was almost complete . . .
>
> . . . The result was that the more "scientific" and "rigorous" philosophy became the less it had to do with the rest of culture and the more absurd its traditional pretension seemed. The attempts of both analytic philosophers and phenomenologists to "ground" this and "criticize" that were shrugged off by those whose activities were purportedly being grounded or criticized.[1]

If the philosopher of mathematics does not wish to be "shrugged off" by those whose activities are being

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[1]Richard Rorty, Philosophy and the Mirror of Nature (Princeton: Princeton University Press, 1979), p. 5.

"grounded" or "criticized," then it is vitally important to consider the history of the mathematics at all times.

The Implications of the Philosophy of Mathematics to the History of Mathematics

Recalling Lakatos' view that "the history of mathematics, lacking the guidance of philosophy, has become blind" it is important to end this chapter with a discussion of the possible aids and implications the philosophy of mathematics may have to the history of mathematics. Among other implications, the philosophy of mathematics should provide historians with critical understanding and criticism techniques in their work. By this I mean that philosophy of mathematics should enable the historian to critically understand and distinguish the senses and implications of his own work. Failure to distinguish or inability to perceive crucial distinctions in historical writings are easy to find. I will quote only one--albeit extreme--example of confusion in interpretation of history.

Morris Kline's most recent book, Mathematics: The Loss of Certainty is an extreme example of a good historian essentially making a mess of his (implicit) philosophical understandings in the course of interpreting his history. Kline knows a great deal of good, accurate history, yet

his interpretations leave much to be desired. He begins his book by confounding several issues:

> This book treats the fundamental changes that man has been forced to make in his understanding of the nature and role of mathematics. We know today that mathematics does not possess the qualities that in the past earned for it universal respect and admiration. Mathematics was regarded as the acme of exact reasoning, a body of truths in itself, and the truth about the design of nature. How man came to the realization that these values are false and just what our present understanding is constitute the major themes.[1]

He is first saying that man has been "forced" to make "fundamental changes" in his "understanding of the nature and role of mathematics." Presumably he means that the more mathematicians learned and knew about mathematics the more they had to change their views on that subject. But there is nothing wrong with that: as a subject matter grows, it changes, in all sorts of ways, so the practitioners' view of the subject will change. His belief that mathematics no longer possesses "universal respect and admiration" is just not true. Most laypeople are in awe of the complexity and difficulty of mathematics. They often fear or dislike it, but that does not diminish their respect for it or mathematicians. Certainly scientists and mathematicians have not lost their respect and admiration for the subject.

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[1]Morris Kline, Mathematics: The Loss of Certainty (New York: Oxford University Press, 1980), p. 3.

That it has some unexpected developments, unsolved problems, and occasional paradoxes and mysteries generally does not lessen scientists or mathematicians view of the subject. And mathematics is still considered the "acme of exact reasoning, a body of truths in itself, and the truth about the design of nature."

The phrase "exact reasoning" is especially confused. Kline does not distinguish between the activity of mathematicians, the informal mathematics of conjectures, hunches and guesses, proposed proofs and corrections, etc., and the formal mathematics of published results. Both informal and formal mathematics have kinds of "exact reasoning." The informal mathematics is primarily heuristic, but within the heuristics there is a kind of exact reasoning. Mathematicians usually have a kind of plan of guesses, of tested or proposed proofs, of possible generalizations and abstractions in their research. By just comparing the way in which a beginning student and a mathematician solve a problem one can understand the existence of a (usually unstated) whole panoply of heuristic devices the mathematician has acquired over the course of his training and career. Some philosophers like Polya and Polyani believe that there is a rationality, a form of reasoning, involved in this heuristic process.

To make this process explicit appears to be very difficult, but that does not mean it is not possible.

"Exact reasoning" also has the sense of rigorous, formal proof. That proofs have been false or difficult to follow and thus uncertain does not mean that the formal proof process is inexact.

The confusion becomes complete when Kline claims that "these values are false." He has confused "understanding" with "values." A mathematician's understanding may be quite separate from his values regarding mathematics. Indeed this is one of the key contributions of the philosophy of mathematics to the history of mathematics. The philosophically-minded historian is careful to distinguish between the understanding of mathematics and the values held by mathematicians during the particular historical period under study.

On page 3 Kline equates "truth" with "certainty." These may or may not be equivalent in a particular historical period. When he says on page 4, that "several differing geometries fit spatial experience equally well. All could not be truths," he is again being vague as to his notion of truth.

It is easy to continue and point out the implied, but never cleared articulated, senses of his philosophical

vocabulary and how it is applied to his historical interpretations. After all the point of his book is historical interpretation. There is no new history in the book; most of his historical discussion has been lifted from his previous books. And while the history is good, the historical interpretation is terribly muddled because, among other things, his imprecise and uncritical use of philosophically loaded words.[1]

In marked contrast with Kline's confusion is Judith Grabiner's careful and thoughtful considerations of the implicit philosophical points in her history of Cauchy's rigorous calculus. As she comments in the introduction:

> . . . The calculus may seem to be about speeds and distances, but its logical basis lies in an entirely different subject--the algebra of inequalities. The relationship between the uses of the calculus and the justification of the calculus is anything but obvious.[2]

Grabiner is clearly aware of the distinction between use and justification; between the applications and the rigorous foundations.

In my opinion, it is her ability to distinguish the mathematical historical results from the philosophical

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[1]Ernest Nagel's review of Kline's book in the New York Review of Books (November 6, 1980) comes to similar conclusions as mine.

[2]Grabiner, p. 1.

implications of those results that gives her book such merit.

> . . . In a very important sense, it may be said that Cauchy brought ancient and modern mathematics together. He cast his rigorous calculus in the deductive mold characteristic of ancient geometry. And unlike his predecessors, he did this successfully; that is, he not only gave his work a Euclidean form but presented definitions that generally are adequate to support the desired results, proofs that basically are valid, and methods that were fruitful sources for later mathematical work. Cauchy, then, brought together three elements: the major results of analysis, most of which he could now prove; some fruitful concepts and techniques from algebra (particularly algebraic approximations) and analysis; and the rigor and proof structure of Greek geometry.[1]

Philosophy of mathematics can help the historian in both establishing a working historiography of mathematics and creating critical historical understandings and interpretations. The subject of the latter part of Chapter II was in establishing an historiography of mathematics, and this can only be aided by considerations of the philosophical implications of the historigraphical method chosen. Noretta Koertge sums up this point well:

> . . . historians must use theories, not only as a sieve to select facts, but also as the mortar which glues them together. The goal of objectivity is best served by welcoming theory in an open, explicit way, while at the same time keeping a critical eye on it.
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> Agassi suggested that historians emulate scientists by using bold theories. Lakatos suggested that they go even further and use idealizations--theories which deliberately neglected factors known to be operative. I think much writing in the history of science could be improved by a bit of bold

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[1] Ibid., p. 165.

> conjecturing and radical reconstructing--but only if it accomplained by the severe criticism and stringent testing which also goes on in the sciences.[1]

Not only in the writing of history, but a philosophically-minded historian is better equipped to evaluate the implications of that history. The only philosophical study I have found which evaluates the philosophical results in history of science is Maurice Finocchiaro's History of Science as Explanation.[2] Unfortunately the book is badly written, confusing, and difficult to follow. For example, Finocchiaro tries to distinguish among "history of science," "history-of-science explanation," and "historiography of science." As he says in a note at the end of the book:

> The term historiography of science will always refer to the historical study of the development of science, i.e., the discipline of the historian of science. Whereas the term history of science will sometimes refer to the development of science and sometimes to the discipline, the context will usually indicate which is meant. Moreover, the use or the omission of the definite article the just before the term history of science has been found to offer a natural way of distinguishing between the development and the discipline, respectively.[3]

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[1] Noretta Koertge, "Rational Reconstructions," in Essays in Memory of Imre Lakatos, p. 368.

[2] Maurice Finocchiaro, History of Science or Explanation (Detroit: Wayne State University Press, 1973).

[3] Ibid., p. 265.

his intention was to evaluate the philosophical implications of historical writings, but his style prevents me from making any useful comments.

The point is that appreciation of the senses of the terms of the philosophy of mathematics and the problems on the philosophy of mathematics can make for a much more sensitive, critically attuned historian. And as I have pointed out above, historical considerations almost always improve philosophical analysis of mathematics. The two subjects--history of mathematics and philosophy of mathematics--reflect on and improve each other.

To investigate and write the history of mathematics is a critical, selective, narrative enterprise. To be a critical, selective narrator implies beliefs or views about the nature of the subjects mathematics and history. Views about the nature of mathematics are ultimately grounded in some philosophy of mathematics and history. And this philosophy of mathematics stems from one's understanding of the subject matter--a large part of such an understanding comes from an understanding of its history.

The history of mathematics and the philosophy of mathematics are inseparable. Questions and problems in one necessarily suggest questions and problems in the other. This circle of questions and problems constitute the depth, complexity, and beauty of this particular branch of the human thought: mathematics.

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