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The Long History of Molecular Electronics: Microelectronics Origins of Nanotechnology

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ABSTRACT Long before nanotechnology, the semiconductor industry was miniaturizing microelectronic components. Since the late 1950s, that industry's dominant material has been silicon. Yet there have always been competitors to silicon that supporters hope will upend the semiconductor industry. It is impossible to understand this industry without a more complete picture of these alternatives – how they come about, how they capture organizational support, why they fail. It is equally impossible to understand nanotechnology without a focus on these alternatives, since research communities devoted to perfecting them today form the backbone of the nanotechnology field. We trace the history of the longest lived silicon alternative – molecular electronics. Molecular electronics arose in the late 1950s as a visionary program conducted by Westinghouse on behalf of the Air Force. We attribute its failure to the difficulties inherent in matching a futuristic vision to a bureaucratically accountable, incremental program that could compete with silicon. Molecular electronics reappeared again at IBM in the 1970s and at the Naval Research Laboratory in the 1980s. In each of these incarnations, molecular electronics' charismatic champions failed to gain the organizational support to make it a mainstream technology. Only at the turn of the century, with new nanotechnology institutions and new models of industry–university collaboration, has some form of molecular electronics neared acceptance by the semiconductor industry.

Keywords IBM, miniaturization, Moore's Law, Naval Research Laboratory, silicon, Westinghouse

The Long History of Molecular Electronics:

Microelectronics Origins of Nanotechnology

Hyungsub Choi & Cyrus C.M. Mody

If there was anything constant during the past half-century of microelectronics, it was the recurrence of radical rhetoric promising rosy futures for the 'next generation' of electronic miniaturization. Long before contemporary nanotechnology, many scientists and engineers predicted the future using colorful neologisms such as 'atomic electronics' and 'angstronics' (Gartner, 1959). During this period, certain institutional conditions repeatedly elicited a constellation of such futuristic scenarios for describing a 'breakthrough' technology that would move society beyond the 'brick wall' imposed by the present technological platform.

The technological achievements in microelectronics were stunning. By 1960, what had been state-of-the-art electronic building blocks at the end of

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World War II – vacuum tubes, crystal diodes, and mercury switches – were replaced by transistors and integrated circuits.[1] Since 1960, we have witnessed an exponential shrinking of electronic components, as predicted most famously by Gordon E. Moore (1965). Indeed, the rapid pace of ongoing technological change that continues to the present provided a fertile ground upon which futuristic language could flourish. Yet, the increasing complexity of institutional networks sustaining (and the size of the investment in) silicon microelectronics set the contours of this futurism. The orthodoxy of silicon is the key to understanding what kinds of people and organizations project which kinds of future for microelectronics, as well as to understanding which futures are acted upon and which are discarded.

Representations of the future of microelectronics during the last 50 years nearly always embodied a particular view of the field's history. One way to justify the *necessity* of a radically different future was to identify a past trend, and show that the trend quickly arrived at a technological dead-end. Were the new technological platform adopted, the protagonists argued, the road ahead would prove itself smooth and well lit. Throughout the history of microelectronics, many new futures were constituted. In turn, each new future reflected its interpretation of the past. Only those able to successfully narrate the past were capable of articulating a compelling vision of the future.

In this paper, we trace the genealogy of one such rhetorical strategy from the late 1950s to the present. In 1958, the US Air Force and Westinghouse adopted the term 'molecular electronics' to describe their radical joint research program, which aimed to leapfrog beyond both the conventional approach to circuit integration proposed by RCA and the US Army Signal Corps and the burgeoning efforts to build monolithic integrated circuits at Fairchild Semiconductor and Texas Instruments. Viewed with hindsight, this program failed, and the term 'molecular electronics' went out of vogue by the mid 1960s. It was revived, however, in the late 1970s by a small group of chemists led by Forrest L. Carter at the Naval Research Laboratory (NRL). Despite the rhetoric of radical, disjunctive change in both versions of molecular electronics, each quickly reverted to an incremental approach. The disjunctive language arose within the context of acute crises – Sputnik in 1958 and the Japanese economic challenge in 1976. But they were soon subsumed into the mainstream technological platform of silicon integrated circuits. We will argue that the fundamental tension between radical rhetoric and incremental practice has characterized the two lives of molecular electronics.[2]

This dialectic can be readily observed in two diagrams constructed in 1958 and 2002. The earlier one was drafted by an Air Force colonel for an opening presentation in the 1958 Conference on Molecular Electronics (Fig. 1); the later one was included in a pamphlet published by the Committee for the Review of the National Nanotechnology Initiative in 2002 (Fig. 2). Beyond the striking visual similarity, both figures deliver much the same message: miniaturization is constant, even predictable; but the technological platform occasionally experiences a radical transition:

FIGURE 1
Adapted from Lewis (1958: 37).

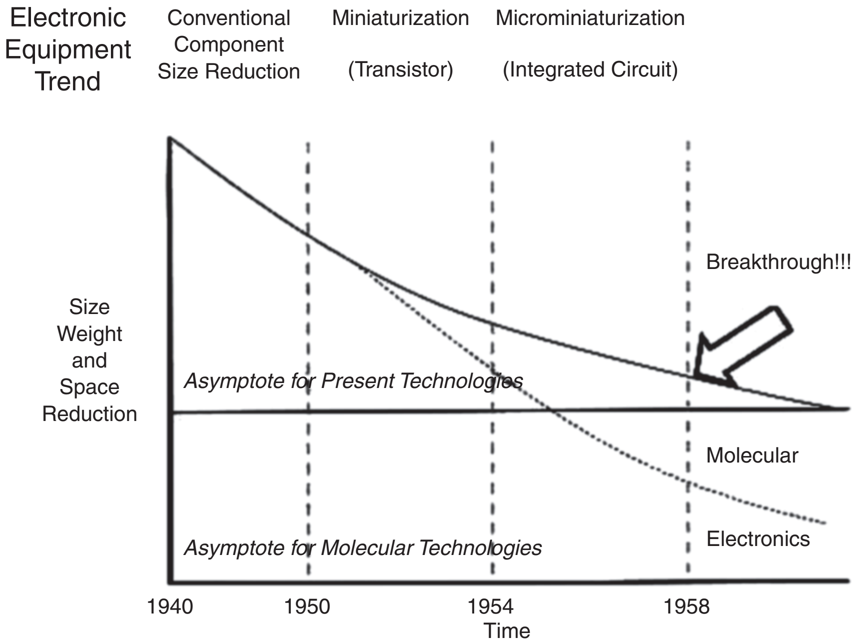
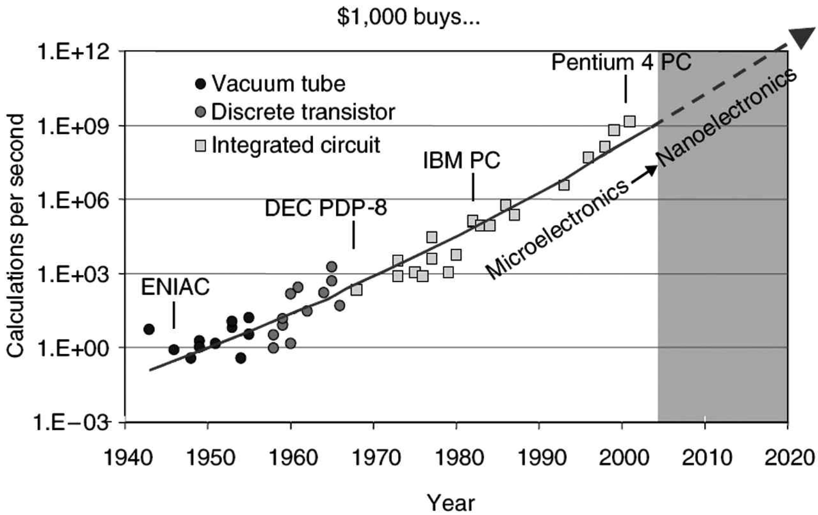


FIGURE 2
From Committee for the Review of the National Nanotechnology Initiative, Division on Engineering and Physical Sciences, National Research Council (2002: 7). Adapted from Kurzweil (1999): used by permission of Viking Penguin, a division of Penguin Group (USA) Inc. (© Ray Kurzweil).



from vacuum tubes, to discrete transistors, to integrated circuits, and on to something else. People have been proposing a post-silicon 'something else' for almost five decades now, yet the massive investment of money, people, and institutions in silicon integrated circuitry has allowed it to persevere.

'Presumptive anomaly' is the term that historian of technology Edward Constant coined to explain this kind of disjunctive leap in the technological trajectory, even when existing technology shows satisfactory performance. As opposed to functional anomaly, presumptive anomaly occurs, in Constant's words, when scientists or engineers begin to *perceive* that 'under some future conditions the conventional system will fail (or function badly) or that a radically different paradigm will do a much better job' (Constant, 1973). This observation fits well with the history of postwar microelectronics; or maybe a little too well. In this high technology field, presumptive anomaly has been a way of life, leading to a mindset of perpetual revolution. This sense of constant and rapid technological change, together with escalating competition within and outside the USA, underlies the two graphs set apart by almost 45 years.

The focus of this paper is how large-scale bureaucratic organizations struggled to remain innovative and profitable during this era of perpetual revolution. A conventional history of microelectronics would emphasize the striking staying power of silicon-based integrated circuit (IC) platforms during the past half-century. Instead, we will focus on the radically new ideas that the microelectronics community continued to produce, such as molecular electronics, spintronics, and quantum computing, along with promises to bring us over the perceived 'brick wall'. Rapid technological change and the urge to leapfrog competitors allowed these new radical ideas to flourish, at least for brief periods of time. Many of them, however, were quickly subsumed to the IC platform, sometimes due to market pressure and at other times due to the managerial urge to impose accountability on freewheeling researchers.

This competition between the radical and the incremental within research bureaucracies defines the long history of molecular electronics. Large research institutions (for example, NRL, Westinghouse, and so on) tended to gravitate toward incremental approaches for the bureaucratic reason that each member of the organization must be seen to be working on well-defined, day-to-day tasks for management to have oversight and because the difficulty of coordinating collaborating organizations (suppliers, contractors, and so on) encouraged small steps rather than large leaps. Yet such institutions occasionally welcome radical visions as a way of jockeying with competitors. As Harry Collins (2004) has shown, large corporate labs and mission-oriented federal agencies have such lavish funding that very small percentages spent on visionary projects amount to large absolute sums. Given the potentially catastrophic consequences of being leapfrogged by a competitor's radical approach, these laboratories are unwilling to place all their eggs in an incrementalist basket. Through time, this dialectic between radical rhetoric and incremental practice produced a series of 'orphans of microelectronics'.

The history of such 'orphans' provides a unique opportunity to understand one of the various strands that make up contemporary nanotechnology. While earlier 'orphans' have been rather easily subsumed into the standard IC platform, since the 1990s revolutionary ideas were met with increased attention, funding, and institutional support that eventually led to the construction of nanotechnology as we see it today (McCray, 2005). Currently, practitioners of nanotechnology are constructing yet another stylized graph of the past in order to project the future of 'nanoelectronics'. However, the story of molecular electronics tells us that the past was not as orderly as the prophets would like for us to believe. Rather, various versions of the past that the visionaries constructed provided frameworks within which they could imagine alternate futures. It is from this perspective that we reconstruct a lineage for nanotechnology, tracing its origin back to the 'orphans' of microelectronics.

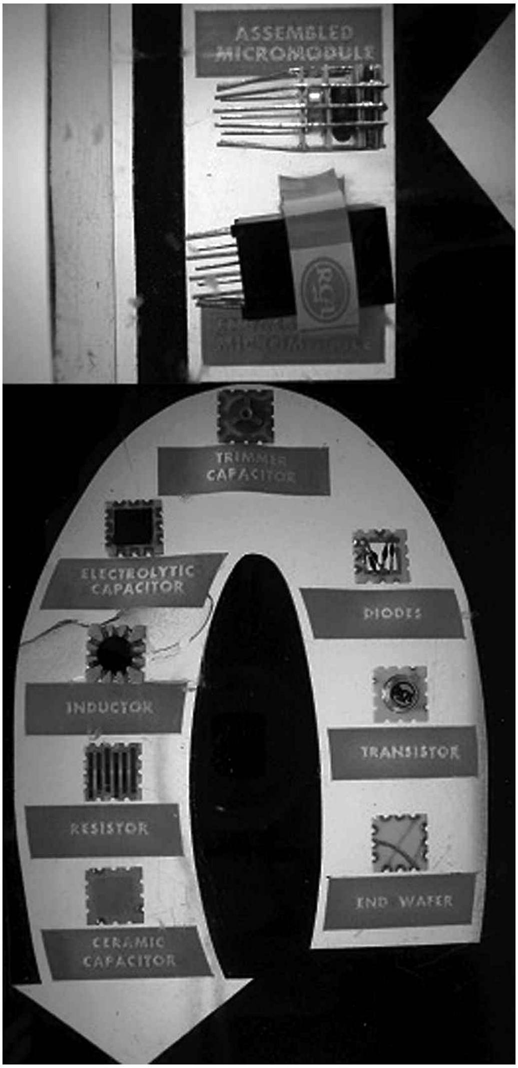
'Tyranny of Numbers'

The transistor was by no means the first step toward miniaturizing electronic components. During World War II, electronics manufacturers developed and produced miniaturized and sub-miniaturized vacuum tubes for various weapons systems under military contract. Nevertheless, it cannot be denied that the invention of the transistor was a watershed moment. The military immediately recognized its merit. As Harold A. Zahl, research director of the Army Signal Corps Engineering Laboratories, noted, the transistor was an 'apparent promise ... to relieve the GI of one of his biggest headaches – carrying weight; with a smaller radio he could carry more food and bullets' (Zahl, 1968: 115). The transistor, it seemed to Zahl, was a perfect solution to many of the engineering problems: it was smaller, lighter, consumed less power, and emitted less heat. It was difficult to ask for more.

Ironically, as transistors gained widespread use in the mid 1950s, their advantages became sources of serious obstacles. The smaller component size and superior heat characteristics made possible the realization of complex circuitry that previously existed only in the minds of circuit engineers. This was all well and good, until the equipment began to fail much more frequently than tube-based devices. The weakest links were the interconnections among components. A linear growth in the number of components yielded an exponential growth in the number of interconnections, any one of which could fail. Jack A. Morton, Vice President of Bell Labs, described this difficulty well when he said 'The tyranny of large systems sets up a numbers barrier to future advances if we must rely on individual discrete components for producing large systems' (Morton, quoted in Reid, 2001: 16).

The US military was among the first groups subjected to the 'tyranny of numbers'. Since the end of World War II, the US Army Signal Corps worked on printed circuits and automatic assembly techniques for its electronic equipment. These efforts continued with the transistor. The first breakthrough came in 1958 when RCA, with an intimate understanding of

FIGURE 3
Micro-elements and the fully assembled micro-module. Accession # 1998.0191 (Danko collection), National Museum of American History. Photograph by Hyungsub Choi.



military needs through its long relationship with the Army, proposed a distinctive packaging technique called a 'Micro-module' (Fig. 3). RCA received an initial US$5 million contract from the Signal Corps for the micro-module program on 1 April 1958. At the core of the program was making 'extremely small elements of uniform shape and size' on a 3/10 inch-square ceramic wafer. These 'micro-elements', such as resistors, capacitors, and transistors, were stacked, connected, and encased into a 'cube-shaped solid', the 'micro-module'.[3]

The Army's choice of the micro-module approach reflected its needs for communication systems on the battlefield. The emphasis was on reduced size and weight, as well as ease of repair. Since electronic circuits were neatly packaged within a module, front-line soldiers could simply 'replace and discard' the malfunctioning module instead of repairing it. RCA engineers claimed that this would be 'economically feasible where subassembly value is in the *several hundred dollar range*'.[4] In order to keep the cost low, however, mass production was necessary. By 1960, RCA began planning for the mass production of 'transmitter–receiver assemblies in combat helmets' (McLean, 1960) using the micro-module technique. In 1962, the Chief Signal Officer Major General Earle F. Cook proudly announced that 'the micro-module program [was such] a success' that by March 1963, he would 'plan to have a capacity of a quarter million modules per year'.[5]

The RCA–Signal Corps micro-module program was but one option among many competing efforts to overcome the 'tyranny of numbers'. Another option, which we will discuss in the following section, was the molecular electronics program advocated by Westinghouse-Air Force.

Constructing the Black Box of Molecular Electronics

The micro-module and other 'Lego-like' alternatives were a clever solution to the problem of interconnections. While these techniques substantially enhanced the reliability of electronic components, however, the interconnections were still there, functioning as a physical limit to further miniaturization. The notion of 'molecular electronics' emerged from three sources in the course of searching for a solution to this problem: Arthur R. von Hippel's materials research program at MIT; Westinghouse Electric's semiconductor research program; and US Air Force's search for next-generation electronic components for its missile and space vehicle program.

In 1936, at the invitation of MIT President and physicist Karl T. Compton, Arthur von Hippel, an émigré physicist from Germany, joined the MIT electrical engineering faculty. He actively participated in war-related research during World War II as Director of MIT's Laboratory of Insulation Research, and at war's end, he articulated a strong vision of restructuring the field of materials research.[6] Although scientists and engineers had developed an impressive array of instrumentations and materials during the war, von Hippel argued that these were largely achieved through empirical methods. Now it was time to make a transition from a 'phenomenological approach' to 'molecular engineering', based on a firm theoretical understanding of the correlation between micro- and macro-properties of materials (Von Hippel, 1956).

Von Hippel's vision was to design the desired material characteristics from the bottom-up. He argued:

> Instead of taking prefabricated materials and trying to devise engineering applications consistent with their macroscopic properties, one builds

> materials from their atoms and molecules for the purpose at hand … . He can play chess with elementary particles according to prescribed rules until new engineering solutions become apparent. (Von Hippel, 1956)

For von Hippel, molecular engineering was not merely an academic program of research. Its development 'require[d] the generous cooperation of industry; it require[d] retraining of engineers in summer courses and by postgraduate fellowships'. Through these training programs, molecular engineering would infuse a 'new mode of thinking' into practicing engineers, which would in turn place engineering upon a 'fundamental foundation' of science (Von Hippel, 1956). Toward this end, in 1956, von Hippel held a 10-day summer course on molecular engineering at MIT; and in 1959 he published a textbook entitled *Molecular Science and Molecular Engineering*, co-authored with numerous researchers not only from academia but also from military and corporate laboratories, including the Air Research and Development Command (ARDC) and Westinghouse (Von Hippel, 1959).

The notion of 'molecular engineering' made its way into the electronics industry through Westinghouse. As early as 1957, Westinghouse had begun a 'Molecular System Engineering' program to implement von Hippel's ideas.[7] This included, among others things, studies of 'dislocations in single crystals of semiconductors, their control and effects'; 'diffusion of atoms of various materials into semiconductors'; and 'dendritic crystal growing, that is, single crystals that can be grown in thin sheets with precise crystal orientation'.[8] By 1958, it was clear that Westinghouse's intention was to use molecular engineering to overcome the limits of miniaturization imposed by the 'tyranny of numbers'. For instance, in a letter that year, S.W. Herwald, Manager of Westinghouse's Air Arm Division, assured an Air Force research manager that through the 'understanding of theoretical materials design' they would be able to 'control the behavior of materials through control of molecular structure'. This, Herwald claimed, would lead to a 'revolution in field electronics. Size, weight, and cost of devices will plummet. Automation will rule our factories and, perhaps best of all, *reliability will increase by orders of magnitude*'.[9]

The notion of 'molecular electronics' crystallized in a prolonged interaction between Westinghouse and the Air Force. In August 1957, Westinghouse approached the Air Force with a presentation on 'Molecular System Engineering' before a group of staff researchers at the Air Force Cambridge Research Center (AFCRC). Either Westinghouse was overly ambitious or the AFCRC was taken by surprise, for the Air Force expressed concern over Westinghouse's 'rosy optimism' for its research program.[10] The Air Force and Westinghouse met a second time, however, in January 1958, this time initiated by the AFCRC. In the second meeting, the Air Force researchers were more conciliatory, revealing their interest in 'initiat[ing] a major start' in the field of 'solid state physics and physical electronics'. The AFCRC solicited presentations on topics such as 'high speed computer devices', 'circuit integration techniques', and 'high temperature semi-conductor devices', applicable to its 'space vehicles and problems'.[11]

Viewed with hindsight, Westinghouse approached the Air Force at a particularly opportune moment. On 4 October 1957, the Soviet Union successfully launched the first artificial satellite 'Sputnik'. This event sparked a deep-seated panic in the US in general, and the Air Force in particular, that the Soviets were forging ahead in advanced science and technology while the Americans were falling behind. Therefore, it is not difficult to imagine that by January 1958 Westinghouse's radical (August 1957) proposal for electronic miniaturization began to look more appealing in the Air Force leaders' eyes (Kleiman, 1966).

Even with an external threat in the background, however, the Air Force did not make abrupt decisions. While the Air Force was clearly interested in Westinghouse's molecular system engineering program, it was not going to seal the deal before conducting a more comprehensive survey. Obviously, Westinghouse was not the only potential contractor at this point. Thus, in November 1958, the Air Research and Development Command (ARDC) convened a conference jointly with the National Security Industrial Association (NSIA) on 'Molecular Electronics'. With close to 300 scientists and engineers from military and private research labs, including Westinghouse, RCA, and GE, this event was, in effect, the Air Force's open invitation to anyone who could fill the black box of molecular electronics.

Colonel C.H. Lewis, Director of Electronics at ARDC, opened the conference with a presentation on 'The Needs of the Air Force'. According to Lewis, airborne electronic equipment grew increasingly complex and vulnerable to failure as aircraft went faster and higher, eventually with orbiting platforms at a height of 200 miles (320 km). Severe operating environments, including radiation, high temperature, and extreme vibration provided optimal conditions for equipment failure. Hence, the key requirement was to ensure 'inherent reliability' of 'at least 1,000 hours (42 days) and preferably for 10,000 hours (approximately a year)!'

The solution to achieving increased reliability, as Lewis envisioned it, was not in 'refining equipment components'. He opined, 'we could refine components again and again, but I am very dubious if we would obtain an order of magnitude of improvement in reliability, compactness, or even consumption'. Lewis solidified his point by presenting a graph showing the trend of electronic miniaturization since 1948 (Fig. 1). This trend had followed an orderly path from conventional components (tubes) through transistors to integrated circuits during the past decade. Lewis argued that 'we have to leave this orderly path and breakthrough to a new curve and to a new approach for solving our electronic problems'.

> Instead of taking known materials which will perform explicit electronic functions, and reducing them in size, we should build materials which due to their inherent molecular structure will exhibit certain electronic property phenomena. We should synthesize, that is, tailor materials with predetermined electronic characteristics. Once we can correlate electronic property phenomena with the chemical, physical, structural, and molecular properties of matter, we should be able to tailor materials with predetermined characteristics. We could design and create materials to perform desired

> functions. Inherent dependability might eventually result. We call this more exact process of constructing materials with predetermined electrical characteristics MOLECULAR ELECTRONICS. (Lewis, 1958: 3–7)

This was a radical vision when transistor fabrication processes were still regarded as 'mysterious witchcraft' (Shockley, 1972). It is worth noting that Lewis employed a language akin to that used in von Hippel's molecular engineering and Westinghouse's molecular system engineering programs. Through the words of Lewis and with the institutional support of the Air Force, molecular electronics made its spectacular debut in the field of microelectronics. The Air Force (personified by Lewis) played the role of an institutional visionary by identifying the breakdown of current trends and articulating a future beyond the perceived limit imposed by conventional methods of circuit integration. Integral to the Air Force's vision of the future was some version of molecular electronics (literally, the post-1958 'Breakthrough!!!' box of Fig. 1). Notably, this box was initially empty – the Air Force knew it wanted a 'breakthrough!!!' called 'molecular electronics', but it did not know what this would look like; the 1958 conference served as an invitation to the microelectronics community to articulate what a 'molecular electronics' might be.

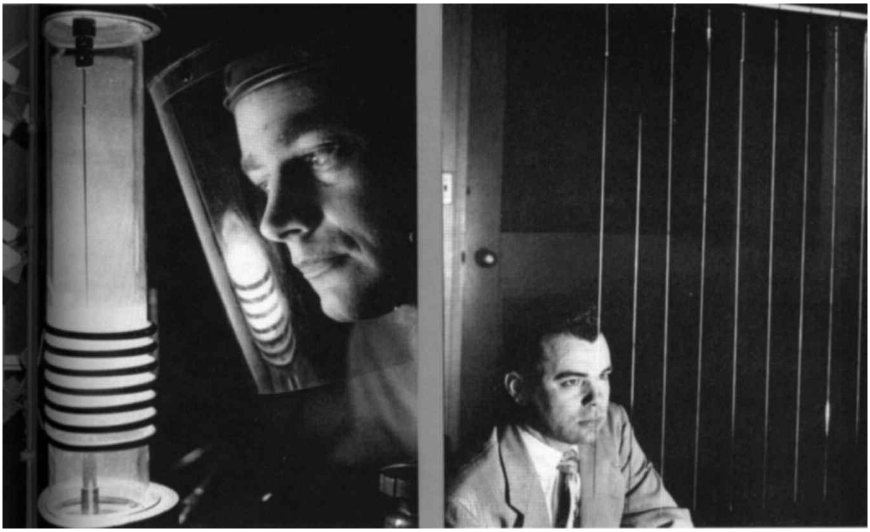
Filling in the Black Box

Several months after the conference in early 1959, Westinghouse submitted a formal proposal to the Aeronautical System Center of the Air Mobility Command. This led in April to a US$2 million development contract to pursue molecular electronics. Thus, the joint Air Force–Westinghouse molecular electronics program began with the Air Force in search of a next-generation technology of electronic miniaturization and integration, one that would go far beyond the current frontier of integrated circuits. By attempting to fill in the black box of molecular electronics, Westinghouse aimed both to fulfill the 'needs of the Air Force' and its own desire to leapfrog into the forefront of solid-state electronics.[12]

The proposal, entitled 'Molecular Electronics – Dendritic Approach', emphasized the radical nature of the program: this was a 'revolutionary new method of electronic system fabrication and design'. The authors of the proposal made it clear that 'molecular electronics' was 'radically different from all concepts of microminiaturization involving standardized assembly with miniature components' (a reference to the Army micro-module program). Building on the scientific foundation of quantum mechanics, the goal of molecular electronics was to make devices utilizing a 'topology of material domains' achieved through a 'controlled inhomogeneity of the material, whereby certain functions are performed by designing a monolithic block of material (in some cases one single crystal) with the proper arrangement of the required domains'.[13]

The centerpiece of molecular electronics, as was indicated in the subtitle of the proposal, was a new fabrication technique called the 'dendritic

FIGURE 4
Dendritic germanium at Westinghouse (*Westinghouse Engineer* [1959]: 113). Reprinted with permission from the Westinghouse Electric Corporation.



approach'. The advantages of this approach were numerous. The *conventional* method of semiconductor preparation, known as the Czochralski method, was – and still is – to 'pull' a cylinder-shaped single crystal from a pool of molten semiconductor. Once a single crystal semiconductor ingot formed, it was cut, perpendicular to the direction it was pulled, using a diamond-tipped saw. This resulted in a thin semiconductor wafer, which could be polished, processed, and diced into the familiar small rectangular pellets. The 'dendritic approach' proposed to eliminate this 'usual time-consuming and expensive processing of germanium' (*Westinghouse Engineer*, 1959).

This was process innovation at its best, if it was realizable. With this approach, Westinghouse engineers even claimed that it would be possible to fabricate a 'three-zone dendritic crystal', which could function as a full-blown transistor without further processing. The idea was to 'pull a pnp or npn crystal strip directly from the melt', or, whenever necessary, to achieve the desired characteristics by 'diffusing suitable impurities upon the newly formed dendrite surfaces'. Either way, 'the purpose of both approaches is the production of a multi-zone semiconductor ribbon essentially in a single operation'.[14] By 1960, Herwald reported that it was possible to grow a germanium dendrite ribbon at a rate of '6 to 12 inches per minute' in a continuous and automatic process (Herwald, 1960).

Westinghouse's effort to fill in the black box of molecular electronics was met with considerable fanfare in the early 1960s, especially from its patrons in the Air Force. In January 1960, the ARDC and Westinghouse held a joint status meeting on 'Molecular Electronics' as the project's public debut. The preliminary results were satisfactory for the patron. At the meeting, Westinghouse delivered eight 'functional electronic blocks'.[15] After the meeting, Colonel W.S. Heavner, Chief of the Air Force Electronics Technology Laboratory, was confident enough to gravely warn the electronics industry of an impending shift in the technological paradigm: 'Component parts will be replaced by molecular electronics. And unless makers of resistors, capacitors and other parts start working on solid circuits, they may be in for serious trouble' (*Electronic Design*, 1960).

Morale was high at Westinghouse, too. Herwald, now the Vice President of Research at Westinghouse, was grasping every opportunity to put molecular electronics on the map. He managed to place publicity pieces in numerous trade and scientific magazines (Herwald & Angello, 1960). Herwald told a reporter that '[t]he concept of molecular electronics, in effect, "leapfrogs" over current attempts to make electronic systems smaller and more reliable'. As could be expected, Herwald put dendritic germanium front and center, arguing that 'it would even be possible to automatically and continuously produce actual electronic equipment, such as radio receivers and amplifiers, starting from a pool of molten semiconductor materials' (*Electronic Industries*, 1960).

The Air Force was satisfied enough with the interim progress of the molecular electronics program to renew Westinghouse's contract for two more years. From 1959 to 1962, Air Force provided Westinghouse with more than US$7 million for the development of molecular electronics. Everything seemed to go as planned. However, retreat from the original radical rhetoric was brewing under the surface.

Domestication of Radical Rhetoric

By mid 1961, the Air Force raised concerns about the promised progress of the molecular electronics program. The major criticism regarded the 'slowness in proceeding with manufacturing development'.[16] Although Westinghouse succeeded in making eight prototypes for the 1960 status meeting, manufacturing these in large quantities was quite another story. The relationship between Westinghouse and the Air Force began to sour as Westinghouse experienced difficulties in turning custom-made prototypes into mass-produced devices. By late 1961, Westinghouse had still failed to deliver the products, fueling growing suspicion within the Air Force whether Westinghouse 'ha[d] a coherent, vibrant organization working on molecular electronics' that could bring the contract to the promised conclusion.[17]

Complaints went both ways. From Westinghouse's perspective, the Air Force did not pick up its side of the stick by 'allow[ing] the various phases of our contract to sort of dissipate in the mist'.[18] For example, the Air Force's demand for sample products 'without financial remuneration but with the hope that this would get for us this large manufacturing methods contract' forced Westinghouse to 'spread thin over a large number of functional blocks'. This impaired Westinghouse's ability to aggressively bring laboratory prototypes into manufacturing. Compared with other military contractors who 'have military funding for specific items', Westinghouse had to struggle with a large number of functional blocks 'essentially with [their] own funds'.[19]

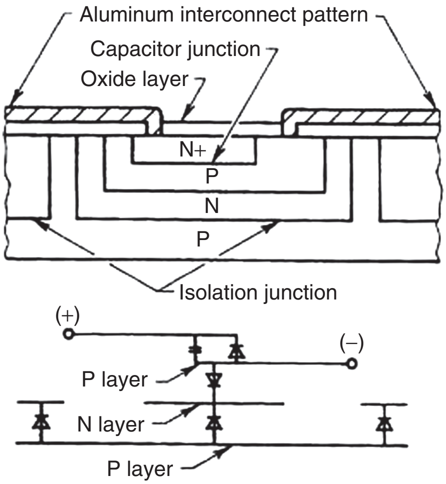
As it turned out, the centerpiece of molecular electronics that won Westinghouse the Air Force contract became the major bottleneck. In April 1962, Gerath wrote: 'A review of molecular engineering contracts [reveals] that our progress which is slower than anticipated is partially related to materials processing and techniques. It is realized that the development of proven processes is a time consuming operation'.[20] This is perhaps not surprising given the relative lack of experience at Westinghouse, and the inherent difficulties of controlling material properties at a molecular level. It was at this juncture that radical rhetoric surrendered to a more incremental approach.

As Westinghouse struggled with manufacturing issues, Air Force enthusiasm toward molecular electronics began to subside. As early as May 1961, a trade publication reported that the 'USAF [was] hedging molectronics bets' by using integrated circuits and discrete components as an 'interim step' toward full 'molecularization' of electronic equipment.[21] When the central technique that defined the identity of molecular electronics proved unfeasible as a manufacturing process, the term rapidly lost its appeal not only for its patrons in the Air Force but also for Westinghouse engineers. In 1962, a new plant was established in the suburbs of Baltimore in Elkridge, MD, consolidating 'molecular electronic activities that had been carried on in laboratories in several other locations'. However, according to a Westinghouse in-house magazine, the process that was implemented was an 'epitaxial diffused planar process', which Fairchild and Western Electric had developed in 1960. Perhaps more significantly for our purposes, Westinghouse began phasing out of the term 'molecular electronics' by, for instance, calling their operation 'the new art of molecular electronics, also called integrated circuits'.[22]

In 1963, when the editor of *The Tool and Manufacturing Engineer* visited the Westinghouse Elkridge plant, the first manufacturing step was to 'lap and polish both sides of the silicon wafers', which clearly indicates that Westinghouse had abandoned the dendritic approach to preparing semiconductor materials. Although the final products were called 'molecular circuits', the structure and manufacturing process were very similar to the integrated circuits made at Fairchild and Texas Instruments (see Fig. 5, which bears a striking resemblance to contemporary patent illustrations for planar integrated circuits). It was no surprise that the Air Force 'hedged' its bet and went for the integrated circuits made by these firms that offered their products at a lower price.

By the mid 1960s, the term 'molecular electronics' had largely disappeared from the technical literature. Westinghouse's semiconductor operation continued under the rubric of 'molecular electronics' until the mid 1960s. However, this only indicates the difficulty of ousting a term once it had made its way onto the organizational chart of a large bureaucracy. At Westinghouse and elsewhere, silicon integrated circuitry became the standard terminology – and practice – for the microelectronics industry. To be sure, Westinghouse made outstanding contributions in defense and especially space electronics

FIGURE 5
From Black (1963: 79–84). Copyright notice 1963. Copyright by Society of Manufacturing Engineers. All rights retained. This image appears with permission from *Manufacturing Engineering*, the official publication of the Society of Manufacturing Engineers (SME).



well into the 1970s.[23] And through its foray into molecular electronics, Westinghouse obtained valuable knowledge and skills in areas such as clean room techniques and scanning electron microscopy.[24] But the company failed to achieve equal success in the commercial market. By the end of the decade, the center of electronics had already migrated across the country to what is now known as Silicon Valley.

Silicon Scaling and the Rebirth of Molecular Electronics

The downfall of the first venture into molecular electronics, then, was that it was sold both as a transition to a wholly new kind of electronics platform (that is, it would fill the next stage in the sequence of vacuum tube→discrete transistor→integrated circuit→?) and as a manufacturable product that could be mass-produced in great enough quantities and at low enough cost to compete with silicon ICs in the Air Force's short time horizon. When faced with the contractual (and competitive) obligation to deliver a product, Westinghouse chose the route of known manufacturability (the silicon IC) rather than that of an untried, possibly unmanufacturable, new platform.

Yet the desire for a new electronics platform beyond silicon ICs never wholly disappeared. Many proposals for radical new platforms have appeared over the years: spintronics, DNA computing, Josephson computing, quantum computing, cellular automata, and so on.[25] Partly by chance and partly because of a genealogical link to Westinghouse, 'molecular electronics' (in a variety of related guises) has continually reappeared since the mid 1970s as a proposed post-silicon platform. The attraction – but also the death knell – of all these revolutionary platforms lies in the tremendous profit and influence that the industry built around silicon. Any person/organization/nation that could develop the next microelectronics platform after silicon ICs could potentially control an industry with a quarter of a trillion US dollars in sales.[26]

Before a radical new platform could supplant silicon, however, it would have to be as manufacturable as silicon ICs – someone would have to make mass quantities of chips based on the new platform that would be faster and cheaper than silicon or provide some other advantage. One strategy for a revolutionary platform proponent would be to develop their technology for some niche application. This would provide the proponent with resources and time to work out the new platform until it is manufacturable enough to compete with silicon. A few exotic forms of microelectronics have, in fact, survived this way.[27] Gallium arsenide integrated circuits, for instance, were widely tipped to displace silicon in the 1980s. They have yet to do so, but today they are commonly used in cellular telephones – a sizeable enough market that manufacturing knowledge about gallium arsenide can continue to grow, with the potential to eventually displace silicon.

Now we can ask: Since the 1970s, what kinds of people and organizations have been attracted to radical platforms such as molecular electronics? One possibility would be an academic lab group or a small start-up company.[28] In fact, academics did become interested in molecular electronics (as well as other post-silicon platforms) in the 1980s, and a few start-ups emerged in the 1990s. These organizations proved adept at making *a* molecular device (or, usually, some part thereof); but so far no such group has acquired the manufacturing know-how *on its own* to make many molecular devices integrated as a single chip, much less to make thousands or millions of such chips.

A second possibility would be the firms in Silicon Valley or their closest international competitors. Since the early 1970s, though, the innovation regime of such firms has discouraged a leap away from silicon to a radical new platform. Among such firms, chip production has been a multi-organizational affair. A piece of silicon goes through well over a hundred process steps on the way to becoming a chip. Each process step involves at least one large, multi-million dollar machine (made to order by one or several equipment suppliers) and numerous smaller tools (photoresists, polishing pads, slurries, and so on) manufactured by an array of materials companies (Hatch & Mowery, 1998). All these materials and pieces of equipment are very precisely engineered and highly dependent on each other; any small change to a process step ramifies through many other steps and therefore affects the practices of a large number of organizations.

For instance, moving from aluminum to copper interconnects (the small 'wires' between transistors in a chip) was predicted for 20 years to enable 10–20% improvement in performance, yet it took a decade of intense negotiation and engineering through the 1990s to make the switch because even this seemingly small modification of one process step had implications for a further 25 other steps.[29] Such changes require a great amount of coordination, either through standards-setting by a dominant firm (for example Intel) or road-mapping through a trade association or quango. These bodies can manage incremental improvements to process steps, but they are extraordinarily averse to large discontinuities in platform that would require rebuilding their infrastructure from scratch.

If not universities, start-ups, or Silicon Valley, then who? The history of molecular electronics since the 1950s points to two types of organization that have both been attracted to radical changes in the microelectronics platform *and* believed they had the wherewithal to make the new platform manufacturable. Those organizations are large, vertically integrated firms and national security research bureaucracies. The latter usually do not have their own manufacturing capacity, but they have unique requirements (for example cryptographic supercomputing or radiation hardening) that make them wary of Silicon Valley's consumer-oriented innovation paradigm; and if their requirements are urgent enough they have the funding to experiment with new platforms.

As for vertically integrated monopolies or near-monopolies (for example Westinghouse, IBM, or AT&T), until the 1990s these firms had large research arms, substantial manufacturing capacity, and exotic requirements that, again, made them leery of Silicon Valley's way of doing things. For instance, as Rebecca Henderson (1995) has pointed out, where Silicon Valley firms have been extremely reluctant to discard optical lithography in patterning transistors onto their chips (and have extended the life of that technology more than 20 years beyond expectations), firms such as IBM and AT&T were the first to develop (and lobby for the widespread adoption of) exotic lithographies such as X-ray, electron beam, and extreme ultraviolet.[30]

Such skepticism extended not just to one piece of the silicon integrated circuit platform (optical lithography) but to the platform as a whole. Indeed, these firms were among the first to call attention to the eventual demise of incremental improvements to silicon ICs. In the early 1970s, Robert Noyce, Carver Mead (1972; Mead & Rem, 1979), Gordon Moore (1975), and others associated with Intel were articulating an open-ended 'Moore's Law' of miniaturization. At exactly the same time, Robert Keyes (1972, 1975, 1977), IBM's microelectronics guru, was announcing that silicon ICs would cease to get any smaller within just a few years.[31] While manufacturers such as Intel – always tightly networked with and mutually dependent on an array of suppliers – saw no presumptive anomaly in silicon, vertically integrated firms such as IBM thought otherwise and believed they could reinvent their microelectronics manufacturing infrastructure from scratch.

Of course, IBM continued to plow money and people into improving silicon technology; but its research arm was easily enticed into adventurous explorations of post-silicon technologies. For instance, from 1969 to 1983 IBM spent more than US$100 million (in 1970s dollars) to develop a supercomputer based on superconducting materials such as niobium or lead rather than traditional semiconductors such as silicon.[32] The company tried to develop all aspects of this computer – from the exotic chips to the refrigerators needed to keep them cool to mundane equipment such as cables and printers. And even though IBM researchers proved adept at making small quantities of superconducting logic elements, by the time they could even assess the manufacturing obstacles to making the millions of such elements needed for a supercomputer, silicon's slow, steady improvement in cost and speed had erased much of the superconducting chip's hypothetical advantage.

The rebirth of molecular electronics was enabled by IBM's ambivalent pursuit of *both* better silicon technology and a post-silicon microelectronics platform. Even as it explored alternatives, IBM was committed to developing the advanced materials needed to make smaller, faster, cheaper silicon integrated circuits. In the early 1970s, one piece of this effort was Bruce Scott's group at IBM's Yorktown Heights lab that was trying to develop new lithographic resists used in patterning of silicon. Resists are lacquer-like organic chemicals that, like photographic film, change their chemical character when exposed to light, x-rays, electron beams or other lithographic beams; this means that when they are exposed to the image of a pattern of transistors, an acid can then etch away the areas that have been exposed to the beam, leaving behind a solid negative of the transistor pattern. Further etches can then be used to transfer that pattern directly into the silicon.

Scott was interested in seeing whether a class of materials known as organic conductors, which had been discovered in the late 1960s, might be used as lithographic resists. Ordinarily, organic compounds are very poor conductors of electricity, but certain charge-transfer salts such as tetrathiafulvalene-tetracyanoquinodimethane (TTF-TCNQ) had been found to be reasonably good conductors. These are compounds made up of alternating layers of an electron donor molecule (for example TTF) and an electron acceptor (for example TCNQ). By moving along an alternating stack of donors and acceptors, an electron is able to pass through the material encountering relatively little resistance.

Scott believed that if a charge-transfer salt could be designed which gained or lost its ability to conduct electrons after it had been exposed to a lithographic beam, then it would make an excellent resist. He therefore tasked the group's synthetic chemist, Ari Aviram, with making a series of charge-transfer salts for the other members of the group (largely physicists) to characterize. But Aviram began to formulate a more far-reaching vision for charge-transfer salts.[33] As a young father with a growing family to feed, Aviram could see three obstacles to career advancement that such a vision might overcome. First, as a synthetic chemist with a master's degree he felt at a disadvantage among the physics- and PhD-chauvinists who were Yorktown's cultural and managerial elite.[34] Second, his current work was largely auxiliary: he made samples to order for other people to build theories and experiments around. Finally, charge-transfer salts were seen as relevant to somewhat low-status applications at Yorktown. At best, they could be used in Scott's photoresists, but more likely they would end up in parts for IBM's line of photocopiers.

By 1970, therefore, Aviram had decided to get his PhD, and for his dissertation research he planned to develop a theory for using charge-transfer salts not for photocopiers but for the kind of radical new micro-electronics platform that was bound to grab attention within IBM Research. So, that autumn, he walked into the office of Mark Ratner, an assistant professor in theoretical chemistry at New York University, and persuaded Ratner to supervise his dissertation on electron propagation in organic molecules.[35] Ratner – 3 years Aviram's junior – agreed to this unusual and forward request partly because Aviram had convinced Scott that IBM should pay his tuition as well as bring Ratner in to consult on organic conductor research.

He also agreed because he could see an interesting theoretical question in Aviram's proposal. Aviram, in preparing bulk quantities of charge-transfer salts, had begun thinking about the properties of a single molecule of a compound such as TTF-TCNQ. This molecule would have a functional unit (TTF) rich in electrons and another unit (TCNQ) poor in electrons. This made the molecule similar to a traditional semiconductor microelectronic component called a diode, in which an electron-poor region of semiconductor is electrically adjacent to an electron-rich region. When a voltage is placed across the diode such that electrons run from the electron-rich region to the electron-poor one, a substantial current is created; when the voltage is reversed, electrons pass poorly through the electron-poor region and little current is created. The theoretical issue for Ratner was whether a single organic molecule could be designed that would have a similar current-versus-voltage graph to that of a semiconductor diode.

The pragmatic issue for Aviram was to take his and Ratner's theory and promote it to his managers as the basis for a new 'molecular' electronics. Abstractly, the step from a molecular diode to a molecular transistor is small. A transistor (especially the bipolar junction transistors on which IBM's machines then depended) is basically two diodes back-to-back – that is, a sandwich of electron rich–poor–rich regions (or poor–rich–poor). The main difference is that the middle region of this sandwich (the 'gate') is used to control current flow across the whole transistor (by the addition or subtraction of a very small voltage on the gate).[36]

Aviram believed that, with both his PhD and a theory of molecular diodes in hand, IBM would allow him to build a program to take the next steps: design and synthesize a molecular transistor, build small devices from these molecules, and eventually wire together millions of these transistors into a full-fledged microprocessor. He framed this program as a radical leap in miniaturization not just beyond Silicon Valley firms, but right to the conceivable limits of microelectronics.

> Thus far, the components which carry out the processing of electrical energy have moved through three 'generations': (1) the vacuum-tube ... (2) the transistor ... and (3) integrated circuits which at increasing levels of miniaturization combine a host of electronic devices ... on single 'chips.' [Aviram and Ratner] have suggested a drastic reduction in component size far below present-day levels of circuit fabrication. ... [T]hey have proposed

> the design of *individual molecules* which would be able to act as functioning electronic devices in circuitry.[37]

Note how this reiterates the notion of molecular electronics as the fourth (and final) generation of microelectronics that is captured in Figs 1 and 2.

Aviram and Ratner (1974) published a now-famous paper on 'Molecular Rectifiers' describing how a modified charge-transfer salt (Ratner added a small barrier between donor and acceptor) could operate in a circuit.[38] And, as Aviram had hoped, this research did spark considerable discussion within IBM. For Ratner, Scott, and Aviram's other colleagues, though, the paper was a theoretical curiosity which could not be tested experimentally, much less scaled up to a product. Aviram's charismatic vision had its moment at IBM – it was taken seriously by Scott, Philip Seiden (director of Physical Sciences at IBM) and the Yorktown semiconductor establishment, some of whom (for example Sokrates Pantelides) eventually defected from semiconductors to molecular electronics in the late 1990s. It even found its way into the mainstream media (*Time*, 1974).

Even Aviram, though, had no answer to the problem of manufacturability. At the time, he could not even synthesize the molecular rectifier he and Ratner proposed, much less put it into a functioning circuit – let alone wire together millions of such molecules! IBM was already throwing hundreds of millions of dollars at a disruptive new form of microelectronics (superconducting computing) that looked much closer to manufacturability. At the same time, it was investing billions into somewhat less disruptive improvements to silicon manufacturing (for example x-ray lithography). In that environment, a research group such as Scott's could afford to pursue the esoteric questions about electron transport that had caught Ratner's interest, but there was little basis to take Aviram's lead and move directly into molecular computing.

Thus, for a few years Aviram was allowed to develop his ideas, and to explore new materials for molecular devices such as conducting polymers (a new kind of organic conductor discovered in 1973). By the late 1970s, though, Aviram's group had dispersed – Scott into administration at IBM headquarters, others to IBM Almaden (in California). Aviram, in Ratner's words, was 'exiled' to work on printer inks, and molecular computing at IBM went into hibernation. The 'Molecular Rectifiers' paper – today seen as the founding statement of modern molecular electronics – sank virtually without trace until Aviram returned to the topic in 1988. As Ratner says, 'nobody read it, and it just laid there for years' (Wolinsky, 2004). Yet in that time, largely independent of Aviram and Ratner, a molecular electronics community – dispersed across regions and organizations and disciplines – came into being for the first time.

Forrest Carter as Transitional and Catalytic Figure

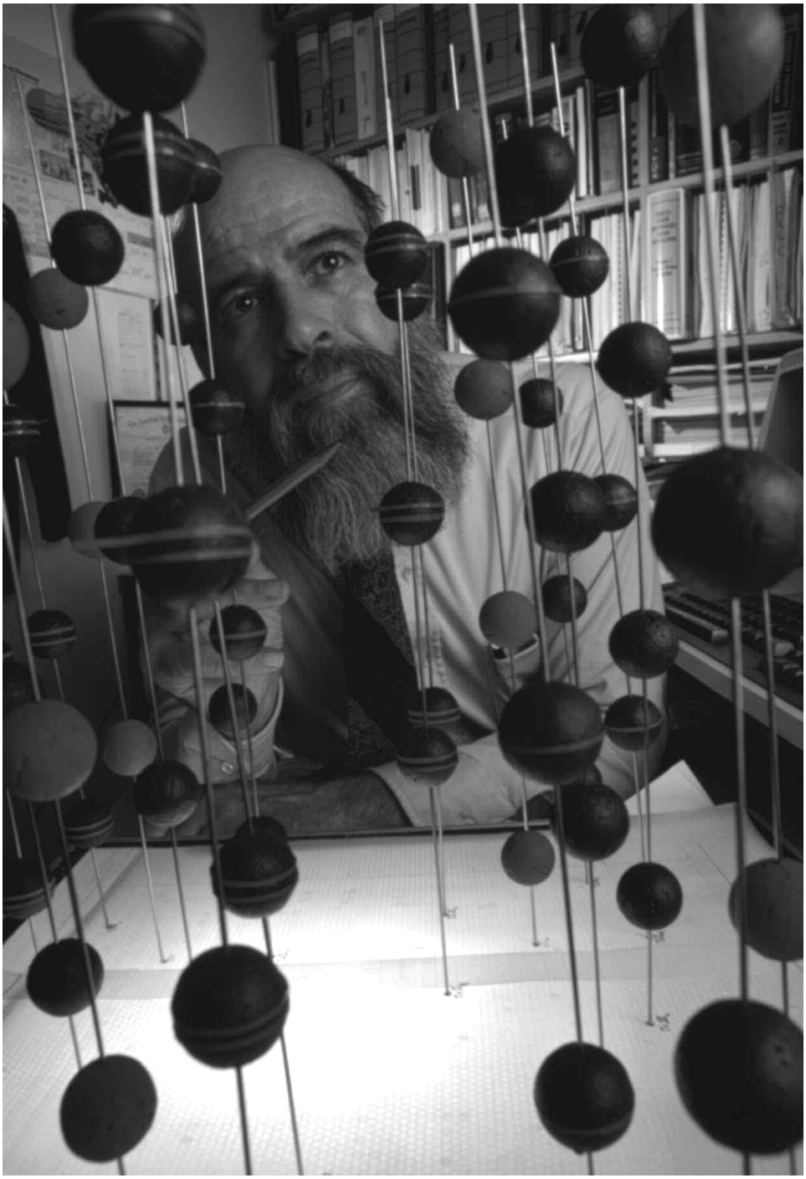
Aviram and Ratner never actually used the phrase 'molecular electronics'. However, their paper is seen as the origin point of the modern field with that name, because it gestured to the general features of what now counts as molecular electronics: the substitution of more-or-less discrete single organic molecules for integrated silicon transistors in a microelectronic circuit. The appropriation of the Westinghouse program's name for this research area and (in large part) the mobilization of practitioners to work on it, however, fell to Forrest Carter, a chemist at the Naval Research Laboratory, in the late 1970s and early 1980s. Aviram and Ratner's 1974 paper was important in spurring Carter – he was one of the few to cite it before 1988, and he included both men in his early community-building. Yet Carter's program developed in parallel with, and was initially much more successful than, Aviram's. In the rest of this paper we follow the molecular electronics community that first nucleated around Carter and then, in the early 1990s, re-formed (with Aviram's help) as a subsidiary of, rather than a competitor to, silicon microelectronics.

Critically, Carter's interest in molecular computing grew out of an institutional and disciplinary environment similar to Aviram's, as well as a personal curiosity dating to his graduate training at Caltech. There, he had studied organometallic chemistry under Howard Lucas, graduating in 1956 (Carter, 1956). His Caltech mentors also included Linus Pauling and Richard Feynman; indeed, by Carter's account, he attended parties at Feynman's house and played bongos and talked science with the older man. It is interesting to note that Carter knew of and was influenced by Feynman's (1960) famous 'Room at the Bottom' speech – much more so than most other early nanotechnologists.[39]

Moreover, Carter incorporated elements of the Feynman persona into his own presentation of self, developing an expansive, charismatic style that helped him promote bold visions and gather protégés, but which also led to institutional conflict. Like Feynman, he had a taste for exotic hobbies (hot rods, motorcycles, fencing, platform diving, salsa dancing); and, like Feynman, he became known for extraordinary parties, risqué banter, and a coterie of young acolytes. Carter's striking appearance, rumbling voice, and colorful banter (cited to this day by skeptics and believers alike) personalized molecular electronics as neither Aviram nor the Westinghouse engineers had before him.[40]

From Caltech, Carter moved to Westinghouse in 1957. While not directly involved in the molecular electronics program, he was aware of the project and even worked on semiconductor materials in collaboration with the Army Signal Corps (Ryan et al., 1962).[41] In 1964, he moved to the Naval Research Laboratory (NRL), where he became one of the laboratory's specialists in x-ray photoelectron spectroscopy (a very recently developed surface analysis technique). As Bruce Hevly (1987) has shown, the postwar NRL portrayed itself as a 'university of applied research' where scientists were expected to contribute broadly to Navy-relevant questions, but were also free (and expected) to pursue basic research questions of interest to academic colleagues. Individual scientists often worked on several projects at once, covering a spectrum of time horizons over which their research would *evolve into* relevance for the Navy. In this environment, Carter man-

FIGURE 6
Forrest Carter explaining molecular electronics. Photograph by Charles O'Rear, courtesy of the National Geographical Society.



aged to balance projects of immediate interest to the Navy with work of indirect or long-term relevance – using the former to build approval for the latter.

Carter saw the new organic conductors as materials that could be parlayed *both* for medium-term research directly relevant to Navy applications and for more speculative explorations of a post-silicon microelectronics platform. Institutionally, he was well placed to take advantage of this view. By the mid 1970s, the hot area of organic conductor research was conducting polymers, and many researchers (including Aviram) had shifted from charge-transfer salts to polymers such as polyacetylene and polysulfur nitride.[42] As it happened, the funding that enabled the initial attention-getting research on conducting polymers had come from Kenneth Wynne, a grant officer at the Office of Naval Research; through the 1970s, Wynne and the Navy were instrumental in mobilizing and coordinating practitioners in this field.[43]

In 1976, Wynne established a Navy Committee on Advanced Polymers to explore naval applications of these materials. Conducting polymers held enormous promise for the Navy – plastic electronics could make sensors and communication equipment cheaper, lighter, more durable, and more resistant to corrosion (always a problem at sea and a major area at the NRL), and might enable far-out applications such as advanced batteries for submarines or super-efficient solar cells for satellites. In conjunction with Wynne's committee, Fred Saalfeld, superintendent of the Chemistry Division at the NRL, organized an Electroactive Polymers program and brought the leaders in the field (most of them Wynne's grantees) in to consult.

Conducting polymer samples are usually prepared as thin films, meaning that surface effects are important. And since Forrest Carter 'owned' the only x-ray photoelectron/Auger electron spectrometers (two key surface analytic tools) in the NRL's Surface Chemistry branch, he could contribute key data to the Electroactive Polymers program. While some researchers resented being dragooned into the project by Saalfeld, Carter took to it with relish.[44] This can be seen particularly in the reports on the program's annual symposia for 1978 and 1979 (Lockhart, 1979; Fox, 1980), where Carter provided the program's broad theoretical outlines (Carter, 1980a), many of its empirical findings (Brant et al., 1980), and – critically – the outlines of a long-range vision linking conducting polymers to molecular computing (Carter, 1979, 1980b). In a high-profile program supported by powerful managers, Carter got results and, therefore, accrued significant latitude to pursue longer term, less directly Navy-relevant research. By 1978, Carter was spending this social capital on a futuristic vision that extended Aviram and Ratner's work.

Carter took Aviram and Ratner's picture of discrete organic molecules substituting one-to-one for silicon diodes and transistors and began filling in the hypothetical details. For instance, their rectifier had simply been a free-floating molecule; Carter now outlined ways that such components could be anchored to a solid substrate. Their rectifier had not been connected to anything; now Carter began sketching a theory for 'molecular wires' to bridge components. At the same time, the raw materials for Carter's speculations were the new conducting polymers, such that he could justify his work as merely the logical next step in NRL's Electroactive Polymers program. For instance, his molecular wires were merely unbranched chains of a conducting polymer such as polysulfur nitride. Polysulfur nitride is made from repeating units of sulfur nitride (SN); so he envisioned anchoring one end of the polymer to a substrate of silicon, then adding SN units as needed and ending the wire with a molecular component: Si–N=SN–SN–SN– ... SN–SN–SN–molecular transistor.

Organizationally as well, he represented this work as a mere extension of the Electroactive Polymers program. Thus, his first effort at building a community around his vision for molecular computing was a symposium in 1981 at (and financed by) NRL on 'Molecular Electronic Devices' (Carter, 1982). This was explicitly modeled on the Electroactive Polymers symposia and included many of the same participants (Kenneth Wynne's conducting polymer grantees), and the talks emphasized those technical problems common to both electroactive polymers and molecular computing, such as ensuring electronic connection between organic and inorganic components of a circuit and the need for an improved theory of carrier mobility in organic molecules.

At the same time, to put Carter's vision for molecular computing into practice clearly required a long leap away from the Electroactive Polymers program, and a substantial leap beyond the NRL's mission and organizational boundaries. Thus, he needed some way to justify molecular computing's relevance to the Navy. Concurrently (and unlike Aviram) he saw the need for an eye-catching rhetoric that would attract influential patrons beyond his own organization. By the early 1980s, therefore, he had found that rhetorical hook by reframing molecular computing to emphasize its implications for national security and economic competitiveness.

It's important to remember how panicky the American state, microelectronics industry, and media were about the Japanese capture of certain semiconductor markets in the late 1970s (Prestowitz, 1988). The incremental progress of Silicon Valley firms was now seen as a liability, because they relied on an innovation pathway for silicon that supposedly uncreative Japanese firms could (so it was argued) simply copy without introducing real high-tech breakthroughs. Carter tapped into these fears by arguing that only a truly radical change, based on advances in fields well outside silicon microelectronics, could deliver a circuit so small, so fast, and so technologically disruptive (and therefore not easily copied) that it would vault American firms back into the lead and put foreign competitors at a permanent disadvantage. In so doing, Carter explicitly justified his molecular electronics work by pointing to the military's mandate to confront *all* threats to national security and competitiveness, including disruptive economic threats.

This was appealing rhetoric at the time, and the Navy was initially very supportive of Carter's molecular electronics. The NRL was eager to sponsor work that would (as one of Carter's bosses put it) 'lead to the discovery of important new phenomena and exciting new technologies' (Jarvis, 1980) – that is, research that contributed both to national security objectives and to basic research. Like the Air Force and Westinghouse (and, to a lesser extent, IBM), the Navy was at the periphery of the microelectronics industry, but in the face of an external threat it was open to an institutional entrepreneur like Carter convincing it that its particular expertise (electroactive polymers) could form the basis of a radical alternative to silicon. As an associate director of NRL, Albert Schindler, put it:

> We are all familiar with the revolution in computer power as exemplified by the hand-held computer. NRL recognizes that this [Molecular Electronic Devices] workshop may lead to another revolution of equal if not greater importance. While we are involved in ... the development of high speed, very large-scale [*silicon*] integrated circuitry, this workshop may point the way to a quantum jump advancement rather than just incremental improvements. (Schindler, 1982)

Finding common ground with his patrons in the language of revolution, Carter began to steer his vision of molecular computing in more speculative, radical directions. His early proposals still bear some family resemblance to traditional microelectronics and computing; even if the wires, diodes, and transistors are 'molecular', they still act like components that anyone can buy at Radio Shack. Later, though, he emphasized the possibilities of less recognizable kinds of molecular computing, particularly so-called cellular automata. For this, he envisioned depositing a periodic array of molecules, with each molecule capable of two states (zero or one) and chemically 'programmed' to change its state depending on the states of its neighboring molecules according to some prefixed set of rules. A set of inputs to this array would chemically cause various changes to the automata molecules until they would reach an end state and yield a set of outputs. Such an array, Carter believed, would not be constrained to process information the way a silicon computer does; instead, his molecular cellular automata would represent a leap to a much more human kind of processing:

> If the data input to such an array of automata is a two-dimensional array of picture elements or pixels then pattern recognition routines and pattern motion could be parallel processed. ... Such a three-dimensional array processor could reduce data in a manner comparable to the optic nerve. (Carter, 1984c)

Such an array could then 'Discern between Sedans, Trucks, Tanks ... between Ships, Boats, Canoes ... between Bombers, 707's [sic], Birds'. With promises like this, Carter found an enthusiastic constituency among generals and admirals beyond the borders of the NRL.

Growing Interest ... and Skepticism

By 1983, the radical turn in Carter's vision for molecular computing was visible in his community-building strategies as well. That year he held the second of his Molecular Electronic Devices workshops (Carter, 1987). Where the first conference had been attended mostly by conducting polymer researchers (many of whom were funded by the Navy), by the second workshop those people were replaced by an eclectic mix of synthetic chemists, biochemists, lithography specialists, and provocative speculators such as Eric Drexler. This broadening of topics and personnel is indicative of Carter's unmooring of molecular electronics from a particular material such as polysulfur nitride or TCNQ, and his construction of a big tent for all things 'molecular'.

Some of the people Carter welcomed under that tent built long-lasting experimental programs in molecular electronics. Robert Metzger at the University of Alabama, for instance, started an Organic Rectifier Project and later a Laboratory of Molecular Electronics to synthesize and test Aviram and Ratner's molecule. For Metzger and a handful of other experimentalists, it was because of 'three scientific conferences organized by the late Forrest L. Carter [that] the idea of "molecular electronics", that is, electronic devices consisting solely of molecules, gained large-scale interest' (Metzger, 1999). For other scientists later associated with molecular electronics – such as Mark Reed (whom we will meet later) and Mark Ratner – Carter's tent was simply too speculative and contained too many visionaries and too few experimental results:

> Forrest was wonderful, but I thought what he was saying was kind of dizzy. ... [H]e was ... really very theoretical and way out there .... [Carter's conferences] were enjoyable, and they were kind of crazy. I'd never been to NRL before so that was fun. I met a lot of good people there. Have you read any of Forrest's papers? Then you know he had some really way out ideas about these soliton switches. He's nice and I like him, but I just couldn't see it – I didn't see how the ideas can be experimentally realized.[45]

> I remember getting the invitation [to Carter's conference], something about molecular structures, or maybe using molecular electronics. I thought 'I don't know anything about that'. But he said 'no, we want you to talk about the fact that you can make these small devices'. And I thought 'sure, I can talk about that'. And I went and talked about that. I remember I met for the first time Eric Drexler there, he was giving a poster. I remember having an argument with him at the time and walking away shaking my head. ... I just got the impression that it's all theory here, no experiment, so I'm not going to take it seriously.[46]

For Carter, it was among the visionaries in that tent – people like Drexler, Stuart Hameroff, and Stephen Wolfram – that he could discuss cellular automata, molecular assemblers, and biological computing and obtain material for the more radical parts of his vision.[47] That vision was quickly attracting widespread attention. Carter was publishing more papers in mainstream physics and surface science journals in the mid 1980s than at any time in his career; but he was also publishing in more off-beat edited volumes that some of his peers and supervisors were beginning to contest.[48] In May 1983, *Chemical & Engineering News* (*C&EN*), a widely read glossy weekly for chemists and chemical engineers, published an article on Carter. Though the tone was generally positive, the article contained negative cues about Carter's place in the field that later become overt:

> A small but zealous group of chemists is convinced that the electronics and computer industry could benefit enormously by being brought down to the molecular level. ... Such talk is not mere science fiction. Some of it has already moved into the realm of theory. ... None of these ideas can be

> tested fully without developing 'whole new chemical synthetic techniques', Carter asserts. ... 'Every person could have his [or her] own world library, or each car its map of the world', he says, cautioning: 'There's an enormous job ahead. Yet, it's so exciting and vital that I'm sticking my neck way out'. (Fox, 1983; gender-inclusive correction in original)

Six months later, *C&EN*'s ambivalence slid toward disapproval with 'Molecular Computers Are Far from Realization'. The article describes one of Carter's many missionary trips to conferences:

> [H]ype seems inevitable for so-called molecular computers. There are claims for the miraculous things they will be able to do compared to present computers, and suggestions that if only research funding were adequate – that is, a lot higher than at present – they would be closer to realization than they are. ... [A]t many levels of organization, such computers currently are barely a step removed from science fiction. ... But such reservations tend to get muted when a group of scientists get excited about new ideas. ... The National Science Foundation funded the conference to find out whether research into chemically based computers is a promising field to support. The straightforward answer to that question, even the enthusiastic conference participants admitted, is: Not really. But that simple answer was stated in muted terms, drowned in a succession of 'gee whiz' tales. (Baum, 1983)[49]

The article highlights Carter as the most egregious of the overly 'enthusiastic conference participants'. A month later, *C&EN* published a letter by Edwin Chandross (1983), a Bell Labs photoresist chemist, excoriating Carter and molecular electronics, followed by indignant replies from Carter and one of his protégés (Carter, 1984a; Siatkowski, 1984).

The exchange in *C&EN* spotlighted resistance to Carter's vision outside NRL. Moreover, changes in NRL leadership triggered a review of projects and greater oversight over Carter's work. NRL, once willing to indulge Carter's voyage into the future, now demanded that he 'include experimental results with his talks rather than simply present hypotheses' (that is, make accountable, interim progress toward the goals he had outlined).[50] For Carter, this was an impossible pill to swallow. Though still popular within the lab (especially among younger researchers), Carter became increasingly isolated from his peers.

Thus, Carter fell into that familiar category of scientist for whom his peers' evaluations of his science was inseparable from their interpretations of his character and mental state.[51] To his critics, Forrest Carter avoided producing experimental results because he was a second-rate chemist; to his supporters, he was a community-builder whose work was more important for national security than the research the Navy demanded. To critics, he was a wild speculator whose misunderstandings of quantum chemistry would bring the Navy into disrepute; to supporters, a charismatic genius who deliberately advanced unfashionable ideas to provoke discussion. Carter's supporters insist he was so brilliant that his arguments went over his critics' heads; yet those befuddled by his arguments include sophisticated theorists such as

Mark Ratner. That is, even if Carter were correct, his claims were so idiosyncratic as to amount to a private language.

Finally, to critics, Carter's molecular electronics was long on vision, but short on experiment; supporters, interestingly, agreed, but insisted that in a new field it was only realistic to expect vision to outweigh empirical results, and that Carter was desperate to bring in experimentalists. Unfortunately, these questions received a curtailed debate, as Carter was diagnosed in 1985 with a brain tumor. In his final two years Carter was unable to take on new postdocs and therefore unable to train a new generation of promoters for molecular electronics; moreover, his illness made it even more difficult to articulate a coherent vision for molecular electronics (which, critics insisted, had been incoherent all along). Despite this, Carter and two younger colleagues, Hank Wohltjen (a veteran of the IBM superconducting supercomputing project) and Ron Siatkowski, assembled a third Molecular Electronic Devices conference (Carter et al., 1988), largely financed on their own and without support of the NRL. Held in October 1986, this was Carter's last hurrah – he died a few months later.

The 'New' New Molecular Electronics

Even before Carter's death, Ari Aviram had reentered the field, publishing new work on the theory and design of molecular logic (Aviram, 1988) and putting together a team to study the electrical characteristics of single molecules with a scanning tunneling microscope (Aviram et al., 1988). In 1989, he started a series of conferences on 'Molecular-Scale Electronics' sponsored by the Engineering Foundation (Aviram, 1989). These new activities attracted much wider attention than his 1970s work. In some ways, Aviram was now following a path created by Carter. After all, neither his program nor his social position differed considerably from Carter's. Both were senior scientists in large, mission-oriented research organizations, attempting to build a constituency for molecular electronics by hosting conferences and instigating experimentalists to realize their proposals. To be sure, high-level current and former IBMers such as Alec Broers and Bob Keyes contributed to Aviram's early conferences – much as NRL managers had participated enthusiastically in Carter's first conference. Yet these senior researchers were sometimes openly critical of molecular electronics – Keyes, especially, gave a very skeptical speech at Aviram's second conference in 1991 (Keyes, 1991). Institutionally, Aviram's position was as precarious as Carter's:

> He was a bit of a lone researcher on this topic at IBM. Even when IBM started getting more and more into nanoelectronics, Ari was pretty much running solo. In some ways he was a man way ahead of his time, and underappreciated at that moment.[52]

The criticisms leveled by skeptics were little changed from Carter's time – How would single molecules be addressed? How would the organic and inorganic components of a circuit be connected? How would manufacture of molecular circuits ever compete with the yield and low cost of silicon integrated circuits?

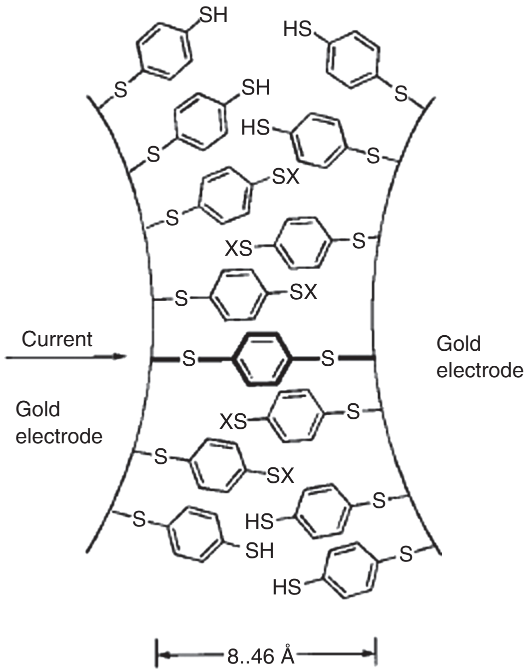
Yet the situation was fundamentally changed from Carter's day, not least because of Carter's legacy. The kernel of a molecular electronics community had formed; people such as Felix Hong and Bob Metzger, who enthusiastically attended Carter's workshops, now enthusiastically attended Aviram's. Internationally, a few other nodes of molecular electronics emerged in the 1980s such as Robert Munn's efforts at the University of Manchester and Robert Birge's at Syracuse University.[53] In a few cases, experimentalists (for example Metzger) who began work in the early 1980s were, a decade later, producing credible results that supported Aviram's program – whereas the little experimental work underlying Carter's vision was too immature to be published in mainstream journals while he was alive.[54]

Moreover, several experimental technologies initially external to molecular electronics (especially the scanning tunneling microscope) had, by the late 1980s, matured to where they could make certain aspects of molecular electronics realizable. Mark Ratner, for one, imputes his reentry to the field to his realization that STM offered access to molecules that was unthinkable in 1974.[55] Within IBM Research, management was strongly encouraging the growth of STM during this period. By showing that STM could generate critical data for molecular electronics, Aviram was able to recruit a growing cadre of IBM's STMers to take up molecular electronics questions, some of whom (Jim Gimzewski, Don Eigler, Phaedon Avouris, Christian Joachim) have become leading figures in today's molecular electronics.

Another research direction instigated by Aviram was the convergence of silicon microelectronics and molecular electronics. Emblematic of this convergence was a collaboration between Mark Reed of Yale and James Tour of the University of South Carolina that came out of Aviram's second molecular electronics conference in the Virgin Islands in 1991. The Virgin Islands conference was attended by roughly equal numbers of STMers, silicon microfabrication specialists, and synthetic organic chemists. For the most part, the last two groups found little room for communication – the fabrication specialists and device physicists talked about small structures built from silicon, while the chemists talked about molecules made in test tubes. One exception was a device physicist, Mark Reed, who had attended Forrest Carter's last workshop and come away viewing molecular electronics as a fuzzy, speculative field with no experimental basis; thus, his motivation for attending Aviram's conference was primarily the nearby scuba-diving opportunities. But one talk by a synthetic chemist, Jim Tour, described some exciting experimental results in a language Reed could understand; and so, after the meeting, he followed up to propose a collaboration.[56]

Tour, winner of an ONR Young Investigator award managed by Ken Wynne (the conducting polymer enthusiast who kicked off the Navy's involvement in the area), had, by 1991, recently gained notice for synthesizing Aviram's molecular rectifier. Reed had recently taken a professorship at Yale,

FIGURE 7
From Reed et al. (1997). Reprinted with permission from the AAAS.



following 8 years at Texas Instruments, where he had invented the class of semiconductor nanocrystals known as quantum dots. Reed brought to the collaboration with Tour both the fire of an academic looking for a new project and the contacts within military funding agencies of a senior scientist at a major defense contractor.

Thus, Reed suggested he and Tour send a white paper, entitled ‘Spontaneously-Assembled Molecular Transistors and Circuit’, to the ONR. In it, they outlined a lasting division of labor – Reed, with his clean rooms, electron beam lithography writers, and so on, would fabricate very small structures out of metals or semiconductors, across which he would string molecules synthesized by Tour. Reed’s small structures would allow them to electrically isolate a single molecule through which they could pass a current; which, if Tour had designed the molecule correctly, would reveal it to mimic a wire, diode, transistor, or other component.[57] Each of them hired postdocs from the other’s discipline, and sent students on expeditions to each other’s labs, creating a ‘trading zone’ (Galison, 1996) in which device physics, microfabrication, and synthetic chemistry could coalesce as molecular electronics.

So here was a molecular electronics that was incremental enough to allow outputs (specific structures built and tested by Reed and Tour) to be delivered, but radical enough that it could anchor futuristic visions of dramatic new technologies enhancing national competitiveness. This new molecular electronics also achieved a balance between capitalizing on the enormous body of (physical and manufacturing) knowledge about silicon while at the same time still claiming to fill the void once further miniaturization of conventional silicon transistors was no longer possible.

Notably, unlike previous attempts, this new molecular electronics was never strongly associated with any single organization – since the early 1990s activity and funding has been widely dispersed among academic, government, and industrial researchers. Instead of a pipeline through one organization – as at Westinghouse, IBM, or the NRL – molecular electronics funding in the 1990s generated a multiorganizational, multidisciplinary network that grew in conjunction (rather than in competition) with the network around silicon. Some funders of molecular electronics envisioned new metrics of progress other than the near-term delivery of manufactured products (the rock on which Westinghouse and, to some extent, Aviram and Carter had foundered). Instead, progress would be charted both by certain benchmarks (a device of a certain size or a molecule of a certain functionality) but also through the migration of persons or materials or techniques from one lab to another in the network, the establishment of regular lunches or emails between group leaders, or the publication of articles jointly authored by personnel from multiple labs.[58]

The entity that brokered this new institutional arrangement for the field was the Defense Advanced Research Projects Agency (DARPA), under program director Jane Alexander. After Reed and Tour had submitted their white paper to the ONR, it somehow circulated into Alexander's hands. She then welcomed the duo as the radical, long-term outliers in DARPA's 'ULTRA' program in 'ultrafast, ultradense electronics'. In a program stacked with microfabrication specialists interested almost exclusively in silicon, Reed played the translator, vouching for his exotic collaborator; while Tour, the only synthetic chemist, outlined how a 'molecular' electronics would be more 'ultra' than ULTRA – the ultimate in small, cheap, fast. This allowed them to do a visionary, as well as an experimental, division of labor. Of the two, Tour has created by far the more public, colorful persona, appearing often in high-profile media outlets as a charismatic, provocative visionary.[59] Consequently, his laboratory research – while still drawing heavily on a silicon infrastructure – has included more radical kinds of computing such as random arrays of molecules that chemically 'learn' over time how to perform logic operations.

Reed, on the other hand, was there through the 1990s to reassure funders and industrial partners that Tour's vision was real enough to fund but no threat to silicon:

> Even though in many ways I was responsible for the generation of this new incarnation of [molecular electronics], I'm a critic. Because I don't see it going into electronic devices in the future. Yet. If I'm going to take my money today, I'm going to put it on silicon. But that doesn't mean that I don't think that there's potential. ... My job description [unlike someone at Intel] is to explore the physics, explore the potential over a lifetime. So my

> time horizon is 20, 30, 40 years. ... On top of that, there's an additional part of my job description, which is ... is there any important physics to be learned from it?[60]

By the time ULTRA morphed into ULTRA 2, the program included several more chemists, and 'molecular' problems began to occupy the funding mainstream. Finally, in 2000, DARPA spun off a 'Moletronics' program from ULTRA 2 to fund (and coordinate) a network of researchers (Tour, Reed, Paul Weiss at Penn State, Stan Williams at Hewlett-Packard, Jim Heath at UCLA, and so on) crossing academic, corporate, and government institutions. Molecular electronics could claim a certain maturity – indeed, in a 2003 paper that brought panic to DARPA and its grantees, a *Science* reporter described the field as undergoing a 'mid-life crisis' (Service, 2003).[61]

Conclusion

This paper has examined 50 years of various avatars of 'molecular electronics'. There is a continuous, if flickering, thread that runs genealogically through those avatars – Forrest Carter links the oldest, Westinghouse-Air Force version to the middle layer; Aviram, Ratner, Reed, Wynne, and others link this middle layer to the most modern version of molecular electronics. Yet there is more holding this story together than the puckish appearance and reappearance of individuals such as Carter, Aviram, and Reed, and organizations such as IBM, Westinghouse, and the US military. Molecular electronics is the product of a dynamic of inter- and intra-institutional jockeying over miniaturization running since World War II. This dynamic is not static, but it is stable enough that the same kinds of solutions to the dilemmas it produces appear again and again.

Since the 1950s, proponents of all kinds of hypothetically post-silicon microelectronics platforms have been prefacing their pitch with statements such as these:

> If one extrapolates gains in transistor size reduction into the future, then it is readily anticipated that electronic switches will be at the molecular size level ... in 20–30 years. (Carter, 1983)

> Since the time of the first room-filling computer, there has been a tremendous drive to compress the size of computing instruments. In order to bring this desire to its extreme, it was conceived that one may be able to construct single molecules that could each function as a self-contained electronic device. (Tour et al., 1991)

That is, activities in and around microelectronics have been structured by such projections backward and forward in time. Some people and organizations look back and see discrete, radical leaps from one electronics platform to the next. Molecular electronics has only been one of many technologies offering to effect the next such leap and bring the desire for radical miniaturization 'to the extreme'.

Other people and organizations look back and see a smooth curve of miniaturization with components getting steadily smaller all the time. They look forward and see the current platform – silicon integrated circuits – continuing to miniaturize with (in the words of Gordon Moore) 'no end in sight' (Moore, 2006). Yet these people know an end to silicon miniaturization must come someday; and in the meantime, they know silicon must outcompete all the radical alternatives advanced by people like Aviram or Carter.

Thus, the history of microelectronics is unintelligible if it does not include these proposed alternatives. None of them has displaced silicon – yet – but, as Daniel Holbrook has shown, each *contributed* to silicon. The Westinghouse-Air Force molecular electronics produced much useful knowledge of how to grow and dope crystals; the superconducting supercomputing efforts of the 1970s enrolled a generation of researchers into microfabrication, spinning off lithographic techniques that were re-imported to silicon; and, as of December 2005, the International Technology Roadmap for Semiconductors has officially noted that 'molecular' electronic components will need to be integrated into silicon manufacturing (by about 2015) in order for Moore's Law to continue to hold true (Markoff, 2005).

Similarly, the history of nanotechnology is incomplete if it neglects the microelectronics industry and its institutions. Many constituent communities of nanotechnology are grouped around spin-offs from silicon technology – objects such as Mark Reed's quantum dots or break junctions fabricated using the lithographic techniques originally developed to make microelectronic components. Many other nano-communities are dedicated to the remnants of failed (or not yet successful) alternatives to silicon. Molecular electronics is one of the most prominent of these. But what does being a 'nano' community offer practitioners of molecular electronics? Some, clearly, are wary of nanotechnology. For instance, one prominent name in molecular electronics, Stan Williams, runs the 'Quantum Sciences Research' group at Hewlett-Packard, named in the mid 1990s explicitly to avoid the 'nano' label.[62] Yet molecular electronics has benefited enormously from the nanotechnology movement; molecular electronic research is sponsored by several agencies under the National Nanotechnology Initiative umbrella, is carried out in academic nano centers, is published in nanotechnology journals, and is presented *as* 'nanotechnology' in commercials and press releases.

Moreover, nanotechnology offers molecular electronics a menu of other 'nano' communities with which to thicken ties. For instance, by being 'nano', molecular electricians gain access to nano institutions (such as academic nanofabrication facilities) where they can easily find collaborators who work on carbon nanotubes or fabrication of small silicon structures. This has firmed up the alliance with nanofabrication (exemplified by the Reed–Tour collaboration); and it has allowed what counts as 'molecular electronics' to expand to include carbon nanotube transistors. Moreover, nano allows the justifications for molecular electronics research to expand beyond commercial microelectronics – by, for instance, going toward biology and claiming that molecular components can function as synthetic neurons rather than transistors. Molecular electronics may never solve the daunting obstacles to manufacturability needed to become part of consumer microelectronics: can molecules be addressed, will electron flow fry the molecule, will molecular switching speeds compete with traditional silicon? But nano may offer practitioners of molecular electronics a space in which to work and proliferate during the years (perhaps the eternity) when those questions are unanswered.

Notes

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1. For an overview of the standard narrative of the development of microelectronics, see Riordan & Hoddeson (1997) and Braun & Macdonald (1977). Bassett (2001) focuses on metal oxide semiconductor technology and the firms that engaged in its development, such as Bell Labs, IBM, Fairchild, and Intel. Lécuyer (2005) tells a broader story of the Silicon Valley focusing on Fairchild, but also its predecessors in the Santa Clara Valley, such as Varian, Eitel-McCullough, and Litton Industries.
2. It should be noted that molecular electronics (ME) of the late 1950s and that of the late 1970s were very different technologies. As it will become clear in subsequent sections of this paper, the 1950s version attempted to use crystalline materials characteristics in microelectronics, whereas the 1970s version envisioned using individual molecules as electronic circuits. Our aim here is to focus on the similar rhetorical and institutional dynamics created under the rubric of molecular electronics.
3. 'The Micro-Module Concept: A New Dimension in Electronics' (prepared for the US Army Signal Corps by the Radio Corporation of America, 1958), Army Communications Electronics Life Cycle Management Command (CE-LCMC) Historical Office, Fort Monmouth, NJ, USA.
4. R.B. Reade, 'Potentialities of the Micro Module Concept' (3 June 1958), Z-424, Box 170, Acc. 2069, Hagley Museum and Library, Wilmington, DE. Emphasis in original.
5. 'Presentation, 28 August 1962 by Major General Earle F. Cook, Chief Signal Officer at Micromodule Press Conference, New York, New York', CE-LCMC Historical Office. The fate of the Army–RCA micro-module program was determined not by its own merit, but by the changing technological environment in microelectronics. By the mid 1960s, RCA quickly abandoned the micro-module and adopted the emerging IC technology. For more details, see chapter 2 in Choi (2007).
6. For a brief biography of von Hippel, see Arthur R. von Hippel, 'Materials Design and Molecular Understanding: A Scientific Biography' (April 1980) available at <www.mrs.org/s_mrs/sec.asp?CID=2198&DID=94209> (accessed 23 September 2008). Also see Frank von Hippel (2005); Wildes & Lindgren (1985: chapter 11); and Leslie (1993: 190–92).
7. J.F. Regan to George C. Sziklai, 25 August 1957, Molecular Electronics Correspondence File, Historical Electronics Museum, Linthicum, MD (hereafter referred to as HEM).
8. S.W. Herwald (Manager, Air Arm Division) to S.W. Sampson (Chief, Components & Techniques Laboratory, AFCRC), 10 March 1958, HEM.
9. Ibid. Emphasis ours.

10. J.F. Regan (Sales Engineer, Defense Products) to George C. Szklai (Assistant to Vice President, Engineering), 25 August 1957, HEM.
11. L.M. Hollingworth (Director, Electronics Research Directorate, AFCRC) to R. Knox, 17 January 1958, HEM.
12. Being primarily an electrical power company, Westinghouse was clearly behind the mainstream electronics manufacturers in the mid 1950s. In terms of semiconductor-related patents, Westinghouse held a mere 7% in 1957; a meager position compared to Bell Telephone Laboratories (34%), RCA (45%), and GE (13%). See Tilton (1977: 57).
13. 'Proposal for Molecular Electronics – Dendritic Approach' (Negotiation No. AAN-45106, 20 February 1959). Copy available in HEM.
14. 'Proposal for Molecular Electronics', 8.
15. The eight functional blocks included: (1) audio amplifier; (2) video amplifier; (3) tuned amplifier; (4) multivibrators; (5) switches; (6) analog-to-digital converters; (7) potentiometers; and (8) low-temperature infrared detectors.
16. J.K. Hodnette (Executive Vice President) to C.J. Witting (Vice President), 11 April 1961, HEM.
17. B.M. Brown (Vice President and General Manager, Baltimore Divisions) to D.W. Gunther (General Manager, Youngwood Semiconductor Department), 27 November 1961, HEM.
18. Ibid.
19. D.W. Gunther to B.M. Brown, 8 November 1961, HEM.
20. J.A. Gerath, Jr. (Project Manager, Air Arm Division) to S.M. Skinner, 4 April 1962, HEM.
21. *Aviation Week* (29 May 1961): 82–83, quoted in Ceruzzi (1998), 181.
22. 'Molecular Electronics – from Laboratory to Mass Production', *Westinghouse Engineer* (November 1963): unpaginated frontpiece.
23. Westinghouse was instrumental in developing the color TV cameras used in the Apollo programs. See Gene Strull, 'Electronic Enterprise: Stories from the History of Westinghouse Electronic Systems' (printed by the Westinghouse Electronic Systems Division, 1996).
24. 'Clean Rooms for Environmental Control', *Westinghouse Engineer* (January 1963): 32; Mackintosh & Thornhill (1964). See Holbrook (1995, 1999) for an elaboration of this point.
25. See McCray (2007) for a history of spintronics that complements this paper's perspective.
26. According to the Semiconductor Industry Association, worldwide sales in the semiconductor industry in 2007 were US$256 billion. See <www.sia-online.org/cs/industry_resources/industry_fact_sheet> (accessed 23 September 2008).
27. Indeed, even the lowly vacuum tube continues to be better understood and improved by manufacturers of high-end audio equipment. See Downes (2006)
28. Special thanks to an anonymous reviewer for prompting this discussion.
29. From a presentation by Robert Wisnieff at the SCI-CHF Innovation Day symposium, Philadelphia, PA, 21 September 2006.
30. Henderson claims that IBM sank more than a billion dollars into developing, but never adopting, X-ray lithography.
31. Gordon Moore (1965) first observed that the number of components on a chip doubled every year or two in the mid 1960s, but it wasn't until the mid 1970s that Mead and Noyce dubbed this 'Moore's Law'. Since then, Moore's 'Law' has been the guide by which the semiconductor industry sets targets for process improvements. For an excellent perspective on the self-fulfilling nature of Moore's Law, see Mollick (2006). A good summary of the origins and evolution of the Law is Brock (2006).
32. See Anacker (1980), Keyes (1979), and Kehoe (1983). There is a national security side to the superconducting supercomputer as well; the National Security Agency funded a great deal of research in this area in the 1970s, and was probably intended to be the first major customer for the IBM computer. Two interviews were helpful to us in better understanding the Josephson project: Hank Wohltjen, phone interview with Cyrus Mody, 13 September 2005; and Robert Buhrman, interview with Cyrus Mody, 24 April 2006, Ithaca, NY.

33. Mark Ratner, interview with Cyrus Mody & Arthur Daemmrich, Philadelphia, PA, 7 April 2006.
34. For background on the disadvantage chemists at Yorktown felt in dealing with the dominant physicists we draw on Robert Hamers, interview with Cyrus Mody, Madison, WI, 9 May 2001.
35. Ratner interview.
36. This gives the transistor its two main applications: as an amplifier (a small difference in input – the voltage across the gate – translates into a large difference in the output – current – through the transistor) or as a binary switch (depending on whether the gate voltage is on or off, current either passes – a one – or it does not – a zero). Abstractly, the Boolean logic elements out of which computers are built are made from these binary switches. In practice, most actual computer logics require their transistors both to switch and to amplify. One obstacle for both IBM's superconducting supercomputing project and Aviram and Ratner's molecular electronics is that the switches these platforms allow are usually not very good amplifiers; thus, critics argued that they would have required not just the substitution of a radical new material infrastructure for microelectronics but a substantial change in computer logic as well.
37. 'Molecules as Electronic Components: Some Basic Considerations,' IBM Archives, Press Release Collection, Research Division, 1974. Emphasis in original.
38. A rectifier, in this case, is merely a diode used in a circuit to convert alternating current into direct current.
39. Feynman's talk is today taken as 'founding' nanotechnology; yet Toumey (2005) nicely shows the limited impact of Feynman's talk. Notably, the Feynman speech was influential among the Moore's Law pessimists – IBM's Bob Keyes being one of the very few to cite the talk before 1990.
40. Wohltjen interview; Ron Siatkowski, interview with Cyrus Mody, Washington, DC, 31 August 2005; Ratner interview.
41. Recall the important role of the Signal Corps in early microelectronics programs such as micro-module.
42. Alan Heeger, interview with Cyrus Mody, Santa Barbara, CA, 13 March 2006.
43. Alan MacDiarmid, interview with Cyrus Mody, Philadelphia, PA, 19 December 2005.
44. Wohltjen interview; Bill Tolles, interview with Cyrus Mody, Alexandria, VA, 2 December 2005.
45. Ratner interview.
46. Mark Reed, interview with Cyrus Mody, New Haven, CT, 7 July 2005.
47. See Hameroff (1987) and Drexler (1986). Toumey (2007) overemphasizes the role of Hameroff and his colleague Conrad Schneiker, but gives a flavor of their style. Regis (1995) is a wonderful introduction to the futurist milieu of Eric Drexler. According to Hank Wohltjen, Carter and Drexler had a sustained correspondence but eventually they 'parted ways in that Forrest was not seeking limelight for this stuff, he was trying to play it down and focus on more doable things. And Eric was more into sensationalism'. On Stephen Wolfram see Levy (2002). That Carter was at least partially in Wolfram's circle can be seen in Carter (1984b).
48. By 'mainstream' we mean papers in journals considered mainstream by Carter's supervisors, such as Brant et al. (1981) or Carter (1983). By 'off-beat' we mean publications that Carter's supervisors, some peers, and trade magazines such as *Chemical and Engineering News* treated as unwarranted speculation (Carter's contribution to Yates, 1984, for instance).
49. The conference described is the Crump Institute meeting described in Yates (1984).
50. Bill Tolles, personal communication.
51. The gravitational radiation researcher Joe Weber is the classic example of such a scientist. See Collins (2004) for a 30-year chronicle of Weber's rise and fall that parallels Carter's more compressed trajectory.
52. Jim Tour, personal communication with Cyrus Mody, 15 August 2007.
53. Munn was editor of the *Journal of Molecular Electronics*, which ran from 1985 through 1991 before merging with *Chemtronics* to form *Advanced Materials for Optics and Electronics*. In general, Munn's editorial orientation was to Langmuir–Blodgett films

(what are now called self-assembled monolayers) and organic resists for lithography along the lines originally envisioned by Bruce Scott.

54. Compare Metzger & Panetta (1987) with Metzger (1991).
55. Ratner interview. Also, Jim Murday, personal communication. For a history of how STM (and its variants) propagated into fields like molecular electronics, see Mody (2004).
56. Tour interview; Reed interview.
57. A philosophical analysis of this experiment (and others like it) that offers useful sociological insights on nanotechnology more generally is Nordmann (2004).
58. Jane Alexander, interview with Cyrus Mody, Washington, DC, 24 July 2006.
59. See Overton (2000) and Chang (2006). Tour (2003) has also written a semi-popular book aimed at encouraging others in the field to commercialize their research.
60. Reed interview.
61. The response to Service's paper shows the incredible emotional heat generated by molecular electronics today and yesterday. *Science* published three indignant letters to the editor in reply – one from Paul Weiss (2004) claiming to have been misquoted and one from Jim Heath, Fraser Stoddart, and Stan Williams (2004) claiming they had been misrepresented: 'the article erroneously stated that ... [we] tried to create "transistors that used the movement of molecules". We were not attempting to create transistors.' The third was from Forrest Carter's old nemesis, Edwin Chandross (2004), irate at the new generation of molecular electronics researchers: 'The proponents of this "technology" have buried us in hype for several years .... Such unreasonable advertising should have aroused the skepticism even of novices.'
62. Stan Williams, interview with Cyrus Mody, Palo Alto, CA, 14 March 2006.

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