DISTORTIONS AND DISCONTINUITIES OF MATHEMATICAL PROGRESS: A MATTER OF STYLE, A MATTER OF LUCK, A MATTER OF TIME, ... A MATTER OF FACT

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Introduction

A close and careful study of the history of mathematics provides evidence that mathematical progress is not necessarily linear or continuous; nor is it always evolving in precise logical steps, according to the rules of logic. Indeed, logic itself has evolved significantly since the days when Kant pronounced it to be a completed science, finished and in its final form with the presentation of Aristotle's *Organon* (see Kant, K.d.r.V., B viii).

In the last years of the nineteenth century and the early years of our century, philosophers of mathematics, many of them trained in mathematics or the physical sciences, sought to understand mathematics in a Kantian sense, that is, as a completed body of knowledge, one which did not, however, develop dynamically in time, but which, rather, developed statically, in logical space, from Euclidean-like axioms, into Euclidean-like theorems according to the rules of logical inference. By 'Euclidean-like' here, I do not intend to suggest that these late nineteenth and early twentieth-century philosophers, among them such crucial figures as Hilbert, Frege, Peano, or Russell, were unaware of the existence or importance of non-Euclidean geometries (for indeed, Russell's 1897 *Essay on the Foundations of Geometry* was devoted to an elaboration of the epistemological foundation of non-Euclidean geometries, with special emphasis on projective geometry; and Hilbert's successive versions of his *Grundlagen der Geometrie* were meant to provide a system of axioms sufficiently general to serve as the inferential basis for all non-Euclidean geometries as well as for Euclidean geometry); rather, these thinkers ignored the nineteenth century developments of non-Euclidean geometries, of noncommutative and nonassociative algebras in the *historical* sense, and treated them as logical deductions from a core of axioms which presented a complete and closed formal system. In short, the formalists and logicists treated mathematics ahistorically.

Our philosophers were writing in response to several "crises". The development of hyperbolic and elliptic geometries, of noncommutative and nonassociative algebras, and of nonlinear or non-Aristotelian (i.e. multiple-valued) logics challenged mathematicians to redefine their conceptions of mathematical *proof*, *validity*, *logical rule*, *deduction*, *formal system* - and this led to a renaissance of logic which began with Boole in the 1850s and continued for half a century, through the work of Peano, Frege, C.S. Peirce, Hilbert, and Russell (to name only the more prominent figures); it led to the creation of *mathematical* logic. While logicians engaged in metamathematical studies in which such logical concepts as *proof* were defined and developed mathematically, and while they built logical systems which would be sufficiently general yet sufficiently powerful to recreate all of ordinary mathematics, their philosophically-minded colleagues created the foundational philosophies of mathematics - Formalism, Logicism, Intuitionism/Constructivism - which would provide the ahistorical epistemological anchors for the mathematico-logical and mathematical enterprises. But almost at the very moment when these mathematical edifices and their philosophical anchors were completed, a new "crisis" arose in the form of Gödel's incompleteness theorems, according to which such systems as Whitehead and Russell's *Principia Mathematica* could be shown to be complete if and only if they were inconsistent; worse still, these systems could not themselves be used to prove their own completeness, despite their claims to universality ("completeness" in the Fregean sense, to encompass all of mathematics deducible from the selected axioms, including any systematic proofs of the completeness and consistency of the system). It then became the business of philosophers to try either to rescue mathematics or to admit that human knowledge - including in particular mathematical knowledge - was itself incomplete, and perhaps even essentially incompletable. It is significant that, faced with Gödel's results, philosophers for the most part felt panicked; and among these were some who took Heisenberg's Indeterminacy Principle, Gödel's Incompleteness Theorems, and Carnap's infinite meta-linguistic regress to be variants or applications of a single, central epistemological Fact. With a few notable exceptions, philosophy of mathematics had not gone beyond this "crisis" for nearly four decades following the initial shock of Gödel's announcement in 1931. It is equally significant, however, that (as John Dawson [1985] has shown) mathematicians have not felt in the least exercised or hampered by Gödel's results, but continued to till their mathematical gardens, then and now, and to make great progress. In fact (as Saunders MacLane [1986] has noted), it was precisely around 1930 that mathematical logic became a separate field, and in the years since, especially after the end of World War II, gradually became a highly specialized branch of mathematics, with distinct sub-branches. We must ask '*Why ?*' Could it be that mathematics does not proceed the way that the foundationalist philosophers of mathematics have said that it must and does? Could it be that mathematics is not simply *logic*, that its methodology is not purely and simply *formal* and deductive? Indeed, when we talk today of "logic", we are really talking, as MacLane has said, about several distinct and highly developed technical branches of mathematics: and many of these branches have grown out of technical responses to the philosophical "crises" of mathematics - for example, proof theory from Hilbert's metamathematics, or recursion theory from Gödel's studies of incompleteness.

Increasing numbers of philosophers of mathematics have begun asking these questions in the past decade, taking their cues from forerunners such as Imre Lakatos, who noted that "whenever the mathematical dogmatism of the day got into a 'crisis', a new version once again provided genuine rigour and ultimate foundations, thereby restoring the image of authoritative, infallible, irrefutable mathematics..." (p. 5 of Lakatos [1976]). These same kinds of questions were raised at the turn of the century by Poincaré, and answered in a similar vein. Writing during the years when mathematical logic was attaining its Fregean-Hilbertian adolescence and with youthful exuberance laying its claim to be the foundation for mathematics, Poincaré asked whether mathematical reasoning was syllogistic or deductive. Today we would not argue that there is a dichotomy between syllogistics and deductive reasoning, of course; we should rather assert that syllogistic is an instance of deductive reasoning, just as we would assert that propositional calculus is just a fragment of the first-order functional calculus known as monadic predicate logic. Still, if we accept Poincaré's distinction, we notice that he is merely asking one form of the Lakatosian question. In *Science and Hypothesis*, Poincaré [1903] worried that, if mathematical reasoning were deductive, then mathematics would be reduced to a vast but empty tautology, while, if it were syllogistic, then it must be asked how anything new could ever be added to the body of mathematical knowledge, what he calls "data". Poincaré's solution is to assert that there is unconscious hypothesizing at work behind the formulation of mathematical theorems, just as there is unconscious hypothesizing behind the formulation of new scientific theories. Mathematical "proof" is in essence *verification* by mathematical reasoning, and this reasoning is in fact syllogistic; mathematical reasoning is a very long and repetitive syllogistic process, and the syllogism applied is a hypothetical syllogism. This places Poincaré as the true precursor of Popper's hypothetico-deductivism and Lakatos' empirico-historicism, based on Popper's hypothetico-deductivism and falsifiabilism. Kitcher's [1984] is devoted to a full exposition of the epistemology of Popperian hypothetico-deductivism for mathematics, and details the logical apparatus for this methodology. Charushnikov [1987] and Van Bendegem [1987], however, have (correctly, I think), argued that Lakatos has gone too far in the direction of identifying mathematics with the empirical sciences, and mathematical methodology with the experimental methodology of the empirical sciences. Van Bendegem [1987] in particular has used the example of Fermat's last theorem, as a problem which has long resisted solution, as evidence that falsifiabilism has no role in mathematics. And while he has resisted generalizing, we can point to similar situations, for example, to Hilbert's tenth problem, for which Iurii Matiiasevich has shown that the solution is that it is undecidable that there is an algorithm for deciding if every Diophantine equation has a solution in the integers. In general, if Gödel's incompleteness theorems have any proper philosophical relevance, it must be that there are some mathematical problems that can be shown to be undecidable, and that, as a consequence, falsifiabilism cannot be a proper test for a mathematical theorem as it is for a scientific theory.

The notion that philosophers discard an old mathematical dogmatism and replace it with a new one when a "crisis" undermines the old one, suggests that philosophies of mathematics have histories, and that the history of philosophy of mathematics is tied to the history of mathematics. If philosophy of mathematics changes paradigms –to use the Kuhnian terminology – when faced with a "crisis", then we need to explain this concept of crisis. If the foundationalist philosopher of mathematics is correct, and if in particular the formalist who holds that mathematical progress consists of the (history of) deductions from axioms to theorems in an ahistorical, Euclidean-like manner, then we may wish to think of a crisis as a situation in which a single set of axioms has led to one or more inconsistent theorems. In this case, the three "crises" of foundational philosophy of mathematics about which Snapper [1979] spoke are traceable back to the crises of mathematics in our sense, that the historical development of mathematics points towards the need to reconceptualize the nature of mathematics, to broaden the scope of our conception of mathematics and to permit wider latitude in our understanding of certain basic mathematical concepts. In this sense, the history of mathematics is the history of learning and struggling to accept as intuitively true certain ideas which had heretofore been regarded as unintuitive, for example the gradual acceptance of the legitimacy, both theoretically and practically, of irrational numbers, which classical Greeks regarded with suspicion, and which, while used (in the guise of incommensurable magnitudes), albeit with reluctance, in computations, were abolished from classical number theory.

As another example of the crisis, we now know that Euclid's parallel postulate is independent - that if we drop it as a postulate of Euclid's system, we can obtain consistent systems for which the parallel postulate fails: in short, given Euclidean axioms without the parallel postulate, we can derive a geometry in which either (a) parallel lines l, m do not intersect and for any line p perpendicular to l, p is perpendicular to m (Euclidean, parabolic, geometry); or (b) for a line l and a point P not on l, at least two distinct lines parallel to l pass through P (hyperbolic geometry); or (c) no parallel lines exist (elliptical geometry) - and all three of these geometries are consistent. Crisis! And a means is sought to resolve or dissolve the crisis. But note that the enforced need to widen our conception of geometry to include non-Euclidean geometries immediately raised as well a foundational question of the scope of the logical or foundational concept of *consistency*: this is how our conception of crisis encompasses both Lakatos' historical sense of crisis and Snapper's foundational sense.

If, however, mathematics lurches from crisis to crisis, resolution to dissolution, then it must be equally clear that mathematics has a history, as much as does philosophy of mathematics. The concept of crisis in mathematics shows that mathematics changes, that in fact it does not proceed exclusively by formal deductions in a logical, ahistorical evolution. Behind the crises of mathematics are false starts, dead ends, crooked paths, guesses. And in fact, the study of the history of mathematics shows that mathematical progress could be defined in terms, as I have just suggested, of the gradual acceptance of formerly nonintuitive or nonintuitively true propositions as, finally, intuitively true. In this case, mathematical progress need not necessarily be linear or continuous. On the contrary, the history of mathematics offers examples, familiar and unfamiliar, of events in the growth of mathematical knowledge that betray the distortions and discontinuities of mathematical progress. Historians of mathematics have been more aware of this fact far longer than have philosophers of mathematics. But the traditional historians of mathematics have nevertheless also seen mathematical progress as essentially linear. This no doubt was due to the fact that, in the past, historians of mathematics have, professionally, tended to be retired mathematicians. It is only recently that history of mathematics has come into its own as an independent profession. With that professionalization has come a plea for a more accurate history of mathematics, one that seeks to understand the mathematics of the past as those bygone mathematicians themselves saw mathematics in their own times (see, e.g., Kitcher & Aspray [1988], especially pp. 24-25). This is in response to an essentially historiographical problem. The historian of mathematics Karen Hunger Parshall (especially p. 129 of her [1988]) has explained the situation thusly:

> Traditionally, historians of mathematics have most often adopted a presentistic approach to their subject. From the vantage point of the state of the discipline in their own times, they have tended to portray the development of mathematics as fundamentally linear in nature. In other words, looking back into mathematical history, they have picked and chosen from among the various contributions and constructed a logical, straight line progression from the past to the present. This kind of history serves to anchor contemporary mathematics in the past by providing it with a clear sense of direction, but at the same time it profoundly distorts the view of the mathematical climate at any given time in history. In the search for predecessors of a particular type of equation, theorem, or idea, other concepts which may have been of prime importance to the authors under scrutiny tend to be ignored or trivialized. Furthermore, competing approaches and underlying philosophies often fall into total obscurity. ...

As a corrective, Parshall suggests that historians of mathematics take as their goal a reconstruction of the dynamics of mathematics at some particular time, with the aim of examining the understanding, and accounting for the failures as well as the successes, of the mathematics of the past, in terms of the process which she calls "the natural selection of ideas" of mathematics, its history, and philosophy.

It is my contention that mathematical progress frequently depends upon the vagaries of style, of luck, of time, and of fact. (This does not, however, mean that I reject altogether the role of logic and set theory in the epistemology of mathematics; on the contrary, our age of mathematical "rigor" requires that, once a new piece of mathematical knowledge, by whatever means, has been discovered, created, or otherwise acquired, it must then be presented as a theorem, drawn through a formal logical deduction from already accepted mathematical knowledge, before it can itself be accepted as true, and thereby validated as indeed *proven*. This is in contrast with Steiner's [1975] study, which largely consists of a repudiation of the formal or deductive character of mathematics. Moreover, I have argued (Anellis [1981]) that developmental psychology patterns the same epistemological processes found in intuitionistic mathematics, and that this pattern is provided by an extensional approach, based on intuitionistic set theory.)

Thanks to writers such as E.T. Bell [1937], whose historical accuracy however is unfortunately sometimes sacrificed for the sake of a "good story", the reader already knows well of the bad fortune of Abel and Galois, whose works Cauchy had managed to misplace. More recently, J.-P. Van Bendegem [1988] has provided an example of the resistance of mathematicians to a recent proof by Roger Apéry, using the "old fashioned" methods of Euler, of the irrationality of solutions for the Riemann zeta function Z(3).

I shall present a number of not very well known examples to illustrate my claims that mathematical progress – or regress – can be a matter of (1) *Style* (Bonasoni's "unpropitious" attempts, against the mathematical practices and currents of his day, circa 1575, to geometricize algebra); (2) *Luck* (Jean van Heijenoort's failure to publish important results in model-theoretic proof theory, and his reasons); (3) *Time* (how World War II interrupted John V. Atanasoff's opportunity to obtain a patent for his world's first electronic digital computer); and *Fact* (Bertrand Russell's deliberate distortions of the history of modern logic – with his reasons, and the consequences). These four factors are sometimes connected.

Using our examples, we shall explore and analyze these factors and examine the import which they have for mathematical progress and for our knowledge of the history of mathematics. If I am right, these examples provide evidence for the contention, formulated by Lakatos and his followers, and borrowed from Popper out of the broader context of the philosophy of science, that scientific knowledge generally, and mathematical knowledge in particular, is rooted in, and defined by, its history. In this case, if we wish to understand philosophy of mathematics as the theory of the formal development of mathematical knowledge, then history of mathematics provides, and must be understood as, the nontrivial content of philosophy of mathematics, and philosophy of mathematics must become the theory of the historical development of mathematics as an evolving body of formal knowledge. If so, then the question of whether Kuhnian revolutions, marked by wholesale paradigm shifts that radically reformulate and reorient scientific thinking, becomes a live question for philosophy of mathematics as well (see, e.g., [Fang, 1973]). Some of the examples which will be considered may suggest that Kuhnian revolutions indeed pertain to the history of mathematics. Whether or not it does depends, as Mehrtens [1976] has suggested, upon how one finally defines 'crisis' and 'revolution'. Glas [1987] does not accept the Kuhnian theory, and uses the example of the "analytic" approach to mathematics, presented by Lagrange and Laplace, with mathematics understood as a language or formal system, in conflict with the older intuitive approach, defended by Monge, which regarded mathematics as the study of the movements of spatial configurations. However, it could be as easily argued that the theoretical conflict between the "analysts" Lagrange and Laplace and the "intuitionist" Monge represents merely the first step in the evolution towards the Bourbakian formal deductive axiomatic style of today. Mehrtens, along with Crowe [1975], rejects the Kuhnian theory of revolutions in mathematical history; but Mehrtens accepts the claim of broader sociological influences on the choices made by the mathematical community. The basis for the rejection of Kuhnian revolutionism is precisely that old mathematics is never altogether discarded; rather, any qualitative jump in mathematical knowledge or mathematical methodology tends to center on the absorption of older results into newer results, leading to an increasing generalization of mathematics that sees older and newer results as specific cases of a more general theory. A good example would be Klein's Erlanger Program, which brought the old Euclidean geometry together with the non-Euclidean geometries and with algebra, by interpreting geometries in terms of transformation groups. It is in fact a truism that the history of mathematics has developed - our examples should make that plain enough to anyone who does not already think so. It is also a truism that the history of mathematics provides the nontrivial content for a philosophy of science which understands itself to be the theory of the historical development of mathematics.

These are truisms which Marxist philosophers of mathematics have known from the outset, beginning with Marx's *Mathematical Manuscripts* and Engels' *Dialectics of Nature*. It is a truism which was first formulated, in its non-Marxist terms, by Lakatos, who, unlike Marx or Engels, was specifically trained as a scientist *and* as a mathematician. And, although we must look to the influence upon Lakatos of Popper for the former's historically-oriented study of the nature and development of the concepts of *mathematical proof* and *mathematical rigor*, we must look to the influence of S.A. Ianovskaia for the Lakatosian conception of a historical philosophy of mathematics; for Ianovskaia, with whom Lakatos briefly studied at Moscow State University (probably in the immediate post-war years), was the leading Soviet historian and philosopher of mathematics of her day and the editor of the Russian edition of Marx's *Mathematical Manuscripts*. (For a brief professional biography of Lakatos, see Hersh [1978], which does not, however, give any indication of Lakatos's ties to Ianovskaia; for a highly critical appraisal of Marx, and especially of Engels, as mathematicians, see van Heijenoort [1986]; van Heijenoort's criticisms of Engels in particular should be taken carefully, however, with the understanding that Engels after all had no real mathematical training. For a discussion of the work of Ianovskaia, see Anellis [1987] and [1987a]). This view of mathematics has thoroughly permeated contemporary Soviet philosophy of mathematics, as well as history of mathematics, and not only in terms of dialectics (for example, as seen in the dialectical historico-philosophical writings of A.D. Aleksandrov of the older generation, but also in the Lakatosian terms, among such leading writers, e.g., as Aleksei Georgeevich Barabashev, who, too young to have been a student of Ianovskaia, was profoundly influenced by her teaching, through her students, such as A.P. Iushkevich, S.S. Demidov, and F.A. Medvedev.

Let us, then, turn to our illustrations.

A Matter of Style

The choice of mathematical style is to a large extent philosophical. Clearly, how one does one's mathematics will depend upon one's conception of the nature of mathematics, of its uses as well as its intellectual role within the larger epistemological structure of the architectonic of knowledge. The commercial and practical applications of ancient mathematics among the Egyptians and Babylonians led to a concrete conception of mathematics as a computational and mensurational tool. Thus, if we examine the kinds of mathematical problems and the methods of their solutions undertaken by the pre-Greek calculators, as Aaboe [1964], for example, has done, we find very specific problems, for which very specific rules of computational manipulations are given. This same concrete style of mathematicizing, rooted in practical usage, is repeated in the highly commercialized era of the late middle ages and early renaissance of the XIII–XVIth centuries, when abacus books, frequently designed as handbooks of commercial mathematics for merchants, dealt with concrete practical arithmetical problems in fairly specific, concrete ways (see, for example, the *Treviso Arithmetic* of 1478 and the accompanying socio-historical essays by Swetz [1987]). (A particularly potent example of this kind of phenomenon occurred in Muscovy during the XIV–XVIIth centuries, during which the practical knowledge of mathematical methods of arithmetic and geometry was roughly at a level equivalent to that found in ancient Egypt in the time of Pythagoras and Herodotus, while, with a few notable exceptions, the Muscovites regarded theoretical knowledge of mathematics, if at all, with fear at best, with hostility at worst, concerned that "number wisdom" was a direct danger and devilish challenge to "divine wisdom"; see [Anellis 1988–89].) For the more theoretically-minded ancient Greeks, who, however, learned much of their mathematics from the Babylonians and Egyptians, mathematics took on a formal style, epitomized in the axiomatic system of Euclid's *Elements*. As is well known, this style is characterized by the abstraction and generalization that typified Hellenic Greek disdain for the concrete and the practical, and its equal glorification of the theoretical that marked the intellectual search for "First Principles" and took mathematics as the exemplar as well as the methodology (*mathesis*) of theoretical scientific knowledge.

The matter of style is not, however, dependent exclusively upon philosophical considerations. As the cases of the pre-Greek accountants and rope-stretchers and the renaissance abacists show, there are historical and sociological factors to consider. The sociological milieu in which mathematics is done must impact the style in which that mathematics is done; a commercial, practical society gives rise to financial arithmetic and a practical, concrete style of mathematics, while a society which stresses the theoretical gives rise to an abstract, formal style. There are also more specific historical considerations which impact upon mathematical style; as the body of mathematical knowledge increases, mathematical styles become increasingly formal, abstract, general, in order to accommodate new mathematics and to tie new fields together, developing their interconnections, in order to explore and unify these results. In a paper on the variations of mathematical style, the well known mathematician of the Bourbaki school Claude Chevally [1935] presented historical examples from the nineteenth century history of real analysis, particularly with regard to the rigorous definition by Weierstrass and his immediate predecessors, in terms of epsilon, of the limit concept developed by Cauchy, and of the set-theoretical concerns of Dedekind and Cantor, as successors of Weierstrass, to formalize the real number system upon which the epsilon definition of *limit* depends. Another result, as is well known, is the development of abstract algebra, where a concern was the formal study of the real field as an abstract system. "The axiomatization of this theory," Chavally wrote [1935; q.v. Chevally 1985, p. 7], "profoundly modified the style of contemporary mathematical writing." The style, formal, abstract, deductive, typified by Bourbaki (of which Chevally was a member), is a long way from the informal, fluid style of Euler. We would not today regard Euler's proofs as proofs in our, Bourbakian, sense, as formal deductions carried out axiomatically according to the rules of mathematical logic - or at least of being capable, if one had the patience, of being written out as formal deductive proofs! J.-P. Van Bendegem [1988], for example, has shown that contemporary mathematicians, having been raised in the spirit and style of Bourbaki, have exhibited intense resistance to a recent proof by Roger Apéry, using the "old fashioned" methods of Euler, of the irrationality of solutions for the Riemann zeta function Z(3). The point being made is that styles of mathematics and corresponding conceptions of the nature and proper structure of proofs vary with the age and with mathematical sophistication. Van Bendegem has shown that the resistance of mathematicians to Apéry's Eulerian proof is based on a suspicion of the relatively unsophisticated methods of Euler for solving a profound mathematical problem that had not yielded to sophisticated contemporary methods. While Wilder's [1953; 1968] anthropological and sociological analyses of mathematical progress help to point up some of these broader sociological questions, Crowe's study [1975] propounds laws to account for the type of example which Van Bendegem [1988] has dealt with. In particular, Crowe can be understood to point out that some new results are unwelcome because they run counter to the mathematically accepted standards of the time.

The case of Bonasoni and his *Algebra Geometrica* shows not only that the mathematical style one adopts affects not only the attitude which one's contemporaries may take, but also can impact, negatively, the course of the development of mathematics.

Little is known about Paolo Bonasoni, other than that he served as a professor at the University of Bologna during the late sixteenth century, and that he was the author of the treatise *Algebra Geometrica*, written some time around 1575, possibly as late as 1585. This work, unpublished in the author's lifetime, was unknown until 1924, when it was discovered in the

University of Bologna archives by the historian of mathematics Ettore Bortolotti, a specialist in Renaissance and seventeenth-century Italian mathematics. He announced his discovery of the Bonasoni manuscript that year [Bortolotti 1924/1925], and gave a brief description of the manuscript in all of its aspects, its physical condition, the situation of mathematics, especially of algebra, in Renaissance Italy, and provided an outline of Bonasoni's thesis, methodology, and results, including textual excerpts, in his article. However, the work of Bonasoni continued to be largely ignored. One understandably finds nothing in Michel Chasles' [1837; 1875] classic history of geometry. Very regretably, nothing will be found about Bonasoni and his work in the modern classic by Coolidge [1940], which appeared after Bortolotti's announcement. A single, but very telling, very important, paragraph is devoted to Bonasoni in Boyer's [1956] scholarly, even pedantic, history of analytic geometry, while Jacob Klein [1968], in an obscure footnote, discussing the role of Vieta's contributions to the founding of analytic geometry, mentions Bonasoni, along with the much better known Rafael Bombelli, as a precursor of Vieta. Only recently was the full text of the Bonasoni manuscript published [Bonasoni 1985] and given serious consideration (see [Anellis 1987c]).

It is easy to understand why mathematical scholarship is willing today to return to forgotten texts; Parshall [1988], for example, has shown that our understanding of the history of mathematics is enhanced when we seek to understand the mathematics of a bygone time as those who worked at that time understood it. The social historian Johan Huizinga, a specialist on late medieval history of the Low Countries, once spoke of "bowing to the spirit of the age" that one studies. Historians are more willing today, perhaps because of the relativism of our age, to explore texts which had once been discarded or ignored because they did not suit the temper of their own times. We need here to examine why Bonasoni's work was ignored by his colleagues and successors. The fact is that it was a matter of style.

Reasons for the obscurity and neglect of Bonasoni may be detected by an examination of the published text and a comparison of Bonasoni's goals and mathematical style with those of his contemporaries. To state the point simply, Bonasoni's work ran counter to the directions pursued by the majority of his contemporaries. We may even conclude that Bonasoni's work, which was, after all, under some of the same influences as affected the work of his more successful colleagues such as Bombelli and Vieta, was nevertheless not a true precursor to the work of Vieta. In order to understand this, we have to examine trends in

Italian mathematics of the sixteenth century.

Historians of mathematics traditionally distinguished two major styles of mathematics, the *synthetic* (or geometric) and the *analytic* (which today we would call the algebraic) styles. Both styles can be traced to classical times. Analysis is characterized by the assumption of the truth of the intended conclusion, from which are derived the conditions necessary and sufficient for the truth of that hypothesis. Synthesis, which the historical tradition associates with the axiomatic method of Euclid, is characterized by the assumption of the truth of the necessary and sufficient conditions of the conclusion, from which conditions the conclusion is then logically derived. Both of these styles represent theoretical approaches to mathematics. For the Renaissance Italian mathematicians, a third approach became dominant. It was a non- theoretical approach, one which was oriented specifically towards performance. This *tour de force* style of mathematics, developed by sixteenth century Italian mathematicians, made successful problem solving the ultimate goal of mathematics. During the Renaissance, Italian mathematicians earned their reputations and built their careers on these *tours de force*. They were expected to get results as quickly and "cheaply" as possible. This style probably owes much to the practical nature of the mathematics of the time, and can be traced to the commercialized, practical nature of Renaissance society (see, e.g. [Swetz, 1987]). The style, with its concern for quick, accurate solutions rather than for procedural finesse, resulted in a "hit-or-miss method" of obtaining results. In the search for a more efficient, less haphazard method, one which could also be generalized to a wide assortment of problems, Viete sought to formalize the *tour de force*, or power-problem-solving, method. Thus, the search for a quick and easy method for mathematical problem solving led Renaissance Italian mathematicians to the concept of a *mathesis universalis* (see [Crapulli 1969]).

Bonasoni shared with his colleagues this goal of developing a "*mathesis universa*". Unlike them, however, he chose a "geometrical algebra" as his mathesis, while his colleagues chose numerical algebra. The *algebra geometrica* is offered in the "Praefatio" of Bonasoni's text as an alternative to the "*algebra numeralis*" being developed and refined by Bombelli, Buteo, Nunez, and Viete in these years. In his *Algebra*, Bonasoni gives the geometrical proofs and solutions to three problems which are the geometrical equivalents of the solutions of the three canonical forms of the quadratic equation in numerical algebra. Moreover, his method allows him to give geometrical solutions to problems which others had been able to solve algebraically but for which they were unable to provide geometric constructions. Bonasoni's methods thus permit geometrical treatment of algebraic problems; but more, Bonasoni uses algebraic reasoning in carrying out his geometrical problems, albeit his proofs, as a consequence, reverse the order of the standard algebraic proofs. He has, in fact, shown by his "backward" algebraic style proofs, that analysis and synthesis are duals. Bonasoni's contemporaries, however, were interested in alagebraicizing geometry, whereas Bonasoni's work led to the geometricization of algebra. Thus, his work became lost in the rush to develop numerical algebra as the mathesis. Bonasoni's two reasons for his choice were simple, and he gave them straightforwardly in his "Praefatio". For one, he simply felt more comfortable operating with geometrical concepts, noting that he wished to rely upon both sensible intuition and intellect, upon visible representations of geometric configurations, rather than dealing with surds as if they actually existed in nature, or in mathematical reality as numbers. He therefore provided his *Algebra* with an algebraic symbolism devoid of algebraic notation; his symbols were geometrical. This geometrical symbolism, based on use of letters, Boyer [1956, pp. 58-59] has argued, allowed Bonasoni, unlike his algebraically-minded colleagues, to deal with whole classes of equations *in terms of parameters*, and thus to anticipate the notation developed by Viete. At the intellectual level, he argued [Bonasoni 1985, p. 3] that "numerical algebra most often exhibits the quotiety in surd numbers, ... while geometrical algebra lacks so great and continual a disadvantage..." In other words, Bonasoni was much more conservative than were his contemporaries who, in their zeal for *tour de force* problem solving, were more ready than Bonasoni to acquiesce in the use of irrational roots.

It could not have been as clear to Bonasoni and his contemporaries as it is to us that they were developing analytic geometry. For them, algebraic geometry and geometrical algebra denoted the same subject, but seen from totally different, competing perspectives and styles; one marks the algebraization of geometry, the other the geometricization of algebra. In Bonasoni's day, the style favored the former. As a result, Bonasoni's work was, as far as we are able to tell, ignored even in its own day, and Viete had the greatest impact of Bonasoni's contemporaries on the future work of Fermat and, more importantly, of Descartes.

In his *Géometrie* (1637), Descartes sought primarily to algebraicize geometry, to free it from its dependence upon diagrams. But he also sought to provide a geometric interpretation for algebraic operations, in short, to geometricize algebra. Since Bonasoni's work was left unpublished, we can convincingly suppose that Bonasoni had very little, or no, influence upon Descartes. It is consequently fair to suppose also that if Bonasoni's work had had the circulation and the public hearing that had been afforded the work of Bombelli and Viete, then analytic geometry, in the Cartesian sense of the full unification of geometry and algebra, might well have occurred a generation earlier than it did in the hands of Descartes himself, since Bonasoni's geometrical algebra could have provided the complement to Viete's "algebraical" geometry, the missing step between Viete's work and Descartes'.

The history of Bonasoni's posthumously discovered and neglected work and belated posthumous publication may be taken as an example of the effects of fads and trends in mathematics, and of the impact of social and professional-political pressures on the history of mathematics. It shows that the success of a worthwhile bit of mathematics, and the impact of that work on the consequent development of mathematics, can well be only a matter of style. Our next case, that of Jean van Heijenoort, shows that the presentation of a bit of mathematics, and the assignation of credit for a result, even of what one does or does not consequently contribute, can be a matter of luck.

A Matter of Luck

Jean van Heijenoort studied mathematics at New York University, writing his masters thesis, *On the correspondence between E. Cartan's method and the vector method in differential geometry* [1946] under the direction of J.J. Stoker. He continued his work in differential geometry, with a doctoral thesis *On locally convex surfaces* [1949]. In this thesis, van Heijenoort proved the theorem that, *if there is a support plane of a set (i.e. a plane through a boundary point of the set such that one of the two open half-spaces it determines does not contain any point of the set) through every boundary point of an open set, or of a closed set having interior points, then that set is convex*. Van Heijenoort's thesis, as originally written, proved this theorem for the two-dimensional euclidean space E^2. Shortly before he was to defend his thesis, however, the Soviet mathematician Aleksandr Danilovich Aleksandrov, well known for his work in geometry and foundations, also gave his proof of this same theorem for E^2 ([Alexandrov 1948]). Van Heijenoort, in discussions with I.H. Anellis in 1976 or 1977, remembered that he had had, because of this, to rewrite his thesis, under great pressure and with great haste; and indeed, was able to generalize his results to E^n. It was this new result, by van Heijenoort's recollection, which he finally defended in order to obtain his doctorate. A check of the evidence, in this case, of the thesis itself, reveals, however, that the thesis, in its E^2 version, was officially accepted by van Heijenoort's thesis committee at New York University on 1 April 1949. It was only later, in 1952, that van Heijenoort wrote, and published his generalization of the original theorem, in the paper *On locally convex manifolds* ([van Heijenoort 1952]). In this case, the memory may be more important than the fact.

It would be fruitless to speculate about what van Heijenoort might have accomplished had he obtained his results on convex surfaces a few months sooner, and therefore have announced those results before they had been announced by Aleksandrov. We cannot know whether van Heijenoort would have attained renown as a geometer, or whether he would have left a larger body of publications as his legacy. Nor can anyone, other than van Heijenoort himself, say what van Heijenoort's true response was to this experience. My own recollection, based on my reading of the nuances of van Heijenoort's words and his comportment when he delivered them, must be tentative and speculative at best; it is that the episode had a deep and abiding impact for him. What can be said with certainty is that van Heijenoort did not establish a reputation as a geometer, but rather as a logician and historian of logic, and, moreover, that he published a very small amount of original work during his lifetime, even in logic. His reputation was built upon his historical scholarship rather than on any new mathematics that he produced. He was best known for his editorship of the anthology *From Frege to Gödel* [1967] and for his publication of the work of Herbrand [1968]. Beyond that, there were several short, largely historical, partially philosophical, papers, many appearing for the first time in his posthumously published *Selected essays* [van Heijenoort 1986], although a few of those appearing in that volume were published previously. The great majority of his published corpus is comprised of the many book reviews which he wrote for the *Journal of Symbolic Logic* between 1956 and 1973.

Van Heijenoort continued to write a small number of papers in differential geometry and related areas, gradually making a shift in his research by way of topology, and in particular of some of Brouwer's work, towards logic. However, most of his technical writings since the appearance of his study of convex manifolds remained unpublished. This includes the important work which he carried out, in the decade between 1968 and 1977, in model-theoretic proof theory. In the mid-1960s, he had become interested, apparently following his participation in an informal discussion group which met in New York City around 1964/65, with Richard Jeffrey and Raymond Smullyan, where much of the talk centered around Smullyan's and Hintikka's work on Beth tableaux as a graphical representation of Gentzen-style sequent calculi and of natural deduction.) In a Beth tableau, two columns appear, each divided into two subcolumns; in the left subcolumn of the first column, one lists all true propositions, in the right row, all false propositions, while in the left subcolumn of the second column, are listed all of the propositions obtained as consequences of the true formulae, and in the right subcolumn of the second column are listed all of the propositions obtained as consequences of the false formulae. This method proved needlessly cumbersome, and it was difficult to keep track of things, having to hop back and forth between subcolumns and columns. Thus, in the mid-1950s, Hintikka and Smullyan began developing a one-sided Beth tableau, in particular a left-sided Beth tableau, which came to be known as the Smullyan tree, in which only truth trees had to be considered. A falsifiability tree was also developed, as a special case of right-sided Beth tableaux, or falsehood trees. Here one negates one or more of one's initial formulae, but assumes them to be true nevertheless. By downward induction, all consequents of these formulae will also be assumed to be true. The falsifiability tree can be used as a test of the validity of either a formula or of a proof. If the negated initial formulae are assumed to be true and if they nevertheless yield, in the same tree path both some formulae and its negation, then one has found a falsifying assignment for the negated initial formulae; if every path of such a tree for the negated initial formulae yield such contradictions, then the negated initial formulae are invalid, and the original, unnegated, initial formulae are valid. (For a history of the development of the tree method, see [Anellis forthcoming].)

Inspired by what he had learned, van Heijenoort began to make his own contributions to the tree method. In [1968], he explored the relation between Herbrand quantification and the falsifiability tree method, showing that the Smullyan tree could be used whether or not all quantified formulae were in prenex form, that is, whether all quantifiers were collected in the preface of one's formulae. Next, he proved [1972] that the falsifiability tree method for the theory of types with extensionality is sound and complete. The method is sound if every formula provably valid by the method is in fact valid, and complete if the method can prove that every valid formula is valid. In [1973], he showed that the method is sound and complete for propositional calculus; in [1974], he proved soundness and completeness of the method for first-order functional logic; and in [1975] and [1975a], he showed the method to be sound and complete for intuitionistic logic. None of these results were published. Instead, they circulated among van Heijenoort's students and to a small number of his colleagues. (For a discussion of these manuscripts, see [Anellis 1988].) We can, of course, only speculate on why they remained unpublished; it could have been that they were never intended as anything other than supplementary classroom materials, or that they did not meet van Heijenoort's high and rigorous standards of excellence - his perfectionism has become almost as legendary as Gauss' - or if, because of his experience with his thesis, he was hesitant to publish "new" results which someone else had, or might have, only a short time ago already published, or some combination of all of these factors. The fact remains that, with the exception of a greatly revised version of a proof of the soundness and completeness of the tree method for classical and intuitionistic first-order logics and corresponding new proofs for propositional and first-order modal logic in [1979], van Heijenoort's results in model-theoretic proof theory remained unpublished, and remain, still, largely unknown. Moreover, because of this unlucky hesitancy, Bell and Machover [1977] published their proof of the soundness and completeness of the falsifiability tree method for classical propositional calculus and classical first-order logic before van Heijenoort published his, although van Heijenoort's proofs of 1973 and 1974 were finished first and are simpler, if also somewhat longer, and van Heijenoort received no credit for his work. Perhaps as a result, a number of logicians have recently begun to publish proofs of the soundness and completeness of the tree method for propositional logic ([Boolos 1984]), and for first-order logic ([Kapetanovic & Krapez 1987]). These are results which van Heijenoort, as we have seen, had already obtained. It is a matter of ill luck that he did not publish these results. We must add, however, that there is enough difference between van Heijenoort's proofs and those of Boolos and the others to allay any suggestion that others appropriated van Heijenoort's ideas. Moreover, the extremely limited circulation of van Heijenoort's manuscripts militates against such a possibility. Had van Heijenoort published his results, then these accomplishments would have enhanced his stature, would have led to his being recognized not primarily, or even exclusively, as an historian of logic, but as an original contributor to technical logic as well.

In this case, van Heijenoort's bad luck in not publishing his results in model-theoretic proof theory, and his consequent failure to claim priority to results on the soundness and completeness of the tree method may have derived initially because of the bad luck of the poor timing of the results on convex surfaces in his doctoral thesis. His experience with his thesis, in my estimation, made him hesitant to publish his original technical work in the succeeding years. In the case of John Atanasoff, bad timing proved to be a critical factor in his loss of recognition as the inventor of the first electronic digital computer, and, beyond that, postponed the development of computer science and widespread use of the computer.

A Matter of Time

Timing is often the crucial factor in priority disputes. The classic case of the Newton-Leibniz priority dispute over the invention of the calculus, with all its bitterness, is well known, and need not be recounted. What is perhaps critical, from the point of view of our study, is the question of timing; Newton probably obtained his results first, but he failed to announce them quickly, fearing they might be stolen, while Leibniz published his results quickly. Very likely, both men obtained their initial results within a very short time of one another. If Newton kept his results secret, then the question of Leibniz having stolen them seems to become superfluous, if not silly. In van Heijenoort's case, his proofs of the soundness and completeness of the falsifiability tree method for various calculi assuredly have priority over the proofs of Bell and Machover, and of Boolos and others. But these proofs circulated hardly at all, privately, in unpublished and uncopyrighted form, among a handful of colleagues, such as Richard Jeffrey, and a very large number of van Heijenoort's students, only a very few of whom, however, such as Anellis, became van Heijenoort's colleagues. Hence, they remained virtually unknown. But if it is not always the case that he who publishes first receives the credit, neither is it always the case that he who fails to receive the credit has not published first. In Atanasoff's case, work that was meant to be secret was not altogether surreptitiously appropriated, but was used without permission. And he who borrowed this material, not bound by the secrecy, published his results first and received credit for Atanasoff's work. For Atanasoff, failure to receive credit for completion of the first electronic digital computer was a matter of time.

John Vincent Atanasoff received a Bachelors degree in electrical engineering from the University of Florida in 1925, a Master of Science degree in mathematics from Iowa State College (now Iowa State University = ISU) in 1926, and a Ph.D. in physics from the University of Wisconsin in 1930. After receiving his Ph.D., he joined the mathematics and physics departments at ISU, where he began his search for a calculating machine which would ease the burden and shorten the time that it took to perform tedious computations even on the most sophisticated calculating machines of the day. In particular, Atanasoff became interested in developing a machine that could easily and quickly handle the computation of approximate solutions for the wave functions with which he had worked in his doctoral thesis on the dielectric constant of helium. Well equiped by his education for the task he was undertaking, Atanasoff began his studies by examining in detail the mechanisms of the calculating machines currently available. He noted that the machinery then current was incapable of handling complex spectral analysis. During these years, especially beginning around 1934, Atanasoff, with the aid of some of his graduate assistants, also tinkered with the machines then available, and sought ways to link together several machines in order to achieve greater speed in handling more complicated problems. On the basis of this research, Atanasoff's graduate students George Gross [1937; 1939] and C.J. Thorne [1941] wrote graduate theses on the use of functionals for the approximate solutioms of linear differential equations, thereby developing a method which made it easier to carry out the required mathematics, while Atanasoff himself in 1936 developed his Laplaciometer, a small analog calculator. It was concluded, on the basis of these studies, that the analog calculators available at the time simply could not solve, efficiently, if at all, large scale systems of linear equations. Atanasoff therefore undertook a study, after 1936, with the assistance of his student Clifford Berry, of the possibility of constructing a digital machine. By 1939, a model had been constructed, and then, by mid-1940, a full-sized, fully operational prototype of the electronic digital computer, the ABC (Atanasoff-Berry Computer). In August 1940, Atanasoff described the ABC in his paper *Computing machine for the solution of large systems of linear algebraic equations*, using the mathematical tools developed by Gross and Thorne. The paper, which was not finally published until 1973 (see [Atanasoff 1973]), gave a full engineering description of the machine, and an account of its logic. The machine used a Boolean-valued machine language which which Atanasoff developed in 1939, taking his inspiration from the binary arithmetic presented, along with several other number-base arithmetics, in a long forgotten elementary school arithmetic textbook which had once belonged to his mother. He did not use Boolean algebra directly, asserting that at the time, in 1939, he did not recognize the application of Boolean algebra to his problem. He also described the logic with which computation would take place, using an addition-subtraction mechanism, rather than the simple enumeration used by the analog devices of the day. On the basis of this work, Atanasoff filed an application for a patent on the ABC. (For a personalized account of this history, see [Atanasoff 1984].) The paperwork for the patent application was still in progress when the U.S. entered World War II. Soon thereafter, Atanasoff left ISU to carry out research at the US Naval Ordanance Laboratory in Washington, D.C., and it was left to the administration at ISU to oversee the progress of Atanasoff's patent application. Apparently, however, in Atanasoff's absence, the application was never completed, although Atanasoff returned to ISU several times during the course of the war to check on the progress of the application and to prod the responsible legal authorities into action. Whether the paperwork was simply lost in the shuffle of the war effort, or the responsible authorities at the college decided that there were other, more pressing, more important concerns, or some combination of these, is not altogether clear. The fact remains that the timing of Atanasoff's application could not have been more unpropitious. Matters were made even worse by the visit to ISU of John Mauchly.

Mauchly visited Atanasoff and Berry at ISU in June 1941, had seen the ABC, had its construction explained to him by Berry, discussed it in detail with Atanasoff, and had read Atanasoff's *Computing machine...* paper during his week at ISU; all of this was done with the understanding that Mauchly would not make use of the information which Atanasoff and Berry were to share with him. In 1943-1946, Mauchly and his colleague J.P. Eckert, using the same logical and engineering principles which Atanasoff and Berry developed for the ABC, built their EINAC computer. Learning about the development of EINAC and its workings, Atanasoff believed that Mauchly had appropriated his ideas. The Sperry-Rand company had purchased Mauchly's patent rights, and a lengthy lawsuit (1971-73) was brought against Sperry by the Honeywell company, on Atanasoff's behalf, and in which Atanasoff was the star witness. The decision was rendered in favor of Honeywell, and finally, after more than three decades, Atanasoff's claim as the original inventor of the modern computer was legalized. Since that time, historians of computer science have begun to disentangle the web of confusions and distortions in the record; and it has only been in the last year or two that popular attention has begun to focus on Atanasoff as the inventor of the computer (see, for example, [Mollenhoff 1988] and [Mackintosh 1988]), to the extent even of becoming an American folk hero on the order of Thomas Edison and Alexander Graham Bell (see, e.g. [Hutchison 1988]). It is evident that, without the intervention and distractions of America's entry into the world war, Atanasoff would have received his earned recognition much sooner than he did, in particular as some historians of computer science have detected evidence that Mauchly dissembled in his accounts of his meeting with Atanasoff and Berry, and eventually falsified too his account of the development of EINAC. It is equally clear that, had Atanasoff's patent been granted in a timely fashion, the computer would have been available for use several years earlier than it was, and thus could have made a difference in the advance not only of computer science but of mathematical researches related to the American war effort.

It is not clear, whether we look at the case of van Heijenoort or the case of Atanasoff, whether bad timing led to bad luck or bad luck led to bad timing, or whether such a distinction is even possible. It is clear from these examples, as it is from the Newton-Leibniz priority debate, that the timing of publication of mathematical results can make a difference in the ascription of priority; but it is also clear from these two recent examples, and perhaps also from the example of Bonasoni, as it is not from the Newton-Leibniz debate, that timing can affect, by years, if not by decades, the course of mathematical progress. Whether a lost result, lost because of bad timing or bad luck, can significantly alter the entire course of mathematical development is no doubt a moot point. The best example here would be the case of Fermat's last theorem; for unless we can find a proof, in Fermat's hand, of his famous theorem, we cannot know how the history of mathematics might have been different, how much of the mathematics designed since Fermat's time to try to prove this theorem might have been lost, or how much new mathematics Fermat's "lost" proof had created. Indeed, "might have been" historical speculations are perhaps interesting thought-experiments; but they are outside the bounds of the factual study of history of mathematics, more suitable perhaps to philosophy of mathematics. They are perhaps also fruitless. Finally, timeliness may also be a question of being too early as much as of being too late, as Benois Mandelbroit will assuredly note in his scheduled talk on "Richardson and prematurity in science" for a recently announced program, on "Lewis Fry Richardson - mathematician and meteorologist", to be held at the School of Mathematics of Bristol

University on 6 May 1989 (see [Drazin 1988]).

If those who have argued that Mauchly deliberately distorted and obfuscated the history of computer science to his own advantage because an accident of timing deprived Atanasoff of his patent are correct, then the case of Atanasoff also shares some points in common with Russell's alteration of the course of the history of mathematical logic through a deliberate manipulation of matters of fact.

A Matter of Fact

Bertrand Russell is universally regarded as one of the greatest, and also one of the most important, logicians since Aristotle, a reputation which rests to a great extent upon his work with Whitehead on the *Principia Mathematica*. Among Russell's contributions are the Russell paradox, along with several "solutions" to the paradox, among these the theory of types. It is incontrovertibly true that Russell was, and to a great extent continues to be, one of the most influential logicians of modern times, perhaps the most influential since Aristotle. Nonetheless, there is mounting evidence that Russell was neither as good a mathematician nor as good a logician as his noteriety suggests. An examination of Russell's pre-*Principia* writings, particularly from the period 1896–1899, many of which remain unpublished, has shown that Russell was unable to understand Cantorian set theory when he first undertook a study of Cantor's work (see, e.g., Anellis [1984], [1987c], and [1987e]); it has also been suggested that the growing sophistication which Russell exhibited in his understanding, through the period 1896–1898, of the modern theory of real numbers may have been largely due to wholesale borrowings from, or even simple imitation of, other mathematicians, principally from textbooks such as Harkness and Morley's [1898] *Introduction to the Theory of Analytic Functions* (see especially [Anellis 1987e, pp. 317–319]). It was also shown that the "inconsistencies" and related problems which Russell professed to detect in both infiniteimal and real analysis were rooted in his misunderstanding of set theory and number theory (see [Anellis 1986]). In these studies, and especially in [Anellis 1987f], it was suggested that the philosophical root of Russell's misunderstanding was his Hegelianism. This does not explain, however, either how, rather dramatically, Russell's mathematical sophistication increased during the first decade of the twentieth century, and in particular in 1899–1900, leading to his work on *Principia*, or how one can account for the anomaly of the sudden, momentary, reappearance, in 1963, of Russell's Hegelian interpretation, based upon an example taken from F.H. Bradley, of Gödel's first incompleteness theorem, whereby Russell concludes that Gödel's result means, not that primitive recursive arithmetic is complete if and only if it is inconsistent, but that "school-boy arithmetic" is inconsistent (that 2+1=4.001) (see [Anellis 1987c, p. 17]). If, however, one examines the surviving documentation related to the writing of the *Principia*, as Victor Lowe has done, or compares Russell's private correspondence during the period 1899–1904 with his publications from those same years, and with his blatantly self-serving autobiographical publications, as Anellis and Nathan Houser are doing, some explanations begin to emerge. None are very flattering to Russell's mathematical or personal legacy.

After examining the surviving Russell–Whitehead correspondence related to the writing of the *Principia*, Lowe [1985, pp. 291–292, 263–264] concluded that Whitehead was responsible for all of the mathematics in the *Principia*, while Russell's contribution was restricted to the work on the theory of descriptions and the theory of types, and he refers to Whitehead's criticisms of Russell's sketchy, incomplete, and sometimes erroneous proofs in the early drafts which Russell prepared for the *Principia*. Elsewhere, Lowe [1974] notes that Russell destroyed correspondence from Whitehead which, Russell admitted, contained harsh judgments of some of his work on *Principia*. Russell, in his autobiographical writings, claims that work on *Principia* was fairly evenly shared ([Russell 1948]), although, somewhat later, he asserts that Whitehead did all of the mathematics for *Principia*, other than the section on series, without, however, disclaiming that the work was not divided fairly evenly between the two ([Russell 1959]). Anellis and Houser [1988] have noted that, although Russell studied Schroeder's *Algebra der Logik*, along with some of Schroeder's smaller works, beginning around September 1900, and was aware of Peirce's work in the logic of relations, largely through Schroeder's citations in the *Algebra*, Russell for the most part either ignored their work in his own publications on the logic of algebra written during the period 1900–1904, or expressed strongly negative views of their work whenever he did refer to it in his publications of the period, as well as in some of his correspondence, in particular with the historian of logic P.E.B. Jourdain, to whom he wrote that Schroeder's methods were "hopeless" (see [Grattan-Guinness 1977, p. 134]) and in his criticisms of Norbert Wiener's comparative study of Schroeder's work and the *Principia* (see [Grattan-Guinness 1975]). At the same time that Russell was denigrating the algebraic logic tradition of Boole-Peirce-Schroeder, he expressed his private view to others that Peirce's work was probably important, although beyond his understanding. At the same time, Russell's own work, both in the the 1900–1904 writings on the algebra of relations and, later, in the *Principia*, were heavily dependent upon the work in particular of Peirce and, even more so, of Schroeder. And although, in his remarks about the history of mathematical logic, Russell sought to create a dichotomy between the algebraic tradition of Boole- Peirce-Schroeder and the "quantification-theoretic" tradition of Frege-Peano-Russell out of which mathematical logic grew, it is clear that no such distinction was made by logicians during this period; Peano in particular, whom Russell especially singled out, was decidedly in the algebraic tradition. An examination of Russell's publications on the algebra of relations during the 1900–1904 period shows unequivocally that Russell, despite his strident deprecation of the work of Schroeder and Peirce in those publications, owed a powerful debt to both men. The dichotomy between algebraic logic/mathematical logic did not exist for logicians at the turn of the century; it seems to have arisen primarily through the efforts of Russell to create a sharp break between the algebraicists, to whom he owed much, and himself and his principal cohorts, Frege, Peano, and Whitehead. Russell argued that mathematical logic, as found in *Principia*, arose out of the quantification-theoretic tradition, and had supplanted the earlier algebraic tradition, which he viewed as a stultified dead-end. But it can be shown that the *Principia* itself, though it is, according to the standard, "Russellian" history, the pinnacle of the efforts by Frege, Peano, and Russell to create a mathematical logic, is heavily indebted on several levels to the algebraic tradition, and simply incorporates the algebra of logic as the algebra of sets and the algebra of classes. In fact, it should be almost superfluous to note that Whitehead himself, as the author of the *Treatise on Universal Algebra* (1898), was an important figure in the algebraic tradition and intended the *Principia* to serve in part as the second volume of his *Algebra* – which indeed it can. Moreover, first-order logic was first developed by Peirce, was incorporated and developed by Schroeder in his *Algebra* and did not begin with either Peano, Frege, or Russell.

My view that Russell deliberately distorted the history of logic in a campaign towards self-aggrandizement, with the aim of amplifying his own role in that history, is augmented by additional discrepancies between Russell's published autobiographical accounts and unpublished private correspondence which I, together with Houser, have examined. Thus, there is, for example, Russell's published assertion [1946] that he did not see or study Peirce's work until he became interested in the logic of relations, that is, in 1900; but one will find correspondence from Russell to the historian and philosopher of logic Louis Couturat ([Russell 1899]; quoted by Russell archivist Kenneth Blackwell [1987]) in which Russell discusses and recommends study of Peirce's *Studies in Logic*. It would be easier to dismiss this discrepancy as an innocent failure of memory were it not for the growing body of evidence of falsification and willful destruction of documents which are being discovered by Anellis, Houser, and Lowe. Coupled with the views which Russell's turn-of-the-century contemporaries shared of Russell's work, most of whom regarded the work of Boole, DeMorgan, Peirce, and Schroeder with great esteem, and, like Couturat [1904, pp. 129–130], regarded, even dismissed, Russell's work as simply a "systematization and synthesis" of the work of his predecessors, this is powerful evidence that, indeed, Russell has sought to influence the perception, in his favor and against in particular the algebraicists, the history of mathematical logic. It is on the basis of this evidence that [Anellis & Houser 1988] have suggested that Russell's propagandizing has altered the historian's and logician's perception of the history of logic, to the extent at least that a historian of logic with the stature, influence, and careful scholarship of van Heijenoort [1967, p. vi] could easily dismiss the half-century of important contributions, from Boole and DeMorgan to Peirce and Schroeder, as a mere sidelight in the development of modern logic. This view of the history of logic has been the traditional one for most of the twentieth century, and has, as a consequence, indubitably influenced as well a large share of the research in mathematical logic that has been undertaken since the publication of *Principia*.

It is only recently that the standard, "Russellian", history has been challenged, for example by Gregory H. Moore, who argued, in his [1977] review of van Heijenoort's anthology *From Frege to Gödel*, that the work ignores a large representative part of the history of mathematical logic. Moore's challenge to consider all aspects of the history of mathematical logic has been taken up by Moore himself [1987], [1988], who has examined, for the first time in a serious way, the contributions of the algebraicists to mathematical logic within the broader development, and as an integral part of, the history of mathematical logic, and by Anellis and Houser [1988], who have examined the perceptions of the state of mathematical logic before the facts of its history had been distorted and considered the factors that led to the distortions. It becomes increasingly clear that a distortion of the actual facts of the history of modern logic has occurred, and that this distortion has contributed to an alternative set of "facts" that have guided the history of logic since that time. It would also appear that the corrupted facts of the history of mathematical logic have influenced the subsequent development of logic in terms of the research which it encouraged on the technical level and in terms of the research which it engendered on the historical level. This distortion, which has undervalued the algebraic tradition and has relegated the algebraic researches of subsequent logicians, such as Tarski, to a logical backwater and associated that work with universal algebra rather than with the mainstream of mathematical logic, seems to have begun with, and been primarily the responsibility of, Bertrand Russell; and is only now beginning to be recovered. What still require explanations are the reasons, if any, for the extraordinary influence which Russell has exercised over the history of logic, and why it is only recently that historians of logic have begun to examine and to question the standard history which Russell and his heirs have presented. But this may simply be one of the imponderables of the history of mathematics. Perhaps it is easier to accept, as a matter of fact, theories which have become entrenched than to challenge them against the dominant current stream.

Conclusion.

The factors which contribute to the distortions and discontinuities of the development of mathematics, which lead to sometimes uneven, sometimes nonlinear, discontinuous mathematical evolution, such as matters of style, of luck, of time, of fact, perhaps cannot be explained, but only recognized. The examples which we have considered, in which Bonasoni's choice of mathematical *style* caused his work on the geometricization of algebra, as a complement to his colleagues' work on the algebraization of geometry, to be ignored, and of necessity led to the recapituation of that work by Descartes a generation lated, in which van Heijenoort's results in logic were withheld because perhaps of a nervous hesitancy to come forward with original results because of his *unfortunate* experience with his thesis, in which Atanasoff's work on the electronic digital computer was interrupted by the *untimely* intervention of external conditions, as a consequence of which Mauchly was able to illegally appropriate Atanasoff's work and gain credit for the invention of the computer, and in which

Russell, as new documentary evidence seems to suggest, manipulated the *facts* of the history of mathematical logic, thereby altering the perception of that history and very probably that history itself, suggest that mathematics cannot be viewed as an unbroken and singular enterprise, that its history does not proceed always in a straight line, acccording to logical rules, towards an overarching and monolithic generalization; it suggests that there are, indeed, distortions and discontinuities in the historical development of mathematics. Many of the examples of these distortions and discontinuities have, undoubtedly been lost, either through negligence, accident, or, as was nearly the case, apparently, through the efforts of Russell, by design.

Working mathematicians often see mathematics in a truer light than do either philosophers or historians of mathematics, because, as working mathematicians, they daily face the false starts, the intellectual puzzles, the dead-ends, the crooked paths, the frustrations, the pressures, of doing mathematics. At the same time, they also see the finished product *as* the final word. Mathematics is, after all, what the mathematicians make it. And in the Bourbakian style of mathematics, mathematics is the finished product, neatly tied and stringently presented. The goal becomes the reality of mathematics; a fine and finished elegant proof is what matters, not the search. It is the business of the history of mathematics to recover as much of this "lost" mathematics as possible, to attempt to understand how mathematics was done, and, "bowing to the spirit of the age" which he studies, to show the mathematics of an age, as much as possible, as it was for those who were doing that mathematics, to study the mistakes as well as the successes. It is the business of the philosopher of mathematics to explain, within the context of the social and philosophical milieu, the dominant influences that impact the choices of styles, the matters of consideration that shape the ways of doing mathematics, that lead to a choice between one set of "facts" and another. The lesson may be that the kinds of matters, of style, of luck, of time, of facts and falsehoods, cannot be predicted; for if they were predictable, then they would not readily lead to the kinds of discontinuities and distortions that our chosen examples have revealed. Moreover, the cases which we have cited suggest that it is not always a straightforward task to differentiate between or disentangle the matters of style, of luck, of time, of fact, that contribute to this uneven, adventurous, human and social adventure called mathematics. But the search itself can prove to be as fulfilling and as exciting as the solution, both for the philosopher who seeks to understand it as for the historian who seeks to recover and reveal it, in the same way that the search for a new piece of mathematics is as fulfiling and exciting for the working mathematician as the solution of a particularly difficult computational problem or the discovery of a new and brilliant theorem.

Several years ago, at the Third Southeastern Logic Symposium, held in Charleston, South Carolina, I had the opportunity to hear the famous peripatetic Hungarian mathematician Paul Erdös give a highly personalized account of unsolved problems in combinatorial set theory. In his remarks, he told the oft-repeated, by now legendary, story of his encounter with Gödel at an airport. Their discussion eventually got to the question of Gödel's views about the existence of God; and Gödel is reported to have said that, should God exist and were he to have the opportunity to ask Him only one question, that one question would be 'Is the Continuum Hypothesis true?' The hypothesis says that the cardinality of the reals is aleph-one, and that there are no cardinals between aleph-null and aleph-one. It assumes that the reals can be well-ordered, so that it bears a connection to the Axiom of Choice. Now mathematicians have raised doubts about the acceptability of both the Continuum Hypothesis and the Axiom of Choice, largely on philosophical grounds. Nevertheless, the working mathematicians frequently assume that both are true, and frequently use them, often only implicitly, in their work, because many otherwise difficult proofs can be rendered easy, straightforward, economical, with their use. But what the cases to which we have referred suggest is that the history of mathematics, like human life itself, may not always be well-ordered or easy.

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