A maverick's apprenticeship

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Many sciences arose from the need to describe and understand sensory data. The visual sensation of bulk and shape, of brightness and color, prompted the development of geometry and optics. The feeling of "heavy" or "light" led to mechanics. The sensation of hot and cold led to the theory of heat. Taming the sense of acoustic pitch began with vibrating strings or pipes. But the sensation of "rough" versus "smooth," had historically been neglected. Being the most obvious manifestation of underlying complexity, it is ubiquitous, always concretely relevant, and often essential. Yet the practical study of metal fractures, measured roughness by some deviation from an idealized plane. This borrowed expression proved oversimplified and not effective.

My whole career became one long, ardent pursuit of the concept of roughness. The roughness of clusters in the physics of disorder, of turbulent flows, of exotic noises, of chaotic dynamical systems, of the distribution of galaxies, of coastlines, of stock-price charts, and of mathematical constructions, – these have typified the topics I studied.

The field of fractal and multifractal geometry, which I have been credited as founding, is the underpinning of a new, emerging theory of roughness. "Fractal," a word I coined for a new concept whose roots are traceable over centuries and millennia, denotes geometric shapes that break into parts, each a small-scale model of the whole. Consider mundane objects already cultivated by the Ancient Romans.
A cauliflower head is easily broken into florets. Each floret is like a smaller cauliflower, therefore again decomposes into smaller florets. Using a magnifying glass, this process can be watched over many steps. A mathematical formula that would imitate this structure could be continued to infinity. The idea, therefore, is that of self-similarity: each part, namely each small floret, is like the whole or any other floret, except for a dilation or reduction.

This concept's simplicity is deceiving: it has generated a surprisingly broad range of important shapes that can model a surprisingly broad range of important phenomena.

To start towards a comprehensive and harmonizing approach to a sensory input that had long defied rational study, a new geometry turned out to be necessary.

How did I come upon one? Answer: Not in one *Eurêka!* flash, but by step by step, in a painful process that continues to reveal new horizons. I had chosen to be an "outsider" to most of the fields where I worked. Conceiving fractal geometry, then developing it in many directions, required contributions to a broad, range of topics. Those included several branches of physics or pure mathematics, and several other sciences.

To what established discipline does my work belong? To none exclusively – and to many. One crucial unifying thread is that I am an avid student of statistical thermodynamics and since 1951 have generalized its key ideas to an increasingly wide variety of unconventional complex systems. These ideas were not used as a metaphor but as the main basis for technical work. That is why every step of my work involves the word "macroscopic."

Another unifying thread: a constant pursuit of scale-invariances and symmetries. Further threads: power-law relations rule all the macroscopic systems I studied and a strong geometric mode of thought eventually led to fractals. In pure mathematics, my main contribution has not been to provide proofs, but to ask new questions – usually, very hard ones – suggested by physics and pictures.

In this essay, I will attempt to explain the crosscurrents of my life that led to this eclectic, outsider-looking-in , manner of thinking. When I was 19, events revealed that I had an innate gift for manipulating complicated geometrical concepts in my head, with limited assistance from drawings or algebraic calculations. But rather than taking the straight track to the physics or mathematics establishment, I pursued a more-varied career: through Ècole Polytechnique in Paris, Caltech, Princeton's Institute for Advanced Study, MIT, Harvard, Yale, and – unconventionally – IBM Research.

All this led in later life to a Wolf Prize for Physics and a Sterling Professorship of Mathematical Sciences at Yale. Yet I did not join any "guild" and kept my choices open. By all odds, allowing my interests to drift should have led to a disconnected and miserable professional life. But only during fleeting moments did I regret my course. Persistent and hard work, readiness to tackle daunting problems "beyond my field," willingness to "think big," a dose of good luck and timing – all these combined with that powerful gift for shape, and made me into a jack of several seemingly unrelated trades, a "maverick" and deeply visual geometer, an outsider successful at his chosen trade.

BACKGROUND (1924-1943)

I was born in Warsaw late in 1924 to a family from Lithuania, a nuance that greatly mattered in Eastern Europe. I remember a happy and carefree childhood in an enormous extended family, and what proved to be the first stage of a peculiar education.

For the first and second grades I was tutored privately by the husband of an aunt, because my mother, a doctor, was scared of epidemics and kept me away from school. That uncle was an intellectual who despised rote learning, including even the alphabet and the table of multiplication: both mildly trouble me to this day. However, he trained my memory and my mind in an independent and creative way through extensive reading. Most of my time was spent playing chess, reading maps and learning how to open my eyes to everything around me. Certainly, these experiences did not harm, probably even helped the geometric intuition which has been my most important intellectual tool.

In the nineteen-thirties' Warsaw, the Depression was terrible and the ethnic and political strife was bad and getting worse. My parents, being very rational and decisive people who closely followed events in Germany and Russia, concluded that our prospects of happiness in Poland were grim. At age 50, my mother, not waiting to be pushed out by events, gave up the medical profession she loved. With two sons, she moved to Paris in 1936; my father had moved ahead and settled in a slum in Belleville.

A year later, I had become fluent in French, but entered secondary school two years older than my peers, another peculiar step in my education.

Living in Paris made me well acquainted with my father's much younger brother, Szolem Mandelbrojt (1899-1983). He preceded us all to France around 1920, became rapidly and thoroughly accepted, and was about to become Professor of Mathematics and Mechanics at the Collège de France, succeeding Jacques Hadamard (1865-1963). After 1973, he occupied at the Académie des Sciences a chair once held by Poincaré and then Hadamard. No one influenced my scientific life nearly as much as this uncle did.

Raised in Warsaw, then Kharkov during the Civil War that followed the Soviet Revolution, my uncle left for France aged about twenty, a refugee driven by an ideology that was not political or economic but purely intellectual. He was repelled by the "Polish mathematics," then being built up as a militantly abstract field by Waclaw Sierpinski (1882-1969). By profound irony, whose work was to become a fertile hunting ground when, much later, I looked for tools to build fractal geometry? Sierpinski! Fleeing his ideology, my uncle joined the heirs of Poincaré who ruled Paris in the 1920s. My parents were not ideological but economic and political refugees; their joining my uncle in France later saved our lives. I never met Sierpinski but his (unwitting) influence on my family had no equal.

In 1939, the outbreak of World War II sent us away from Paris to Tulle, a small town in poor central France, where my uncle had a house and friends. Among first-rate teachers, Marie-Thérèse Tronchon (1907-1997) honed my skills in French and Monsieur Guitton recognized my gift for mathematics. They helped me graduate in 1942 at the normal age with the first *Summa* in the school's history.

Soon, the German occupation tightened and the desire to maximize the chances of survival made me keep away from big cities, therefore skip most of early college education.

For some months I was in Périgueux as apprentice toolmaker on the railroads. For later use in peacetime, the experience was better than another wartime stint as horse groom, but I did not look or talk like an apprentice or groom and, at one point, narrowly escaped execution or deportation. Some good friends eventually arranged for admission to the Lycée du Parc, in Lyons. While much of the world was in turmoil, it was almost business as usual in a class preparing for the feared examinations of the French elite universities called "Grandes Écoles."

SELF-DISCOVERY

The few months that followed in Lyons were among the most important of my life. Stark poverty and deep fear of the German boss of the city (we later discovered his name to be Klaus Barbie) tied me to my desk for most of the time.

It was then and there that a gift was revealed. During high school and the wandering year and a half that followed, I became intimately familiar with a myriad of geometric shapes that I could instantly identify when even a hint of their presence occurred in a problem. “Le Père Coissard,” our marvelous mathematics professor, would read a list of questions in algebra and analytic geometry. I was not only listening to him but also to another voice. Having made a drawing, I nearly always felt that it missed something, was aesthetically incomplete. For example, it would improve by some projection or inversion with respect to some circle. After a few transformations of this sort, almost every shape became more harmonious. The Ancient Greeks would have called the new shape “symmetric” and in due time symmetry was to become central to my work. Completing this playful activity made impossibly difficult problems become obvious by inspection. The needed algebra could always be filled in later. I could also evaluate complicated integrals by relating them to familiar shapes.

I was cheating but my strange performance never broke any written rule. Everyone else was training towards speed and accuracy in algebra and reduction of complicated integrals; I managed to be examined on the basis of speed and good taste in translating algebra back into geometry and then thinking in terms of geometrical shapes.

Where did my gift come from? One cannot unscramble nature from nurture but there are clues. My uncle lived a double life as weekday mathematician and Sunday painter. My gift for shapes might have been destroyed, were it not for the unplanned complication of my life during childhood and the War. Becoming more fluent at manipulating formulas might have harmed this gift. And the absence of regular schooling influenced many life choices, but ended up not as a handicap but as a boon.

A LIFE AT CROSSROADS

In June 1944, the Allies landed in Normandy. After a hectic summer, I went to the exclusive Lycée Louis-le-Grand in Paris, facing the Sorbonne. The exams were only delayed by a few months. I crammed hard but took them only “for practice.” However, not only did I pass, but many poor marks were compensated for by top marks in mathematics. I barely missed being ranked first in the competitions for the two leading schools that, since the French Revolution, have drawn applicants from the whole of France. École Normale Supérieure “Rue d'Ulm” was tiny and extraordinarily prestigious but focussed. In research its reputation was high in pure mathematics and low in physics. École Polytechnique (“l'X”) was more diversified and its alumni ran many diverse aspects of French life.

Fellow students who faced such a choice had long been preparing for it. Regular schooling creates full awareness of what ambitions ought to be pursued. To the contrary, I was, in effect, an underschooled stranger. Only months before, I had been desperately scheming to keep alive. Suddenly, a marvelous long-term choice was available for me alone to make.

Several family members, brilliant and forceful but not politically savvy, offered sharply conflicting advice. My uncle dreamt I would follow straight in his footsteps, though he worried endlessly about my taste. My father remembered what we lost in Poland and strongly believed that a scholar's happiness and independence hinged on a steady income and a job largely independent of politics or the will of a state – contrary to medicine or teaching in the model my family had experienced. My father was a fiercely independent person, anything but an “organization man.”

Initially, I chose École Normale because of the school's glamor, and out of deep respect for my uncle. But “Bourbaki,” a Utopian movement towards abstraction, was poised to take over French mathematics and my uncle kept telling me that geometry was dead and that I must forget about it. This dealt me a wound that did not heal until much later, when I was privileged to help the subject rise from its long sleep. Normale was the absolutely wrong place for a strong-willed person of my tastes. After two days, I yielded to my father and left for École Polytechnique. This switch of schools was widely criticized and many potential friends never understood or forgave it. But as I look back, it was the right decision, carrying me closer to what was to become my goal in life.

ÉCOLE POLYTECHNIQUE (1945-1947)

Most classmates coasted along, confident that being “ancien Élève” would serve them for life. Classmates who were aiming at a high graduation rank – and the splendid state jobs it provided – had to work not only very hard but “efficiently.” But a state job required French citizenship and the staff did not know that my French naturalization, signed after the war, should have been effective from the day it was approved, before the war. Free of the pressure to cram, I received a very fine and broad education.

Since I had fled mathematics as practiced at Normale, my Professor of Physics greatly mattered. Louis Leprince-Ringuet (1901-1999) was actively recruiting and came close to convincing me to join his team. But he was fully committed to revive experimental physics in France after many years of inactivity and I was definitely bound to become a theorist of some kind. At Polytechnique, no one could help.

The Professor of (Differential) Geometry, whom I mostly saw from far away, was Gaston Julia (1893-1978). In 1917, Julia had published a 199 page masterpiece “Mémoire sur l'itération des fonctions rationelles.” It received a Grand Prix then fell into thirty years of neglect and increasing scorn, being overly concrete and specialized. My uncle suggested I pick it up but I failed to move an inch. Who could have imagined that, years later, I would overwhelm that field with new questions? My work fired with enthusiasm a band that revived Julia's theory of iteration of functions and brought it to fuller and well-deserved glory.

The Professor of Mathematical Analysis, Paul Lévy (1886-1971), was nearing sixty at the time and suddenly was being "discovered" as a very great man in probability theory; and his field was becoming politically correct within mathematics. But he never became a probability theory insider, nor did I. Lévy's self-directed boldness and insight cost him in terms of "career" and early recognition, but I found it admirable and felt ready to pay the same price.

GOOD AND BAD ADVICE

During the last term at Polytechnique, I looked for ways to apply my gift for shape, and a growing knowledge of various fields, to real, concrete, and complex problems. I wanted to keep far from organized physics and mathematics and instead find a degree of order in some area – significant or not – where everyone else saw a lawless mess.

My uncle might have been an obvious source of advice, but he harshly mocked my thoroughly romantic dream. He never followed or understood my aspirations, feeling to the end of his life that I persisted in squandering my native intellectual gifts. But later in life many similarities emerged.

One obvious difference is that my uncle joined a powerful established "guild," while I managed to thrive without either joining an existing school or creating one for the few formal students I had. Therefore, his scientific life's path was straight as an arrow; mine seemed continually... fractal.

One similarity is that I, too, became an "ideological" refugee from abstraction, first when Bourbaki made me leave école Normale in 1945, and then when this group's pervasive influence made me leave France in 1958.

Yet another feature is that my uncle's beloved Taylor and Fourier series had started centuries ago in the context of physics, but in the 20th century developed into a field self-described as "fine" or "hard" mathematical analysis. In my uncle's theorems, the assumptions could take pages. The distinctions he enjoyed were so elusive that no condition was both necessary and sufficient. The long pedigree of the issues, for him a source of pride, was for the younger me a source of aversion.

A wandering scientist should never say never, and a beautiful part of abstract mathematics should never be called exhausted. Like that figure of Greek mythology, an occasional contact with our Mother Earth will revivify it.

In due time, persistence in the study of roughness made me encounter increasing depths of wild complexity and conclude that the world is not fundamentally mild and simple. In due time, the intellectual landscape that I chose to visit as scientist turned out to have been previously visited by my uncle and his friends. The hard messiness found in his mathematics may well reflect the irreducible messiness of the scientific frontier where I have chosen to work.

CALTECH, DOCTORATE, AND POSTDOC WITH VON NEUMANN (1947-1954)

In lieu of the splendid state job that my city of birth was believed to preclude, Polytechnique arranged a scholarship in the United States at Caltech (the California Institute of Technology). I made many good friends there and the course of my life was profoundly influenced.

Firstly, I learned about turbulence and attended the "swan song" lectures on statistical physics of Richard C. Tolman (1881-1947). He did not teach technique but "explained thermodynamics" and affected much of my work.

Secondly, I had the privilege of becoming close to several postdocs and their maverick leader, Max Delbrück (1906-1981). There and then, molecular biology was being created against the hostility of all the guilds. This provided an exhilarating – and dangerous – proof that someone with my bent might have a chance, after all.

Back to Paris, the University provided none of the pluses or minuses of having an advisor or mentor. In 1952, I submitted a Ph.D dissertation, unfinished and poorly presented, that largely determined the course of my life.

Roughly speaking, the first half was about mathematical linguistics, the second, about statistical thermodynamics, the first half being a very exotic form of the second.

Onlookers – even friendly ones – all warned me that the combination was by no means "natural." Not only did the first half concern a subject that didn't yet exist, but it was clear that my main goal was not to help linguistics become mathematical, but to explain a mysterious power-law relation called Zipf's law. George Kingsley Zipf (1902-1950) claimed that power laws characterize everything interesting in the social sciences, and provide them with an element of unity in contrast to the physical sciences.

At this point, it is useful to comment on the later development of this story. During the fifties and sixties, much of my work centered on other power laws in economics and other social sciences. Zipf's first claim proved to be sound, but I soon disproved his second claim by also discovering or identifying many power laws in different branches of the physical sciences, other than the mainstream. My vocabulary was different, but the methods I soon adopted were those of renormalization and the search for fixed points.

Worse even than the first half of my thesis, the second half dabbled with a subject that was viewed as no longer part of active physics. This was reasonably true in 1952, but not for long. In the late 1960s and early 1970s, the study of critical phenomena ushered a brilliant period in the history of statistical physics. What did it bring? It brought renormalization, fixed points, and a contingent of new scaling laws that became better understood in that context than anything I used to know.

All those developments came too late to influence my work on financial prices, on long-term dependence in physical science, and on turbulence. Conversely, the explosion of interest in critical phenomena owed nothing to my work. However, to anticipate again on this story, I found it proper – in fact exhilarating – to break my youthful vow. New friends made it easy to join a mainstream of physics and contribute my share to the studies of critical phenomena and/or the basic physical clusters. They have become generally viewed as truncated fractals.

All in all, the details of my 1952 dissertation were never important. My explanation of Zipf's law keeps being rediscovered only to sink back into oblivion.

To the contrary, the broad central argument of my thesis – the appreciation of the significance of the power laws and their explanation (soon broadened by renormalization and fixed points) – proved to have very "strong legs." Also, much of my work outside of mainstream physics has been directed by a "generalized thermodynamics." The great J.W. Gibbs has been criticized for fathering a theory that went beyond gases, to include assemblies of sewing machines (I quote from memory). The thermodynamics of roughness may have gone even beyond sewing machines!

Back to 1952: my dissertation had to be pigeonholed. The physicist Alfred Kastler (1902-1984), a close family friend, was consulted; on the basis of positions about to open up, we decided that my Ph.D. was in applied mathematics.

In launching this unorthodox career, I was greatly influenced by the examples of *Game Theory and Economic Behaviour* by John von Neumann (1903-1957) and *Cybernetics* by Norbert Wiener (1894-1964). Each seemed to be a bold attempt to put together and develop an appropriate new mathematical approach to very old and very concrete problems that overlapped several disciplines.

I spent 1953-1954 at the Institute for Advanced Study in Princeton as the last post-doc sponsored by von Neumann; a Rockefeller Foundation grant was arranged by Warren Weaver. At one point in the sixties, Weaver revealed having been asked by "Johnny" (then dying of cancer) to keep an eye on me, because my chosen lifestyle was dangerous and I may need help.

Though von Neumann was an originator of computers, he died before I became aware of their potential. Years later, I became fully involved and they played key roles in my career and research.

IBM RESEARCH (1958-1993); YALE (SINCE 1987)

After four years in Geneva and France, I returned to the United States to work at IBM. As an environment to flesh out my ambitions, IBM Research seemed unpromising. But it proved to be far better than any university department, whether in France or America. Research universities are loose conglomerates of various departments. A department is like a small specialized business whose principal products are narrowly focused articles, books, and new graduates ready to join the corresponding guild. On the other hand, in order to serve the corporation, IBM's Research Division took a longer and deeper view. Scientifically, it was broadly based and historical circumstances prepared it to risk sheltering a few one-man projects not limited to an established area. It started in the 1950s with none of the glamor of its competitors in academia and industry. By 1993 (when the policy changed), the quality of its staff, most notably the abnormally large numbers of resident mavericks, had made it one of the most respected scientific powerhouses, worldwide.

My direct contributions to IBM's business were intermittent but, all counted, I think IBM "lost" no money on me. I started in the Mathematical Sciences Department, united by a common interest in the sophisticated use of computers. Later, interests and daily scientific interactions made me move to Physical Sciences. Notwithstanding those labels, every discovery I made while at IBM fell well outside the scope of any university department. I did not add new fields because of boredom or coercion, but because of new opportunities – typically represented by pictures.

In my case, the advantages offered by IBM clearly overwhelmed its drawbacks. Geographical isolation made it harder to keep up with prevailing scientific fashion, but this did not much matter for me. There was hardly anybody with whom I could discuss technical matters, so my investigations moved more slowly than I wished. But being at IBM handed me, again and again, marvellous opportunities to make long strings of discoveries that, under different conditions, would have been either discouraged by funding agencies or shared by immediate competition. An unperturbed research environment was a totally unexpected gift.

I became popular in diverse departments of great universities as a visiting professor, but no major institution wanted a permanent professor with unpredictable interests. Once, when still relatively young, I received from a top U.S. university, a very glamorous offer that was retracted the next day, after the Dean became concerned by my professional activity in several other fields. Not knowing how to fit me in an existing peer-review pigeon-hole, that university saw no way to employ me. IBM did. In 1974, I was made an "IBM Fellow" and given a very small staff. IBM Fellows received the enviably vague "freedom to choose and carry out their work in areas related to their specialization in order to promote creative achievements."

Anyhow, no real choice was open until very much later. By the time a new policy made me retire from IBM, I was fortunate to have become associated with the Yale Mathematics Department. Building on its existing tradition of thinking of people first and area next, it proved extremely receptive to my approach to the sciences. Everyone followed one's work wherever it led, and twists and turns required no justification. Incidentally, Yale may have, in my case, set a world record for the latest age an individual was tenured for the first time.

IT'S A WILD WORLD OUT THERE

Back to my life-long pursuit, the taming of roughness. Each time this concept is described, there is wide agreement that it is ubiquitous. Yet, one might argue that specialists in metal fractures, proper physics, and financial prices consistently deny the specificity of roughness, at least by implication. Indeed, tools like root-mean-square or Fourier spectrum are well-suited to handle nice phenomena that clearly separate into a deterministic "true value" and a small "perturbation." All are forms of what I called the state of "mild randomness."

I have argued that the cases of roughness I studied (exemplified early in this essay) are very different and mark a new stage of indeterminism. In handling the rough surface of a broken metal or of Earth's relief, widely used approaches amount to invoking a pile of elementary shapes, for example, pyramids.

However, a sufficiently detailed specification of such a pile demands a multiplicity of rules that defeats the primary goal of every scientific model. A properly parsimonious model is required to have a deceivingly simple input and its output must not merely rephrase the input. It must be rich in unanticipated structure, open unanticipated new issues and, of course, fit the facts.

In strikingly visual fashion, this primary goal is fulfilled by the simple scale-invariance principles that I first used in the 1950s and that led to my fractal model of Earth's relief and of metal fractures. Beyond fascinating both professionals and amateurs, those fractals' goal was pragmatic: to gain acceptance for my study (less perspicuous but famously challenging) of turbulence and exotic ("1/f") noises and of financial prices. Fractals/multifractals do not explain turbulence (nobody has) but provide a more accurate description.

In addition, they raise a major theoretical issue by threatening or even breaking a separation that is a characteristic of "mild" variability, between a true value and random perturbation. Not only variability does not disappear by averaging, but it creates extraordinary shapes that are often hard to distinguish from the "real structures."

For example, striking configurations inevitably appear in my fractal models of the distribution of galaxies. Their being hard to tell from galaxy clusters raises a question. Are galaxy clusters real objects, or merely a name concocted by humans to achieve a quick sense of control over a mystery perceived as threatening? This deep and troubling question has many counterparts elsewhere in my work.

Altogether, turbulence is best understood if viewed as lying well beyond that which I called the state of mild variability. It seems to belong, instead, to that which I called the state of "wild variability and randomness."

Like turbulence, roughness was slow to be studied and its study continues to encounter perceived delays and difficulties. This fact may solely reflect the scientists' inadequacies but I do not think so. It seems that, by attacking those neglected phenomena, science has come to a frontier of qualitatively increased complexity and changing ambitions.

THE "BOOK-LENGTH ESSAYS" (1975, 1977, 1982)

The fate of being called a good physicist by mathematicians and a good mathematician by physicists was one I always feared and fought. And I discount all praise for my work in economics by people other than economists. While it is good to be free of the economists' "before the fact" censorship, I also want to win "after the fact" understanding and approval of at least a part of their community.

However, I always kept a strong foreign accent, scientifically speaking. Several of my papers were rejected and other papers' drafts did not seem worth finishing. Thus, my work on the distribution of galaxies would not be published until it became known and understood, but could not become known and understood until it was published.

This dilemma was solved by publishing a great deal of original work in successive versions of a broadly-based "Essay." To write and illustrate those books demanded enormous effort, a full-time programmer and one or two part-time associates. Furthermore, no book is published without some expectation of success and, in this case, the chances of success could not even be estimated in advance. But I was able to provide the first publisher with a camera-ready product. The stigma attached to being, to a degree, a vanity-press book is erased by large sales, many translations (including one into Basque), and wide influence.

DIVISION OF LABOR BETWEEN "GUILDS" AND THE MAVERICKS' ROLE IN SCIENCE

History proves that it is a misconception that theoretical scientists do their best work when very young; that a person who hasn't already earned admiration by the age of 25 or 30 will never catch up. Were it valid, this rule would have annihilated all the mavericks. But I view it as being in good part a self-fulfilling prophecy that pleases the scientific "guilds" but is harmful.

To the contrary, mavericks' kind of research requires slow and gradual buildup and maturation – and seems to extend longer than "the norm." The coming generation will have to change professions and activities much more often than mine could get away with. They will find it vitally important to be more adaptable and open to change, a step that is incompatible with the worship of youth.

Mavericks need not be encouraged. Should they be tolerated? A century ago, the famous sociologist Emile Durkheim (1858-1917) wrote that the division of labor is "a moral rule for human behavior... a categorical rule... that should be imposed as a duty. It is true that those who infringe it are not meted out any precise punishment laid down by law, but they do suffer rebuke."

A more recent justification for the microscopic subdivision of labor forsakes duty and calls it the fruit of necessity, fully and uniquely determined by the inevitable sweep of history. Needless to say, mavericks read the tea leaves of history differently.

Consider the scientific funding rule that preserves each branch of science from interference from other branches. It started in the USA, when Emanuel Piore (whom I knew later as the real founder of IBM Research) set the rules of the National Science Foundation. Later, it conquered the world. As a result, every established field, at any given instant, is entitled to select a single "best way" to train and co-opt new participants. The elimination of every pressure from other fields was not a reform but a revolution. Helped by lavish funding, it led to decades of productive activity.

But today, it is leading to catastrophe. Every old civilization knows that even the best intentioned and best designed rule, if made absolute, eventually creates a terrible situation. To an astonishing and dismaying degree, science has willingly lowered itself to a feature of professional sport, where an athlete's worth is solely assessed within a narrowly defined event. To me, this makes the Olympics a very dull form of professional entertainment.

Every so often, the Olympics adds new events, and similarly new academic fields keep being created. They quickly organize themselves as guilds opposed to all other guilds. Even a token of unplanned diversity is intolerable. Contrary to Durkheim, I believe that *for its own pragmatic welfare, society must allow some persons to opt out of the strict division of labor.* Given the attractiveness of a guild's protection, the so-called "malcontents" or troublemakers will not be numerous. Rebuking all of them (for all practical purposes, crushing them) and labeling them as "immoral" or simply as "avoidable waste of money and positions," does nothing to improve society.

A serious threat that must be addressed is due to the fact that science as a whole (as well as large areas like physics or mathematics and smaller guilds) have neither an endowment they can live off, nor (with the possible exception of healers and star-gazers) the automatic constituency of sports. To survive, they need the approval of the broader society, hence deeply depend on credible interpreters. This service was provided by veterans of World War II laboratories who were selected when guild rules were suspended and mavericks were not rebuked. Today, guilds get away without ever explaining themselves to other guilds, not to mention the "common man."

How does all this relate to mavericks? If guilds have no choice but to acknowledge that explaining their activity has become a matter of survival, they will have to either identify and train or co-opt a few members well suited to the challenge. This is a task best performed by mavericks.

While a maverick's story is not in the least an example to follow, it may carry the following useful message: a good sprinkling of diversity is just as indispensable to the good functioning and survival of science as it is to the welfare of society as a whole.