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What are revolutions in mathematics? Mathematical truth in the light of Thomas S. Kuhn's philosophy of science[1]

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RESUMEN

A principios de los años sesenta Kuhn propuso un nuevo punto de vista sobre la ciencia y el concepto de las revoluciones científicas, las cuales excluyeron, deliberadamente, discusiones sobre matemáticas. En 1975, Crowe concluyó que: 'Las revoluciones nunca ocurren en las matemáticas'. A través del análisis de los orígenes del teorema fundamental del cálculo diferencial e integral, el autor pone especial énfasis al papel crucial que jugó el modo o estilo algebráico de las matemáticas en la formulación de la forma algorítmica del teorema. Desde el punto de vista del autor, la formulación de Leibniz fue revolucionaría, él mismo puede ser comparado con el Lavoisier de la revolución química. La verdad en las matemáticas está menos restringida por el mundo natural real que la verdad en las ciencias naturales. El cambio en las matemáticas ha sido o gradual o revolucionario, y ha afectado tanto al contenido como a las instituciones. Como una actividad histórica intelectual con cierta base hermenéutica, el conocimiento matemático ha experimentado, por supuesto, revoluciones.

Palabras clave: Kuhn, Crowe, scientific revolutions, fundamental theorem of the calculus, historical philosophy of mathematics.
MSC: 01Axx, 01A85

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ABSTRACT

During the early 1960s, Kuhn proposed his new view of science and his concept of scientific revolutions, which deliberately excluded discussions of mathematics. In 1975, Crowe concluded that ‘Revolutions never occur in mathematics’. By examining the origins of the fundamental theorem of the differential and integral calculus, the author pays attention to the crucial role played by the algebraic mode or style of mathematics in formulating the algorithmic form of the theorem. In the author's view, Leibniz's formulation was revolutionary, and Leibniz can be compared to the Lavoisier of the chemical revolution. The truth of mathematics is less restrained by the real natural world than the truth of natural sciences. Change in mathematics has been either gradual or revolutionary, and it has affected both content and institutions. As a historical intellectual activity with a certain hermeneutic basis, mathematical knowledge has indeed experienced revolutions.

Keywords: Kuhn, Crowe, scientific revolutions, fundamental theorem of the calculus, historical philosophy of mathematics.

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1. Toward a newhistoriographyof mathematics: ThomasS.Kuhn's historical philosophy of science and the responses to it by some historians of mathematics

The second half of the twentieth century witnessed a kind of revolution in the history and philosophy of science, and one book played a key role for that revolution, Kuhn's *Structure of Scientific Revolutions*, published in 1962. Today, his view of science is generally labeled ‘historical philosophy of science’, in contrast with ‘logical philosophy of science’ represented by Carnap [Kuhn 2000, 91& 309, for example. See also: Gillies 1993, 68].

In what follows I consider whether the ‘historical philosophy of science’ can be applied to mathematical truth. For many years since the appearance of Kuhn’s monograph, historians and philosophers of mathematics have tended to focus on the dynamic process by which mathematical knowledge has been acquired rather than on the static and formal structure of the products of mathematical research. Many of them, however, have been skeptical about the applicability of Kuhn's view of science to mathematics. Crowe wrote:

> In the historiography of mathematics, no comparable group of authors seems to have emerged. Moreover, most historians of mathematics acquainted with the new historiography of science have been skeptical as to whether the insights embodied therein can be applied in any direct way to the historiography of mathematics. [...] [T]he major differences between the conceptual structures of mathematics and of science make it questionable whether their histories should exhibit similar patterns of development [Crowe 1975, 162; Reprinted in Gillies 1992, 15].

Following nine ‘laws’ concerning patterns of change in the history of mathematics briefly, he added a tenth law: “Revolutions never occur in mathematics” [Crowe 1975, 165; Gillies 1992, 19].

Several historians of mathematics, nevertheless, attempted to refute Crowe's tenth law. By taking examples from Eudoxus's theory of proportion applicable to both commensurable and incommensurable quantities and Georg Cantor's transfinite set theory, Dauben has proposed a new thesis: “Revolutions obviously do occur *within* mathematics” [see Dauben [1984]. The cited sentence with the original emphasis is from Dauben 1992, 81].[1]

Crowe has modified his views of 1975. In 1988, he states: “I have become convinced that ten claims I formerly accepted concerning mathematics and its development are both seriously wrong and a hindrance to the historical study of mathematics” [Crowe 1988, 260. See also Crowe 1992]. Despite Crowe's conversion, it is worth considering Crowe's views of 1975, since they exert a firm hold on practicing mathematicians.

An autobiographical note is relevant. As a student of Kuhn at Princeton University, I attended his course on the philosophy of science and offered him an outline of my

1. The papers useful for reviewing the debate since the Crowe essay of 1975 have been compiled in Gillies [1992] and in Ausejo & Hormigón [1996].

immature views on the historiography of mathematics in May 1977. Before then, Kuhn had not thought that his theory of science was applicable to mathematics, and this view certainly influenced, it was said, Crowe's essay of 1975. But, in his comment on my report, Kuhn seemed to me to have changed his attitude toward mathematics, although slightly. About this event, see 8. “Concluding remarks” of this essay.

In the following pages I support Dauben's understanding by defending the Kuhnian approach to the historical philosophy of science. To do so shall require rejecting the Platonic or formalist understanding of mathematics and presenting mathematics as a quasi-empirical science, to use Lakatos's [1978b] terminology. Historians of mathematics have discussed the problem from point of view of mathematics in modern Europe and ancient Greece. In treating this question, however, it is vital to examine how traditional mathematical culture in East Asian countries transformed into a culture based on modern Western mathematics. Surely any general understanding of the development of mathematics must be ecumenical rather than parochial.

2. An examination of Crowe's thesis of 1975

In his tenth law of 1975, Crowe does not advocate a radical evolution as the nineteenth-century mathematician Hermann Hankel did when he stated, “In most sciences one generation tears down what another has built and what one has established another undoes. In Mathematics alone each generation builds a new story to the old structure” [cited from Moritz 1942, 14]. Crowe tells us that such a quotation “cannot stand alone and without qualification”, for his own ‘law’“depends upon at least the minimal stipulation that a necessary characteristic of a revolution is that some previously existing entity (be it king, constitution, or theory) must be overthrown and irrevocably discarded” [Crowe 1975, 165; Gillies 1992, 19]. According to Crowe, “a number of the most important developments in science, though frequently called ‘revolutionary’, lack this fundamental characteristic”. He then distinguishes ‘transformational’ or revolutionary discoveries (*e. g*. the Copernican revolution) from ‘formational’ discoveries. In the latter case, new areas are ‘formed’ or created without the overthrow of previous doctrines. He exemplifies this case by energy conservation or spectroscopy. Later he states:

> It is, I believe, the latter process rather than the former which occurs in the history of mathematics. For example, Euclid was not deposed by, but reigns along with, the various non-Euclidean geometries. Also the stress in law 10 on the preposition "in" is crucial, for, as a number of the earlier laws make clear, revolutions may occur in mathematical nomenclature, symbolism, metamathematics (*e. g*. the metaphysics of mathematics), methodology (*e. g*. standards of rigor), and perhaps even in the historiography of mathematics [Crowe 1975, 165−166; Gillies 1992,19].

Here Crowe presupposes a fixed belief which appears to occupy most working mathematicians of today. Nevertheless, we have to ask with Mehrtens what ‘in’ mathematics means [Mehrtens 1976, 301; Reprinted in Gillies 1992, 25]. Can we strip the content or the substance of mathematics from mathematical nomenclature, symbolism, metamathematics, and methodology?

To take an example which Crowe has used, the creation of non-Euclidean geometries, Crowe states, “Euclid was not deposed by, but reigns along with, the various non-Euclidean geometries”. That is, however, a Whiggish judgment. Körner [1967, 120−121] observes:

> Until the discovery of non-Euclidean geometries the doctrines of the uniqueness of mathematical reality or intersubjective intuition, of its accessibility, of the solvability of all classes of mathematical problems and of the possibility of the unambiguous and exhaustive reflection of mathematical reality in conceptual or linguistic formulations were neither called into question nor regarded as needing a more precise analysis.

The discovery of non-Euclidean geometries changed epistemology by refuting the Kantian apriorism. In turn, epistemology reinterpreted the content of non-Euclidean geometries. Crowe, furthermore, seems to be referring to Book I of Euclid's *Elements* or rather to Euclidean geometry in the Kantian sense. It is well to recall Book V containing the theory of proportion applicable to geometrical magnitudes, and Book X which consists of the complicated theory of irrational magnitudes. Crowe has selected a part of the *Elements* for his view of the historiography, for Books V and X of the *Elements* have been discarded and superseded totally by the real number theory of Richard Dedekind. Are the propositions of Books V and X still true? Is Euclid's geometrical mathematics entirely different from Aristotle's physics? The questions are taken up in the next section.

3. Mathematics as quasi-empirical and time-dependent knowledge

Why does it appear as if mathematical truth is always true and not time-dependent, perennial in the strict sense of the word? One reason for this is that mathematics acquired a firm paradigm from prehistory, or at least ancient Greece.[1] Mathematics was one of the first sciences to employ deductive inference. Thus, mathematics is always related to the ideal world, as distinct from the real world, through the procedure of abstraction. Mathematics, from the time of ancient Greece, is also characterized by the axiomatic method. Mathematics in Islamic civilization, in the Latin Middle Ages, and in modern Europe all absorbed this *modus operandi*.

Observe here that these characteristics of mathematics are not a priori, but are rather historical acquisitions. We can contrast the paradigm of Greek mathematics with that

1. We do not argue this point although Szabó's [1969] contribution is important. Kuhn [1962, 15] regards mathematics and astronomy as doctrines “in which the firm paradigms date from prehistory”.

of traditional Chinese mathematics most effectively. Needham observed:

> "On account of its more abstract and systematic character"—so came the words of themselves to the keys of the machine. Systematic, yes, there no doubt is possible, but abstract—was that wholly an advantage? Historians of science are beginning to question whether the predirection of Greek science and mathematics for "the abstract, the deductive and the pure, over the concrete, the empirical and the applied" was a wholly a gain. [...]. In the flight from practice into the realms of the pure intellect, Chinese mathematics did not participate. [...]. For the 1st century B. C., the time of Lohsia Hung and Liu Hsin, the *Chiu Chang SuanShu* (Nine Chapters of the Mathematical Art) was a splendid body of knowledge. It dominated the practice of Chinese reckoning-clerks for more than a millennium. Yet in its social origins it was closely bound up with the bureaucratic government system, and devoted to the problems which the ruling officials had to solve (or persuade others to solve). Land mensuration and survey, granary dimensions, the making of dykes and canals, taxation, rates of exchanges—these were the practical matters which seemed all-important. Of mathematics 'for the sake of mathematics' there was extremely little. This does not mean that Chinese calculators were not interested in truth, but it was not abstract systematized academic truth after which sought the Greeks.[1]

1. Needham 1959, 151-153. The significance of *The Nine Chapters of the Mathematical Art* is richly discussed both in *The Nine Chapers of the Mathematical Art: Companion & Commentary*, by Shen Kangshen, John N. Crossley, and Anthony W.-C. Lun (Oxford: Oxford University Press; Beijing: Science Press, 1999) and in *Les Neuf Chapitres: Le Classique mathématique de la Chine ancienne et ses commentaires*, Edition critique bilingue traduite, présentée at annotée par Karine Chemla et Guo Shuchun (Paris: Dunod, 2004). See, in particular, Geoffrey Lloyd's 'préface' to the latter French version.

   Incidentally, it was after a reintroduction of Chinese mathematics that the Japanese became really observant about mathematical studies in the seventeenth century. Yet the Japanese, while they were expanding their own mathematics during the Tokugawa period, had the concept of mathematics for its own sake under the general influence of the notable artisanship [*geidō*], which might be compare to 'l'art pour l'art'. This fact seems to have made an important difference between the Chinese and the Japanese when they introduced modern European mathematics. As for the method of demonstration, the premodern mathematics in Japan [*wasan*] had, at least, the concept of direct or intuitive proof. For example, Seki Takakazu (d. 1708) proved the Pythagorean theorem by this method without the

Moreover, the paradigms acquired in ancient Greece, whose main characteristic was geometrical, had to be reinterpreted in algebraic language, particularly by Viète and Descartes [Sasaki 2003]. The rigor of modern European mathematics is based on algebraic thinking, as can be seen in Augustin-Louis Cauchy's *Cours d'analyse:1repartie, Analyse algébrique* of 1821.

Mathematicians have generally believed that mathematics is a 'pure deductive science'. Kalmár [1967, 188−189] summarizes the reasons why this belief is sustained:

> (i) The rise of a philosophy of the *a priori*, according to which it was unnecessary to test in practice intuitively evident axioms and rules of inference; (ii) mathematics' becoming proverbial as an "infallible" science, due to its appropriate methods; (iii) mathematics' becoming, because of its precision, an ideal for other fields, beginning perhaps with Spinoza's *Ethica more geometrico demonstrata*, down to the mathematical economics or mathematical linguistics of our own day; (iv) the success of mathematics itself, and the extension of its method of abstraction to more and more general regularities holding for empirical facts, which was reflected in more and more abstract mathematical theorems and algorithms.

These circumstances, however, do not deny that mathematics has empirical origins and that its foundation has empirical aspects. Rather it would be more suitable to call this characteristic 'quasi-empirical', for the mathematically given is closely related to, but not identical with, the empirically given. It was Lakatos who characterized 'the present

Western influence. See Seki's *Collected Works* (Osaka: Osaka Kyoiku Tosho, 1974), p. 125. A similar, but more elementary proof can be seen in *Chou Pei Suan Ching*, or *The Arithmetical Classic of the Gnomon and the Circular Paths (of Heaven)*. Needham comments on the Pythagorean theorem which is not proved in the Euclidean way: "we cannot now regard it, with Biot, as certainly 'five or six centuries earlier than Pythagoras (fl. 530 B. C.)', but there is not much reason for putting it any later, and it may well be older" [see Needham 1959, 21-22]. The comparative study of such fundamental concepts of mathematics in different cultures is one of the desiderata of the historiography of mathematics.

empiricist mood' of the philosophy of mathematics, relying on his historical studies of theorems of topological geometry and analysis, and his understanding of post-Gödelian mathematics. He explains the impact of Gödel's incompleteness theorem of 1930−31 concisely: "The infinite regress in proofs and definitions in mathematics cannot be stopped by a *Euclidean* logic. Logic may *explain* mathematics but cannot *prove* it" [Lakatos 1962, 178; reprinted in Lakatos 1978a, 19. The emphases are in the original].

Consequently, his comment on Kalmár's presentation has the title, "A Renaissance of Empiricism in the Recent Philosophy of Mathematics?" Lakatos [1967, 202（the emphasis is in the original)]states his thesis:[1]

> The present empiricist mood, so strongly represented by Professor Kalmár, originates in the recent recognition that mathematics is not Euclidean or quasi-Euclidean—as had been expected in the first heroic period of foundational studies—but quasi-empirical, and in the increasing attention drawn to the part of mathematics that falls *outside* its Euclidean kernel.

In a word, mathematics is 'pure deductive science' normatively, but it is, in fact, 'quasi-empirical'.

What about the infallibility of propositions of mathematics? Are the propositions of Books V and X of Euclid infallible and totally different from, for example, the natural philosophical statements or Aristotle's *Physics*?

There are two kinds of mathematical theories. One is the group of mathematical theories which give mathematicians active problems. We may call these theories active ones, and they often correspond to what occurs in Kuhn's normal science; by extension comes the concept of normal mathematics, following Mehrtens [1976, 305; reprinted in Gillies1992, 29]. The other group consists of theories which are not active any longer.

1. This point of view was much extended in his paper with the same title [see: Lakatos 1978b, 24-42].

Mathematics, if we regard this discipline as a science which develops historically, has empirical origins and cannot obtain absolute rigor. Körner [1967, 131−132] has analyzed the philosophical significance of post-Gödelian mathematics, comparing it with the existence of competitive non-Euclidean geometries:

> Disregarding always possible appeals to some dogmatic metaphysics, one must conclude that the metamathematical discoveries of the present century imply the falsehood of the common doctrines shared by the classical philosophies of non-competitive mathematical theories, but not that they imply the positivist doctrine that if mathematics is not true-or-false in the actual or every possible world, it is therefore meaningless.

Körner [1967, 132] then refutes the dogmatic formalism by stating that competitive mathematical theories have their relation to experience and that their historical validity and survival is sustained by their applicability to the real world:

> They support on the other hand the view that the various un equivalent, mutually compatible theories are meaningful, *i. e.,* true in different possible worlds, even though none of them is actual, and that their agreement or conflict with each other lies in their relation to experience, namely the coidentifiability or otherwise, of at least some of their axioms or theorems with empirical propositions.

If the reasoning of syllogism is always true, a mathematical proposition is necessarily true if its postulates are true. But the postulates and even the standard of rigor are time-dependent. The world of mathematics is essentially open to the real world even though it is idealized. Moreover, such idealization cannot transcend history. Mathematical terms are fastened to the historical world in which they flourish. Let us quote the first definition of Euclid's *Elements*: ‘A point is that which has no part’. As commentaries on this definition show, the concept of point has been reinterpreted again and again. Exactly speaking, the mathematical terms of Hilbert's *Grundlagen der Geometrie* [1899] are not identical to Euclid's. If Euclid's theory is eternal, it would be true that the theory is concerned only with elementary statements and with the world which is common both to the Greeks and the modern men. But from the seventeenth to the beginning of the twentieth century Euclideanism has experienced a great retreat. As Lakatos [1962, 166; reprinted in Lakatos 1978, 7] states: “The fallible sophistication of the empiricist programme has won, the infallible triviality of Euclideans has lost. Euclideans could only survive in those underdeveloped subjects where knowledge is still trivial like ethics, economics etc.”

We explain historically discarded physical theories —for example, Aristotle's natural philosophy— by examining their related *Weltanschauung* and terms such as motion, void, space, and place. There is certainly a discontinuity between ancient Aristotelian physics and modern Newtonian mechanics. Kuhn [2000, 13−32] called this discontinuity ‘incommensurability’ and saw in it a scientific revolution. Although we might admit that the statements of natural sciences are much more easily refuted by new statements than mathematics, we cannot dream the nineteenth-century mathematicians' dream: ‘In mathematics alone each generation builds a new story on the old structure’.

In an essay of 1974, Grabiner pursues the reason why standards of mathematical rigor change. Nineteenth-century analysts, beginning with Cauchy and Bolzano, demanded rigorous proofs of propositions in infinitesimal algebraic analysis. Avoiding errors became more important near the end of the eighteenth century, when there was increasing interest among mathematicians in complex functions, in functions of several variables, and in infinite trigonometric series. Grabiner appeals to social change: “Before the last decade of the century, mathematicians were often attached to royal courts; their job was to do mathematics and thus add to the glory, or edification, of their patron. But almost all mathematicians since the French Revolution have made their living by teaching” [Grabiner 1974, 360; reprinted in: Tymoczko 1998, 207. See, also, Grabiner 1981].

Grabiner asks, "Why might the new economic circumstances of mathematicians—the need to teach—have helped promote rigor?" Then, she answers, "Teaching always makes the teacher think carefully about the basis for the subject. A mathematician could understand enough about a concept to use it, and could rely on the insight he had gained through his experience" [Garbiner 1974, 360; Tymoczko 1998, 208].

Certainly, mathematical propositions appear to be infallible in the light of a standard of rigor which is time-dependent but easily transferred across epochs and civilizations. They are understandable even if they depend on the *Weltanschauung* in which they were created. This aspect of mathematics is, no doubt, quite different from the natural sciences. In this sense, mathematical knowledge is concerned with the ideal world in the Platonic sense.

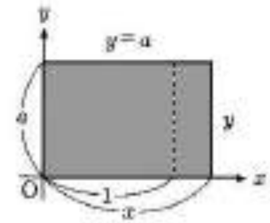
But, there is still the matter of active mathematics. Books V and X of the *Elements* are not active any longer except as a subject of special historical interests. Even Book I is not so active among mathematical practitioners, because the way of mathematical thinking has changed. Before returning to Euclid, we focus on the differential and integral calculus.

4. The measure of areas and the tangent method before the establishment of the differential and integral calculus: from Archimedes to Pierre de Fermat

Now we should take a look at how the differential and integral calculus was formed in seventeenth-century Europe and for this, we consider forerunners from Archimedes to Fermat.[1]

1. For a general outline of the development of infinitesimal mathematics, see, among others, Edwards [1979]. Some useful source materials are available in Struik [1969].

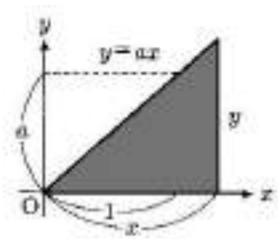
The theory of integration can be said to have been derived from the art of measuring or calculating areas in classical antiquity. The ancients knew that the area of a rectangle with one side an and the other side x is equal to



$$\int_0^x a dx = ax .$$

(Figure 1.a).

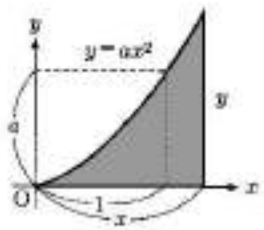
And the area of a triangle with its basis x and its height $y = ax$ was also known to be equal to



$$\int_0^x a dx = \frac{ax^2}{2}.$$

(Figure 1.b).

Then, what Archimedes attempted at in his the *Quadrature of the Parabola* was to get the area of a segment of the parabola. Through both sophisticated geometrical and mechanical methods, he obtained the area of a segment of the parabola to be in modern algebraic language.



$$\int_0^x ax^2dx = \frac{ax^3}{3}$$

(Figure 1.c)

Rashed [1993] has shown that Ibn al-Haytham [latinized as *Alhazen* from his first name al-Ḥasan] of the late tenth and early eleventh centuries became a mathematician in no way inferior to Archimedes on the infinitesimal geometry in the Arabic mathematical tradition.

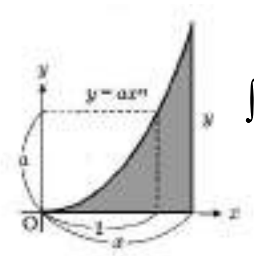
With the almost total recovery of the works of Archimedes during the Renaissance European humanists and mathematicians studied the Archimedean method and tried to go beyond it. In his *Geometria indivisibilibus continuorum* published in Bologna in 1635, Bonaventura Cavalieri (*c*. 1598−1647) introduced a new method of integration and derived the Archimedean formula

$$\int_0^x ax^2dx = \frac{ax^3}{3}$$

for the parabola, but he failed to get a new result. In his later work, *Exercitationes geometriae sex,* published in Bologna in 1647, however, he obtained the following formulae for some generalized or higher parabolas for n = 3, 4, ..., 9:

$$\int_0^x ax^n dx = \frac{ax^{n+1}}{n+1}.$$

As may be imagined, the method which Cavalieri used for those results was a natural extension of the Archimedean geometrical method for the quadrature of the parabola. It would not be so difficult for talented contemporary mathematicians of the mid-seventeenth century to conjecture the following formulae inductively and generally (Figure 1.d):



$$\int_0^x ax^n dx = \frac{ax^{n+1}}{n+1} \text{ for } ax^n \quad (n \geqq 3).$$

Fermat further generalized the above formulae for *n* being positive rational numbers and negative integers other than −1.

A modern method of drawing tangents can be said to have been born with Fermat. In his manuscripts, "Methodus ad disquirendum maximam et minimam et de tangentibus linearum curvarum", which may be ascribed to the period of 1629−36, he obtained algebraic methods for values of maxima and minima and how to draw the tangent to the parabola [Fermat 1891 t. I, 133−167].[1]

We see his treatment of the problem of finding the tangent *PT* to a curve at a point *P* on the curve. He did this by finding the length of the subtangent *TN*. We may think that he further extended his method of tangent for the generalized parabola of which equation is $y = x^n$for *n* being a positive integer.[2] In Figure 2, let *P* be (x, y) and *P*' is a neighbouring point $(x + e, y + d)$ on the curve. Now we have

$y + d = (x + e)^n = x^n + nx^{n-1} e$ + {terms in e^2 and higher powers of *e*}.

1. For a French translation see: Fermat 1896, t. III, 121-147.*Cf.* Mahoney 1973, 143-213.
2. For the following interpretation, I am indebted to Hollingdale [1989,141-142]. Figure 2 has been taken from p. 142.

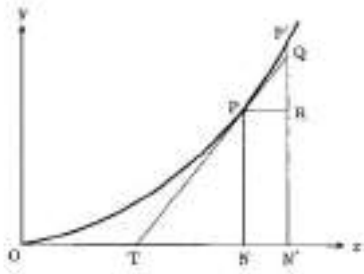


Figure 2

Then, $d/e = nx^{n\text{-}1}$+ {terms in e}. Consequently, the slope of the curve at P is obtained as the formula which is familiar to us,

$$\frac{dy}{dx} = \frac{dx^n}{dx} = nx^{n-1}.$$

For the algorithmic method of tangent, Fermat thus attained the result

$$\frac{dy}{dx} = nax^{n-1}$$

for the generalized formula $y = ax^n$ with some anachronistic modifications. But, unfortunately, he failed to see the mutual reciprocal relationship between the algorithm of quadrature and that of tangent.

5. The recognition of fundamental theorem of the calculus: Five mathematicians with their geometrical vs. algebraic mode of thought

In the middle of the seventeenth century, five mathematicians gradually recognized the mutual inverse relationship between the method of quadrature and that of tangent: the Italian mathematician Torricelli, the Scot James Gregory, Barrow and Newton, and the German 'uomo universale' Leibniz.

It was Torricelli who understood that mutual relationship for the first time. He recognized that finding quadratures and solving inverse-tangent problems were essentially identical for particular geometrical curves, *i.e.*, the case of the generalized parabolas and hyperbolas. The so-called inverse-tangent problem consisted in finding a curve, given a law characterizing the tangent. This achievement was made through kinematical considerations acquired from Galileo and the geometrical method of indivisibles of Cavalieri. Torricelli's mathematical writings remained unpublished until the publication of the *Opere di Evangelista Torricelli* [4 vols., 5 pts. in 1919−1944]. Certainly Torricelli was a remarkable mathematician, but he consciously avoided the algebraic method, relying totally on the orthodox geometrical method. He was the 'geometras ummo', as his contemporaries called him.

James Gregory was the first to publish the recognition that the method of quadrature and that of tangent were mutually reciprocal in his *Geometriae pars universalis*, appearing in Padua in 1668. The curves which Gregory treated were quite general, and were not algebraic but entirely geometrical, as in the case of Torricelli. What Prag stated in 1939 seems completely correct:

> in fact we have here a first proof of the Fundamental Theorem of the Integral Calculus. But tangent and area problems are still strictly geometrical. As long as differentiation and integration are not conceived of as operations, as calculating processes, we are hardly justified in describing or explaining old results by applying our modern abstract conceptions which are based precisely on the idea of 'operation' [Prag 1939, 491. See, also, Dehn *et al*. [1943, 162].

Newton was the first mathematician who saw that the method of quadrature and that of tangent were mutually reciprocal in an algebraic manner. He arrived in this result as early as May of 1665. Newton [1967, 42], in his draft of "The October 1666 tract on fluxions", set down Problem 5:

"To find y^e nature of y^e crooked line whose area is expressed by any given equation".

Newton's 'resolution' is essentially as follows: In Figure 3, consider the area x with its basis x and its height 1, and let the area under the curve $ac = q = f(x)$ be y. Compare the fluxions p (= $\dot{x} = dx/dt$, in the a nachronistic Leibnizian notation) and q (= $\dot{y} = dy/dt$) so that p:q = be : bc. That is to say, p :q = 1 : $f(x)$. Since

$$y = \int_0^x f(t)dt\ ,$$

the previous formula indicates that

$$\frac{d}{dx}\int_0^x f(t)dt = f(x)\ .$$

This formulation and its 'resolution' is dazzlingly simple, and it is nothing other than the fundamental theorem of the calculus.

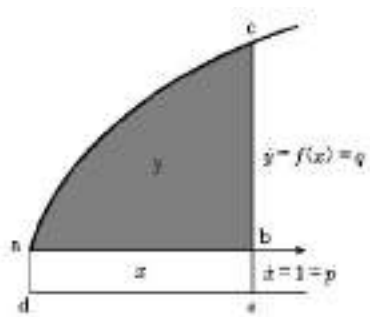


Figure 3

Newton, however, kept this remarkable result from the public. It was not until the publication of "De quadratura curvatum", in 1704, that his method of fluxions became generally and openly known.

Newton's predecessor of the Lucasian professorship of mathematics at Cambridge University, Isaac Barrow, is known to have made public a result which could be translated algebraically into the fundamental theorem of the calculus. It was in his *Lectiones geometricae*, delivered at

Cambridge in 1668 and published in 1670. We read Lecture X, Proposition 11 as follows (Figure 4):

> Let *ZGE* be any curve of which the axis is *VD* and let there be perpendicular to ordinates to this axis (*VZ*, *PG*, *DE*) continually increasing from the initial ordinate *VZ*; also let *VIF* be a line such that, if any straight line *EDF* is drawn perpendicular to *VD*, cutting the curves in the points *E*, *F*, and *VD* in *D*, the rectangle contained by *DF* and a given length *R* is equal to the intercepted space *VDEZ*; also let *DE* : *DF* = *R* : *DT*, and join [*T* and *F*]. Then *TF* will touch the curve *VIF*. For, if any point *I* is taken in the line *VIF* (first on the side of *F* towards *V*), and if through it *IG* is drawn parallel to *VZ*, and *IL* is parallel to *VD*, cutting the given lines as shown in the figure; then *LF* :*LK* = *DF* : *DT* = *DE* : *R*, or *R*×*LF* = *LK* ×*DE*.
>
> But, from the stated nature of the lines *DF*, *LK*, we have *R*×*LF* = area *PDEG*: therefore *LK* × *DE* = area *PDEG* < *DP* × *DE*; hence *LK* < *DP* = *LI*.
>
> But, from the stated nature of the lines *DF*, *LK*, we have *R* × *LF* = area *PDEG*: therefore *LK* × *DE* = area *PDEG* < *DP* × *DE*; hence *LK* < *DP* = *LI*.
>
> Again, if the point *I* is taken on the other side of *F*, and the same construction is made as before, plainly it can be easily shown that *LK* > *DP* = *LI*.
>
> From which it is quite clear that the whole of the line *TKF* lies within or below the curve *VIFI*.

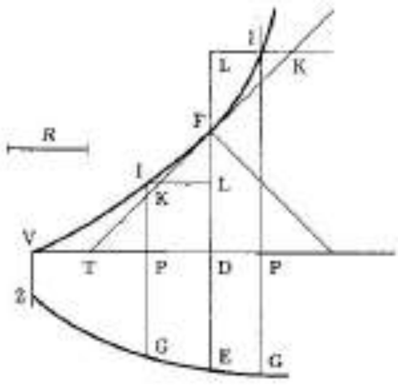


Figure 4

> Other things remaining the same, if the ordinates, *VZ*, *PG*, *DE*, continually decrease, the same conclusion is attained by similar argument; only one distinction occurs, namely, in this case, con-

> trary to the other, the curve *VIF* is concave to the axis *VD* [Barrow 1860, 243–244].[1]

Now we translate this theorem into the algebraic language. Let the curve *ZGE* be expressed by $y = f(x)$, and the curve *VIF* by $z = g(x)$. Then the previous formulae can be represented by

$$Rz = \int y dx \text{ and } y : z = R : DT.$$

From the demonstration shown above, *DT* is shown to be the subtangent of *VIF*. Thus, $y : z = R : zdx/dz$. Therefore,

$$y = R\frac{dz}{dx}, \text{ i.e. } y = \frac{d}{dx}\int y dx .$$

This is certainly a form of the fundamental theorem of the calculus.

But, we must ask, is this truly the fundamental theorem of the calculus? That is, did Barrow obtain the core part of the differential and integral calculus? After having examined Barrow's *Lectiones geometricae*, Child [1916, 124] came to the conclusion that

> the theorem of §11 [*i.e.*, Lecture X, Proposition 11] is a rigorous proof that differentiation and integration are inverse operations, where integration is defined as a summation. Barrow not only, as is well known, was the first to recognize this; but also, judging from the fact that he gives a very careful and full proof [...], and in addition, [...], he takes the trouble to prove the theorem conversely, —judging from these facts, I say, —he must have recognized the importance of the theorem also.

Consequently, according to Child [1916, ix], "Barrow *had got the calculus*".[2]

Is Child's assertion correct? Surely Barrow was familiar with the symbolic algebra as represented by Oughtred's *Clavis mathematicae* [1631], as can be seen in his edition

1. See, also, Child [1916, 116-117]. There are some mistranslations in Child, which I have corrected, referring to the original Latin text.
2. The emphasis is in the original.

of Euclid's *Elements* published in 1655. But a very careful examination shows that Barrow's theorem in Lecture X, Proposition 11, was not in the form of the calculus, but rather it was totally geometrical. Barrow continued to admire the ancient Greek manner of geometry [Sasaki 1985].

Quite different from Barrow is Leibniz, who entertained from his boyhood the ambitious program of the *characteristica universalis*. Leibniz acquired his knowledge of Cartesian symbolic algebra through reading the second Latin version of Descartes's *Geometria*, edited and published by Frans van Schooten in 1659−1661, especially Van Schooten's *Principia matheseos universalis*, edited by Erasmus Bartholin and published originally in Leiden in 1651.

Leibniz's mathematical ability became enhanced following his encounter with Huygens in Paris. What he was encouraged to study in 1673 by Huygens was Pascal's geometrical works. He soon became familiar with Pascal's treatises, not in the geometrically rigorous Archimedean style of Pascal, but in the Cartesian style of algebraic analysis. In his posthumous manuscripts entitled "Historia et origo calculi differentialis", Leibniz describes the moment in which he saw a geometrical figure of Dettonville (a pseudonym of Pascal) in a new way leading to its algebraic formulation:

> Later on from one example given by Dettonville, a light suddenly burst upon him, which strange to say Pascal himself had not perceived in it. For when he proves the theorem of Archimedes for measuring the surface of a sphere of the solid formed by a rotation round any axis can be reduced to an equivalent plane figure. From it our young friend [i. e. Leibniz] made out for himself the following general theorem [Leibniz 1858, 399. For an English translation, see: Leibniz 1920, 38].

The first great fruit of his mathematical study in Paris was the arithmetical quadrature of the circle and some conic sections, the most known result of which was the Leibniz formula of

$$\frac{\pi}{4}=\frac{1}{1}-\frac{1}{3}+\frac{1}{5}-\frac{1}{7}+\ldots.$$ [1]

Toward the end of October 1675, a crucial step was taken towards Leibniz's infinitesimal calculus. In the manuscript with the title "Analyseos tetragonisticae pars secunda", dated October 29, 1675, he began to use the symbols ∫(italic letter of 'ſ') for 'summa' and *d* for 'differentia', and wrote that "then just as∫will increase, so d will diminish the dimensions. But, ∫means a sum, and *d* a difference" [Leibniz 1899, 155; for an English translation, see: Leibniz 1920, 82].

During the 1680s, Leibniz started to publish his essays on the new infinitesimal calculus in the newly established journal *Acta eruditorum* in Leipzig, the first of which was "De vera proportione circuli" in its issue of February 1682. As its title suggests, it was an announcement of the result of his arithmetical quadrature of the circle. In the issue of October 1684 of the *Acta eruditorum* appeared "Nova methodus pro maximis et minimis, itemque tangentibus", in which he published basic rules of his differential calculus. There the differential calculus was called an 'Algorithmus' [Leibniz 1858, 222].[2] As is now well-known, the Latin word 'algorismus' came from the Muslim mathematician Muhammd ibn Mūsā Al-Khwārizmi and was used for the art of calculation by Indo-Arabic numerals. Here Leibniz seems to have applied this vocabulary to his new symbolic calculation. 'Algorithmus' rather than 'algorismus' was used probably because he misunderstood it to have been derived from a Greek word, not an Arabic personal name.

1. The most detailed text on the arithmetical quadrature is available in Leibniz [1993]; With a French translation, Leibniz [2004].
2. Leibniz's papers on the differential calculus in the *Acta euditorum* were translated into French and published asLeibniz [1989]. The word 'l'Algorithme' appeared at page 110.

In “De geometria recondita et Analysi Indivisibilium at que infinitorum” in the June issue of 1686, after writing the formula “$d.\frac{1}{2}xx = xdx$” and its inverse "$\frac{1}{2}xx = \int xdx$", Leibniz paid attention to the fact that: “For, as powers and roots in common calculations, so for us sums and differences, namely ∫and *d*, are reciprocal” [Leibniz 1858, 231;Leibniz 1989, 137−138]. This was an announcement of the fundamental theorem of the infinitesimal calculus in the fully algebraic form. As Hofmann [1974, 194] put it, what Leibniz attempted at during the process of the invention of the new calculus was the ‘algebraization’ of infinitesimal problems. In the September 1693 issue, Leibniz published “Supplementum geometriae dimensoriae [...] similiterque multiplex constructio lineae ex data tangentium conditione”, where he demonstrated that “the general problem of quadratures can be reduced to the finding of a line that has a given law of tangency” [Leibniz 1858, 298; Leibniz 1989, 263; Struik 1969]. This is a statement of the fundamental theorem of the infinitesimal calculus.

The “calculus differentialis et integralis” in the terminology by Johann Bernoulli, Leibniz’s most influential follower, was thus nothing other than an infinitesimal extension of the Vietan and Cartesian algebraic analysis for finite quantities. This characterization was clearly made by Leibniz himself. In the “Historia et origo calculi differentialis”, he claimed that Viète's and Descartes's mathematics was “Geometria communisseu Apolloniana” and that his own calculus was “Geometria Archimedea” [Leibniz 1858, 393−394]. If we call Cartesian mathematics a form of ‘mathesis universalis’, the Leibnizian infinitesimal calculus was the Archimedean form of ‘mathesis universalis’.

Seventeenth-century Europe witnessed mathematical geniuses who were equally ‘geometra summo’ with Torricelli. Pascal was one ‘geometra summo’, and his mathematics was a kind of culmination in the Archimedean infinitesimal geometrical tradition. Leibniz saw Pascal’s works of the infinitesimal geometry with the new mathematical mode of thought which Viète and Descartes had cultivated. In Leibniz's own words in the “Historia et origo calculi differentialis”,

> it certainly never entered the mind of anyone else before Leibniz to initiate the notation peculiar to the new calculus by which the imagination is freed from a perpetual reference to diagrams, as was made by Viète and Descartes in their ordinary or Apollonian geometry; moreover, the more advanced parts pertaining to Archimedean geometry, and to lines which are called 'mechanics' by Descartes, were excluded by the latter in his calculus [Leibniz 1858, 393–394; for an English translation, see: Leibniz 1920, 25–26].

In a word, Leibniz was ‘algebrista summo’ *par excellence* from the very beginning of his mathematical career. It is sometimes pointed out that the basic mode of mathematical thought was transformed in the seventeenth century from geometrical to algebraic [Mahoney 1980]. With this transformation, the way of characterizing geometric curves changed, *i.e.*, the shifting of the foundations in geometry occurred with Descartes, among others [Molland 1976]. Consequently, Leibniz's novel calculus should be called most appropriately the ‘infinitesimal algebraic analysis’. In this sense, Leibniz was a mathematical revolutionary [Grosholz 1992], if we see a ‘conservative’ element in Torricelli's and Pascal's geometrical mode of thought.

Newton might be also called a mathematical revolutionary. But with his mathematical maturity, he became ‘conservative’. He, who began his mathematical study with Cartesian algebraic analysis and invented the method of fluxions, tended to return in the 1680s and later years to the standard of rigor in Barrow and Huygens, both classicists and geometers, and came to say that “Algebra is the Analysis of the Bunglers in Mathematics”.[1] What he admired in his last

1. Cited from Hiscock [1937, 42]. This passage was written in May 1708. The editor noted: “this must be a rare utterance from the supposedly humourless Newton”.

years was the analysis in the form of ancient Archimedes and Pappus.

In the differential and integral calculus, we should compare Leibniz to Kuhn's scientist hero in the chemical revolution, Antoine-Laurent Lavoisier, who proposed the oxygen theory of combustion instead of the old fashioned phlogiston theory. There was a revolution in mathematics. We cannot claim Torricelli, Gregory, and Barrow, who preserved the geometrical mode of thought, to have been discoverers of the fundamental theorem of the calculus. Certainly Newton was the first to discover the fundamental theorem, but in later years he became more conservative. It was Leibiniz who initiated an ‘analytical revolution’ in Boyer's terminology [1956, 218, 256, & 268] which found a pedagogical instrument in the opening of the École Polytechique soon after the French Revolution.

6. Truth in mathematics *vs*. truth in the natural sciences

How, then, can we characterize mathematical truth? Is it different from the truth in the natural sciences? If so, how does the former differ from the latter?

In *The Structure of Scientific Revolutions*, Kuhn has argued that the discovery of oxygen by Lavoisier was different from its intimation by Scheele and Priestley, both of whom continued to believe in the so-called phlogiston theory. “What Lavoisier announced in his papers from 1777 on,” wrote Kuhn [1962, 56], “was not so much the discovery of oxygen as the oxygen theory of combustion. That theory was the keystone for a reformulation of chemistry so vast that it is usually called the chemical revolution”. The phlogiston theory was discarded with the advent of Lavoisier's new theory of combustion.

What happens with revolutions in mathematics? As we have pointed out in Section 3, mathematical theories must be quasi-empirical and time-dependent. They are not totally free from the real natural world. But, at the same time they are not much restrained empirically or ontologically by the real natural and life world, to use Edmund Husserl’s philosophical term. Mathematical truth is at most conditional truth. In today’s understanding, “mathematics can be regarded as the science of possible idealized structures”, to use Paul Bernays’s [1974, 603] characterization of mathematics. With mathematical revolutions, the new theory must become fashionable, and the old theory may become unfashionable, but not totally discarded. For example, with Leibniz's analytical revolution, the infinitesimal calculus became very influential and much used by mathematicians, but truth values of the infinitesimal geometry by Torricelli and Pascal, for example, were preserved. In this sense, revolutions in mathematics are not so radical as in the natural sciences.

Truth values in mathematics are ordinarily kept, since mathematics is not relatively restricted ontologically. The natural sciences, however, are concerned with the real empirical world, and thus ontologically restrained to a much greater degree than in mathematics. This is why older theories in the natural sciences are often simply discarded, for example, the phlogiston theory. But, some theories continue to be studied, such as Newtonian mechanics. We should remember that even some political revolutions were not destructive. For instance, the so-called Puritan Revolution during the 1640s−50s in England certainly was accompanied with a civil war killing the King Charles I, but, the Glorious Revolution of 1688−89 was glorious because of an absence of violence [for the evolution of the word ‘revolution’, see Rachum 1999].

Kuhn characterized revolutions in the natural sciences as a shifting of basic paradigms. Aristotelian and Newtonian physics differ in their paradigms, or hermeneutic bases. In the Kuhnian interpretation, Aristotelian physics was not simply false, for it can be understood in a certain sense, *i.e.*, in a hermeneutic framework. As Kuhn [2000, 216−223, esp. 221] has admitted both similarities and differences between the natural and human sciences, we have to recognize similarities between the natural sciences and mathematics (principally because they are both a historical product) as well as differences. Admittedly, there may be a reason to state with the Crowe of 1992 that “The question of whether revolutions occur in mathematics is in substantial measure definitional” [Crowe 1992, 316]. Nevertheless, it must be unnatural and odd for us to admit the conception of revolution in the natural sciences and not to admit that in mathematics, for both are a historical product of human intellectual activity and both have certain hermeneutic bases.

In sum, both in the natural sciences and in mathematics, revolutionary changes occur. There exist differences between the two fields of knowledge. On one hand, in the natural sciences, which are empirical and ontologically restrained by the real natural world, revolutionary changes insulate the old and new theories from each other. On the other hand, in mathematics, which is conditional and hypothetical, thus ontologically not so restrained by the real world, in revolutionary transformations the losses of the former theory are not so great, and its properties are usually transmitted through translations of mathematical ‘languages’, or by shifting the modes of mathematical thought. Nevertheless, there must be incommensurable elements between the old and new mathematical theories. Contents of mathematical theories cannot be separated from their forms and other meta-contentual factors. Both are, *pace* the Crowe of 1975, mutually interwoven. Therefore, there exist revolutions even in mathematics.

7. Conceptions of revolutions in mathematics

We cannot deny any longer that there have existed revolutions or drastic transformations in mathematics. Now we distinguish four patterns in the development of mathematical knowledge: It develops not only both in content and in its institutions, but it is also develops either gradually or by revolution.

(1) There can be gradual changes of mathematical knowledge; we do not need to explain this type of change, for most mathematicians must believe that mathematics has usually developed gradually through discoveries step by step.

(2) There can be revolutionary changes in fundamental conceptions, and forms or mathematical mediae. We use the word 'revolutionary' for this kind of change, since mathematics has sometimes progressed by drastic changes of its fundamental conceptions and forms, experiencing strong resistance. In these junctions, it should be noticed that mathematical content and the so-called outer-mathematical forms (*e.g.*, modes of thought, epistemological presuppositions, or symbolisms) are mutually inseparable. We refer to some examples for this category of change: The emergence of axiomatics in ancient Greece, the formation of the program of algebraic analysis by Viète and Descartes and of the new infinitesimal analysis by Newton and Leibniz in seventeenth-century Europe, and the discovery or invention of non-Euclidean geometries by Gauß, Bolyai János, Lobachevsky, and Riemann, mostly in the first half of the nineteenth century. These revolutionary changes came through drastic alternations of mathematical ideas as well as through elaborating mathematical techniques.

Two other categories of mathematical development concern institutional changes.

(3) In general, institutional and social settings in which mathematical activities are carried out develop gradually.

(4) In some cases, however, revolutionary transformations occur in institutional circumstances. We count several examples for this category of external revolutionary change: The systematic introduction of Greek mathematics

into Islamic civilization in Baghdad during the Ninth and Tenth centuries, the translation movements of mathematical works in the Twelfth-Century Renaissance and during the Italian Renaissance, and the drastic institutional transformation of mathematical education soon after the French Revolution. Along with the last revolutionary change, the analytical revolution by Monge and Lacroix took place at the École Polytéchnique. This was a continuation of the Leibnizian program in infinitesimal algebraic analysis.

Finally, we should refer to the transformation of mathematical culture from traditional to modern Western mathematics in Japan in the second half of the nineteenth century. Before the Meiji Restoration of 1868, Tokugawa Japan cultivated the highest form of the mathematical tradition called *wasan*, having been originated in China. But with the adoption of Western arithmetic for the centralized school system in 1872, and the establishment in 1877 of the University of Tokyo and of the Tokyo Mathematical Society, Japan's mathematical system became almost totally European, except counting with the abacus called *soroban* [Sasaki 1994 & 2002]. This kind of drastic transformation associated with shifts of fundamental social and cultural structures took place in the other East Asian countries, China, Korea, and Vietnam, as well. But the case of Japan was the most radical. In this type of change, institutional circumstances have overwhelmingly determined the further course of the development of mathematics. They were like a massive, slow-moving Juggernaut. In the original Kuhnian formulation, incommensurable elements, thus revolutions, are mostly admitted in diachronic developments of the knowledge concerned. Now we should recognize them in geographically diverse cultures, as well.

8. Concluding remarks

In his essay entitled "What Are Scientific Revolutions?" reconfirming the argument in *The Structure of Scientific*

Revolutions, Kuhn introduces the distinction between the two types of scientific development, normal and revolutionary, and states:

> Revolutionary changes [...] involve discoveries that cannot be accommodated within the concepts in use before they were made. In order to make or to assimilate such a discovery one must alter the way one thinks about and describes some range of natural phenomena [Kuhn 2000, pp. 14-15].

As an example of this sort of discovery, Kuhn refers to the discovery of Newton's second law of motion. He must also have had in mind other examples, such as Lavoisier's discovery of oxygen along with the modern theory of combustion.

So far we have argued that there exist revolutions in mathematics, but that the degree of destruction in mathematical revolutions is much lower than in the natural sciences. Kuhn's differentiation of the types of scientific development between normal and revolutionary corresponds to our (1) gradual development, and (2) revolutionary development in our categorization of the development in mathematical content.

The point of our argument has been that Crowe’s distinction between ‘transformational’ and ‘formational’ discoveries is not sustainable in the history of mathematics. The main reason for this is that we may not strip the content of mathematics from modes of mathematical thought. We examined this in the case of the fundamental theorem of the differential and integral calculus. What we can call the fundamental theorem was proposed only in the algebraic form by Newton and Leibniz, not in the purely geometrical form by Torricelli, Gregory and Barrow.

With the formation of non-Euclidean geometry, the truth value of Euclidean geometry was retained in a certain sense, but not totally. Euclid’s geometry with the parallel postulate in ancient Greece had a different meaning after the publication of Hilbert's *Grundlagen der Geometrie*. A similar phenomenon can be observed in Newtonian mechanics after Einstein presented his theory of relativity. With some modifications, Kuhn's observation quoted above can be applied to the history of mathematics as well. Thus, we have to admit revolutions in mathematics *mutatis mutandis* as in the natural sciences.

In the same essay, Kuhn recalled a moment in the summer of 1947 when he conceived the notion which he later called 'incommensurability', for example, between Aristotelian and Newtonian physics, and then the idea of 'scientific revolutions'.

I turn to my autobiography again. I myself conceived a certain sense of eschatology in contemporary mathematics when I listened to some pieces of twelve-tone music by Arnold Schönberg and Alban Berg with a friend at the Mathematical Institute of Tohoku University in Sendai, Japan, in 1968. Then, I was an undergraduate student aged twenty one studying contemporary abstract algebra in the formalist style of Hilbert and Bourbaki. It was a kind of emptiness of meaning that I sensed both in abstract algebra and in music, a branch of the mathematical sciences since the time of Pythagoras. Since then, to me the 'style' of music and 'style' or 'mode' of mathematical thought have seemed crucial. This experience was before my encounter with Kuhn's work of 1962. In 1971, I certainly read *The Structure of Scientific Revolutions* in the Japanese translation of its second enlarged version with a considerable sympathy, and wrote a review of it, in which I was not totally favorable to it, since it concentrated on internal aspects of scientific activities, ignoring their external aspects.

In my Princeton years between 1976 and 1980, I began to entertain a version of Kuhn's program of historical philosophy of science for my own field of history of mathematics. In May of 1977, I submitted my preliminary report entitled "Thomas S. Kuhn's Theory and the History and Philosophy of Mathematics" for his course "Introduction to

Philosophy of Science”. Kuhn wrote to me his critical commentary upon it: “This is a very good topic, and I'm interested in what you've said about it. [...]. I think they [revolutions in mathematics] must exist, but I am as puzzled as anyone else to account for the extent to which mathematics seems to preserve all its old theorems”. Despite Crowe’s claim of 1992, I would like to continue to use the word of ‘revolution’ in mathematics, following Kuhn.

I don't know whether Prof. Kuhn would have finally supported my present notion of revolutions in mathematics. In *The Essential Tension: Selected Studies in Scientific Tradition and Change*, published in 1977, he wrote his dedication “For K. M. K., still my favorite eschatologist” on the front page. Many historians of mathematics have rejected Kuhn’s conceptions in *The Structure of Scientific Revolutions*. We should certainly avoid the uncritical use of conceptions such as ‘crisis’ and ‘revolution’. Yet we now are living on the horizon that Kuhn’s historiography of science opened. From my perspective it seems incontestable that there are revolutions in mathematics in the scholarly and strict sense of the word. For this reason, a program of ‘historical philosophy of mathematics’ should be pursued as an extension of Kuhn’s ‘historical philosophy of science’.

Acknowledgement

On the same page of his dedication "For K. M. K., still my favorite eschatologist" of my own copy of *The Essential Tension*, Prof. Thomas S. Kuhn kindly wrote to me in Tokyo on May 2, 1986: “For Chikara Sasaki: Many warm memories of and thanks for several important days. Tom Kuhn”. He is my lifelong teacher of the history and philosophy of science. I say many thanks to the late Prof. Kuhn. Also, I am warmly grateful to Prof. Lewis Pyenson of Western Michigan University for revising the present English text.

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