

Understanding the Creativity of Scientists and Entrepreneurs

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Preliminary

Introduction

Economists' theoretical models consistently identify technological change as a major source of economic growth. Simon Kuznets (1966, p.9) long ago observed that modern technological change has been above all a product of the application of scientific knowledge to problems of economic production. Kenneth Arrow (1962, p. 624) in turn noted that talented individuals are the primary source of innovations. Studying how innovators make their discoveries therefore holds out the possibility not only of understanding how technological change is produced, but perhaps also of increasing it. This paper demonstrates how the innovations of individual scientists and entrepreneurs can be understood within a general analytical approach to creativity.

During the past decade, I have discovered that there are systematic patterns that connect the motivations and methods of individual innovators in the arts to both the nature of their work and their life cycles of creativity. There are two very different types of artistic innovator. *Experimental* innovators seek to record their perceptions. They work tentatively, proceeding by trial and error, and build their skills gradually over time. As a result, they tend to arrive at their greatest contributions gradually, after long periods of study, late in their lives. In contrast, *conceptual* innovators use their art to express their ideas and emotions. The precision of their goals allows them to plan their work, and to execute it decisively. Their most radical new ideas, and consequently their greatest innovations, occur suddenly, early in their careers, when they are least constrained by fixed habits of thought (Galenson 2006).

These patterns have been established empirically, by studies of a large number of important artists — painters, poets, novelists, sculptors, architects, filmmakers, songwriters, and

photographers (Galenson 2010a). The task of this paper is to show that these same patterns apply to scientists and entrepreneurs. This will be done through studies of the careers and methods of individuals — two of the greatest scientists of the modern era, and two great contemporary entrepreneurs.

Charles Darwin (1809-1882)

My industry has been nearly as great as it could have been in the observation and collection of facts.

Charles Darwin (2005, pp.114-15)

Charles Darwin always loved the outdoors; he believed that he “was born a naturalist” (Mayr 1991, p. 3). He had less interest in school, and was a mediocre student. The biographer Gavin de Beer (1965, p. 23) observed that “Darwin was a striking example of the fact that in educational matters the race is not always to the swift; he was an outstanding late developer.” His father sent Charles to Edinburgh University to study medicine, but Charles hated his studies, and left without a degree. His father then sent him to Cambridge, to prepare for a career as a clergyman. Charles again had little interest in his courses, but loved collecting beetles in the local fens, and as a result met several professors of natural history. Shortly after he graduated, one of these professors offered him passage on a ship bound for a voyage around the world. This voyage stretched to five years, and Darwin returned to England in 1836 with a new sense of purpose.

Darwin (2005, p. 64) later wrote that “The voyage of the *Beagle* has been by far the most important event in my life and has determined my whole career.” He considered it his true school: “I owe to the voyage the first real training or education of my mind . . . I was led to attend closely to several branches of natural history, and thus my powers of observation were improved.” On the voyage he wrote more than 2,500 pages of diary and notes, and collected

more than 1,500 samples of species in spirits and nearly 4,000 labeled animal skins and bones.

Darwin's time on the *Beagle* effectively started his career: he would devote the rest of his life to questions that first occurred to him during the five years of the voyage.

In 1837, Darwin became convinced of the existence of evolution. He arrived at this belief empirically: a key contribution was the determination by an ornithologist that the mockingbirds Darwin had found on three different islands in the Galapagos were actually three different species. In his autobiography, Darwin (2005, pp.98-99) recalled that in 1838, he first read Malthus on *Population*,

and being well prepared to appreciate the struggle for existence which everywhere goes on from long-continued observation of the habits of animals and plants, it at once struck me that under these circumstances favorable variations would tend to be preserved, and unfavorable ones destroyed. The result of this would be the formation of new species. Here, then, I had at last got a theory by which to work.

This was the theory Darwin would later call *natural selection*, that explained by natural causes all the adaptations of living organisms that had previously been attributed to divine design. In 1842, Darwin wrote a 35-page abstract of his theory; in 1844, he expanded this to a manuscript of 230 pages. He had no intention of publishing this, but he did leave instructions to have it published in case of his death.

Darwin did not in fact publish his theory of evolution until 1859, two decades after his initial formulation of natural selection. Scholars have speculated about the causes of this long gap. The Nobel laureate James Watson (2005, pp. xiii, 340) stressed the magnitude of Darwin's contribution: "Copernicus, Galileo and Newton had removed the Earth from its central position in the universe . . . But the position of man, as the image of God on earth, was left unchanged by their revisions of the received cosmogony. Darwin changed this." Hence the delay: "Extraordinary claims require extraordinary evidence, so Darwin began a long process of

accumulating fact after fact that could be explained better from an evolutionary perspective than any other.” Darwin (2005, p. 102) himself commented in his autobiography that “I gained much by my delay in publishing from about 1839, when the theory was clearly conceived, to 1859.” And much of what he gained was unanticipated.

In 1846, Darwin began to study the last specimens that remained from the *Beagle* voyage, of small crustaceans. He intended to spend only a few months, to write a few articles. Instead, he eventually spent eight years making a complete survey of all known species of barnacles, during which he examined 10,000 specimens, published four volumes, and earned the Royal Society’s Royal Medal for Natural Science. The great expansion of the barnacle studies was a result of several major discoveries that emerged from the project: one provided powerful evidence that evolution had taken place, while another demonstrated the pervasiveness of natural variation within a species, which provided the basis for natural selection. Both the expansion of the barnacle project and its serendipitous results were products of Darwin’s inductive method, for he never focused narrowly on any aspect of a problem, but always tried to learn all he could about any subject he touched.

In 1859, at the age of 50, Darwin published *On the Origin of Species by Means of Natural Selection, or: The Preservation of Favoured Races in the Struggle for Life*. The biologist Edward Wilson (2009, p. xv) recently wrote that “I believe we can safely say that the *Origin of Species* is the most important book of science ever written . . . No work of science has . . . so profoundly altered humanity’s view of itself and how the living world works.” Initially intended as an article, the *Origin* grew into a book of 500 pages, because of the complexity of the argument and the vast amount of evidence it contained. The philosopher David Hull (La Vergata 1985, p. 925) observed that “Although there is, broadly speaking, a deductive core to the *Origin*,

by and large it is one long, involved argument conducted in the midst of a mass of very concrete facts. Darwin's argument as presented in the *Origin* is a genuinely inductive argument." Nor was the *Origin* a final product in 1859, for it would go through another five revised editions during Darwin's lifetime (one of the most famous phrases associated with the book, "survival of the fittest," appeared for the first time in the fifth edition) (Ruse and Richards 2009, p. 334; Browne 2002, p. 312). In one respect, Darwin's cautious publication delay continued even after 1859, for the *Origin* contained no analysis of human evolution. It was not until 1871 that he published *The Descent of Man, and Selection in Relation to Sex*. This was based on "a great deal of additional hard work, much of it highly original," as Darwin passed the age of 60 (Gruber 1981, pp. 24-33).

Great scientists must explain things, so no important scientist can be purely an observer. Darwin (2005, pp.114-15) understood that useful observation necessarily had to be guided by theory, but he stressed that he had always "endeavored to keep my mind free, so as to give up any hypothesis, however much beloved . . . as soon as facts are shown to be opposed to it. Indeed . . . I cannot remember a single first-formed hypothesis which had not after a time to be given up or greatly modified. This has naturally led me to distrust greatly deductive reasoning in the mixed sciences." The biologist Ernst Mayr (1991, p. 10) considered Darwin a great empiricist: "He was not only an observer but a gifted and indefatigable experimenter whenever he dealt with a problem whose solution could be advanced by an experiment." Late in his life, Darwin (2005, p. 113) provided a striking characterization of himself as an inductive scholar: "My mind seems to have become a kind of machine for grinding general laws out of large collections of facts."

Albert Einstein (1879-1955)

Imagination is more important than knowledge.

Albert Einstein (Viereck 1930, p. 447)

In the history of science, the name *annus mirabilis* has long been given to 1666, when 24-year-old Isaac Newton developed calculus, an analysis of the light spectrum, and the laws of gravity. Today that title is also given to Albert Einstein's work of 1905. Prior to 1905, Einstein had published a total of five little-known papers. He had completed four years of studies in physics at Zurich, but his dissertation had been rejected, so he had not yet earned a Ph.D. After failing to find an academic job of any kind, he was working in an entry-level position in the Swiss Patent Office in Bern. But during 1905, the 26-year-old Einstein wrote five papers that transformed the discipline of physics.

The diversity of the subjects of the *annus mirabilis* papers is a continuing source of amazement to scholars. But the physicist Gerald Holton (1988, pp. 193-94) observed that the three epochal papers — on the quantum theory of light, Brownian motion, and special relativity theory — shared a common structure: “Each begins with a statement of formal asymmetries or other incongruities of a predominantly aesthetic nature (rather than, for example, a puzzle posed by unexplained experimental facts), then proposes a principle . . . which removes the asymmetries as one of the deduced consequences, and at the end produces one or more experimentally verifiable predictions.” Holton stressed that these papers all represented attempts “to solve problems by the *postulation* of appropriate fundamental hypotheses and to restrict those hypotheses to the most *general kind* and the *smallest number* possible.” The Nobel laureate Louis de Broglie (1959, p. 110) compared Einstein's early papers to “blazing rockets,” produced by “the originality and genius of a mind which can perceive in a single glance, through the complex maze of difficult questions, the new and simple idea . . . suddenly to bring clarity and

light where darkness had reigned.”

After he created the special theory of relativity, Einstein realized it was incomplete. He devoted most of the next decade to developing a new field theory of gravity, and to generalizing his relativity theory. He now discovered that more sophisticated mathematics was necessary, and enlisted a mathematician to help him. After intensive effort, Einstein declared victory, stating on November 25, 1915, that “the general theory of relativity is closed as a logical structure” (Pais 2005, p. 256). At the age of 36, he knew even without reactions from other scholars that he had achieved “the most valuable discovery of my life,” as he told a friend that “The theory is of incomparable beauty.” And his peers soon agreed. Paul Dirac, a Nobel laureate in physics, called general relativity “probably the greatest scientific discovery ever made” (Isaacson 2007, pp. 223-24).

Many conceptual innovators turn from youthful revolutionaries into aging reactionaries, and Einstein is a classic example, as the story of his later intellectual life is inextricably tied to his resistance to quantum mechanics, a revolution created by younger physicists, that ironically built in part on Einstein’s own quantum theory of light of 1905. Einstein was never able to accept the idea that nature was probabilistic, for he was committed to classical notions of determinism. Many of his colleagues were frustrated by Einstein’s unwillingness to accept the new paradigm, in spite of the accumulating empirical evidence that supported it. So, for example in 1926, Einstein wrote to a friend, the physicist Max Born, that “Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing.” Born (2005, pp.88-89) was exasperated not only by Einstein’s negative verdict, but by its unsatisfactory basis: “he rejected it not for any definite reason, but rather by referring to an ‘inner voice.’”

Many scholars have pondered why Einstein would not accept the new paradigm. A

friend and biographer of Einstein, the physicist Abraham Pais (2005, pp. 463-64), suggested a cause of Einstein's late intransigence: "As a personal opinion, it seems to me that making great discoveries can be accompanied by trauma, and that the purity of Einstein's relativity theories had a blinding effect on him. He almost said so himself: 'To the discoverer . . . the constructions of his imagination appear so necessary and so natural that he is apt to treat them not as the creations of his thoughts but as given realities.'" Werner Heisenberg (2005, p. xxxvii), a Nobel laureate whose uncertainty principle was a foundation of quantum mechanics, agreed that Einstein became constrained by habits of thought: "in the course of scientific progress it can happen that a new range of empirical data can be completely understood only when the enormous effort is made . . . to change the very structure of the thought processes. Einstein was apparently no longer willing to take this step, or perhaps no longer able to do so." Several scholars have suggested that over time Einstein became concerned more with mathematical elegance than physical reality. So for example Dirac (1982, p. 83) reflected that "Einstein seemed to feel that beauty in the mathematical foundation was more important, in a very fundamental way, than getting agreement with observation," and the Nobel laureate Steven Weinberg (2005, pp. 102, 108) contended that "The oracle of mathematics that had served Einstein so well when he was young betrayed him in his later years." Weinberg described the cost of Einstein's stubbornness: "Tragically, Einstein spent almost all of the last 30 years of his life pursuing [a theory that gave a unified account of gravity and electromagnetism], not only without success, but without leaving any significant impact on the work of other physicists."

Albert Einstein was an iconoclastic rebel, who consistently challenged authority and celebrated individualism. He was an archetypal conceptual innovator, who made dramatic imaginative leaps through theoretical thought experiments rather than careful analysis of

empirical evidence. His prodigious ability to solve scientific problems through highly abstract deductive reasoning made him a symbol of modern scientific genius. Yet his faith in his own intuition prevented him from accepting the discoveries of a younger generation, and caused him to spend the last three decades of his life in growing intellectual isolation from the discipline he had once dominated. Perhaps he was not surprised by this, for at 38, just two years after his greatest achievement, he lamented to a friend that “Anything truly novel is invented only during one’s youth. Later one becomes more experienced, more famous – and more *blockheaded*” (Isaacson 2007, p. 316).

Muhammad Yunus (1940-)

I hoped that if I studied poverty at close range, I would understand it more keenly.

Muhammad Yunus (2003, p. ix)

Muhammad Yunus was born into a prosperous family in Chittagong, the largest port in East Pakistan. He was a good student, and after receiving an MA from Dhaka University, he accepted a job as lecturer in economics at Chittagong University. To advance his career, he applied for and received a Fulbright scholarship to study development economics in the United States, and earned a Ph.D. from Vanderbilt University. In 1969, he became an assistant professor at Middle Tennessee State University in Murfreesboro.

Early in 1971, Yunus was excited by the news that Bangladesh had declared its independence from Pakistan. When Bangladesh won its war for independence, he returned home to help build the new nation. He took a government job, but had little to do, and soon became bored. He returned to Chittagong, as head of the university’s economics department. He enjoyed teaching, and looked forward to a long academic career. Once again, however, events disrupted his plans.

In 1974, Bangladesh suffered a devastating famine, that killed hundreds of thousands of people. The poor did not march or demonstrate: they simply lay down in the streets and died.

Yunus (2003, pp. viii-ix) felt guilty for living in a privileged fantasy world:

I used to feel a thrill at teaching my students the elegant economic theories that could supposedly cure societal problems of all types. But in 1974, I began to dread my own lectures. What good were all my complex theories when people were dying of starvation on the sidewalks and porches across from my lecture hall? My lessons were like American movies where the good guys always win. But when I emerged from the comfort of the classroom, I was faced with the reality of the city streets.

Yunus lost his faith in the abstractions of economic theory: “I needed to run away from these theories and from my textbooks and discover the real-life economics of a poor person’s existence.” He went to a small village near his university, and began talking to the residents. Instead of repeating the theories of others, as he had done as a economist, Yunus now became an innovator. And the innovations were experimental, with concepts that grew directly from empirical observation, and practices constructed by trial and error:

My repeated trips to the villages around the Chittagong University campus led me to discoveries that were essential to establishing the Grameen Bank. The poor taught me an entirely new economics. I learned about the problems that they face from their own perspective. I tried a great number of things. Some worked. Others did not (Yunus 2003, p. ix).

Yunus’ most surprising discovery was that many villagers were trapped in poverty because they lacked very small amounts of capital for small-scale manufacturing: forced to borrow from moneylenders, their profits were effectively confiscated by usurious loans. He was amazed at the tiny sums involved: “I had never heard of anyone suffering for the lack of *twenty-two cents*” (Yunus 2003, p. 48). Yet when Yunus appealed to bankers to remedy this problem, they laughed at him: the loans the villagers needed were too small to justify the necessary paperwork, and the illiterate poor were in any case unable to fill out the forms. Nor would banks

lend to borrowers who had no collateral. Out of frustration, Yunus initiated a pilot lending project in 1976. The next year, he named the project *Grameen*, “of the village.” In 1979, he took temporary leave from Chittagong University, to manage Grameen full-time. In fact, he would never return to teaching.

Over time, Yunus and his assistants developed an effective set of lending practices. Central to these was the use of groups. Loans were made only to villagers who organized themselves into groups of five, each of whom was responsible for the loans of all the group members. Peer pressure was thus used to assure repayment. There were no legal documents for loans: “the bank [was] built on human trust, not on meaningless paper contracts” (Yunus 2003, p. 70). In spite of the fact that Grameen’s early programs consistently realized repayment rates over 95% on loans, conventional banks refused to take them over, insisting — contrary to the evidence that Yunus produced — that lending without collateral could not succeed. As a result, in 1983, when Yunus was 43 years old, he launched Grameen Bank as an independent company. He based Grameen on the experimental principle that experience is the best guide. All the bank’s employees have always been encouraged to suggest changes in any of the bank’s rules if they see better ways of dealing with problems they encounter in their daily work.

Grameen’s success was dramatic. It rapidly gained new borrowers, and diversified into loans for housing and irrigation. By 2006, Grameen was making loans to nearly 7 million people, 97% of whom were women, in 73,000 villages throughout Bangladesh. The bank’s success was such that Yunus (2007, pp. 237-48) reported these figures in his acceptance speech for the 2006 Nobel Peace Prize, awarded jointly to him and the Grameen Bank, in recognition of the development of microcredit into a powerful tool in the struggle against poverty. Within Bangladesh, Grameen has expanded into an industrial empire, comprising more than two dozen

separate nonprofit companies, including the largest telecommunications company in the country. And beyond Bangladesh, independent of Grameen, microcredit has spread throughout the world.

Microlending violated a fundamental tenet of banking — one so fundamental that it had never been tested. Muhammad Yunus’ challenge to that assumption was based entirely on empirical observation and experimentation. His ability to solve the problem of usurious moneylending was a product of his rejection of the conceptual approach to economics he had learned in school. Instead of assuming collateral was necessary for lending, he approached banking without preconceptions, by speaking to villagers about their problems:

I had no intention of lending money to anyone. All I really wanted was to solve an immediate problem . . . I had no idea what I was getting myself into. I was walking blind and learning as I went along (Yunus 2003, p. 57).

In 1974, when he became a witness to an economic catastrophe, Yunus decided that it was not sufficient to *imagine* the world, but that he had to *see* it. His desperate decision to learn about poverty directly from those who suffered from it led him to become a banker and entrepreneur, and a major figure in the fight against world poverty.

Steve Jobs (1955-2011)

I understand the appeal of a slow burn, but personally I’m a big-bang guy.

Steve Jobs (Beahm 2011, p. 66)

When illness forced Steve Jobs to leave Apple, Ken Auletta (2011) of the *New Yorker* called him “the twentieth century’s Thomas Edison.” The description of Jobs as an inventor prompted a flurry of protests. But these objections were not new. In 1976, when the 21-year-old Jobs and 26-year-old Steve Wozniak were starting a company, Wozniak’s father, who was an engineer, objected to the partnership agreement that gave Jobs and his son equal shares:

“You don’t deserve [anything],” he told Jobs. “You haven’t produced anything.” Jobs began to cry . . . He told Steve Wozniak that he was willing to call off the

partnership, “If we’re not fifty-fifty,” he said to his friend, “you can have the whole thing” (Isaacson 2011, p. 73)

Steve Wozniak was an electronics genius, who entirely by himself had invented the world’s first practical home computer, the Apple II, which would eventually sell nearly six million units. Yet Wozniak accepted the equal partnership with Jobs, because he understood the value of their division of labor. Thus he explained that Jobs “was the one who thought we could make money . . . I was the one who designed the computer . . . and I had written the software, but Steve is the one who had the idea we could sell the schematics” (Malone 1988, p. 68). Wozniak was correct: Jobs’ entrepreneurial ability would eventually make Apple the most valuable company in the world. John Sculley (1987, pp. 160-62), who was hired by Jobs to be president of Apple, later forced Jobs out of the company, and still later was forced out by Jobs, observed that Jobs “didn’t create anything really, but he created everything . . . Steve lacked the engineering ability to create a product, but he instinctively knew what needed to be created to succeed.”

Jobs ran Apple as a dictatorship, in which he made all major, and most minor, decisions. His approach was highly conceptual; his understanding of creativity did not involve extensive effort or research, but rather thought and momentary inspiration. He believed creativity was greatest in the absence of expertise: “There’s a phrase in Buddhism, ‘beginner’s mind.’ It’s wonderful to have a beginner’s mind.” Experience destroyed creativity: “Our minds are electrochemical computers. Your thoughts construct patterns like scaffolding in your mind . . . In most cases, people get stuck in these patterns, just like grooves in a record, and they never get out of them.” He believed that creativity ended at age 30: “It’s rare that you see an artist in his 30s or 40s able to contribute something amazing” (Sheff 1985). One consequence of Jobs’ conceptual approach was his disdain for market research in conceiving and selling new products. He maintained that consumers couldn’t know they wanted revolutionary products: “Our job is to

figure out what they're going to want before they do. I think Henry Ford once said, 'If I'd asked customers what they wanted, they would have told me, "A faster horse!"'" (Isaacson 2011, p. 567).

Jobs called Bob Dylan "one of my all-time heroes" (Isaacson 2011, p. 416). He opened the public unveiling of the Macintosh by quoting a verse of Dylan's "The Times They Are a-Changin,'" and played "Like a Rolling Stone" at the public launches of both the iPhone and the iPad. Jobs considered Dylan a role model:

As I grew up, I learned the lyrics to all his songs and watched him never stand still. If you look at the artists, if they get really good, it always occurs to them at some point that they can do this one thing for the rest of their lives, and they can be really successful . . . That's the moment that an artist really decides who he or she is. If they keep on risking failure, they're still artists. Dylan and Picasso were always risking failure (Schlender 1998).

The reference to both Picasso and Dylan suggests that Jobs had thought deeply about how to avoid losing his creativity as he grew older. Picasso was the archetypal example of a great conceptual innovator who made not a single major innovation, but a series. The key to this was the independence of Picasso's innovations, for each was a new, fresh idea, unrelated to the others. Producing an important conceptual innovation requires the ability to approach familiar problems in entirely new ways. Few people can do this once, and fewer can do it more than once, for even the greatest conceptual innovators normally become wedded to their own innovations. Picasso was a unique case in modern painting, who made three fundamental, revolutionary innovations within the span of less than two decades (Galenson 2009a, Chap. 7). Dylan deliberately followed Picasso in this respect, making several major innovations by abruptly changing the style and content of his music (Galenson 2009b). And Steve Jobs became the greatest serial innovator of Silicon Valley, changing not only the market for computers, but also subsequently those for movies, music, and telephones.

Walter Isaacson (2011, p. 566) called Steve Jobs “the greatest business executive of our era, the one most certain to be remembered a century from now.” Jobs was responsible for creating Apple, and later for transforming it from a computer company to a manufacturer of consumer products. He had a talent for identifying markets that were populated by inferior products, for hiring excellent engineers and designers, and, through a mixture of charm and intimidation, inspiring them to create excellent products. He had a flair for creating excitement around these products, and convincing a broad public that they needed them. He became a symbol of innovation, who stood out even among the technology titans of Silicon Valley, because he made not one but a series of innovations, which served very different purposes, and spanned markets that were not previously connected. His longtime rival Bill Gates reflected that Jobs had “a sense of people and product that, you know, is hard for me to explain. The way he does things is just different and I think it’s magical” (Isaacson 2011, p. 464).

Collaboration

The analysis presented above refers exclusively to the creativity and life cycles of individual innovators. Yet collaborative research is now the norm in the physical sciences, and entrepreneurs work with many other people within firms. An analysis developed from the experiences of individuals might consequently appear irrelevant for understanding innovations that are the joint products of groups.

In fact, however, groups of innovators, whether in universities or corporations, in most respects collectively conform to the models presented here, working like either the experimental or the conceptual innovator. This is a consequence of a necessary condition for successful collaboration. Specifically, collaboration can succeed only if participants share a body of knowledge and techniques, that the scientist Michael Nielsen (2012) calls a *shared praxis*. A

shared praxis does not exist when there is disagreement over basic values. Such disagreements destroy collaborations, for arguments cannot be settled. Arguments can be resolved only if there is general agreement on standards for what it means for analyses or procedures to be valid or correct. And this agreement will normally exist only when all participants in a project are either experimental or conceptual in approach. Scientists typically choose to work only with others who share their approach, and entrepreneurs generally hire employees who share their approach. Teams of scientists, or groups of researchers in corporations, can therefore be treated collectively as either experimental or conceptual innovators.

Apple provides an example of a company that has functioned as a conceptual innovator, due to the leadership of Steve Jobs. Jobs would imagine a new product, that hadn't yet been invented: "I can see the product as if it's sitting there right in the center of the table. It's like what I've got to do is materialize it and bring it to life." Lacking the technical skills to create products himself, Jobs hired engineers who would translate his vision into a real product. He supervised their work carefully, often meeting daily with a technical team. When an engineer complained that some feature or process was not practical, or too difficult to produce, Jobs' response was simple: "If you can't do it, I'll find someone else who can" (Sculley 1987, pp. 162, 165) As a result, teams of workers at Apple collectively took on the personality of a conceptual innovator.

Apple and Grameen illustrate a common difference between conceptual and experimental entrepreneurs. As noted above, Jobs was a dictator: he made decisions, and his employees executed them. In contrast, Yunus is a democrat: Grameen's policies are made only by meetings of department heads, and branch managers are encouraged to try out their own ideas, and to pass on successful practices for testing and replication elsewhere (Wahid 1993, p. 12).

The two models produce very different practices. Whereas Jef Raskin, a manager at Apple, described Jobs as a mercurial despot who “would have made an excellent king of France,” Grameen has open debates among its employees, because Yunus believes that “innovation can only sprout in an atmosphere of tolerance, diversity, and curiosity” (Moritz 2009, p. 268; Yunus 2003, p. 102).

Conclusion

In the arts, great conceptual innovators are often precocious prodigies, who make revolutionary contributions early in their careers. Albert Einstein and Steve Jobs both fit this description. At the age of 26, before he had even secured an academic appointment, Einstein made three discoveries that changed physics — one of these later earned him a Nobel Prize, and another included what would become the most famous equation in all of science. And at the age of just 22, Steve Jobs formed a new company, and created the market for a radically new product, the personal computer.

Great experimental artists are wise old masters, whose work matures gradually over long periods, and who arrive at their greatest contributions late in their careers. Charles Darwin and Muhammad Yunus equally fit this description. Darwin spent decades studying evolution and its mechanisms, and published the first edition of what many consider the most important book in the entire history of science at the age of 50. Yunus became an entrepreneur gradually and unintentionally, founded a fledgling bank when he was 43, and built it into the foundation of one of the greatest industrial empires in Southeast Asia over the course of the next three decades.

These examples demonstrate that the careers of great scientists and entrepreneurs follow patterns similar to those of great artistic innovators. And this is not surprising, for they share the same basic approaches and motivations. Experimental scientists and entrepreneurs are inductive

empiricists. They immerse themselves in their subjects of interest, learning everything they can about them without strong preconceptions. The power of their generalizations becomes greater over time, because they are based on ever larger amounts of evidence. Their discoveries are often serendipitous, for their broad accumulation of evidence not only increases their knowledge, but frequently serves to make them aware of problems they had not previously recognized.

In contrast, conceptual scientists and entrepreneurs make dramatic breakthroughs early in their careers, by taking radical new approaches to old problems. For them, experience is the enemy, for it brings fixed habits of thought that inhibit new departures, as well as growing awareness of the complexity of their subjects, which equally interferes with the radical simplifications that characterize important conceptual innovations.

Conceptual and experimental scholars inhabit very different mental worlds. Experimental scientists observe reality directly, and study it inductively. Their conclusions are consequently provisional and probabilistic. Conceptual scientists create models of reality, which they analyze deductively. Unlike nature, models can be known with certainty, and conceptual results can be proved. These differences in method are manifest in the very language scholars use. Gillian Beer observed that Darwin's prose followed from his goal of describing sensory reality: "Conversation rather than abstraction is the predominant mode, and the emphasis is on things individually seen, heard, smelt, touched, tasted" (Beer 1983, p. 67). Jacques Roger observed that Darwin's prose not only stated, but mirrored, his view of nature: "Darwin's way of writing . . . [corresponds] to a particular view of nature, according to which diversity, polymorphic interrelations, and evolving equilibria are of higher value and significance than clear-cut distinctions, well-defined entities and one-to-one relationships" (Roger 1985, p. 817). In contrast to the detail and complexity of Darwin's prose, Einstein privileged abstraction and

simplicity. Abraham Pais (2005, p. 417) noted that “The early Einstein papers are brief, their content is simple, their language sparse.” Einstein (1954, pp. 225-26) declared that theoretical physics “demands the highest possible standard of rigorous precision in the description of relations, such as only the use of mathematical language can give.” To achieve this goal, he explained that the theorist “must content himself with describing the most simple events . . . Supreme purity, clarity, and certainty at the cost of completeness.”

Conceptual scholars pose precise questions, and answer them definitively, freeing them to go on to study very different problems. The diversity of the subjects of Einstein’s *annus mirabilis* papers provides an archetypal case in point, as does their conclusiveness. Thus Pais (2005, p. 417) observed that these early papers “exude finality even when they deal with a subject in flux. For example, no statement made in the 1905 paper on light-quanta needs to be revised in the light of later developments.” In contrast, experimental scholars often remain tied to a single problem for long periods because of their uncertainty as to their goals; they often spend as much time finding problems as solving them. The expansion of Darwin’s barnacle project is a classic example: as John van Wyhe (2007, pp. 191, 194) recently observed, Darwin “did not make a single conscious decision to do the entire group of barnacles in four volumes instead of just some papers; it was a gradual process.” After he finished the *Origin*, Darwin explained to a friend that the long period of preparation for that book was a result not only of his search for answers, but even more of the difficulty of formulating the questions:

I suppose that I am a very slow thinker, for you would be surprised at the number of years it took me to see clearly what some of the problems were, which had to be solved . . . Looking back, I think it was more difficult to see what the problems were than to solve them, as far as I have succeeded in doing; this seems to me rather curious.

The implications of this paper go beyond the strictly academic. More than 50 years ago,

Simon Kuznets (1962, p. 32) wrote that “we need far more empirical study than we have had so far of the universe of inventors; any finding concerning inventors . . . would be of great value . . . for public policy in regard to inventive activity.” The analysis of this paper points to how an improved understanding of innovators might affect public policy. Specifically, the demonstration that there are two very different life cycles of creativity in science and business contradicts a widely held, and potentially damaging, belief that creativity is the unique domain of the young. So for example the growth economist Paul Romer recently stated that “Young people . . . tend to be more innovative, more willing to take risks, more willing to do things differently,” and the scientist Francis Collins, director of the National Institutes of Health, argued that younger scientists should consequently be given more research funds than their older colleagues (Galenson 2010b). Romer and Collins both appear unaware of the existence, and importance, of experimental creativity. Janet Rowley, a medical scientist who discovered the genetic basis of cancer during the 1970s through careful observation, recently remarked that her research could not be funded today, because it was not motivated by theory. Yet her discovery made possible today’s treatments for leukemia (Dreifus 2011). Romer, Collins, and many others need to be made aware that science and business, like the arts, are populated not only by young geniuses like Einstein and Jobs, but also by old masters like Darwin, Yunus, and Rowley. Most of our current policies for fostering creativity are effectively aimed primarily or exclusively at increasing conceptual creativity. Devising additional policies aimed at increasing experimental creativity should be a high priority for economists. Helping both types of innovator may be critical for maximizing innovation, technological change, and economic growth.

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