Ed Fredkin and the Physics of Information: An Inside Story of an Outsider Scientist

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This article tells the story of Ed Fredkin, a pilot, programmer, engineer, hardware designer, and entrepreneur whose work inside and outside academia has influenced major developments in computer science and in the foundations of theoretical physics, and, in particular, in the intersection thereof, for the past fifty years.

No one likes outsiders. They usually claim to know more than anyone else, they pay little attention to the rules everyone else must abide by, and they rarely pay their dues. In science, however, outsiders are powerful engines of innovation,[1] and sometimes their work is instrumental in shaping an entire discipline. This article tells the story of Ed Fredkin, one such outsider scientist whose influence on the intersection of physics with computer science and information theory spans more than half a century.

A self-made millionaire, a USAF jet fighter pilot, an inventor, an entrepreneur, and an independent intellectual and autodidact, Fredkin has been working mostly outside the corridors of academia all his life. As a freshman at Caltech in 1952, he studied with scientists such as Linus Pauling but dropped out and joined the air force in the middle of his sophomore year. As one of the early hardware and software designers and computer experts in the United States, he befriended geniuses such as John McCarthy (1927–2011), Richard Feynman (1918–88), and Marvin Minsky (1927–2016). As a full professor at MIT (without so much as a bachelor's degree), he shared his innovative and novel ideas about computers, programming, robotics, graphics, and relationships between physics, information, and computation with many colleagues and students, guiding the latter through projects ranging from the

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Information & Culture, Vol. 51, No. 3, 2016

DOI: 10.7560/IC51306

world's first computer navigation system in an automobile, to exploring how arbitrary synchronous counters could be constructed using nothing but J-K Flip Flops, or how a computer could be built that could operate without dissipating any power whatsoever.

The physics of information as it is practiced today is based on the premise, advocated forcefully by Fredkin since the 1960s, that the most fundamental physical processes are likely to be discrete and deterministic in character, similar to the bits in a computer, and that any ordinary computer could exactly mimic them. Fredkin, however, is rarely credited for his ideas within academia and has remained an outsider scientist all his life. Based on his personal recollections, spelled out in a series of interviews that took place in the summer of 2013 and the winter of 2014, as well as on several published and unpublished manuscripts, I will here focus on three key themes in Fredkin's thinking and demonstrate how these have helped in establishing what, at the beginning of the twenty-first century, has turned out to be one of the most successful industries in the intersection of physics and computer science.

My goal in depicting Fredkin as an outsider scientist is threefold. First, my article is intended as a contribution to the historiography of science, in this case as yet another example of the important role played by outsider scientists in facilitating scientific and conceptual breakthroughs and the high price they sometimes pay for maintaining this role. Second, it is intended also as a contribution to the foundations of the physics of information, where issues such as the physical limits on computation and the disciplinary boundaries between computer science and physics are still under dispute even today. Finally, Fredkin's story exemplifies the roles of features of personality, institutions, and prior training that shape scientific novelty, and as such it is yet more evidence for the fruitfulness of combining the history and the philosophy of science in the study of the development of scientific ideas.

I start with a brief biography of Ed Fredkin that portrays his intellectual trajectory and how it was shaped by his exceptional career as one of the first computer hardware designers in the United States. I then focus on Fredkin's key contributions to the physics of information and conclude with an evaluation of his role as an outsider scientist both from a historical perspective and from a more foundational one.

Milestones

Ed Fredkin was born in Los Angeles in 1934 to often-poor Russian immigrant parents.[2] His childhood was not an easy one. His mother died from cancer when he was eleven, and her husband was unsupportive of and indifferent to Fredkin's achievements.[3] Despite that, Fredkin was a confident child. Scoring the highest in California for the recently adopted standardized Iowa ninth-grade school test, he had a strong desire to become a physicist. The obvious choice was Caltech, where some of his peers at the smallest high school in Los Angeles at that time, John Marshall High School, went after graduation. Caltech was indeed the only college Fredkin applied to, and he managed to graduate high school a semester early so that he could earn his way to college working full time as an actuary clerk and part time in a movie theater.

Based on his test scores and an interview with Caltech's recruiter, Fredkin entered Caltech in the fall of 1951,[4] but since he was considered an LA resident, he was not given on-campus housing and had to pay his own rent and tuition. His freshman year was uneventful, but by the end of the first trimester of the second year Fredkin had taken a construction job so he could support himself and gradually started skipping classes. He took the final exam in physics, his passion, and got the top score in the sophomore class. Nevertheless, he got a D+ due to poor attendance, no homework, and no lab reports. At that point he decided to leave Caltech, for a while at least, to join the air force as an aviation cadet. The Korean War was winding down, but people were still being drafted, and Fredkin had taken some flying lessons earlier at the Glendale airport.

Fredkin excelled at air force flight training and graduated in the spring of 1955. He then served three years as a jet fighter pilot and an officer. He refused to sign an additional one-year commitment for gunnery school and was assigned as an intercept controller at Eglin Air Force Base in Florida. Sheer luck brought him, along with thirty-five other airmen, to Lincoln Labs at Hanscom Air Force Base in Bedford, Massachusetts, where SAGE (then a new integrated and computerized air defense system) was being developed.[5] The Eglin group was supposed to be involved in testing SAGE, but upon arrival the airmen were told that the software was still not done and that it would be another year before SAGE could be tested. That year was to be spent, according to the air force, in getting them up to speed with the newly introduced computer technology, and it was here that Ed Fredkin would begin his career as a hardware designer and a world-class programmer.

At the start of the SAGE project there were only about five hundred programmers in the world. The computer itself was a recent invention,[6] and programming languages such as Fortran hadn't yet made their first appearance. After a week, all of his colleagues dropped out of these classes, which were especially designed for newly hired personnel at the SAGE project. Fredkin, on the other hand, immersed himself in the best technical education available—in effect, the best practical training a computer industry novice could hope for at that time from actual practitioners in the field. Here is his recollection:

> So they had taken over a facility called Murphy Army Hospital, which was a World War I army hospital in Waltham. It was just a giant structure that was empty and abandoned. They took it over and created a school. What they did was—very clever—they went out to industry and hired people from Bell Telephone Laboratories, various universities, industries, IBM, Burroughs, Western Electric, I remember, and others etc. They brought all these people in together to teach and to do work and so on, but to teach. So they had all these classes running. There was a programming class, and I sat in on the programming class. There was a digital logic class. I sat in on the digital logic class. . . . I stayed in these classes, and it was just fantastic because they were telling me in a very quick way—they weren't being academic. They were trying to be just practical. "You have to learn how digital logic works. You have to learn this and that" and I learned all this stuff and how to program. I just sopped it up like a sponge because it was exactly just the kind of thing I wanted to learn.[7]

It did not take long before Fredkin established himself as the quickest, most efficient (hardware or software) problem solver at Lincoln Labs, and he earned this reputation playing around with the latest, most powerful computer in the world, the IBM XD-1, which had just been installed in another building not far from his office. In an interview with Richard Wright, he described this period as the one in which his passion for physics and his discovery of the computer machine led him to digital physics, which is the idea that nature is fundamentally computational in character.[8]

In 1958 Fredkin left the air force and was hired by Bolt, Beranek and Newman (BBN), a Boston-based consulting firm now known for its work in AI and computer networks. His direct superior was J. C. R. Licklider, who immediately recognized Fredkin's talent.[9] Fredkin was the only person in the company with any computer knowledge or experience at that time, so his main job initially was to educate Licklider and others at BBN about computers. In December 1959 Fredkin discovered the PDP-1 prototype computer and convinced BBN to get the first one. He suggested many hardware modifications to Ben Gurley, the designer of the PDP-1, and to his surprise Gurley incorporated most of those modifications into the production design.[10] Occasionally, Fredkin used this computer to simulate physical processes. These simulations, along with his implementations of cellular automata, following a suggestion by Marvin Minksy, would strengthen his belief that an ordinary computer could exactly model fundamental physical processes. If this were the case, then the physics of the entire universe could be a consequence of processes that could be mimicked exactly by any computer that was large enough and fast enough.

Fredkin left BBN in 1962 and in a typical move founded a private IT company, Triple-I (International Information Incorporated). After six years his company went public, and Fredkin, then thirty-four years old, suddenly became a millionaire. During these years he slowly moved back into academia. Here again, the combination of his exceptional abilities and the serendipity of being in the right place at the right time led him to become a full professor at MIT and the director of the most important computer lab of that institute.

In the early 1960s MIT had set up a project known as Project MAC (Man and Computer, or Machine Aided Cognition). The initial support for Project MAC came from DARPA (Defense Advanced Research Projects Agency), where Licklider, Fredkin's former direct superior at BBN, had gone and obtained funding for the development and application of John McCarthy's concept of time sharing. Bob Fano, the first director of Project MAC, appointed MIT polymath Marvin Minsky, Ben Gurley (the designer of the PDP-1), and Ed Fredkin as members of the project steering committee. In 1968 Minsky and Licklider, who by then had become the project director, convinced Louis Smullin, the head of the MIT Electrical Engineering Department, to hire Fredkin for a year as a visiting professor. During this time Fredkin taught a very successful course to freshmen called Problem Solving.[11]

At the end of the one-year appointment and after interviewing the students who took his course, Fredkin was given an appointment as a full professor with tenure.[12] From 1971 to 1974 he also served as Project MAC's director. The college dropout had thus made it back into academia. This move was unusual for MIT, even for the young discipline of computer science, whose faculty members were mostly without any academic background in computers but who had other academic backgrounds and typically a graduate degree in some other discipline in the exact sciences.

In 1974 Fredkin left Project MAC on a one-year sabbatical to Caltech as a Fairchild Distinguished Scholar, hosted by Richard Feynman. The two had met by chance in 1961, when Fredkin was visiting Caltech with Marvin Minsky and John McCarthy, and their close friendship lasted for almost thirty years, only ending with Feynman's death.[13] After successfully absorbing everything about quantum mechanics that Feynman felt Fredkin needed to know, Fredkin returned to MIT in 1975 and began enlisting others to work on the details of his revolutionary conceptual framework of depicting physical laws as discrete-state algorithms.

At MIT Marvin Minsky was undoubtedly Fredkin's greatest supporter. He played a key role in bringing Fredkin to the institute, and he always seriously considered all of Fredkin's ideas, trying to help with constructive criticism and various suggestions. Minsky was, along with Feynman, the most influential person in Fredkin's intellectual life, first convincing Fredkin to want to come to MIT and then influencing MIT to accept him. Besides Minsky, Fredkin had at least three additional intellectual supporters from MIT (he had hired Tomasso Toffoli, and Roger Banks and Norman Margolus became his graduate students) and three inside the New York IBM Research Laboratory (John Cocke, Rolf Landauer, and Charles Bennett). Yet despite this support and his winning formal recognition within computer science,[14] opposition to him and his ideas was strong among the MIT faculty; his lack of formal credentials and his economic independence still irritated many, and in 1986, after a year as an adjunct professor, he left MIT. He remained attached to academia, however, spending six more years as a professor of physics at Boston University, and more recently he has been a Distinguished Career Professor at Carnegie Mellon University. More importantly, the opposition didn't stop him from continuing to develop his ideas, to which we now turn.

Finite Nature

The thesis that ultimate reality is finite, discrete, and deterministic, like a digital computer, is not new. Leibniz had already envisioned the world as an automaton in his *Monadology*; Charles Babbage thought natural laws were like programs run by his analytical engine; and Konrad Zuse hypothesized that the universe is a digital computer in his book *Calculating Space*.[15] Fredkin, however, made explicit the idea that was only implicit in this view of physical processes as computational processes, namely, the rejection of actual infinities, infinitesimals, and randomness. In effect, he translated into physics a position in the philosophy of mathematics known as finitism, according to which the physical world is seen as "a large but finite system; finite in the amount of information in any volume of spacetime, and finite in the total volume of spacetime."[16] On this view, space and time are only finitely extended and divisible, all physical quantities are discrete, and all physical processes are only finitely complex.

According to Marvin Minsky, Fredkin was thinking about such a hypothesis as early as 1961.[17] In his first encounter with Feynman, they discussed the possibility of miniaturization of automata, an issue that interested Feynman for quite some time.[18] According to Fredkin, it is here where he first brought up the idea that physical laws should be regarded as computations; hence, the physical world must be ultimately finite and discrete.[19] Several years later, in 1965, Feynman would write in his popular book *The Character of Physical Law*: "It always bothers me that, according to the laws as we understand them today, it takes a computing machine an infinite number of logical operations to figure out what goes on in no matter how tiny a region of space and no matter how tiny a region of time. How can all that be going on in that tiny space? Why should it take an infinite amount of logic to figure out what one tiny piece of spacetime is going to do?"[20] Today we call the thesis that physical processes are computational in character the physical Church-Turing thesis (PCTT). This physical version of the original Church-Turing thesis says that any physical system can be simulated (to any degree of approximation) by a universal Turing machine and that complexity bounds on Turing machine simulations have physical significance.[21] For example, if the computation of the minimum energy of some system of n particles requires at least an exponentially increasing number of steps in n, then the actual relaxation of this system to its minimum energy state will also take an exponential time. An even stronger version of the thesis says that a probabilistic Turing machine can simulate any reasonable physical device at polynomial cost.[22]

In order for the PCTT to make sense, we have to relate the space and time parameters of physics to their computational counterparts: memory capacity and number of computation steps, respectively.[23] There are various ways to do this, leading to different formulations of the thesis.[24] For example, one can encode the set of instructions of a universal Turing machine and the state of its infinite tape in the binary development of the position coordinates of a single particle. Consequently, one can physically realize a universal Turing machine as a billiard ball with hyperbolic mirrors.[25]

Despite the fact that the original notion of computation has been generalized with models that can handle real numbers and continuous time functions,[26] the question remains whether such mathematical niceties are applicable to the actual physical world. At least two counterexamples to the PCTT exist that purport to show that the notion of recursion, or Turing-computability, is not a natural physical property that must hold a priori.[27] But the physical systems involved in these counterexamples—a specific initial condition for the wave equation in three dimensions and an exotic solution to Einstein's field equations, respectively—are somewhat contrived; consequently, most physicists believe, if only for practical reasons, that the PCTT is a physical fact in our world. While ideal (mathematical) classical mechanical systems can be used to realize actual infinity and simulate non-Turing computable processes, if one allows noise, quantum effects, finite accuracy, and limited resources, one can never achieve infinite precision and hence can never tell precisely the *exact* value of a parameter x: If x is a velocity, we need to wait an infinite time to see if it moves; if x is a probability, we need an infinite number of ensembles to see if it happens; if χx is a position, we need light of infinite frequency to locate it; if χx is a critical temperature, we need an infinite number of particles for the thermodynamic limit to be meaningful. So in the world we actually live in, the physical Church-Turing thesis seems safe.[28]

Our best theories of the material world follow suit, at least up to a point. While noncommittal with respect to the most fundamental structure of space-time, the standard model of particle physics has evolved to what it is today mostly due to renormalization techniques that could eliminate divergences and singularities, hence removing non-Turing computability from the effective description of physical phenomena. Such techniques ban actual infinity but still allow potential infinity: since the physics above the "cutoff" is independent thereof, one may remain agnostic about the physics below it, which is declared beyond the theory's domain of applicability. Fully fledged finitism may be found only if one goes "farther down" to the Planck scale, the domain of applicability of the speculative and yet-to-be-agreed-upon theory of quantum gravity.[29]

For Fredkin, who never completed his formal education in physics and who was extensively exposed early in his life to the world of algorithm design and computing machines, discrete math was much easier to fathom as the correct description of the world than the standard, continuum-based mathematics with which physics had been working for so many centuries. His views on the matter are best exemplified in the following excerpt from his 2014 interview:

> Yes. Well, what they want, they suppose, without any evidence whatsoever, that the properties of the mathematical model are properties of physics. But there's no reason to suppose that. So here's an example. Just take something like continuity. We have continuous functions of continuous variables. We could describe the motion of a particle in space very concisely with these wonderful equations that are continuous. Therefore, it seems space is continuous, and time in our equations is continuous, so time is continuous and so on. Who says? Now if you go to a sophisticated physicist, he'll tell you, "Well, at the real bottom of physics are the great symmetries."

> That's what at the fundamental part. There is translation symmetry. There's rotation symmetry, and there's Noether's Theorem. Now what most people fail to notice at all is [that] Noether's Theorem has a very interesting property. It relates conserved quantities to continuous symmetries. So if you have an exactly conserved quantity, you can say there's continuous symmetry. But say I have an exactly conserved discrete quantity. What do I get? Well, what you get is something that is asymptotic to a continuous symmetry because you can't have a continuous symmetry out of it. But it can be asymptotic to it. So what I've discovered is that there are these discrete models of physics that conserve the discrete quantities exactly! In other words, in this model I have—say we're talking about momentum. The momentum is conserved exactly! But it's discrete. But there's no . . . it's always conserved exactly. It happens to be a discrete number, and it's always the same. If you take any closed system, it's the same number. It does not change. Well, what that says is that if I have something that is conserved exactly like momentum (at a microscopic level it's conserved exactly), then you should get out of it something that's asymptotic to a continuous symmetry in the limit. So that's how I believe the relationship is, that we have some quantities that are conserved exactly, but they're discrete. In the limit, it looks like a continuous thing, but it's not.

Fredkin's defensive attitude is typical of the long-standing finitist stance on the question of the nature of space-time and its appropriate mathematical description. Lest we forget, this description has long been, and still is, based on calculus, a tool that is fundamentally continuous; it employs variables that range over continuous sets of values, and the functions it deals with are continuous. One such continuous function describes motion in space and is treated as a function of a continuous time variable. The continuity of the motion function is essential, for velocity is regarded as the first derivative of this function, and acceleration as the second derivative. Functions that are not continuous are not differentiable, and hence they do not even have derivatives.

The applicability of calculus to spatial events is achieved through analytic geometry, which begins with a one-to-one mapping between the points on a line and the set of real numbers. The set of real numbers constitutes a continuum in the strict mathematical (Cantorian) sense; consequently, the order-preserving one-to-one mapping between the real numbers and the points of the geometrical line renders the line a continuum as well. If, moreover, the geometrical line is a correct representation of lines in physical space, then physical space is likewise continuous. This continuity is buried deep in standard mathematical physics and in field theories, where the physical descriptions consist of a continuum of values assigned to a continuum of spatial points whose temporal evolution is also continuous (an impressive infinity for Nature to keep track of), yet such constructions have been most successful in describing physical phenomena for almost three centuries, to the extent that any cutoffs, such the ones we find in renormalization techniques mentioned above, are interpreted as merely a practical requirement and not as resulting from an underlying fundamental finite structure.

This success of continuum-based mathematics poses a double challenge to finitists such as Fredkin: they need to demonstrate not only that finite mathematical models are inherently consistent, at least insofar as the continuous models are, but also that these models are capable of approximating the continuous models and of reproducing the predictions they yield.[30] But this challenge was well met by Fredkin. First, he invented the concept of proving the Universality of Cellular Automata by demonstrating a universal logic gate (such as the two-input NAND Gate) and communication by wire or glider, as opposed to the prior method of demonstrating the construction of a Turing machine. That concept allowed for an enormous reduction in the complexity of the necessary machinery in minimal universal cellular automata design.[31] Roger Banks, Fredkin's PhD student, used Fredkin's invention of a cellular automaton that implemented logic and communication to reduce the complexity of Fredkin's prior three-state achievement to the minimum possible two states per cell. As usual, Fredkin's motivation was simplicity, since his goal was finding something like a cellular automaton as the most basic mechanism at the most fundamental level of physics. This motivation led him to the next step, namely, to use cellular automata in approximating continuous and apparently random physical behavior.

Fredkin, however, has gone a step further and has moved from physics into metaphysics. Today he believes that if indeed space and time at the most fundamental level were to be discovered as discrete, then a variant of the three-state so-called SALT model cellular automaton (see below) would be the best candidate as a possible substrate for the most fundamental processes in physics. Note that in this view reality itself is regarded as computational; it is like a hardware whose basic building blocks are cellular automata on which physical reality runs as software. The cellular automaton is thus not only a mathematical *model* that can simulate physics; it *is* physics: "Finite nature does not just hint that the informational aspects of physics are important, it insists that the informational aspects are all there is to physics at the most microscopic level."[32] Clearly, such metaphysical ideas have generated a lot of opposition among practicing theoretical physicists,[33] and no doubt they have been instrumental in the tagging of Fredkin as a "crackpot." But one need not accept Fredkin's metaphysics in order to understand the discrete physics behind it. Whether space-time is ultimately finite and discrete or whether the cutoff we impose of the continuum when describing the physical world is merely practical or signifies a deeper, fundamental spatiotemporal discreteness, are two legitimate scientific questions. These questions touch upon deep and ancient puzzles about the fundamental nature of space, time, and matter. As it turns out, they have also become fashionable recently in the quantum gravity community, where theoreticians are trying to build phenomenological models that incorporate finite spatial resolution and a fundamentally discrete space-time into field theories with the hope of testing them with cosmological observations in the not-too-distant future.

Reversible Computation

Perhaps the most evident of Fredkin's contributions to the physics of information and one of the best examples of his role as an outsider is the idea of reversible computation.

Since 1961, the year Rolf Landauer published his paper on the inevitable energy dissipation that accompanies irreversible logical operations such as ERASURE, the prevailing opinion among physicists and computer scientists was that computation is necessarily irreversible.[34] Combined with the abundant evidence for heat dissipation in macroscopic computers, Landauer's principle has established itself as a cornerstone in the physics of information.

During his tenure as a Fairchild scholar at Caltech, Fredkin conceived of a new universal logical gate (later known as the Fredkin gate or the controlled swap gate [CSWAP]) that could, in a sense, conserve information by having the same number of inputs and outputs. Accepting Landauer's insight that the erasure of information necessarily leads to dissipation, Fredkin has nevertheless found a way to compute without disposing of information. Motivating this discovery was the conviction that while thermodynamics may describe macroscopic physical systems, it is not a fundamental theory, and so on the microscopic scale, where the dynamics are, as far as we know, time reversal invariant, computation need not be irreversible if it is to be equated with the dynamical laws:

> The author's motivation in demonstrating the possibility of reversible and universal cellular automata models of computation was very different from the apparent motivation of others. If physics

> (space, time, state, and all other quantities) is finite and discrete, then it should be capable of being modeled bijectively by some such computational process. In addition, fundamental physics is characterized by conservation laws, while the 1s and 0s in ordinary computers are certainly not conserved. When two 1s go into a NAND gate, a single 0 comes out! The so-called conservative logic gate (three inputs and three outputs) was invented to do two things: serve as a reversible gate which would allow for the construction of reversible computers, and at the same time operate in such a way as to microscopically conserve both 1s and 0s. Conservative logic showed that computational models were not inconsistent with time symmetry and the conservation laws that govern the simplest and most fundamental interactions in physics.[35]

The Fredkin gate, universal as it is, was the first building block in the theory of reversible computation but soon attracted criticism from inside academia because it was misinterpreted as yet another attempt at constructing a perpetual mobile of the second kind. To demonstrate his idea, Fredkin invented an ideal model of computation that has become an inspiration to generations of information scientists. It is called the billiard-ball computer. The model consists of billiard balls ricocheting around in a labyrinth of "mirrors," bouncing off the mirrors at 45-degree angles, periodically banging into other moving balls at 90-degree angles, and occasionally exiting through doorways that would also permit new balls to enter. To extract data from the machine, you would superimpose a grid over it, and the presence or absence of a ball in a given square at a given point in time would constitute information. Such a machine, Fredkin showed, would qualify as a universal computer; it could do anything that normal computers do. But unlike other computers, it would be perfectly reversible; to recover its history, all you would have to do is stop it and run it backward.

Any implementation of such a model would, of course, be noisy, and so it remains a technological challenge to minimize the friction and the entropy produced by the collisions. The important point is that, modulo this noise, no other entropy is produced in the computation, since there is no information loss. With this model the task of overcoming these challenges and physically realizing a reversible computer has been reduced to purely technological. As Fredkin says, "The cleverer you are, the less heat it will generate."[36]

Fredkin did not publish his work immediately, and it took him several years to channel his intuition to proper academic venues. Indeed, when one searches today for bibliography on reversible computation, three other names besides Fredkin's come up: Charlie Bennett, Tomasso Toffoli, and Norman Margolus. The latter two worked with Fredkin at MIT in the late 1970s and the early 1980s and developed reversible computation with his guidance. The former actually published an article on the possibility of reversible computation as early as 1973 and did so independently of Fredkin, but even he acknowledges that Fredkin's billiard-ball computer is in some respects a more elegant solution to the problem than his own.[37]

The billiard-ball model, when combined with a cellular automaton, realizes Fredkin's finite nature hypothesis. Consider, for example, a variant of the model invented by Norman Margolus, the Canadian in MIT's information-mechanics group that Fredkin founded. Margolus showed how a two-state cellular automaton that was itself reversible could simulate the billiard-ball computer using only a simple rule involving a small neighborhood. Margolus's model, inspired by Fredkin's intuition that such a simple automaton rule must exist, demonstrated how a seemingly more complex behavior of microscopic particles bouncing off each other can arise from a simple underlying discrete structure. That model would later on lead to another of Fredkin's inventions, which he is constantly improving even today, namely, the SALT model, a two- or three-state, three-dimensional, deterministic, reversible cellular automaton that is capable of approximating circular orbits, wave-like undulations, and particle-like configurations that decay in accordance with a half-life law.[38] Ironically, such complex phenomena that the SALT model can simulate are commonly regarded as inconsistent with discrete structures![39] Reversible computation has thus brought Fredkin's finite nature hypothesis to its final test, and, against all odds, it has prevailed. In this respect one can count Fredkin's model as yet another example of a discrete dynamical structure that can approximate continuous behavior and as such, a part of a distinguished line of thought that started long before him that suggests itself as an alternative to field theories in modern physics.[40]

Feynman and the Birth of Quantum Computing

In 1961 John McCarthy was looking for a job, and two of his close friends, Ed Fredkin and Marvin Minsky, went with him to California to his job interview at Caltech. McCarthy ended up at Stanford, but while visiting Pasadena, his two companions wanted to have some face time with some "great people." Fredkin suggested Linus Pauling, whom he remembered from his freshman year at Caltech, but Pauling wasn't home. Minsky suggested Feynman, whom both men had never met. A phone call and a motorbike ride later, they were at Feynman's house. The conversation that ensued was so lively that they left only early the next morning.

Feynman was deeply involved in those days with miniaturization ideas, and according to Fredkin, they discussed the hypothesis of finite nature and the possibility of a computer performing algebraic manipulations.[41] Fredkin credits the origin of MIT's MACSYMA algebraic computing project, a precursor to the ever-present MATHEMATICA, to that discussion in Pasadena. Be that as it may, one thing is certain: that visit has led to a friendship that lasted almost thirty years, until Feynman's untimely death.

Their interaction was reinforced when in 1974 Fredkin came to Caltech under Feynman's invitation. The deal, as Fredkin retells it, was that he would teach Feynman more about computer science, and Feynman would teach him more about physics. Although he believes he got the better end of it, Fredkin recalls that interactions with Feynman could also be frustrating at times. The following recollection may also explain why Fredkin is also rarely cited in print within academia:

> I never pressed any issue that would sort of give me credit, okay? It's just my nature. A very weird thing happened toward the end of my time at Caltech. Richard Feynman and I would get into very fierce arguments. . . . I'm trying to convince him of my ideas, that at the bottom is something finite and so on. He suddenly says to me, "You know, I'm sure I had this same idea sometime quite a while ago, but I don't remember where or how or whether I ever wrote it down." I said, "I know what you're talking about. It's a set of lectures you gave someplace. In those lectures you said perhaps the world is finite." He just has this little statement in this book. I saw the book on his shelf. I got it out, and he was so happy to see that there. What I didn't tell him was he gave that lecture years after I'd been haranguing him on this subject. I knew he thought it was his idea, and I left it that way. That was just my nature.

Indeed, direct evidence for Feynman's interest in Fredkin's ideas is not abundant and consists of two published references: one is Feynman's paper, discussed below, and the other is the inclusion of Fredkin's name and the billiard-ball model in Feynman's *Lectures on Computation*.[42] The reason, according to Fredkin, is that while Feynman was truly intrigued by Fredkin's finite nature hypothesis, he nevertheless kept this interest to himself and wanted no public mention thereof, fearing perhaps to be tagged as a crank. This is understandable, as the finite nature hypothesis, if true, makes most of modern physics an approximation; its clash with the continuous symmetries of special relativity and the continuum of the Hilbert space being perhaps the most problematic consequence.[43]

In 1981, however, quiet interest turned into an international debut. Fredkin, by then on his way out from the directorship of project MAC, organized with Toffoli and Landauer a conference on the physics of information and suggested to his successor, Mike Dertouzos, that Feynman should be the keynote speaker. The tentative title of the conference was "Computational Models of Physics." When Feynman heard it, he declined the invitation and only accepted it when the title was changed to "The Physics of Computation."

Fredkin, in passing, told Dertouzos about Feynman's reaction, and when it was time to introduce the keynote speaker, Dertouzos thought it funny to relay the incident to the audience. Feynman, however, had in the meantime changed his mind, and his talk, as well as its title, was exactly on the subject he had tried to distance himself from earlier.[44]

Feynman's talk, which also appeared in print as the 1982 paper "Simulating Physics with Computers," starts with explicit credit to Ed Fredkin: "I want to talk about the problem of simulating physics with computers and I mean that in a specific way which I am going to explain. The reason for doing this is something that I learned about from Ed Fredkin, and my entire interest in the subject has been inspired by him."[45] The finite nature hypothesis is mentioned too (Feynman even expanded on it in the question period), as well as the idea of reversible computation. The paper is famous today, however, for the fact that in it Feynman first discusses the possibility of quantum computers and the difference between classical and quantum physics in terms of the probability measure they make use of in predicting physical phenomena.[46] Feynman saw this difference as evidence that no classical computer can simulate quantum physics efficiently; today many regard this difference as one of the possible sources of the putative power of quantum computers over their classical counterparts.

The paper ends with another credit for Fredkin, and this is where the subtle disagreement between the two reveals itself:

> The program that Fredkin is always pushing, about trying to find a computer simulation of physics, seems to me to be an excellent program to follow out. He and I have had wonderful, intense, and interminable arguments, and my argument is always that the real use of it would be with quantum mechanics, and therefore full attention and acceptance of the quantum mechanical phenomena—the challenge of explaining quantum mechanical phenomena—has to

> be put into the argument, and therefore these phenomena have to be understood very well in analyzing the situation. And I'm not happy with all the analyses that go with just the classical theory, because nature isn't classical, dammit, and if you want to make a simulation of nature, you'd better make it quantum mechanical, and by golly it's a wonderful problem, because it doesn't look so easy. Thank you.[47]

Feynman, along with almost all of the physics community, considers quantum mechanics universal. Fredkin, on the other hand, faithful as he is to the finite nature hypothesis, cannot regard it to be so. For him there is an underlying deterministic and discrete structure, under and below the discreteness of space-time, that can account for physical phenomena, quantum or otherwise. This is the reason why Fredkin, as a true outsider, counts himself today as one of the few vocal opponents to the quantum computing industry, notwithstanding his being one of its backstage facilitators: "The problem is that Hilbert space is a computational technique that gets you the right answer. Not every computational technique that gets you the right answer is a part of physics. But it has to exist for a quantum computation to work. . . . I just don't believe it does."[48]

Discussion: Historiography and the Foundations of Physics

In this article I have attempted to shed light on several milestones in Ed Fredkin's intellectual life that allow us to portray him as an outsider in science. I'd now like to explain why such a historiographical portrayal is instructive not only for the history of science but also for the foundation of physics.

The story of Ed Fredkin and his influence on the physics of information may serve to sharpen the category of the outsider scientist from a historiographical perspective. This category has so far been delineated between two possible extremes: on the one hand, an outsider scientist is seen as a working scientist in one discipline who has crossed disciplinary boundaries, for example, from physics to biology; on the other hand, outsiders in science are sometimes regarded as fringe characters, underdogs who challenge the authority of mainstream science by testing its boundaries.

A good example for the first side of this spectrum is the recent edited volume on outsiders in the life sciences.[49] Here outsiders are regarded as sources of significant innovation who bring with them tools and techniques from their own discipline that are unknown or uncommon in the discipline they “migrate” to. As an example for the other side of the spectrum, consider *How the Hippies Saved Physics*.[50] In this book outsiders are portrayed as fringe academics, members of the counterculture who, by injecting heavy metaphysical speculations into quantum phenomena, created a reactionary wave that made the interpretations of quantum mechanics a respectable scientific research discipline and in so doing, according to David Kaiser, have helped shape quantum information science.

I believe that Fredkin’s story positions him with neither of these extremes; instead, he is somewhere on the spectrum between them. What we gain from such juxtaposition is a sharper definition of the category of an outsider. One need not “move into” a discipline (as in the case of the physicists who entered the life science) or generate hype based on mischaracterization of some scientific topic outside academia (as in the case of the hippies and quantum telepathy) in order to influence a specific domain of inquiry. Outsiders may be better defined less by their origin and more by their intrinsic and contextual properties.

Indeed, regardless of their origin, within or without academia, outsiders are considered historically significant relative to their influence on the discipline. In the first case of the outsider scientist, Linus Pauling and Erwin Schrödinger are familiar exemplars for physicists who translated their wisdom into the life sciences and helped create the foundations of molecular biology. In the second case, the influence was more of a form of reaction within physics to fringe ideas such as quantum telepathy and action-at-a-distance that were entertained by counterculture groups on the West Coast during the 1970s and led, according to Kaiser, to discussion within physics on the violations of Bell’s inequalities and their practical implications for information transfer. In both cases, however, a target discipline was interrupted and sometimes reshaped by new ideas that originated in outsiders to that discipline.

Another distinguishing feature of an outsider is the aspect of character. As Oren Harman and Michael Dietrich emphasize, outsiders are genuine transgressors who see little point in respecting existing boundaries or conventional wisdoms.[51] Linus Pauling, for example, admonished young scientists in his Nobel laureate lecture in 1954 never to take anything on authority and always to think for themselves. Pauling, as we recall, was one of Fredkin’s professors at Caltech a couple of years before he won his first Nobel Prize.

The final aspect that may be important from a historical or even a sociological perspective is the process of shaping a discipline or instantiating a change therein. This process, as has been shown in the two case studies above, is rarely done by the outsider alone. The role of facilitating it is often taken by an individual patron to the outsider or by forward-looking institutions who in their own ways support the outsider and his or her ideas.

Returning to Fredkin, although he differs in his origin from these two types of outsiders, he nevertheless shares with them similar characteristics: first, a fiercely independent mind that reveals itself to anyone interested in entering into a conversation with him; second, many revolutionary ideas, the scope of which is so vast that it would take many a lifetime to implement them all; third, an evident influence on a discipline that sometimes goes unaccredited (see below); fourth, support both of individual patrons (Licklider, Minsky, Feynman) and, up to a point, of forward-looking institutions (MIT Electrical Engineering Department, Boston University Physics Department), which allowed him to disseminate his views inside academia and facilitated their acceptance with the clout and prestige these institutions had (and he lacked) in their corridors. In all these aspects, Fredkin is a true outsider scientist, regardless of the actual trajectory of his career.

The similarity of Fredkin's story to other stories of outsiders in science would be of less interest were his ideas not as influential. In the final part of this section, I will try to substantiate the claim, made here, that Fredkin's ideas were instrumental to the creation of quantum information science and to the research program of deterministic and discrete alternatives to quantum mechanics.

First note that to evaluate such an influence and to trace it back to an outsider could be an elusive historical project; this is true for the first class of outsiders within academia and even more so for the second, counterculture class. Ideas can appear in many forms, for example, in published articles, in research grants, in proceedings of workshops, or, most often, in a conversation. Written evidence may not be found, and even if it is found, it is not clear that such written evidence is indeed evidence, that is, that it had any influence at all on the discipline either because it was read but not cited or because it remained unread.[52]

In Fredkin's case, the task is even harder because he published so little in academic venues.[53] As relatively young a research program as quantum computing is, its historiography is almost nonexistent, and those who do reflect on the origin of the discipline usually stop at Feynman: the keynote lecture from 1981 has become part and parcel of the folklore of the field, and most working scientists in quantum information, if not all of them, would gladly point at Feynman as one of the fathers of their discipline.[54] Nevertheless, besides Feynman's recognition that reversible computation inspired his ideas about quantum circuits, at least in the nonscientific and popular literature it has already been acknowledged that (1) the line of thought that started with the PCTT and continued with reversible computation and the billiard-ball model has culminated in Feynman's famous talk on the possibility of quantum computers;[55] and (2) this talk has inspired a generation of physicists and computer scientists to the extent that, shortly after it, the idea of a quantum Turing machine appeared and was followed a decade later by the quantum algorithm of factoring.[56] The latter has generated massive interest from various funding agencies outside academia, all eager to put their hands on the first scalable quantum information processing device; consequently, these agencies have injected a huge amount of funding into the discipline, turning it into a small industry. The quest for a large-scale and computationally superior quantum computer, incidentally, is still going on more than thirty years after Feynman's keynote lecture.[57]

Two points are worth mentioning here. First, given its historiographical importance, Feynman's decision to align himself with Fredkin and his ideas precisely at that particular occasion in 1981 is rather fortunate for the purpose of completing the story quantum computer scientists tell themselves about the origins of their research program.[58] This fact stands in stark contrast to another talk by Feynman from 1959 on the possibility of nanotechnology, the historiographical significance of which is rather questionable;[59] recently, it has been convincingly shown that, rather than this talk, which was almost never cited until the 1980s, it was the creation of the scanning tunneling microscope (STM), whose proprietors never read Feynman's prophecy from 1959, that ended up being the actual trigger for the establishment of the discipline.[60]

Second, it is rather ironic that Fredkin has nevertheless positioned himself today as one of the few vocal quantum computer *skeptics*, who argue that a large-scale quantum computer will never be built that can outperform its classical counterpart.[61] For Fredkin the reason for this skepticism lies in his commitment to the finite nature hypothesis, which, if true, would make the Hilbert space and the quantum state that inhabits it mere mathematical constructs and not part of physical reality. This reality, as one recalls, is for Fredkin deterministic, finite, and discrete, and quantum theory, despite its success, should not be seen as a universal theory, especially when there are deterministic and discrete models (e.g., his SALT model) that can reproduce the description of many phenomena under its domain of applicability.

One may plausibly assume that his lack of formal indoctrination in state-of-the-art physics has actually better equipped Fredkin to develop his alternative framework and allowed him to express such heretic ideas in public; as an outsider he was not committed to an academic career and could freely contemplate ideas that even now are considered blasphemy. Indeed, today only a minority of physicists allow themselves to publicly propose discrete alternatives to quantum field theory.[62] Those who do so, however, rarely credit Fredkin for his trailblazing models.

Yet the evidence for Fredkin's influence on current attempts to construct a deterministic and discrete subquantum theory is striking. Consider a recent arXiv manuscript posted on May 2014 by Gerard 'tHooft, the 1999 theoretical physics Nobel laureate who has known Fredkin and his work for many years. (According to Fredkin's recollection, in 1988 both he and 'tHooft arrived at Boston University, Fredkin as a research professor of physics, since he didn't want to teach, and 'tHooft as a visiting professor, and were given an office to share. Fredkin introduced 'tHooft to cellular automata, and from that point until very recently, they have been in communication often. The relationship was more than scholarly and included many mutual home visits.) The manuscript is titled "The Cellular Automaton Interpretation of Quantum Mechanics."[63] Its 207 pages depict nature as a universal cellular automaton and develop this idea into a full-fledged alternative to quantum theory. The only mention of Fredkin appears in one item in the bibliography, referred to only twice in the manuscript, and one of these references credits the automaton rule (discussed above) to Norman Margolus. Margolus, on the other hand, has acknowledged in print on many occasions that the original idea was Fredkin's.

Be that as it may, and despite the fact that the community interested in discrete alternatives to quantum theory is much smaller than the community of quantum information science, here one can establish a clear trajectory of ideas and trace them back to Fredkin, whether credited or not. 'tHooft's recent manuscript is thus another illustration of Fredkin's character as an outsider. Since he was independent of the academic system most of his life, Fredkin never bothered with the academic currency of publications and citations, yet his ideas resonated with many scholars who have met him, and through them his ideas have become part of academic knowledge.[64]

This final point brings us to our closing insight: the outsiders we have compared Fredkin to were either already members of the scientific community (and just moved between disciplines) or nonmembers without any aspirations to penetrate it. In both cases the question of academic recognition was never an issue; what mattered was the intellectual influence, and such an influence is often apparent either with hindsight or to those who are prepared to view matters in finer resolution. This, I believe, should be the case also with Ed Fredkin and his impact on the physics of information.

Notes

1. See, for example, David Kaiser, *How the Hippies Saved Physics* (New York: W. W. Norton, 2011); Oren Harman and Michael Dietrich, eds., *Outsider Scientists* (Chicago: University of Chicago Press, 2013).

2. This section is based on personal interviews with Ed Fredkin conducted in June 2013 and January 2014. Fredkin's life story became publicly known in April 1988, when Robert Wright published his "Did the Universe Just Happen?," *Atlantic Monthly*, April 1988, 29–44.

3. Years later Fredkin would confirm what he had long suspected, namely, that his mother's husband was not his biological father.

4. Fredkin also scored among the highest in a national test in high school chemistry that year, 23rd from almost 100,000 nationwide.

5. See Kenneth Schaffel, *The Emerging Shield* (Washington, DC: Office of the Air Force History, USAF, 1991).

6. Paul Ceruzzi, *A History of Modern Computing* (Cambridge, MA: MIT Press, 2003).

7. Edward Fredkin, personal interview with the author, Boston, MA, 2014.

8. Wright, "Did the Universe Just Happen?"

9. Ibid., 36.

10. Fredkin is the father of the modern "Interrupt" protocol found in all modern computers and one of the fathers of time sharing. See David Walden and Raymond Nickerson, *A Culture of Innovation: Insider Account of Computing and Life at BBN* (Cambridge, MA: Waterside Publishing, 2011), 13–14, 28–29.

11. According to Fredkin's recollection in the 2014 interview, when Louis Smullin asked him, "How will your problem solving be related to what we teach in our department?" he answered, "It's not directly related!" Smullin then asked, "So why should it be taught in our department?" Fredkin's answer was, "Because it's also not related to anything taught in any other MIT department." Smullin shrugged.

12. A member of the tenure committee who had to review Fredkin's accomplishments grumbled to him that a normal appointment, with tenure, required a serious publication record with lots of references, but in Fredkin's case he could only find one single referenced publication, called "Trie Memory," published in the *Communications of the Association of Computing Machinery* in 1960. Eight years after publication, the committee couldn't find a single reference to that paper. Today, Google Scholar claims that about one thousand papers reference Fredkin's 1960 "Trie Memory" paper, which is now considered the predominant method of storing and retrieving information from a database and the basic algorithm used by Google to do searches.

13. In 1980 Feynman and his family all traveled to the Caribbean, where Feynman was best man at Fredkin's marriage to Joycelin Wheatley.

14. In 1984 Fredkin won the Dickson Prize in Science, given to "those who have notably advanced the field of science."

15. Konrad Zuse, *Calculating Space* (MIT Technical Translation, 1970).

16. Edward Fredkin, "Digital Mechanics: An Informational Process Based on Reversible Universal Cellular Automata," *Physica D* 45 (1990): 255.

17. Richard Feynman, *No Ordinary Genius: The Illustrated Richard Feynman* (New York: W. W. Norton, 1994), 178.

18. Richard Feynman, "There's Plenty of Room at the Bottom: An Invitation to Enter a New Physics," first presented at the American Physical Society at the California Institute of Technology on December 29, 1959. Subsequently published in the journal *Engineering and Science*, Caltech, February 1960. See also Feynman, *No Ordinary Genius*, chap. 7.

19. Fredkin, 2014 interview.

20. Richard Feynman, *The Character of Physical Law* (Cambridge, MA: MIT Press, 1965), 157.

21. Steven Wolfram, "Undecidability and Intractability in Theoretical Physics," *Physical Review Letters* 54 (1985): 735.

22. See Itamar Pitowsky, "From Logic to Physics: How the Meaning of Computation Changed over Time," *Lecture Notes in Computer Science* 4,497 (2007): 621–31. This stronger version of the PCTT is challenged by quantum computing. See Amit Hagar, "Quantum Computing," in *Stanford Encyclopedia of Philosophy*, ed. Ed Zalta (2006) for an accessible review. Further variants of this thesis can be found in Jack Copeland, "The Church-Turing Thesis," in Zalta, *The Stanford Encyclopedia of Philosophy* (1996).

23. It should be stressed that there is no relation between the original CTT and its physical version (see Oron Shagrir and Itamar Pitowsky, "Physical Hypercomputation and the Church-Turing Thesis," *Minds and Machines* 13 [2003]: 87–101), and while the former concerns the concept of computation that is relevant to logic (since it is strongly tied to the notion of proof, which requires validation), it does not analytically entail that all computations should be subject to validation.

24. Itamar Pitowsky, "The Physical Church Thesis and Physical Computational Complexity," *Iyyun* 39, no. 1 (1990): 81–99.

25. See Chris Moore, "Unpredictability and Undecidability in Dynamical Systems," *Physical Review Letters* 64, no. 20 (1990): 2354–57; and Itamar Pitowsky, "Laplace's Demon Consults an Oracle: The Computational Complexity of Prediction," *Studies in the History and Philosophy of Modern Physics* 27, no. 2 (1996): 161–80. For the most intuitive connection between abstract Turing machines and physical devices, see the pioneering work of Robin Gandy, "Church's Thesis and the Principles for Mechanisms," in *The Kleene Symposium*, ed. H. Barwise, H. Keisler, and K. Kunen (North Holland, 1980), 123–48, simplified later by Wilfrid Sieg and J. Byrnes, "An Abstract Model for Parallel Computations," *Monist* 82 (1999): 150–64.

26. See, for example, Joel David Hamkins and Andy Lewis, "Infinite Time Turing Machine," *Journal of Symbolic Logic* 65, no. 2 (2000): 567–604; Jack Copeland, "Accelerating Turing Machines," *Minds and Machines* 12, no. 2 (2002): 281–300.

27. See Pitowsky, "The Physical Church Thesis," for an exposition.

28. Chris Moore, "Recursion Theory on the Reals and Continuous-Time Computation," *Theoretical Computer Science* 162, no. 1 (1996): 43.

29. Amit Hagar, *Discrete or Continuous? The Quest for Fundamental Length in Modern Physics* (Cambridge: Cambridge University Press, 2014), chaps. 4 and 7.

30. For more on this philosophical debate, see ibid., chap. 1.

31. Von Neumann's original Universal CA had twenty-nine states per cell. The best CA design using Turing's model of universality was done by Edgar Codd, who demonstrated universality in a CA with the von Neumann neighborhood

with only eight states. See Edgar F. Codd, *Cellular Automata* (New York: Academic Press, 1968).

32. Fredkin, “Digital Mechanics,” 259.

33. Counterexamples are rare. See, for example, Max Tegmark, *Our Mathematical Universe* (New York: Knopf, 2014).

34. Rolf Landauer, “Irreversibility and Heat Generation in the Computing Process,” *IBM Journal of Research Development* 5, no. 3 (1961): 183–91.

35. Edward Fredkin, “Five Big Questions with Pretty Simple Answers,” *IBM Journal of Research Development* 48, no. 1 (2004): 31–45.

36. Wright, “Did the Universe Just Happen?,” 40.

37. Ibid., 44.

38. See Dan Miller and Ed Fredkin, “Circular Motion of Strings in Cellular Automata, and Other Surprises” (2012), available at arXiv.org/pdf/1206.2020, accessed September 15, 2014.

39. The following anecdote (Fredkin, 2014 interview) is telling. In 2008 Gerard ’tHooft was visiting Perimeter Institute, and Fredkin invited him to give a talk at CMU. In his lecture ’tHooft pointed out that one trouble with deterministic and discrete ideas like Fredkin’s was particle decay half-life phenomena, as no deterministic model of half-life was possible. After his lecture, Fredkin told ’tHooft that he had already solved this problem and described a deterministic model of half-life to him. That “impossible” model was realized several years later. See Miller and Fredkin, “Circular Motion of Strings.”

40. The clash between the continuous and the discrete bothered even luminaries such as Einstein, who in a letter from 1935 to Pierre Langevin wrote: “One does not have the right today to maintain that the foundation [of theoretical physics] must consist in a *field theory* [emphasis in the original] in the sense of Maxwell. The other possibility, however, leads in my opinion to a renunciation of the space-time continuum and to a purely algebraic physics. . . . For the present, however, instinct rebels against such a theory” (Einstein to Paul Langevin, 3 October 1935, as translated in John Stachel, “The Other Einstein,” *Science in Context* 6 [1993]: 275–90).

41. Fredkin, 2014 interview.

42. Richard Feynman, *Feynman’s Lectures on Computation* (Boulder, CO: Westview, 1996).

43. As an example of this attitude, a recent interview with Marvin Minsky on his relation with Feynman reveals that Feynman thought he really should be working on Fredkin’s cellular automata because there was something a bit wrong with modern physics if it allowed infinite information to be packed into a finite space, but first he had to fix quantum chromodynamics. See http://www.webofstories.com/play/marvin.minsky/146, accessed September 4, 2014.

44. Julian Brown, *Minds, Machines and the Multiverse* (New York: Simon & Schuster, 2000), 83–85.

45. Richard Feynman, “Simulating Physics with Computers,” *International Journal of Theoretical Physics* 21 (1982): 467–88, quote at 467.

46. More than a generation later, Feynman’s paper would become part of the canon of quantum information science and would be cited endlessly as one of the first explicit mentions (in the Western Hemisphere at least) of the dream of the quantum computer.

47. Feynman, “Simulating Physics with Computers,” 488.

48. Fredkin, 2014 interview.

49. Harman and Dietrich, *Outsider Scientists.*

50. Kaiser, *How the Hippies Saved Physics.*

51. Harman and Dietrich, *Outsider Scientists.*

52. For an example of the latter case, see Chris Toumey, "Plenty of Room, Plenty of History," *Nature Nano* 4, no. 12 (2009): 783–84.

53. Here one should note that the fact that Fredkin's ideas were not published is due mostly to his character as an outsider and not to an injustice in the scientific publishing method (as suggested by an anonymous referee). A bias in that method may well exist, but Fredkin never tested it, since, with the exception of the TRIE algorithm (1962) and his talk at the 1981 MIT conference on the physics of information, he never submitted anything for publication until the 1990s (indeed, once he did start sending articles for publication, they were published).

54. See Michael Nielsen and Isaac Chuang, *Quantum Information and Quantum Computation* (Cambridge: Cambridge University Press, 2000), 7. Ideas of harnessing the quantum non-Boolean probability structure for information processing purposes had appeared in the Soviet Union in the 1970s but reached the West only in the mid-1980s. See Hagar, "Quantum Computing," for further references.

55. See Feynman, "Simulating Physics with Computers"; and Brown, *Minds, Machines and the Multiverse.*

56. David Deutsch, "Quantum Theory, the Church-Turing Principle and the Universal Quantum Computer," *Proceedings of the Royal Society A* 400, no. 1,818 (July 1985): 97–117; Peter Shor, "Polynomial-Time Algorithms for Prime Factorization and Discrete Logarithms on a Quantum Computer," *SIAM Journal of Computing* 26, no. 5 (1997): 1484–1509.

57. Klint Finely, "The Man Who Will Build Google's Elusive Quantum Computer," *Wired,* September 2014, http://www.wired.com/2014/09/martinis/, accessed September 5, 2014.

58. Fredkin, in an interview from 2013, recalls that Feynman's talk was filmed, but because of some technical mishap the camera started working only *after* the preface, in which Feynman acknowledged Fredkin's strong influence over him, so it was never recorded. What remains, instead, is the acknowledgment in print.

59. Richard Feynman, "There's Plenty of Room at the Bottom (Data Storage)," *Journal of Microelectromechanical Systems* 1, no. 1 (1992): 60–66. A reprint of the talk delivered at an APS meeting at Caltech on December 29, 1959.

60. Toumey, "Plenty of Room."

61. Other skeptics include Rolf Landauer, Leonid Levin, Oded Goldreich, and Gil Kalay. See Rolf Landauer, "Is Quantum Mechanics Useful?," *Philosophical Transactions of the Royal Society A* 1,703, no. 353 (1995): 367–76; Leonid Levin, "The Tale of One-Way Functions," *Problems in Information Transmission* 39 (2003): 92–103; Oded Goldreich, "On Quantum Computing," http://www.wisdom.weizmann.ac.il/~oded/on-qc.html, accessed September 5, 2014; Gil Kalay, "Detrimental Decoherence," http://arxiv.org/abs/0806.2443, accessed September 5, 2014.

62. For an alternative to Schrödinger's equation that introduces physical collapse to standard QM, see, for example, Stephan Adler and Angelo

Bassi, “Quantum Theory: Exact or Approximate?” (2009), http://arxiv.org/pdf/0912.2211.pdf, accessed September 15, 2014. For an explicit discrete and deterministic subquantum theory, see Gerard ’tHooft, “Quantum Mechanical Behavior in a Deterministic Model,” *Foundations of Physics Letters* 10 (1997): 105–11. It is noteworthy that both these authors are distinguished physicists (Adler is a former Albert Einstein Professor of Theoretical Physics at Princeton’s Institute of Advanced Studies, and ’tHooft is a Nobel Prize laureate in theoretical physics) who are also known for theoretical work that is a lot more mainstream.

63. Gerard ’tHooft, “The Cellular Automaton Interpretation of Quantum Mechanics: A View on the Quantum Nature of Our Universe, Compulsory or Impossible?,” http://arxiv.org/abs/1405.1548, accessed September 5, 2014.

64. It is unclear why ’tHooft has gone out of his way to disassociate himself from Fredkin. According to Fredkin (2014 interview), it is certainly true that ’tHooft has been influenced by his numerous discussions with Fredkin about cellular automata models of physics to a far greater extent than he has by the sum of all his interactions with everyone else he credits in the above manuscript.