2 DISCOVERING COMPUTERS

Early in my apprenticeship at Eberstadt, when I was splitting my time between serving concurrently as a political economist and a trainee investment banker, I discovered computers. That is to say, I discovered why computers are interesting. This came about as an unexpected result of the collapse of the post-World War II golden age. From the autumn of 1973, under the impact of the oil embargo and energy crisis triggered by the Yom Kippur War, both political and market processes broke down, nowhere more definitively than in the United States. Making sense of the new economic environment in which the financial markets were functioning was as challenging as it was necessary.

By the early 1970s, the macroeconomics of Samuelson's neoclassical synthesis, universally and misleadingly termed Keynesian, had come to be intimately associated with large-scale econometric models. Otto Eckstein's DRI (Data Resources, Inc.) Model, based on his research at Harvard, led the field, with competition in the commercial world from Michael Evans's Chase Econometrics and Lawrence Klein's Wharton Model. Every major central bank had its own version, as did the Treasury Department. Derived from the work that had won Jan Tinbergen his share of the first Nobel Prize in Economics, these models all deployed a statistical methodology intended to define consistent relationships between variables, using the correlations between historical time series to establish predictable patterns of systemic behavior.

From the beginning of the econometrics enterprise in the late 1930s, Keynes had raised objections to the whole procedure, even

though he had championed the development of the national income statistics that populated the models.[1] Tinbergen himself emphasized the practical promise of econometrics:

> The establishment of a system of equations compels us to state clearly hypotheses about every sphere of economic life and, in addition, to test them statistically. Once stated, the system enables us to distinguish sharply between all kinds of variation problems. And it yields clear-cut conclusions. Differences of opinion can, in principle, be localised, i.e., the elementary equation in which the difference occurs can be found. Deviations between theory and reality can be measured.[2]

Keynes, in response, identified a number of technical issues that infest any attempt to derive causal relationships from statistical correlations. He then turned to the core structural issue: the instability of behavioral relationships through time. This is what undermines Tinbergen's project at the most fundamental level and renders econometrics finally unable to serve Tinbergen's second purpose: to test the validity of alternative economic theories. Fifty years later, Pesaran and Smith evaluated the prewar argument between Tinbergen and Keynes:

> Given that there were not strong *a priori* reasons for believing economic relations to be stable over time, and the fact that estimated equations are prone to structural change, one is forced to agree with Keynes that at a logical level econometric inference, like other forms of inference, is unsupportable.[3]

In practice, Keynes himself had emphasized that in the face of uncertainty

> we have tacitly agreed, as a rule, to fall back on a *convention*. The essence of this convention ... lies in assuming that the existing

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[1] R. Frydman and M. Goldberg correctly point out that Keynes's critique was shared by Friedrich Hayek: R. Frydman and M. Goldberg, *Beyond Mechanical Markets: Asset Price Swings, Risk, and the Role of the State* (Princeton University Press, 2011), p.250.

[2] J. Tinbergen, *An Econometric Approach to Business Cycle Problems* (Paris: Herman & Cie, 1937), p.73, cited in H. Pesaran and R. Smith, "Keynes on Econometrics," in T. Lawson and H. Pesaran (eds.), *Keynes' Economics: Methodological Issues* (London: Croom Helm, 1985), p.136.

[3] Lawson and Pesaran (eds.), *Keynes' Economics*, p.147.

state of affairs will continue indefinitely, except in so far as we have specific reasons to expect a change.[4]

As David Hume had asserted some 150 years before Keynes, reliance on the convention of continuity underlies the observable stability of behavioral relationships in "normal" times, even though the "precariousness" (Keynes's term) of such foundations can also be observed in the drastic regime shifts represented by bubbles and crashes.[5] And so, as Pesaran and Smith conclude, "it does not follow that econometrics is useless." Indeed, it was their "practical usefulness in decision-making and policy formation" that drove the proliferation of econometric models in the postwar decades.[6]

In the winter of 1973–1974, however, Keynes's attack on "the promise of structural stability" dominated.[7] For the first energy crisis drove all the key variables of the models – interest rates, inflation rates, unemployment rates and, with the contemporaneous collapse of the Bretton Woods international financial system, exchange rates – beyond the ranges that had been observed during the mere quarter-century in which national economic statistics had been systematically collected. The functional relationships that had been defined on the data from this period and that constituted the guts of the models were left floating in air, decoupled from empirical observation.

In a series of papers written for Eberstadt's institutional clients, I defined this as "the database problem." The econometric models represented a statistical economy whose behavior was supposed to evolve in close emulation of the underlying complex networks of agents and institutions, stocks and flows, goods and services, money and credit. But, beginning in 1973, the world economy was ejected from the models. We were living "outside the database." Whether or not a given dependent variable would exhibit the same relationship to the supposedly relevant independent variables was an entirely arbitrary judgment once the latter moved to levels never before observed. So, quite

[4] J. M. Keynes, *The General Theory of Employment, Interest and Money*, in E. Johnson and D. Moggridge (eds.), *The Collected Writings of John Maynard Keynes*, vol. 7 (Cambridge University Press and Macmillan for the Royal Economic Society, 1976 [1936]), p.152 (emphasis in original).

[5] D. Hume, *An Enquiry Concerning Human Understanding* (Oxford University Press, 2007 [1777]), pp.4.19, 4.21.

[6] Pesaran and Smith, "Keynes on Econometrics," p.137.

[7] Keynes, *General Theory*, p.146.

apart from the econometric models' standing in economic logic, as practical tools for prediction they had broken down as thoroughly as the political economy they were supposed to represent.

Agent-based Simulation Models

In 1975, my persistent search for alternative tools with which to evaluate global economic discontinuity led me to a warehouse off Kendall Square in East Cambridge, Massachusetts. There I found a band of academic refugees, led by a young scholar named Nathaniel Mass. They had been students of Jay Forrester, renowned first for his leadership of MIT's pioneering "Whirlwind" computer project and then for his development of a methodology for representing the behavior of complex systems by capturing both the positive feedback effects that amplify initial movements and the negative feedback that dampens them.

At the end of the 1960s, in collaboration with Donella Meadows, Dennis Meadows and others, Forrester applied his system dynamics to economic systems, an effort that culminated notoriously in *The Limits to Growth: A Report for the Club of Rome's Project on the Predicament of Mankind*.[8] Forrester's design goal was to represent complex systems with parsimonious models that would reveal the systems' modes of behavior, and he did so with the discipline of an engineer. But the system dynamics model deployed in *The Limits to Growth* was so parsimonious that it lacked a price mechanism. As a result, increasing demands on resources, driven by population growth and rising incomes, led monotonically to resource exhaustion, since rising consumption and stretched supply generated no price signals to ration demand and divert investment toward the development of alternatives.

Under assault from economists of all persuasions, the younger members of the team had learned the appropriate lessons. Now isolated from MIT's engineering and economics departments, they set about constructing a national economic model piece by piece, from the bottom up, incorporating both price mechanisms and actions by financial

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[8] D. H. Meadows, D. L. Meadows, J. Randers and W. W. Behrens III, *The Limits to Growth: A Report for the Club of Rome's Project on the Predicament of Mankind* (New York: Universe Books, 1974).

institutions. Their goal was to simulate the behavior of a monetary production economy by tracking the collective behavior of agents that were realistically defined with respect to the data they could observe, the instruments they could control and the constraints to which they were subject. This was the opposite of the reductionism of neoclassical economics, and therefore all the more appealing. The work was an early exercise in what have come to be known generically as agent-based models: comprehensive, alternative approaches to representing how a market economy evolves through time.[9]

When I learned what Mass and his colleagues were up to, a very large penny dropped in my mind. I realized that the transformational function of computers went far beyond their ability to perform arithmetic and statistical operations on ever larger quantities of data. Computers could serve as simulation engines, making it possible to address problems too complicated to solve analytically and enabling analysts to represent the behavior of systems too complex to model by hand. My immediate response was to engage actively with the System Dynamics National Modeling Project as its practitioners moved from specification of the production sector to construct the financial and government sectors of their model.

In the spring of 1977, I attempted to lay out for our clients how agent-based simulation models differed from econometric models driven by statistical correlations. Econometrics generated "prediction models" that were valued to the extent that they yielded accurate forecasts of economic and financial variables. The experience of the previous few years had shown how unreliable such tools could be in the face of radical discontinuities. The MIT agent-based simulation project, by contrast, was an "exploration model" whose explicit microstructure offered the prospect of being able to trace the nonlinear, disequilibrium consequences of the behavior of the participating agents. It is true that agent-based simulation had limited immediate utility as a prediction model, but the opportunity to follow the simultaneous evolution of individual behaviors and emergent systemic phenomena was novel and provocative. Writing more than thirty years later, Doyne Farmer and Duncan Foley expressed the promise I saw then:

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[9] See J. M. Epstein, *Generative Social Science: Studies in Agent-Based Computational Modeling* (Princeton University Press, 2006), and J. D. Farmer and D. Foley, "The Economy Needs Agent-Based Modeling," *Nature*, 460 (2009), pp.685–686.

> To understand what such a model would be good for it is useful to make a comparison to climate models. We specifically compare to climate rather than weather because we think that it will be a long time before such models will be useful for short term forecasting (though this is not impossible). We think the main utility of such models will be to model the equivalent of the economic climate: For example, when the economy is at a given point in the business cycle, what central bank actions tend to be most effective?[10]

My hope that agent-based simulations would develop into a full-blown methodological alternative was frustrated in the field of economics, even though it has thrived in such fields as epidemiology and climate studies. Indeed, all the natural sciences have adopted simulation techniques as they grapple with the dynamics of systems revealed by ever more fine-grained tools of analysis to be dauntingly complex. As discussed in the Coda to this second edition, their reappearance in economics largely reflects the shift toward more realistic engagement with the world, induced by the Global Financial Crisis and its economic consequences. Back in the 1970s, however, Mass and his team took their project out of MIT on a quixotic venture to apply their model as a tool for macroeconomic forecasting, with the predictable (and predicted) result: the assertion of a superior methodology was irrelevant to potential clients whose sole criterion of merit was the short-term accuracy of the model's prediction of such variables as GDP growth rates and market interest rates.

The demise of the MIT Systems Dynamics National Model in no way inhibited my determination to learn more about the uses of computing. This, in turn, required learning as much as I could digest about the range of contributing disciplines, including semiconductor physics, digital logic and software engineering. It also involved constructing access to the commercial activities that were emerging not only within the behemoth of IBM but also on the Route 128 periphery north and west of Boston and, barely discernibly, in the potato fields south of Palo Alto.

Along the way, I discovered a history – still living then, now all but forgotten – that represented perhaps the most productive collaboration ever in the game between the American state (or, indeed, any other state) and the market economy. Understanding how the US government's

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[10] Farmer and Foley, "The Economy Needs Agent-Based Modeling," p.686.

unprecedented investment in fundamental science and related technologies fostered the emergence of computers and all things digital is central to understanding, first, the emergence of a venture capital industry focused predominantly on information technology and, second, the creation of the new digital economy that venture capitalists and the financial markets have funded over the past generation.

Government Investment in Science and Technology

During World War II, the United States had followed the United Kingdom's example in mobilizing science for war. In addition to funding the development and procurement of advanced technological products, from radar to the atomic bomb, the US government invested in the scientific sources of technological innovation. At war's end, Vannevar Bush, who had served FDR as founder and Director of the Office of Scientific Research and Development (OSRD), delivered to President Truman a prospectus for continuing this investment of public funds. In *Science, the Endless Frontier*, Bush argued:

> The Government should accept new responsibilities for promoting the flow of new scientific knowledge and the development of scientific talent in our youth. These responsibilities are the proper concern of the Government, for they vitally affect our health, our jobs and our national security. It is in keeping also with the basic United States policy that the Government should foster the opening of new frontiers and this is the modern way to do it. For many years the Government has wisely supported research in our agricultural colleges and the benefits have been great. The time has come when such support should be extended to other fields.[11]

Bush explicitly advocated funding basic research, calling it the "scientific capital" that "creates the fund from which the practical application of knowledge must be drawn,"[12] and he argued for making the results of scientific research broadly available to industry and the public at large.

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[11] V. Bush, *Science, the Endless Frontier: A Report to the President on a Program for Postwar Scientific Research* (Washington, DC: US Office of Scientific Research and Development, 1960 [1945]), pp.8–9.
[12] *Ibid.* 9.

For five years, implementation of Bush's vision for permanent programs of state investment in fundamental science was delayed by arguments over the extent and manner of political control. Into the institutional vacuum moved the more entrepreneurial agents in the public sector, which took control of elements of the OSRD's domain: the newly created Atomic Energy Commission assumed responsibility for nuclear research; the National Institute of Health pluralized its name and took over OSRD's programs of extramural grants for life sciences research; and the Office of Naval Research emerged as the vanguard of the newly formed Department of Defense, focusing on the range of sciences and technologies that supported the development of microelectronics and digital computing. In 1950, the outbreak of the Korean War finally induced the creation of the National Science Foundation (NSF), a relatively modest version of the all-encompassing National Research Foundation envisioned by Bush.[13]

The NSF was endowed with a broad mandate across both the natural and the social sciences, but the Office of Naval Research's initiative pointed the way. National funding of the basic research that enabled the IT revolution emerged largely from the Defense Department. The Soviet threat, crystallized in the years following 1945 and amplified by the Korean War in 1950 and the launch of Sputnik in 1957, was the context for the US military's massive commitment to renewing its wartime role as the principal financier of scientific and technological research and the principal customer of the products generated therefrom.[14] Indeed, the fact that various arms of the Defense establishment had become sophisticated purchasers of advanced digital technology may have been more significant than the government's direct funding of research, for it both enabled substantial investments in productive capacity and know-how by the industrial side of the military–industrial complex and encouraged the

[13] For a thorough analysis of the competing priorities, rationales and policy entrepreneurs out of which the Cold War consensus emerged, see D. M. Hart, *Forged Consensus: Science, Technology and Economic Policy in the United States, 1921–1953* (Princeton University Press, 1998), pp.145–205.

[14] The role of the Cold War in legitimizing novel state interventions is evident in the names of two signal acts of legislation that passed during the Eisenhower Administration with overwhelming bipartisan support: the National Interstate and Defense Highways Act (1956) and the National Defense Education Act (1958).

sharing of expertise by requiring second sources of supply and cross-licensing of patents.[15]

Kira Fabrizio and David Mowery summarize the essential elements of federal policy:

> The IT sector, which scarcely existed in 1945, was a key focus of federal R&D and defense-related procurement spending for much of the postwar period. Moreover, the structure of these federal R&D and procurement programs exerted a powerful influence on the pace of development of the underlying technologies and the structure of the industries that developed these technologies for defense and civilian applications.[16]

And the scale was substantial: for twenty-five years through 1978, federal sources accounted for more than 50 percent of national R&D expenditures and exceeded the R&D expenditures of all other OECD governments combined.[17] As Henry Kressel, my partner and collaborator at Warburg Pincus, would write in retrospect, drawing on his own entry into the digital research enterprise at RCA's Sarnoff Laboratory around 1960: "The real visionaries in the early days were to be found in U.S. defense organizations."[18]

The Computer Industry in the 1980s

By 1980, the world of computing had stabilized. IBM dominated commercial data processing across the corporate world. The "seven dwarves," including the BUNCH companies (Burroughs, Univac, NCR, Control Data and Honeywell) plus the computer divisions of GE and RCA, all knew that IBM was more than a mere competitor. IBM defined and managed the environment in which they sought to survive. Digital Equipment Corporation (DEC) led the minicomputer industry. At the

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[15] D. C. Mowery and N. Rosenberg, *Technology and the Pursuit of Economic Growth* (Cambridge University Press, 1989), pp.126–128, 143–146.

[16] K. R. Fabrizio and D. C. Mowery, "The Federal Role in Financing Major Innovations: Information Technology During the Postwar Period," in N. R. Lamoreaux and K. L. Sokoloff (eds.), *Financing Innovation in the United States, 1870 to the Present* (Cambridge, MA: MIT Press, 2007), p.283.

[17] *Ibid*. 283, and Mowery and Rosenberg, *Technology and the Pursuit of Economic Growth*, p. 125.

[18] H. Kressel, *Competing for the Future: How Digital Innovations are Changing the World* (Cambridge University Press, 2007), p.13.

peak of that segment there were some 200 companies focused on auto-mating manufacturing management and financial reporting for smaller companies and for divisions of large ones.

All the computer companies were vertically integrated: that is, the core processing engines were built according to proprietary designs that ran proprietary operating systems often bundled with their own application software and peripheral devices. The goal was to manage complexity for the customer – an important need given the novelty of the technology and the scarcity of trained personnel. The cost was lag-gardly innovation and customer lock-in. The resultant profit margins were too good to last.

The disruptive transformation of the vertical, centralized com-puter industry into a horizontally layered, distributed industry was the work of two decades. The developments unfolded on multiple fronts. One was in computer-aided design, manufacturing and engineering software. Here, the technical members of staff required the continuing, dedicated power of a machine that could run complex, data-intensive algorithms. The engineering workstation, networked to specialized ser-vers, challenged and defeated the minicomputer: Sun Microsystems defeated DEC. It was especially significant that the software technolo-gies deployed by the victors embodied open standards. Although the software was customizable by particular vendors, who were subject to competitive pressure to experiment and innovate, the interfaces were collectively agreed and were accessible to all. Thus, products from different vendors could be integrated into a working system.

The UNIX operating system was developed in the Bell Laboratories of AT&T and licensed to the world as a consequence of the 1956 consent decree with the US Department of Justice that pre-cluded AT&T from competing in the nascent commercial computing markets. The Ethernet networking protocol was developed at Xerox's PARC and successfully promoted as an open standard, in collaboration with DEC and Intel, in competition with IBM's proprietary offering. The profit margins of the new players who used these open standards were distinctly lower than their entrenched competitors', but their growth was positive and accelerating.

Client–server computing was proven in niche technical applica-tions where the scale of the addressable market was measured by the number of "seats" occupied by distinct categories of engineers. In time, as the technology matured and as the performance of general-purpose

microprocessors grew to match and then to exceed the custom hardware at the core of IBM's mainframes, client–server systems would penetrate the enormously larger commercial markets, IBM's domain. The manner in which I was educated to appreciate the economic and investment significance of this revolution came by way of an abstruse academic exercise.

Real Lessons from Artificial Intelligence

A persistent interest in innovative applications of computing had led me to the frustrating frontier of artificial intelligence (AI). Around 1980, Phil Meyer, a colleague of mine at Eberstadt who had followed the oil boom from the electronics industry to the sophisticated end of the oilfield equipment and supply industry, discovered that the chair of the oilfield services company Schlumberger, Jean Riboud, had recruited the entire team of AI researchers from the Stanford Research Institute. On the simple premise that if Schlumberger was interested in AI, then we should be as well, Phil set out to learn all he could, in the style of the unreconstructed, old-fashioned investment analyst that he was. Riboud's immediate purpose was relatively easy to discern. The core of Schlumberger's extraordinarily profitable business was the generation of data from proprietary instruments inserted into oil wells to enable analysts to estimate the likelihood of finding hydrocarbons and to assess the magnitude of any find. The data were being interpreted by human experts; if their work could be even partially automated, then Schlumberger's productivity and profits would rise in tandem.

Phil correctly read Schlumberger's project as indicative of a broader potential. And so he set out, with me in tow, to explore the broad world of academic and industrial AI research. MIT's Artificial Intelligence Laboratory, established in 1970 and managed by Pat Winston, was the first stop. MIT was the logical place for us to start because of its early engagement with the field and because we had already built a relationship with its Industrial Liaison Program. But the range of relevant research establishments was broad, covering a multitude of academic and industrial labs and a growing number of start-ups staffed therefrom. Projects included primitive exercises in machine learning of the physical, and the definitely more problematic metaphysical, worlds that humans find it natural to navigate. The center

of gravity of these research endeavors lay in "expert systems," software programs populated with relevant rules for decision-making in specified domains, from medical diagnosis (University of Pittsburgh) to management of shipboard propulsion systems (Bolt, Beranek and Newman). That the researchers in the field had their own chosen language, LISP, and required specialized workstations optimized to run LISP added a certain esoteric allure and exemplified the cult nature of the enterprise.

Over the course of a year or more, we visited all the labs and met all the start-ups. We managed not to lose any of our own or our clients' money in the process largely because along the way we had met a legendary figure on the West Coast who, we were persistently informed, was a radical critic of the AI research agenda and spoke from a position of authority within the world of computer science. At the suggestion of Howard Austen, a knowledgeable, credible and connected consultant whom Phil had retained, one evening after dinner I found myself knocking on the door of a small, nondescript building on a side street off Page Mill Road in Palo Alto. Eventually I was admitted and escorted to a conference room, where I was joined by a tall, bearded man. This was John Seely Brown, or JSB as he had already come to be known. JSB was still an independent scientist and not yet director of Xerox PARC, but he was already a figure with extraordinary reach across the entire space of information technology, from the physics that constituted its foundation to the epistemological issues that attended its applications. For the next two hours, he and I set out to discover what we were talking about – which meant discovering *how* to find out what we were talking about.

JSB and I bonded over the impossibility of deriving semantic information – "meaning," that is – from the syntactical rules of language. The purportedly expert systems could replicate only the most simplistic of intelligent behaviors, those following well-defined rules. Genuine expertise, by contrast, is a function of, first, perceiving patterns that distinguish possible signals amid a world of noise and, next, bringing experience to bear in order to interpret them. Meaning, that is to say, is relative to context, and learning to read context adequately is a lifetime's work.

The ability of computers to track the evolution through time of the elements of complex systems, like their ability to estimate correlations across ever larger sets of data, made them increasingly useful tools for extending the application of human intelligence. However, the

expectation that they could be developed into autonomous substitutes for human intelligence was doomed then and remains problematic today, even as the second great wave of investment in AI, funding innovative methods of machine learning, gains cumulative strength.

Lessons from the collapse of AI's first hype cycle in the 1980s remain relevant in assessing the prospects of the second cycle in which we are now embedded. This time, machine learning techniques are properly focused on pattern recognition in the oceans of data generated and captured in an increasingly digitalized world. We will consider the promises, threats and limits of contemporary AI in Chapter 12. Here it is appropriate to note that transforming data into information, and from that information extracting actionable meaning, remains context-dependent. Bootstrapping the understanding of context from the bottom-up examination of data remains a daunting challenge. That is why the increasingly rich spectrum of machine learning successes share a common, general character. Either there *is* an objectively definable pattern to identify (as in the case of image recognition) or there are defined rules exogenously established (as in the case of games, from Chess to Go to Poker). When the meaning of the pattern in question is subject to real-time negotiation – as was precisely the case in my first conversation with JSB – comprehending the context is necessarily a shared creative act for which humans remain better equipped than machines.[19]

JSB offered me informal access to PARC, where I got to play with the Xerox Star, the first PC that could run a graphical user interface and be managed by a mouse, and where I had the opportunity to be a naive guinea pig for the assertedly intuitive directions for operating the holy grail: the digital copiers that would transform Xerox's core business. At this frontier of digital innovation, a future of intelligent client computers distributed across networks and drawing on the power of dedicated servers could be lived in real time.

This was some years before Xerox finally learned how to earn a return on PARC's extraordinary innovations in the architecture, technology and application of digital systems.[20] The profitability of

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[19] For an informed assessment of the state of AI, see Gary Marcus, "Artificial Intelligence is Stuck: Here's How to Move It Forward," *New York Times*, July 29, 2017.

[20] For the history of Xerox's failure to exploit PARC's innovations, see D. K. Smith and R. C. Alexander, *Fumbling the Future: How Xerox Invented, then Ignored, the First Personal Computer* (San Jose, CA: Excel, 1999).

Xerox's patented position in the copier market meant that no start-up business could compete with the economics of the existing business, so the company was passively watching entrepreneurs depart to start new companies when headquarters refused to commit the funds required to turn invention into commercially significant innovation. This was a powerful lesson in one way that the innovator's dilemma expresses itself in action, crippling the ability of a company with surplus resources to exploit commercially the innovations generated by research funded with those resources.[21] Warburg Pincus would benefit hugely from such corporate paralysis when it came time, more than a decade later, to challenge IBM's core engine of monopoly profit. As for Xerox, it finally began earning a return on PARC's innovations years later by taking minority stakes in spin-off ventures.

In March 1983, we at Eberstadt sponsored a colloquium on artificial intelligence at MIT.[22] One of my assignments was to induce JSB to participate in what was bound to be a festival of promises that he profoundly disbelieved could be kept. His lecture, on "the high road and the low road" of AI research, stays with me to this day. The high road was the project to give computers the ability to think like human beings. When that project had failed, as JSB correctly anticipated it would, we would be rewarded nonetheless for having, of necessity, followed the low road of incrementally improving how human beings and computers interact. Three years later, the Dreyfus brothers nailed the lid on the pretensions of the first generation of AI research with their definitive work, *Mind over Machine*.[23] With their behavior rigidly dictated by rules imposed from outside, the so-called expert systems were actually emulating human apprentices.

My engagement with Xerox PARC provided an education at the frontier of innovation in information technology. More than twenty years later, it would pay an exceptional dividend by validating our shared critique of the initial, fundamentally misguided approach to AI. As JSB had anticipated, the work "wasted" on AI would contribute

[21] See C. Christiansen, *The Innovator's Dilemma: When New Technologies Cause Great Companies to Fail* (Cambridge, MA: Harvard University Press, 1997).
[22] The proceedings of the conference were published: P. H. Winston and K. Prendergast, *The AI Business: Commercial Uses of Artificial Intelligence* (Cambridge, MA: MIT University Press, 1983).
[23] H. Dreyfus and S. E. Dreyfus, *Mind over Machine* (New York: The Free Press, 1986).

to the integration of computers into the working and social lives of human beings for three decades and more.

Paul Ricci was a young member of the PARC staff when JSB introduced us in 1983. In September 2000, he left Xerox, where he had risen to the role of group Vice President of Marketing, to become CEO of one of Xerox's family of sponsored spin-offs, Scansoft. Endowed with Xerox's optical character recognition technology, which is used to scan paper documents into digital formats, Scansoft was struggling to reach profitable scale as an independent public company. Once Paul had acquired Scansoft's principal competitor and established a sustainable – albeit slow-growing – base of cash flow from operations, he decided that the time was ripe to address automatic speech recognition, a domain of technological invention that seemed to be persistently a decade from commercial maturity.

Automatic speech recognition was one of those fields of AI research where the rules-based approach had failed to produce adequate results. Paul's bet succeeded largely because research and development in speech recognition was turning toward the application of increasingly sophisticated statistical techniques to ever larger datasets using ever more powerful computers to identify ever more subtle correlations. In other words, researchers broke the code in speech recognition by using computers as computers, not as pathetically inadequate simulacra of the human mind. It should come as no surprise that much of the underlying science and technology was funded by the Defense Department and other arms of the government.

The first time Paul approached me at Warburg Pincus to ask me to consider backing his vision was in 2002. I listened with academic interest and minimally restrained skepticism. But within two years Paul had purchased relevant technology, and Scansoft had begun to demonstrate both step-function increases in the accuracy of speech recognition and meaningful revenue from a variety of applications of the technology. Moreover, Paul understood that the critical factor was not the raw accuracy of the recognition engine but, as always, customer satisfaction. Unlike the techno-geeks who had been driving the technology for decades, Paul recognized that turning automatic speech recognition from a laboratory curiosity and a science-fiction fantasy into a large-scale commercial solution required taking seriously the delicate process of engineering human beings into the system as back-up and for quality control.

Warburg Pincus acquired Xerox's residual ownership of Scansoft in March 2004, when annual revenues were somewhat above $100 million. We subsequently funded several strategic acquisitions while the company was on its way to approximately $1.5 billion in revenues in 2011, prior to Warburg Pincus's exit from its successful investment (and $2 billion in revenues today). One of those acquisitions was the Stanford Research Institute's entry in the game, Nuance Communications, which carried a far more relevant name for the leader in speech recognition than Scansoft. As Nuance, the company established leadership positions in a range of major markets: voice control of mobile devices; automation of enterprise call centers; dictation, both general purpose under the Dragon brand and with specific applications, such as medical transcription. Extension of the statistical approach to automatic speech recognition led Nuance to the frontier of the current, second generation of AI research: computerized natural language understanding, applicable not only to digital transcripts but to all media of human communication.

The Return of the IPO Market

The passage from PARC in the early 1980s to Nuance in one professional and multiple technological generations illustrates the continuity available from human relationships through discontinuous shifts at the frontier of technology. Long before I reconnected with Paul, when I was still in the microworld of Eberstadt's research-based investment banking practice, the first hint of a major shift in the capital market context came in the autumn of 1980, when the hugely successful offerings of Genentech and Apple signaled the end of the IPO drought in which we had thrived. The Volcker credit crunch that broke inflation with double-digit interest rates postponed the revival, but by the autumn of 1982 the writing was on the wall or, rather, the growl was in my ear.

The year before, we had financed Daisy Systems, a pioneer in computer-based electronic engineering software. Daisy's lead venture capital investor was a remarkable individual named Fred Adler. With extraordinary analytical powers and intense purpose, Fred had worked his way from poor Jewish Brooklyn through Harvard Law School to a leading Irish Catholic law firm in New York. From that base, he had

put his talents to work as a turnaround artist, taking operational control of troubled businesses and driving them to positive cash flow. His successes reached from Loehmann's, a chain of discount women's clothing stores based in New York, to a Silicon Valley semiconductor company backed by some of the Valley's venture capital elite. Fred's motto, displayed on needlework pillows in his office, was: "Corporate Happiness Is Positive Cash Flow." Paying bills by selling products to customers for more than it cost to develop and deliver them endows a company with strategic freedom: independence from the volatile vagaries of the capital markets. This, is the always relevant lesson today's Unicorns sooner or later will be compelled to learn, and in the vast majority of cases it is bound to be learned the hard way.

Fred had made his decisive step to become a venture capitalist in 1969 by mobilizing the capital to back a brilliant engineer, Ed de Castro, who had left DEC to start Data General, one of the top tier of minicomputer companies that emerged in the 1960s and 1970s. By 1980, Fred had built a substantial venture capital firm, based in New York, with a portfolio that extended from Israel to Silicon Valley. Daisy Systems, a leader in computer-aided engineering for the design of printed circuit boards, was one of his most promising investments when I proposed that we do a second private placement to fund its growth. Specifically, I proposed using roughly the same valuation metrics as the year before, despite hints that the IPO window might be finally opening. My argument was that returning to our institutional clients, long-term equity investors who already knew the company and owned the stock, was a safety play for which a substantial discount from a hypothetical future IPO was appropriate. I knew the game had changed when, on a cold evening in November, I stopped at a pay phone in Columbus Circle on the way home to catch up with Fred, who reported, "Sandy Robertson just told me he'll do Daisy at Janeway-plus-10 percent!"

Fred and Daisy stayed with Eberstadt for this financing despite the offer of a higher valuation, but the game had indeed changed. As the window opened with breadth and depth and even some speculative excess, both the institutions and the bankers woke up. The latter were led by the "Four Horsemen" of the venture capital ecosystem: Alex. Brown, Hambrecht & Quist, Robertson Stephens (Sandy Robertson's firm) and Rothschild, Unterberg & Towbin. Even the major firms, such as Morgan Stanley and Goldman Sachs, were drawn to the new business

opportunity represented by financing venture-backed IT and biotech companies in the public equity market. And all recognized that pairing research analysts with investment bankers was the way to win the business and to market the stock.

What only some fifteen years before had been a marginal, hardly respectable activity – peddling shares in speculative, early-stage companies to risk-seeking retail investors – had become a worthy and substantial line of business. In what seemed like a heartbeat, our innovation of the late 1970s, research-based investment banking, had become business as usual, although in the generic model it was the bankers who told the analysts where to go and what to do.

By 1984, it had become clear that fundamental investment research as a product of the "sell side" of the market had two possible futures that were emphatically not mutually exclusive: commoditization and prostitution. As food for institutional investors, the path forward was toward commoditization. By the mid-1980s, commission rates on large institutional blocks of shares had fallen more than 50 percent from their former fixed levels, breaking through 10 cents per share ("a dime a dance") with no bottom above zero in sight. In the absence of a cartel-based subsidy from brokerage transactions, sell-side research could not pay for itself: why would any institution pay for an investment idea if the vendor was simultaneously offering it to all others? In economists' jargon, the output of sell-side research was a nonexcludable, nonrivalrous good that, once published, any number of competitors would consume and replicate simultaneously without the protection that copyright or patent law offered other forms of intellectual property.

No wonder, then, that talent started leaving the research departments of Wall Street brokers in order to monetize the value of their knowledge almost as soon as commission rates began to fall. By 1980, two of the top analysts of the computer industry had shown the way. Gideon Gartner had left Oppenheimer & Company to start his highly successful business, advising corporate clients on their IT purchasing decisions. And Ben Rosen had left Morgan Stanley to start his newsletter and conference business before he co-founded the most successful new venture capital firm of the 1980s.

The alternative path, the one toward prostitution, was already apparent fifteen years before the revelations that followed the dotcom/telecom bubble. A direct example of how economic incentives constantly threatened analytical objectivity came in our corporate advisory

business. Ed Giles, in addition to serving as President and Research Director of Eberstadt, continued to function as the best-ever investment analyst of the chemical industry. In the mid-1970s, he had hired arguably the second-best analyst out of a provincial trust department. One of the chemical companies with whom they had built a close and rewarding relationship was Hercules. There came a day when number two came into Giles's office with the news that the numbers did not add up to what Hercules was forecasting. So Giles called the Hercules CEO, whose response was: "Ed, ignore it. You're a president and I'm a president. We have people who worry about the numbers."[24]

Research-driven investment banking existed in a sea of conflicting interests. When we first met with the management of an interesting company, we would begin by explaining that we had no idea whether we would end by proposing to sponsor the company to our institutional clients (and join them in buying the stock), or propose a merger or acquisition or invite them to put us on retainer to provide strategic advice. We did intend to demonstrate that we understood their business better than any other financial firm. I used to say, "Conflicts of interest exist; the difference between children and grown-ups is that the latter know how to manage them." Perhaps it should not be surprising that, when the stakes increased exponentially during the Internet Bubble, so many senior bankers and analysts proved themselves to be children. But by then we had long since declared victory and sold our firm.

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[24] After 2000, the major banking firms' abuse of our innovative alignment of research analysts with investment bankers would become notorious during the dotcom/telecom bubble and result in the "Global Settlement," which established a regulatory wall between the two.