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Robber barons and politicians in mathematics: a conflict model of science*

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Abstract. This paper argues for a conflict sociology of science that contrasts with idealist and functionalist sociologies of science, notably championed in the works of R.K. Merton and T.S. Kuhn. The basis for the argument is an analysis of major scandals and controversies in the history of mathematics: the conflicts between Cardan and Tartaglia; Newton and Leibniz; and Cauchy, Abel, and Galois take place in the "robber baron" era of mathematics. The conflict between Cantor and Kronecker marks a transition to the era of "saintly politicians." Each conflict represents a watershed in the history of the social organization of mathematics, and a transition to new competitive conditions.

Résumé. Cette étude défend le principe qu'il existe une atmosphère de conflit au sein de la sociologie des sciences qui contraste avec les sociologies des sciences de type idéalistes et fonctionnelles, dont notamment R.K. Merton et T.S. Kuhn se font les champions dans leurs oeuvres. L'argument de base de cet abrégé est fondé sur une analyse des scandales et des controverses majeures de l'histoire des mathématiques: les conflits enter Cardin et Tartaglia, Newton et Leibniz, et Cauchy, Abel et Galois trouvent leur place qui leur reviennent au cours de l'ère des "nobles voleurs" des mathématiques. Le conflit entre Cantor et Kronecker opère une transition dans l'ère des "politiciens aux allures de saints." Chaque conflit représente une ligne de faîte dans l'histoire de l'organisation sociale des mathématiques, et une transition vers de nouvelles règles d'émulation.

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According to one long-standing approach to the sociology of science, scandals and injustices reflect the institution of science "acting" to support its normative structure. In this idealized interpretation, scientists are portrayed as actors guided by "norms of science" such as: (1) disinterested pursuit of knowledge; (2) public recognition of individual contributions; and (3) communal possession of intellectual property, or communism (Merton, 1957: 551-561; Barber, 1952: 122-134; Parsons, 1949: 343-345). The norms themselves are called on to explain deviations from the norms. Thus, priority disputes exhibit the commitment of the scientist to the value of knowledge, and the scientific community's concern for awarding credit where credit is due. Cases in which famous scientists receive more credit than novices for similar work show that the institution protects itself from fragmentation by focusing on certain leading figures and ideas (Merton, 1973: 286-324; Cole and Cole, 1973).

Kuhn's (1962; 1970) model of scientific paradigms and revolutions shifts the idealist line of argument slightly. His work has often been viewed as a radical and relativist alternative to idealized sociology of science; in fact, there is considerable rapprochement between Kuhn and Merton (cf. Merton, 1977: 71-109). For Kuhn, resistance to new ideas and discoveries is not a violation of the commitment to knowledge, but rather evidence for consensus maintenance, a necessary condition for unhampered, routine puzzle solving in normal science. Kuhn, like Merton, interprets deviations from the norms of science in the best possible light. Moreover, the entire *social* mechanism of Kuhn's model is designed to explain scientific conservatism; scientific revolutions are not due to social causes but to the sheer accumulation of empirical anomalies that finally force the introduction of a new paradigm. In spite of socially patterned lags, science is — according to Kuhn — a viable institution for discovering empirical truths.[1]

We propose a different approach to understanding scandals and injustices in science. Major scandals and controversies reveal significant historical shifts in the social organization of science. There is no enduring set of norms that guides the behavior of scientists. What does endure is the activity of scientists (and related types of intellectuals) pursuing rewards such as wealth, fame, and the power to control the flow of ideas and to impose their own ideas on others. The organization of the scientific community determines the nature of the reward system. Under some conditions, ideas are considered most useful when held as *secret* resources; they can then be the basis for prestigious cults or used as weapons in competitive situations. In some cases,

1. This view of Kuhn is discussed at length in Restivo (1983). Restivo opposes the view defended by Barnes (1982) that Kuhn has made "one of the few fundamental contributions to the sociology of knowledge," and that his work is an important point of departure for sociologists of scientific knowledge.

egotistical scientific "robber barons" appropriate or suppress the ideas of other scientists in order to build new or maintain old, dominant organizations and intellectual systems. In other cases, "saintly," community-conscious scientists meticulously recognize the contributions of peers, and subordinate themselves to the ideal of scientific progress.

Scientific behavior is a variable. The ideals of science do not *cause* scientific behavior, but emerge from the struggle for individual success under different conditions of competition. Norms of publicity or secrecy, individual or communal intellectual property, recognition of priority or ruthless self-aggrandizement emerge under specific organizational conditions. The great scandals in the history of science occurred precisely because organizational conditions were changing. Commonplace behavioral patterns become less and less appropriate as the nature of the resources available for competition changes. Our study of certain major scandals in the history of mathematics can thus be likened to looking into the fault-lines between geological eras.

Scientific change is not the result of established paradigms breaking down occasionally under the pressure of accumulated evidence. The Kuhnian model relies too heavily on the *conservative* nature of social organization in science, and on *empirical findings* as "agents" of change. In mathematics, opposition to innovators has not usually come from conservative defenders of old paradigms but from rival innovators. Innovations in mathematics have not been stimulated by the accumulation of empirical (or logical) anomalies, but rather by the drive to find general rules that might hasten the solution of problems. This is not the "puzzle-solving" of Kuhn's normal science. Mathematicians are fond of posing puzzles for each other, but not because they possess a paradigm for solving them. On the contrary, they challenge each other by picking out puzzles that are too difficult for existing concepts and methods. Innovations and revolutions are rooted in the social structure of intellectual competition.

In Kuhn's model, innovation is unpredictable; our view is that the probability of innovation varies according to the organizational conditions of scientific competition. Certain minimal levels of competition produce a continuous drive towards new ideas. When organizational resources shift and new forms of competition emerge, extraordinary changes in mathematical ideas occur. The analysis of scandals reveals these aspects of mathematical change. Mathematics is the theoretical core of most empirical sciences that have reached any level of complexity. Thus, if mathematics reveals the dynamics of theoretical competition in more or less pure form, then it may be a model for innovation in all sciences insofar as they are theory-driven.

We begin by examining cases in the robber baron era of the history of mathematics: Cardan and Tartaglia (the 1540s), Newton and Leibniz (1670-1730), and Cauchy, Abel, and Galois (1826-1832). Our study of Cantor and Kronecker in the late nineteenth century focuses on the transition from egotistical, robber baron competitiveness to the conflicts among schools characteristic of twentieth-century mathematics. These schools are led by "saintly politicians" who emphasize the collective and non-egotistical side of science. Each case represents a transition to new competitive conditions.[2]

Robber barons: Cardan vs. Tartaglia

In the early 1500s, mathematics contests were popular in the commercial cities of northern Italy (Smith, 1958: ch. 2, 454-464; Ball, 1960: 217-226; Gliozzi, 1971; Jayawardine, 1971; Masotti, 1971; 1976). Mathematicians issued public challenges, often with money stakes for the winners. The teaching of commercial arithmetic was rapidly expanding at this time, and the public contests enabled rival teachers to secure fame and attract pupils. Problems called for particular numerical solutions, but sometimes required solving higher-order algebraic equations.[3] Contest results were made public, but problem-solving methods were kept secret, since they were valuable resources in the struggle for individual reputations and incomes.

In the 1530s, the medical doctor, astrologer, gambler, and brawler Girolamo Cardano (Cardan) was in Milan, a little over one hundred miles from Venice. Mathematical contests were becoming popular at the Milan court and with the Cardinal of Mantua, a town midway between Venice and Milan. Cardan, barred from practicing medicine following a dispute with the local college of physicians, was eking out a living teaching and writing on practical arithmetic. He heard of a Venetian mathematics teacher, Niccolo Tartaglia, who had won mathematical duels with Zuanne da Coi (Colla) and Fiore by solving two cubic equations: $x^3 \pm bx = c$ and $x^3 + ax^2 = c$. Fiore had the first equation, which had been bequeathed to him by his teacher, Scipione del Ferro, but not the second. Upon hearing of Tartaglia's victories, Cardan invited Tartaglia to Milan by posing as a wealthy aristocrat offering patronage. This was an attractive offer to the penurious Tartaglia, and he

2. We want to stress that our focus in this paper is on scandals and controversies as watersheds in the organizational history of mathematics. We stop short of a full-fledged constructivist interpretation of mathematical knowledge for practical and not intellectual reasons. We note that research and theory in the sociology and social history of mathematical knowledge has been undergoing a renaissance in recent years: see in particular Bloor (1976), Bos and Mehrtens (1977), and MacKenzie (1981). For an introduction to the historical development of, and contemporary research and theory in the sociology of mathematics, and the relationship between mathematics and the sociology of knowledge, see Restivo (1981a, 1982).

3. For example: "Divide 10 into 3 proportional parts so that the product of the first and second is 6," a problem posed by Colla to Cardan in 1540. Such problems were stated in words — algebraic symbolism did not yet exist. In modern terms, if one defines the mean of the three terms as x, the solution is implied in the equation $x^4 + 6x^2 + 36 = 60x$.

must have been rather disillusioned with Cardan's imposture when he arrived in Milan. But after a good deal of pressure from Cardan who was, by his own admission, prone to violence, Tartaglia eventually revealed his formula. At first he disguised it in a cryptic verse, but later he added a full explanation — after swearing Cardan to secrecy. Cardan then used this secret in mathematical contests such as the ones in which he accepted challenges from Colla.

In 1542, Cardan met Scipione del Ferro's son-in-law, Annabale della Nave, who had inherited Scipione's teaching position at Bologna. He revealed to Cardan (presumably during a braggardly dialogue) that Scipione had, sometime during the early 1500s, discovered the same formula Cardan now possessed. Cardan used this fact to justify breaking his promise to Tartaglia. In 1545, he published the solution for cubics in a mathematical book, *Ars Magna*. Cardan credited Ferro with the discovery, and noted that Tartaglia had discovered the same solution ("in emulation of" Ferro) in his contest with Fiore. This was not strictly true; Ferro had solved the special case $x^3 \pm bx = c$, whereas Tartaglia had discovered (and revealed to Cardan) the solution for $x^3 + ax^2 = c$. Tartaglia was furious and published the solution himself under his own name the next year in his *Inventioni*, along with a vituperative attack on Cardan's perfidy. A series of angry exchanges followed in which Cardan's assistant Ferrari wrote to Tartaglia accusing him of plagiarism and of making unjust accusations against his master. It was finally agreed to settle the matter in the traditional manner, by a mathematical duel. The contest took place in 1548 on Cardan's home territory, in a Milan church with the governor of the city acting as judge. Ferrari appeared on Cardan's behalf. Tartaglia eventually withdrew, claiming that Cardan's boisterous supporters did not give him an opportunity to state his case. Ferrari was declared the winner.

Cardan earned most of the credit for the cubic solution. The solution became known as "Cardan's Rule," partly because Cardan had published in Latin, the scholarly language.[4] Tartaglia had published in Italian and presented his case in an addendum to a practical book on ballistics, compasses, surveying, and related topics. Cardan came from a wealthy family, and studied and taught at universities; he became famous throughout Europe for his

4. Ironically, Scipione del Ferro, the probable discoverer (although he may have had it from a secret source) got little posthumous fame, even though Cardan explicitly credited him in *Ars Magna*. Cardan's motivation, more than likely, was to undercut Tartaglia, who was his main living rival. In fact, one might formulate this as a general principle: intellectuals are most likely to give priority credit to third parties in order to dispute the claims of a close and current rival. In his confessional autobiography published twenty-five years later, Cardan (1962: 225-226) says that he got the first part of his *Ars Magna* from Tartaglia, and makes no mention of Ferro, or of Ferrari's contributions.

medical practice and writings. Tartaglia, by contrast, had no formal education and subsisted by teaching commercial arithmetic. Given these differences, it is not surprising that Cardan's work is much more general and theoretical than Tartaglia's. Cardan clarified the significance of the new solution. He generalized the cubic solution beyond the special cases dealt with by Scipione and Tartaglia by carrying out a linear transformation to eliminate the second degree term in equations of the form $x^3+-ax^2+-bx=c$. He made the general observation that an equation of degree higher than the first has more than one root, and noted the relation between roots and coefficients of equations, and between the succession of signs of terms and signs of roots. Whereas earlier European mathematicians had sought only numerical solutions, Cardan initiated work on the general theory of algebraic equations.

The controversy between Cardan and Tartaglia reveals the transition from a situation in which secrecy was normal to one in which it was normal to share intellectual property. There was nothing unusual about Tartaglia's, Ferro's, and Fiore's concerns for secrecy, or Cardan's subterfuge in prodding the secret from Tartaglia. Fiore (and perhaps Ferro) made a living by winning contests with methods appropriated from others. Cardan's advantage over his rivals was the result of his decision to *publish* the solution for cubic equations. Unlike most of his mathematical rivals, Cardan had an orientation to publishing scholarly books. Before turning his attention to mathematics, Cardan had written treatises on medicine and astrology. In effect, Cardan shifted the competitive scene from mathematical contests to an arena in which the printed word became the basis for establishing reputations. Cardan's rivals were furious because he revealed privately held solutions that they relied on for winning contests and for subsistence. But this shift from mathematical contests to books stimulated the development of mathematics by creating conditions conducive to the development of *general* rules for solving problems.

Cardan deviated from the norm of secrecy, but he continued the tradition of property relations dominant in his time. He can be described as a robber baron in an era when competition among private, commercially oriented households of mathematics was giving way to intellectual competition in print over more generalized and increasingly abstract matters. There are other indications of intellectual piracy in the Cardan-Tartaglia case besides those we have mentioned so far. Cardan published scientific materials similar to unpublished works of Leonardo da Vinci. Duhem and other historians suspect that Cardan used da Vinci's notes, which he could have received from his father, a friend of da Vinci's at Milan a generation earlier (Gliozzi, 1971: 66). Tartaglia published as his own a thirteenth-century translation of Archimedes made by William of Moerbeke. He also claimed as his own practical techniques (such as the procedure for raising sunken ships) developed by others; and he took credit in print for the solution to the problem of the equilibrium of a body on an inclined plane that he had found in a manuscript by Jordanus de Nemure (Masotti, 1976: 260). That this sort of activity was not uncommon is illustrated by the behavior of other intellectuals of this time. For example, in 1494, Pacioli freely borrowed from earlier unacknowledged sources in writing the major Italian mathematics book of this period (Smith, 1958: 252-253).

Violence was also a part of the cultural scene. The brawling between Cardan and Tartaglia drove Ferrari out of Cardan's household. Ferrari was poisoned by either his sister or his brother-in-law; one of Cardan's sons was executed for murdering his wife; and Cardan cut off the ears of another son for some offense. The same type of morality carried over into the intellectual affairs of Cardan and his rivals.[5]

For all that we have noted, the competition was propitious for intellectual advance. The competition among Colla, Tartaglia, and Fiore not only spurred the rediscovery and extension of the cubic solution, but gave rise to a rapid escalation in intellectual standards. By 1540, a particular case of the biquadratic had been posed by Colla and solved by Ferrari (Cajori, 1974: 126). Cardan was a systematizer and generalizer, and founded the abstract discipline of the theory of equations. His behavior, and the new competitive milieu it reflected, signaled the beginning of an important period of mathematical advances.

Leibniz and Bernoulli vs. Newton

Challenges continued to play an important part in mathematics after the time of Cardan and Tartaglia. The mathematics chair at the Collège Royale in Paris was established in 1576 with the stipulation that the incumbent was to be replaced by any challenger who beat him in a public contest (Hall, 1980: 3). Descartes was allegedly recruited into mathematics in 1617 when he saw a placard in Breda, Holland that issued a challenge to solve a geometrical problem (Ball, 1960: 260-270). Later, Pascal, Leibniz, Newton, and Bernoulli participated in famous challenges. But the social context of such challenges was changing. Instead of commercial mathematics teachers building their public reputations to attract students, mathematicians were becoming more concerned with attracting royal patronage. Vieta, who worked out much of the basis of modern mathematics, resided at the French court in the 1590s and made his reputation by answering challenges (Ball, 1960:

5. Cardan's overriding aim was to make his own reputation and mathematics meant much less to him than his successes in medicine, gambling, and astrology. In his autobiography of 1570 (Cardan, 1962), he describes himself as having received more commendations in his lifetime than Aristotle or Galen, and shows the greatest contempt for the intelligence of those (e.g., Tartaglia) he thought of as enemies. By modern standards this is an astoundingly egotistical attitude.

229-230). In the 1660s and thereafter, royal patronage for science began to be institutionalized in academies: the English Royal Society (1662); the French Académie des Sciences (1666); the Prussian Academy of Science (Berlin, 1700); and the Russian Academy at St. Petersburg (1716). Commercial arithmetic teachers dominated sixteenth-century mathematics; seventeenth-century mathematics witnessed the rise of mathematicians in academies and university professors of mathematics and astronomy. Barrow, and, subsequently, Newton at Cambridge were among the most prominent of the university mathematicians, along with Wallis at Oxford and Gregory at Edinburgh. But this was a period of declining enrollments and intellectual activity at universities, and courts and academies were the main centres of scientific activity (Collins, 1981).

A second important organizational change was in progress at this time. The book-publishing industry was developing. In the sixteenth century, a small number of books were published which were devoted to or at least included some mathematics. More efficient and specialized networks of scientific communication emerged in the seventeenth century. Individuals like Mersenne in Paris in the early seventeenth century, and Henry Oldenburg and John Collins in London later on, served as informal "message centres." By keeping up an active correspondence with scientists and mathematicians both in their own countries and abroad, they were able to inform an "in-group" of interested persons of current intellectual developments. At the same time that ad hoc patronage was being transformed in the 1660s and 1670s into regular appointments at royal academies, the informal correspondence network began to be replaced by the first scientific journals (Price, 1975: 165). These two organizational changes are the context for the next mathematical controversy we discuss (Hall, 1981; Broad, 1975; Hofmann, 1972; 1973; Cohen, 1974).

During the mid-1600s, a number of mathematicians had made advances in dealing with infinitesimals while working on squaring the circle, measuring curvilinear figures, problems of motion, and algebraic series. In the years after 1665, the young Cambridge mathematician Newton developed a general method in what we know as the calculus. He apparently did not have a clear idea of its importance, and worked with a clumsy and unsettled notation. In 1669, Newton sent Collins, at his request, a rather obscure paper on the subject; shortly thereafter, he began but failed to finish a longer treatise on his "method of fluxions." Newton was more interested in publishing his theory of optics (in the *Philosophical Transactions of the Royal Society*). But this work was widely criticized by Newton's peers, and this prompted him to withdraw from scientific activity for a number of years to pursue interests in theology and alchemy (Westfall, 1980).

In 1672, Leibniz arrived in Paris as a young diplomat in the service of a German prince. Leibniz was educated in law and philosophy; he knew virtually no mathematics. He was, however, very ambitious, and had already framed a proposal for reforming all intellectual discourse on the basis of a universal logical symbolism. There was a great deal of interest in science in Paris, fostered by the new Académie des Sciences. In this atmosphere, Leibniz established personal connections with leading scientists, and learned mathematics from Christian Huygens and others. He visited London in 1673 as a member of a diplomatic mission, and quickly associated himself with scientific circles there. On the strength of his invention of a rudimentary calculating machine, Leibniz was elected to the Royal Society. At the same time, he made an unfavorable impression on many scientists and mathematicians by making unsupported claims about his accomplishments and proposing an algebraic series for squaring the circle already published by several mathematicians. Such rash pronouncements prevented Leibniz from being appointed to a position at the Collège de France in 1675 (Hofmann, 1973: 161). Nevertheless, he became part of Oldenburg's and Collins' correspondence network, and inquired about the work of English mathematicians. With Oldenburg and Collins as intermediaries, Newton and Leibniz exchanged letters in 1676 and 1677. Leibniz managed to persuade Newton to send him a description of his work on infinite series. Apparently suspicious about Leibniz's motives, Newton mentioned his fluxional calculus in a single cryptic sentence in the form of an anagram. This same strategy had been employed by Tartaglia in his initial response to Cardan's inquiries about the secret formula for the cubic equation.

Leibniz did not get much direct information from Newton. Yet it was precisely during this period when reports of English achievements in science and mathematics were circulating that Leibniz rapidly perfected his own calculus, using a clearer and more useful notation than Newton's He described this work to Newton, but Newton did not consider it significant. Because Leibniz was still in many ways a novice, Newton may have underestimated his mathematical abilities.

Leibniz left Paris to enter the diplomatic service of an important German statesman, the Duke of Brunswick. As a result, in part, of Leibniz's genealogical researches and diplomatic maneuvers, his employer was elevated from Duke to Elector of the Holy Roman Empire in 1692, and eventually became heir to the English throne (succeeding as George I in 1714). During his travels, Leibniz made important contacts in the rising German state, Prussia, as well as with the emperors of Russia and Austria. Leibniz became a respected and successful politician at several courts. His political connections and intellectual reputation reinforced each other. In 1682, the first specialized intellectual journal in Germany, *Acta Eruditorum*, was founded at Leipzig by members of Leibniz's intellectual circle, in emulation of the Académie des Sciences' *Mémoires* and the Royal Society's *Philosophical Transactions*. Now that he controlled a publication independent of English and French influences, Leibniz published the algebraic series he had bragged about in London without citing any predecessors.

In 1684 and 1686, Leibniz published brief descriptions of his calculus and suggested that it opened up a new epoch in the history of mathematics. His exposition was highly compressed, but it revealed the programmatic value of his method. The Swiss mathematician Jakob Bernoulli (Professor at Basle) and his younger brother Johann promptly recognized the power of Leibniz's method. The superiority of the new calculus quickly became known among Continental mathematicians through a series of challenges published in *Acta*. A Parisian nobleman, the Marquis de l'Hospital, hired Johann Bertoulli to teach him the new method. In 1696, l'Hospital published the first calculus textbook and became the leader of a rapidly expanding group of French mathematicians. Leibniz published relatively little in mathematics, but through his correspondence with the Bernoullis, l'Hospital, and many others, he became known as the leading mathematician in Europe. He enjoyed a similar reputation in philosophy as a result of his extensive correspondence with Arnaud, Bayle, and other leading intellectuals; this was so even though his philosophical works were not to appear in print, for the most part, until after 1710.

During most of this time, Newton remained comparatively obscure. Cambridge had rapidly deteriorated as an intellectual centre (Stone, 1974). Oldenburg and Collins were dead, and Newton was isolated from the London intellectual community. Newton's reputation rebounded after he published his synthesis of terrestrial and astronomical physics in the *Principia* (1687). Shortly thereafter, Newton left his seclusion to become a vehement advocate at Cambridge of the 1688 revolution, agitating against the danger of a Catholic restoration and representing the university in Parliament. In 1690, rewarded with a London post as Warden of the Mint, he left Cambridge for good. As Britain worked out a limited monarchy and a parliamentary party system during the next decade, Newton's popularity as England's leading intellectual grew. In 1703, he became president for life of the Royal Society. In the mid-1690s, Newton's nationalistic followers began to defend his claims to priority in the calculus, and to attack Leibniz. Under pressure from his advocates, Newton finally published his old papers on the fluxional calculus in an appendix to his book on *Optics* in 1704 and again in 1711.

As the attacks against him escalated, Leibniz retaliated by publishing (anonymously) a review of *Optics* in *Acta* that supported his own claims for priority. Subsequently, a letter by Johann Bernoulli accusing Newton of plagiarism was published anonymously in *Acta*. Leibniz and Bernoulli behaved courteously to Newton in their public statements, but carried on a covert attack. There was probably a political motive operating in this controversy. The settlement of royal succession negotiated between the English parties in 1701 placed Leibniz's employer, the Elector of Hanover, in line to inherit the

English throne; it was therefore important for Leibniz not to alienate himself in English political circles. Conversely, the attacks by Newton's English supporters on Leibniz and the Continental science establishment escalated at just the time that their political position within England improved; they may have felt threatened by the possibility that Leibniz's well-organized Continental machine might be brought to London under royal patronage.[6]

The Newton-Leibniz dispute became a matter for official investigation. In 1713, Newton rigged the report of a Royal Society committee (which included token representation from the international diplomatic world) in his favor. Leibniz and Newton eventually accused each other of plagiarism, misrepresented the facts of the case, and wrote supposedly impartial vindications under the cover of anonymity. The behavior of their partisans was even worse. The result was a major split between English and Continental science. Newton's physics was attacked by the Leibnizians as a quasi-religious system containing "occult" qualities (the force of gravity), and hence a retreat from Cartesian materialism to the metaphysics of the Middle Ages — in short, it was seen as a return from the intellectual position of the liberal regimes to that of the reactionary clerical ones.[7] Newton's physics eventually made its way into Holland in the 1720s, and France in the 1730s, but Germany held out for the Leibnizian position to the end of the century. The British stayed faithful to Newton's fluxional calculus until the early 1800s, cutting themselves off from the major mathematical developments of an entire century.

The sociological significance of the Newton-Leibniz controversy is not a simple matter of priority or simultaneous discovery. The notion that it is the sheer logic in the development of ideas that accounts for multiple discoveries is an idealist rather than a sociological position. What we see in the cases examined so far are situations involving intense competition among ambitious individuals. As in many other cases in the history of science, *the very fact that a problem is explicitly posed and that a solution is known to exist* is crucial for stimulating intellectual advance. Although the problem of the

6. See Thackray, 1970-1971.

7. We cannot pursue the deeper connections between mathematics, theology, economics, politics, and social issues hinted at in this paragraph. For important insights on these connections see, for example, Merchant (1980: 275ff.), and Restivo (1981b). It should be noted that the idea that Newton and Leibniz "invented" *the* calculus needs to be qualified in two ways. First, and more trivial, their work built on centuries of work, paralleled in many respects the work of their contemporaries (e.g., Barrow), and was followed by further work on what is best viewed as a developing mathematical idea and system, rather than a static concept discovered or invented "once-and-for-all" at a specific point in historical and cultural time and space. Second, and more important, there was not *one* calculus. The systems of Newton and Leibniz were rooted in different philosphies of nature and ultimately in two different world views (see Hall, 1980; Shapin, 1981).

cubic equation had defied solution for millenia, within a few years after the Tartaglia-Fiore contest in 1535 the cubic solution was generalized beyond special cases; and the biquadratic was posed, solved, and generalized through the competitive activities of Cardan, Colla, Ferrari, and Bombelli. The social situation that produced Leibniz's rather arrogant ambition was crucial for stimulating the advance from the fragmentary efforts of earlier mathematicians to a general, programmatic formulation of the calculus. Individual ambition and competition were *intensified* by an organizational shift in the social resources available for rewarding mathematicians during the Cardan-Tartaglia and Newton-Leibniz periods. Intellectually ambitious individuals like Leibniz were bound to appear because of the opportunities provided by the rise of patronage in the academies, especially the opportunity to control their own subsidized publications, the new scientific journals.

Leibniz was an advocate of the new organizational forms and their entrepreneur *par excellence*. He engineered the first scientific journal in Germany and used his political connections to found the Berlin and St. Petersburg academies, becoming president for life of the latter organization. He also tried, unsuccessfully, to establish academies at Dresden and Vienna. He controlled academy publications and staffed their paid positions with his followers. Several generations of the Bernoulli family, their student Leonard Euler, and other major European mathematicians like Legendre held the mathematical posts at Berlin and St. Petersburg during the 1700s, and used these organizational resources to produce significant advances in the Leibnizian calculus. Leibniz must rank as one of the most successful organization builders in the history of science; he created both organizational forms and an intellectual content to fill them.

Leibniz acted like an innovative captain of industry in the age of the robber barons. He was aware of every opportunity — organizational, political, and intellectual. In his early days in Paris and London, he worked his way into the most prominent networks and aggressively familiarized himself with the most important intellectual trends. There is no evidence that he plagiarized, but rather that he learned as much as he could about what the leading intellectuals were thinking and exploited this in his own interests. He went through the unpublished manuscripts of Descartes and Pascal (Hofmann, 1973: 161). He induced Spinoza to let him see the manuscript for *Ethics*, in which Spinoza deduced a system of philosophy in geometric (axiomatic) form. Leibniz's philosophy (which goes beyond Spinoza) became famous while Spinoza's remained unpublished and obscure for a century. Leibniz was adept at picking up hints through his inquiries, developing them rapidly, and beating the originators into print. After reading a review of Newton's *Principia* in 1689, he hurriedly wrote a series of articles for *Acta* outlining his own theory of astronomical physics without mentioning Newton (Hofmann, 1973: 151).

Although not as organizationally innovative as Leibniz, Newton also, once he was firmly established in power, acted like an arrogant intellectual baron. He was a tyrannical president of the Royal Society, controlling its membership and cutting short its debates. He and his collaborator Halley published the findings of the Astronomer Royal, Flamsteed, without Flamsteed's permission. Newton staffed positions at the Mint with his scientific followers as a form of patronage (Cohen, 1974: 83). It seems clear that Newton, especially in his later years, was more interested in establishing his own "school" than in the advancement of mathematics. His attention during the dispute with Leibniz was focused entirely on establishing his priority forty years earlier, not with what might be done to foster mathematical developments using either his own or Leibniz's system. Leibniz tended to be programmatic and forward-looking, whereas Newton was more of an intellectual traditionalist who rarely saw the significance of his own advances until others pointed them out to him. His *Principia* was argued entirely in the style of traditional Euclidean geometry, with scarcely any mention or use of his calculus (even though he had used the new methods to reach his results). If Newton had been motivated primarily by a concern for advancing science, he would have recognized the superiority of Leibniz's formulation, adopted it, and used it to further English mathematics. Ironically, Newton's mathematical "comeback" after his work in physics had helped to make him a powerful figure in London, enabled him to champion a school of thought that had become reactionary compared to Continental mathematics.

Newton operated in a traditional intellectual structure. He was a university professor at a time when medieval-style universities were in decline. He achieved fame when the correspondence network was active, and faded from attention when it was not. The Newton-Leibniz controversy in fact points up the weaknesses of the informal *message centre* system. It depended too heavily on a few key individuals; the British network fell apart after Oldenburg and Collins died in the 1670s. The system did not spread ideas or reputations very widely, since relatively few individuals could actually receive letters. Posting letters abroad was especially expensive, since no postal system existed — and a "message centre" like Collins or Mersenne had to find travellers to act as couriers when they wanted to send information abroad. And the dependence of the network on personal good will made it difficult to handle controversies, even in the form of mere differences of intellectual opinion (Hall, 1980: 63). Oldenburg often lost contact with correspondents who took issue with what he reported. And Newton's suspicions in corresponding with the inquiring Leibniz are indicative of a system of communication that neither insured safe public claims to priority nor the open and free exchange of information.

There are other instances of robber baron behavior in this period. L'Hospital's calculus textbook was actually written by Johann Bernoulli, who was pressured to communicate his methods only to his employer, l'Hospital (i.e., as secret, private property). This recalls the relationship between Cardan and his servant Ferrari, and the patrimonial household of Scipione del Ferro. The Bernoulli family, too, operated almost as a patrimonial unit in which intellectual creativity was not individually credited but was used as the property of the head of the family. Johann Bernoulli learned his mathematics from his older brother Jakob (fifteen years his senior), and later inherited Jakob's position of professor of mathematics at Basle. In the newer cosmopolitan market for mathematics that was now emerging, patrimonial control of intellectual property could no longer be readily maintained. Jakob and Johann Bernoulli had furious struggles over intellectual property, and Jakob expelled his younger brother from his house. After Jakob's death in 1705, Johann published Jakob's solution to the isoperimetric problem as his own (Hooper, 1948: 344); and during the dispute with Newton, Johann claimed credit for the discovery of a mathematical flaw in Newton's work actually made by his nephew Daniel Bernoulli (Hall, 1980: 198). Similarly, the Scottish mathematician, David Gregory, took credit for results inherited from his uncle, James Gregory, his predecessor in the mathematics chair at Edinburgh (Hall, 1980: 36-37).

Without the organizational shifts we have noted, the patrimonial household would have gone unchallenged and the head of the household's right to subordinates' intellectual products would have been no more a matter for controversy than the Guild Master's right to sell journeymen's products. As prominent figures in the seventeenth-century organizational shift, Leibniz, Newton, l'Hospital, and the Bernoullis not only acted like robber barons, they were involved in the creation of a genuine mathematical empire.

Abel and Galois vs. Cauchy and the French Académie

The organizational forms pioneered by Leibniz dominated European mathematics until the early 1800s. Leibnizian mathematics dominated the intellectual content of European mathematics too. The danger of the national academies system was that the relatively small in-group that controlled it might eventually lose its intellectual élan. This was especially likely to occur as time passed and the enthusiasm and ambition generated by new opportunities declined; an academy might even fall into the hands of mediocre intellectuals or non-scientists as in the case of several academies in the 1700s (Ben-David, 1971: 77). There was also the danger, illustrated by the Royal Society in the eighteenth century, that the academies might become nationalistic and exclude non-native researchers and their creative products.

At the turn of the nineteenth century, world mathematics was dominated by the French Académie. The Académie offered a few well-financed positions for its leading members and publicized mathematical achievements in its journal and in international prize competitions. Nevertheless, by the early 1800s, the Académie was becoming stagnant. Innovative mathematics was now becoming associated with a rival organizational form: the new research-oriented university, pioneered at Göttingen in the late 1700s and made famous by the founding of the University of Berlin in 1810. The new university form went along with the rise of public primary and secondary education; a major purpose of the university was to train school teachers (Ben-David, 1971: 108-138). France, like England, did not reform its universities and establish public schools until late in the nineteenth century. As a result, innovations in fields such as mathematics tended to come from Germany and from other peripheral countries undergoing educational reform as part of their own nationalistic movements. The major mathematical scandals in the early 1800s reflect this conflict between the old academy system and the newer university-based, mathematical community (Ore, 1970; Freudenthal, 1971; Taton, 1972; Costabel, 1978; Raven and GrattanGuiness, 1972).

In 1826, a young Norwegian, Neils Henrik Abel, travelled to Paris on a small stipend from his government to present a major mathematical discovery at the world mathematical centre. Norway had recently become independent of Denmark and had established its own educational system. Abel studied at Norway's first national university. His father was a leading nationalist politician. When his father died, Abel was left dependent upon slender means of support.

Abel had solved the major mathematical puzzle of his day. He proved that it was impossible to solve an equation of the 5th degree by means of a general formula such as those available for cubic and biquadratic equations); and he had discovered an entirely new realm of mathematics: transcendental functions. The Paris mathematical establishment ignored both of Abel's contributions. His paper on transcendental functions, submitted to the Académie, was "lost" by one of the referees, Cauchy. Abel was in no position to protest effectively, and could not afford to stay in Paris. He died in 1829 of tuberculosis, penniless and without an academic position. The scandal emerged when German mathematicians who knew of Abel's other works made his research on transcendental functions known in France, and the Norwegian government formally protested the loss of Abel's paper. Under these pressures, Cauchy found the paper and it was awarded a posthumous Grand Prize by the Académie in 1830.[8]

8. There were two referees for this paper. The second, Legendre, apparently never saw the paper before Cauchy lost it. After corresponding with the German mathematician Jacobi, he became aware of its existence, and took the initiative in having Abel given posthumous credit (Ore, 1970: 12-17). The fact that Legendre and Cauchy together officially proposed Abel for a prize in 1829 has been taken by some historians as evidence of Cauchy's good faith (Freudenthal, 1971: 134), but it is clear that Cauchy was acting under pressure when he made these belated amends.

A similar case occurred a few years later. In 1829, Evariste Galois, a young radical student at the École Normale Superieure in Paris, submitted a paper to the Académie on the general theory of the solvability of equations by means of the theory of groups. Cauchy received the paper. He declared (erroneously) that Abel had priority, rejected the paper informally, and made no official report to the Académie. Galois prepared a second paper that was officially submitted for the prize in 1830 and assigned to the aging mathematician Fourier for evaluation. Fourier died a few months later and the paper was lost among his possessions; the Académie did not inquire about it, and Galois' claims were ignored. In 1832, a third version of Galois' paper was rejected by the referee Poisson, who labelled it incomprehensible. Shortly thereafter, Galois was killed in a politically motivated duel, and his contribution was buried for fourteen years.

The cases of Abel and Galois reflect an academy structure that provided an elite membership with extraordinary power. A paper lost or buried by a single referee could be denied recognition. Cauchy kept Legendre from even knowing of the existence of Abel's 1826 memoir; no one followed up on what happened to Galois' second paper after Fourier died; Galois' third paper was rejected on the evaluation of Poisson alone, an uninspired mathematician lording it over the Paris establishment in the wake of Cauchy's departure. The highly centralized Académie was not self-policing, and there were no safeguards against mediocrity or bias within its own ranks.

There is no evidence here of a conservative old guard rejecting the innovations of a paradigm-breaking new guard. It is rather a matter of a new guard versus a rival new guard. Although he was the villain in these instances, Cauchy nevertheless was one of the two great mathematicians (along with Gauss at Göttingen) who led the nineteenth-century mathematical community's ascent into higher mathematics. Cauchy was already a leader in the areas in which Abel and Galois were working — the theory of groups (Galois), and the use of new rigorous standards of proof, the basis of Abel's new proofs and functions. Cauchy was acting to protect his turf.

Cauchy's behavior was that of a robber baron, but not in the organization-building style of Leibniz. Cauchy played the advantages of controlling an already established Leibnizian organization for all they were worth. He used the publications of the Académie virtually as a private preserve; members of the Académie could publish their own works without review, and Cauchy worked at a furious pace — swamping the official printers and becoming one of the two most prolific mathematicians of all time (the other being Euler, at the Berlin and St. Petersburg academies, who had a similar privilege of publishing whatever he wrote). The ability to publish rapidly helped Cauchy dominate European mathematics. In his haste, he often presented his ideas sloppily (in a way reminiscent of the young Leibniz), and without recognizing their significance. Cauchy specialized in skimming the cream off each new area as it opened up. He frequently used his position as a referee for the Académie to advance himself. He would hold up a submitted paper while he wrote his own contribution on the paper's topic, publish his paper first, and then require the author to recognize his priority (Freudenthal, 1971: 134-135). Cauchy was involved in many priority disputes and was often accused of greed and unfair play (Bell, 1937: 293).

Unlike the political libertarian Leibniz, Cauchy was an outspoken conservative. For him science was part of the privileged establishment, and he habitually mixed science with political preferences. It is not unlikely that Cauchy was antagonistic to Abel and Galois for political reasons. Both young men were radicals; Abel was a Norwegian nationalist, and Galois was a participant in the 1830 revolution and was arrested in subsequent demonstrations pressing for greater liberalization. One can hardly imagine Cauchy giving disinterested advice when he rejected Galois' paper shortly before the revolutionary outbreak.

Cauchy's extreme conservatism was perhaps appropriate for the last great figure of the French Académie. As the period of its intellectual domination came to a close, the Académie became an intellectually reactionary force. While Cauchy's political biases are idiosyncratic, his behavior is consistent with the highly centralized, elitist structure of the French scientific world (Ben-David, 1971; Clark, 1973). The power of the entire system was concentrated in a few positions in Paris, in organizations acknowledged to be the most prestigious in the world. This structure encouraged extreme arrogance and egotism. Cauchy's behavior has earlier parallels in other fields. Lavoisier was another extremely ambitious man, a great systematizer who created the nomenclature and the theoretical foundations of modern chemistry. He had no compunctions about publishing other people's findings without acknowledgements. His discovery of oxygen in 1775 came after dining with Priestly, who later accused him of stealing his ideas (Guerlac, 1973: 74-76). Perhaps Lavoisier's behavior was related to his conviction that he was bringing chemistry to a state of final perfection; he may have felt that the contributions and disagreements of earlier scientists were irrelevant. Laplace, another ambitious system-builder, and a political opportunist, was also careless about acknowledgements. A good deal of what he published on the theory of universal gravitation, for example, was taken verbatim from the more retiring Lagrange (Hooper, 1948: 360). Laplace too seemed to think that science was being brought to perfected closure in his works. This attitude was widespread among the elite French scientists of the late eighteenth century. Even the modest Lagrange wrote in 1781 that he thought there was nothing further to be discovered in mathematics (LeLionnais, 1971: 244).

The French scientific elite did not have to face rival powers. These scientists often felt that if they did not achieve something, no one else would. The very existence of scandals, however, indicated the rise of countervailing forces in opposition to the dominant structure. Abel and Galois eventually came to prominence in centres rivalling those dominated by Cauchy. A rival centre in Berlin came to Abel's defense. The new German university spawned independent journals open to a variety of scholars. In 1826, Leopold Crelle founded the first journal in the world devoted exclusively to mathematics (Bell, 1937: 314-317). In his first volume, Crelle published a number of Abel's papers, including his great contribution on equations of the fifth degree. Through Crelle's sponsorship of the talented Abel, the German mathematician Jacobi heard of the missing memoir on transcendental functions and inquired about its whereabouts. The memoir was eventually recovered and brought to the attention of mathematicians. Similarly, Galois was rediscovered by Joseph Liouville, whose goal was apparently to establish a French rival to the publications of the Académie. Galois' paper was published in the first issue of Liouville's new journal in 1846 (Kramer, 1970: 25-26).

In contrast to Lagrange's time, when leading intellectuals thought their fields were coming to an end as realms of discovery, in Cauchy's era the organizational structure of the scientific was becoming pluralized, reflecting and stimulating new pathways in science. The new, reformed universities became competitive with the French system of centralized, elite science. The density of scientific competitors increased sharply, causing a shift in mathematics to much more rigorous and abstract methods. This was the beginning of the end of the robber baron era. From now on, institutionalized competition among organizational centres would no longer allow the ruthless scientific egotism characteristic of the giants of the past.[9]

Cantor vs. Kronecker: the transition to saintly politicians

University professors increasingly dominated nineteenth-century mathematics, especially in the network of competing German universities. The tendency towards abstraction and systematization characteristic of educational settings (Collins, 1975: 487-492) produced a mathematics far removed from the empirical world and the categories of common sense. Controversies began to develop over the proper status of these levels of abstraction. Georg Cantor (1845-1918) was an outspoken leader of the movement for extreme abstraction without regard for the paradoxical conclusions to which this might lead. In the 1870s and 1880s, Cantor developed the theory of transfinite sets. By contrast, the Berlin professor Leopold Kronecker

9. Grabinger (1981) identifies Lagrange as a crucial transition figure separating the era of Newton, Maclaurin, Euler and d'Alembert from the era of Cauchy, Abel, Bolzano, Weierstrass and Dedekind. While Grabinger's rhetoric of revolutionary change obscures important continuities, her sensitivity to the relationship between teaching and rigorization is noteworthy and congruent with our analysis.

(1823-1891) argued that only natural numbers (positive integers) exist, and that all mathematics must be derived from them in a finite series of operations. Cantor and Kronecker became bitter rivals, each trying to prevent the other from publishing his work (Dauben, 1979; van Heijenoort, 1967; Parpart, 1976). Kronecker was an associate editor of *Crelle's Journal* (in collaboration with Borchardt, who had succeeded Crelle); in 1878 he tried to block publication of Cantor's major paper on dimension. The paper was finally published by Borchardt, but thereafter Cantor refused to submit articles to *Crelle's Journal.* Kronecker also tried to suppress Heine's paper on trigonometric series because it deviated from the integer program. Kronecker's tactic was much like Cauchy's; he held onto the paper without informing Heine about it. But power was less centralized than in Cauchy's situation, and Heine was finally able to get Borchardt to publish the paper by coming to Berlin and demanding an accounting.

Kronecker initially had more resources at his command than Cantor. After Borchardt's death in 1880, Kronecker became editor of *Crelle's Journal.* Kronecker was a member of the Berlin Academy and of many foreign academies. He was also independently wealthy. He had influential connections in government and his advice was widely solicited in filling university professorships with leading mathematicians. Cantor had been a student at Berlin (where Kronecker had been one of his teachers) as well as at Göttingen, the other leading mathematical centre; but he was consistently disbarred from an appointment at either of these universities. He bitterly noted that he earned half the salary of other full professors, and attributed his career frustrations to Kronecker's opposition.

Cantor was not without resources of his own. Mittag-Leffler, editor of *Acta Mathematica*, the rival to *Crelle's Journal*, was instrumental in publishing Cantor's research. When Kronecker proposed to submit a paper to *Acta Mathematica* in 1884, showing the insignificance of the results of modern function and set theory, Cantor threatened to withdraw his support for the journal if any of Kronecker's polemical articles appeared. Cantor also played a Kronecker-like role in the controversy over infinitesimals; he used the same polemical devices to oppose the Italian mathematician Veronese that he complained Kronecker used to oppose him.[10]

Cantor built a new organizational base to counteract Kronecker's hold over German mathematics. He was the driving force behind the establishment of a separate mathematical society, independent of the older associa-

10. Cantor accused du Bois-Reymond of finding the doctrine of mathematical infinitesimals "excellent nourishment for the satisfaction of his own burning ambition and conceit." Cantor viewed the mathematical quest, not in terms of the disinterested search for truth, but as "*a question of power, and that kind of question can never be decided by way of persuasion*; the question is *which* ideas are the most powerful, comprehensive, and fruitful, Kronecker's or mine; only success will in time decide our struggle!!" (Parpart, 1976: 56).

tion that linked German mathematicians and astronomers in a section of the *Gesellscahft Deutscher Naturforscher Und Aertze* (Society of German Researchers and Doctors). In 1891, the *Deutsche Mathematiker-Vereinigung* (German Mathematical Union) was founded, with Cantor as its first president. In his further efforts to break from the Berlin-centred "conspiracy," Cantor organized the First International Congress of Mathematicians, which met at Zurich in 1897 (Wavre, 1971).

Cantor's efforts succeeded both intellectually and organizationally. The increasing density of the mathematical population and specialization and professionalization in mathematics facilitated acceptance of Cantor's work. With a sharp rise in the number of practising mathematicians, the increasingly viable peripheral universities broke the hold of centres like Berlin and Göttingen on world mathematics. The struggle between Kronecker and Cantor, however, was not a conflict between traditional and innovative forms of mathematics, but between rival new paradigms. Kronecker was not a mathematical traditionalist; in opposing an actual infinity and irrational, transcendental, and transfinite numbers, he was forced to reconstruct mathematics on a radically new basis. He foreshadowed the twentieth-century school of intuitionists, just as Cantor pioneered in what became the formalist program. Both sides pressed for greater rigor in mathematics, but were divided sharply on the issue of how to achieve it.

By the turn of the century, the large scale of the mathematical community and its academic bias toward rigor and systematization had outdated the strictly interpersonal competition between mathematicians over solutions to particular problems. The social conditions that had produced the robber baron had given way to a setting that involved collective conflict among synthesizing schools with rival programs. Even Kronecker and Cantor did not battle simply for individual credit in the manner characteristic of earlier mathematicians. Their successors shifted their style to efface themselves before the collectivity. The robber baron gave way to the "saintly politician."

In the twentieth century, for the first time, mathematicians began to publish co-authored papers. By the 1960s, 60 percent of mathematicians were publishing collaboratively at least some of the time (Hargens, 1975: 51). The Cambridge mathematician G.H. Hardy was one of the first mathematicians to publish with a collaborator. Hardy published hundreds of co-authored papers, many of them with the poorly schooled Indian, Ramanujan (Barkill, 1972). Whereas a sixteenth or seventeenth-century mathematician might have picked Ramanujan's brain or appropriated his results without compunction, Hardy sponsored Ramanujan's way to England and credited the Indian's independent work. Hardy's compatriot, Bertrand Russell, made similar efforts to credit and publicize Frege for anticipating his results, even though Russell had finished his own work before reading Frege, and even though Frege was in a different country and virtually unknown at the time (Russell, 1938: xvi, 501-522; Whitehead and Russell, 1910: viii). Russell had published his own most famous work, *Principia Mathematica*, with himself as second author (Whitehead and Russell, 1910), although the work contained a doctrine that he had himself already worked out and published in his *Principles of Mathematics* (Russell, 1903).

The leader of the Göttingen school of formalists, David Hilbert, was a saintly politician in virtually every respect. Unlike Cauchy, he championed academic underdogs, opposed restrictions on women and political radicals (even though his own politics were conservative), and combatted academic anti-semitism. In contrast to the nationalistic behavior of the Newton-Leibniz era, Hilbert in Germany, like Russell in England, opposed chauvinism in mathematics and honored enemy mathematicians even during the furor of World War I (Kramer, 1970: 467-469; Freudenthal, 1972; Reid, 1970).

Twentieth-century mathematicians have emphasized by word and deed that science is a collective enterprise. The most extreme case is that of Nicolas Bourbaki, not a person but a pseudonym for a group of French mathematicians who published collectively (Kramer, 1970: 467-469; Boas, 1970). "Bourbaki" represents an effort to unify modern mathematics in terms of the theory of groups. Similarly, Russell and Whitehead sought to deduce all of mathematics from a simple, logical foundation; and Hilbert's formalist program extended his Göttingen predecessor Felix Klein's program for unifying geometry around an axiomatic structure to all of mathematics. These "unifiers" were hyper-conscious of the history of mathematics as a collective enterprise. They not only meticulously acknowledged earlier contributors, but tended to efface themselves before the advances they expected in the future. They differed in this respect from Lavoisier, Laplace, and Lagrange, who believed there would soon be no new contributions to be made in their fields. Russell was explicit about where his work needed to be extended, and he credited methods he thought should supercede his (Whitehead and Russell, 1927: xiv-xv). Hilbert, a strong supporter of the International Congress of Mathematicians, gave a famous programmatic speech at its second meeting in 1900; he identified a set of unsolved problems for future mathematicians. "A branch of science is full of life," he said, "as long as it offers an abundance of problems; a lack of problems is a sign of death" (Weil, 1971: 324). Five years later, a leader of the Bourbaki group, Andre Weil, proposed a similar program and invoked his "political" theme for the discipline:

> ...there are very few really important problems which are not intimately related to others which, at first sight, seem to be far removed from them. When a branch of mathematics ceases to interest any but the specialists, it is very near to its death, or at any rate dangerously close to a paralysis, from which it can be rescued only by being plunged back into the vivifying sources of the science. (Weil, 1971: 333)

This view of mathematics as an ongoing and interconnected enterprise led mathematicians to submerge themselves in the collective.

Collectivist attitudes among twentieth-century mathematicians have been structurally induced. Mathematicians have had to become altruists in order to pursue any major intellectual ambitions. The growth of the mathematical community and the development of numerous special fields threatened to make it difficult or nearly impossible for individual mathematicians to have their publications recognized (Hagstrom, 1964; Hargens, 1975). To excel under the new circumstances, one could no longer try to solve all concrete mathematical problems oneself, in the manner of Cardan. Nor could one, following Leibniz's example, found an intellectual program capable of dominating the world of mathematics. It was no longer possible to emulate Cauchy and try to personally rule the mathematical world with fanatical work habits and by controlling a centralized publishing system. In the twentieth century, the ambitious mathematician had to produce results applicable in many different areas of mathematics; the object had to be system building on a highly abstract level. The modern concern with the foundations of mathematics is one result of this structural characteristic of the mathematical community.

Despite structural changes, the engine of mathematical innovation continues to be fueled by aggressive and competitive behavior. What has changed is that this behavior is now grounded in collective, organizational forms. The successful empire builder can no longer create a *personal* empire; he or she must act politically and create organizations. Extreme politeness, crediting others, exhorting others to work in certain directions, and a collective, organizational consciousness are, in general, indispensible for success in the modern environment. We are not saying that collectivism and altruism extend without limits. If individuals who belong to one's school are acknowledged for their contributions, rival schools are treated with less courtesy. This is especially true of the antagonistic schools, like Brouwer's intuitionism, that arose in opposition to the systematizers.[11] Even the anti-systems movement has become a rival system under modern conditions.

The era of system builders enforces ideals of altruism, self-effacement, dedication to collective goals, and an orientation to transcendent purposes — "for the glory of the human spirit," in the words of Hilbert and Weil. The Mertonian image of science is based on twentieth-century ideals. Underlying these ideals is a structure of collective competition within which ambitious

11. The intuitionists' program necessitated jettisoning large sections of mathematics that were considered untenable. A Bourbaki leader expressed disdain for the intuitionist position: "Only a few backward spirits still maintain the position that the mathematician must forever draw on his 'intuition' for new, alogical, or 'prelogical' elements of reasoning. If certain branches of mathematics have not yet been axiomatized. . . this is simply because there has not yet been time to do it" (Weil, 1971: 324; cf. Bourbaki, 1971: 29).

individuals can succeed only by portraying themselves as selfless representatives of the scientific group — in short, as saintly intellectual politicians.

Conclusion

The cases we have considered are not simply reflections of individual personalities. Personalities are partly formed in and reflect conditions of work, and serious intellectuals invest a great deal of self, time, and energy in their work. Nor are the cases trivial, a washing of dirty linen in public, or epiphenomena of intellectual history. The general solution of the cubic equation was an epochal event. It marked the first time that European scientists had solved a problem the ancient Greeks had been unable to solve. In this sense, Cardan's *Ars Magna* can arguably be viewed as the beginning of the Scientific Revolution. It also initiates an era of new algorithms and a trend toward higher and higher levels of abstraction. Leibniz and Newton were involved in developing the basic methods of analysis. They opened new vistas to mathematicians and established the foundations for most of eighteenth-century mathematics. Cauchy, Abel, and Galois developed the theory of groups, and introduced new abstract methods and rigorous proofs, the keys to the great nineteenth-century developments in higher mathematics. Cantor's treatment of the infinite marked the beginning of a period in which foundational issues became central in mathematical work. Hilbert, Russell, and Bourbaki were the major systematizers of the entire period of mathematics after Euclid, and together with their anti-systems opponents (Brouwer, Gödel) established the major schools of twentieth-century mathematics.

The controversies do not reduce to a simple matter of multiple discoveries and priority disputes. The Newton-Leibniz case involves such themes, but we also find scandals over violations of secrecy (Cardan-Tartaglia); suppression of rival ideas (Cauchy-Poisson-Abel-Galois); outright appropriation of others' ideas under one's own name (l'Hospital, Gregory, Bernoulli); nationalistic exclusivity about ideas (the aftermath of the Newton-Leibniz dispute); and factional struggles over control of university positions, journals, and scientific associations (Cantor-Kronecker). If the Mertonian model does not fit, neither does the Kuhnian. In no case do we find a mathematical change *centred in* a struggle between advocates and critics of an established paradigm, but always between rival innovators. Moreover, the long-term trend in Western mathematics has not been towards a single, dominant paradigm, but rather towards rival schools at odds over fundamental questions about methods and knowledge. If mathematics is the most "advanced" of the sciences, it has moved, in its "maturity," to greater paradigmatic pluralism than at almost any time in its history. It has thus come to resemble the social sciences or other allegedly "pre-paradigm" fields more than it does Kuhn's image of a science.

The scandals and controversies we have examined and the intellectual developments that accompanied them are best analyzed in terms of the changes they reveal in organizational forms. The Cardan-Tartaglia dispute marks the beginning of a breakdown in patrimonial organization of intellectual property and in the system of contests between mathematicians; the secrecy of general methods and the publicity of particular problems and solutions is superceded by intellectual competition over more abstract ideas. The Newton-Leibniz controversy reveals a shift from traditional forms of patronage to more permanent government patronage through organized academies, and the related shift from an informal communication network linked by "message centres" to the more impersonal arena of scientific journals. The Abel-Galois scandals in the French Académie, in turn, indicate the decline of centralized institutional patronage and the rise of research-oriented universities. And the Cantor-Kronecker disputes occurred at a time when the relatively small, elite-dominated university network was expanding into a large, world-wide mathematical community.

In each case, ambitious intellectuals pursuing self-interested paths to fame and fortune took advantage of whatever organizational resources new situations offered. The emergence of "saintly politicians" is one source of the ideals Merton mistakenly identified as the universal norms of science. But even in the twentieth century, self-interested competition is still the root of intellectual advance. Structural conditions have merely forced ambitious intellectuals to compete in collective rather than individualistic forms. Like the industrial robber barons, the intellectual robber barons have not so much disappeared as changed their stripes. To the extent that intellectual communities have become pluralized, vulgar robber baron behavior has been curtailed. To some extent, collective forms obscure such behavior. The "saintly politician" is the "civilized" robber baron.

The era of saintly politicians is not without its own scandals. The major scandals of recent years have not occurred in mathematics, but in the bio-medical sciences (Broad and Wade, 1983). Some have involved fabrication of data, others the pirating of papers by referees during the journal review process; some scientists have taken advantage of the large number of publication outlets to publish other scientists' research under different titles. Given the extreme fragmentation of specialties in mathematics and the low rate of readership for most articles (Hagstrom, 1964), such scandals have probably occurred in mathematics as well; but fragmentation is so extreme that no one has noticed! Fame and intellectual advance in a science cannot occur without attracting attention. The lack of major scandals or violent controversies suggests that the mathematical community is not undergoing significant organizational changes — at least, it is not at an organizational watershed.

Mathematics and other sciences need not follow organizational stages such as those we have described. The challenge contests of Renaissance Italy, the academies of the 1600s and 1700s, the German university reforms of the early 1800s, all had particular historical causes that impinged upon intellectual life. Other combinations of conditions might have produced a different sequence. For example, although secrecy about methods has characterized mathematics at relatively early periods in its development in different cultures and has been superceded by public competition over advances in the methods themselves, there is no reason to believe that secrecy cannot become a "norm" in the future. We see current indications of this possibility in the form of government efforts to turn all mathematical advances relevant to cryptography into "classified information."

The nature and availability of organizational and material resources can change the organizational structure of mathematics. If mathematicians are increasingly dependent upon military funding or upon expensive computers, they may experience just such an organizational shift. The old patrimonial organization of intellectual property might return if mathematics were located primarily in commercial laboratories, where discoveries are treated as company rather than individual property.[12] We cannot expect organizational developments in science to follow a simple linear evolution. Our analysis suggests, furthermore, that the development of mathematical knowledge, rooted as it is in organizational forms, will follow those forms and not reflect a simple linear evolution according to some type of "inner logic" (cf. MacKenzie, 1981).

The challenge for sociological theory is to build generalized explanations from the analysis of incidents like those above. Neither the Mertonian nor the Kuhnian theory can be used to predict intellectual shifts. The Kuhnian model proposes only that dominant paradigms are eventually broken down by the accumulation of empirical anomalies. The Mertonian model is even weaker because it describes a static set of norms and proposes no causal variables affecting intellectual productivity. One model that does seem to be congruent with our data is the theory-groups model proposed by Griffith and Mullins (1972; Mullins, 1973). Leibniz was a *theory-group builder*. He was both an intellectual and an organizational leader. The Bernoullis and l'Hospital provided *training centres* at Basle and Paris, and a *standard text*. All this constitutes what Griffith and Mullins call the "network stage." The English attacks on the Leibniz school, and the counterattacks and increasing dogmatism during the period 1700-1720 are exactly what the model predicts for the "cluster stage." The timing of these stages is roughly congruent with Mullins' findings for theory groups in twentieth-century social science and

12. This suggestion is due to Paul DiMaggio.

other fields. The model might be integrated with a more general perspective on scientific innovation if it could be extended to encompass the structure of rival theory groups and the long-term sequences through which they move.

The intensity of scientific creativity is greatest during major shifts to new organizational forms that structure activities and communication. These same organizational shifts are also a major cause of scientific scandals; hence, scandal-free eras are likely to be intellectually placid. The effects of various degrees and kinds of competition, and the effects of particular institutional arrangements on the content of intellectual creativity remain to be more clearly identified, refined, and formally stated by further analyses. Such a theory would apply not only to mathematics, but with appropriate modifications to all theory-driven sciences.

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Robber Barons were vilified for using the capitalist system to exploit workers, form anti-competitive trusts, and place the accumulation of wealth above all else. The belief that the rich could use whatever means necessary to increase their riches seemed to counter the ideals upon which the United States was founded.Â Josephson viewed the Robber Barons as unscrupulous pirates fighting to control the nation's economy. The 1940s and 1950s witnessed a revival of the view of business leaders as industrial statesmen, which reflected the image of America's postâ€"World War II economic, military, and cultural hegemony. "Robber Barons and Politicians in Mathematics: A Conflict Model of Science," The Canadian Journal of Sociology, Vol. 8, No. 2 (Spring 1983), pp. 199-227. With R. Collins. "Development, Diversity and Conflict in the Sociology of Science," Sociological Quarterly, Vol. 24, No. 2 (1983), pp. 185-200. With R. Collins.Â "Science, Social Problems, and Progressive Thought: Essays on the Tyranny of Science" (including "Technoscience or Tyrannoscience Rex," a review of Jurassic Park), pp. 39-87 in S.L. Star (ed.), Ecologies of Science, SUNY Press, Albany, 1995. With Jennifer Croissant. "How to Criticize Science and Maintain Your Sanity," Science as Culture, 6, Part 3, 28 (Spring 1996): 396-413. The robber barons denounced regulators in the name of the free market, but monopoly suited them better. Rockefeller rued the â€œdestructive competitionâ€ of the oil industry, with its cycle of glut and shortage, and set about ensuring continuity of supply.Â The age of the robber barons led inexorably to the age of populist revolt, with mass strikes, anti-monopoly legislation, social reforms and, eventually, the New Deal of the 1930s. The robber barons had ruined too many people and broken too many rules.Â A cohort of politicians and lawyers fairly swiftly translated the backlash into policy. Teddy Roosevelt thundered against the â€œcriminal richâ€. Woodrow Wilson followed up with even more vigorous attacks on corporate America.