

# The Diverse Ecology of Electronic Materials

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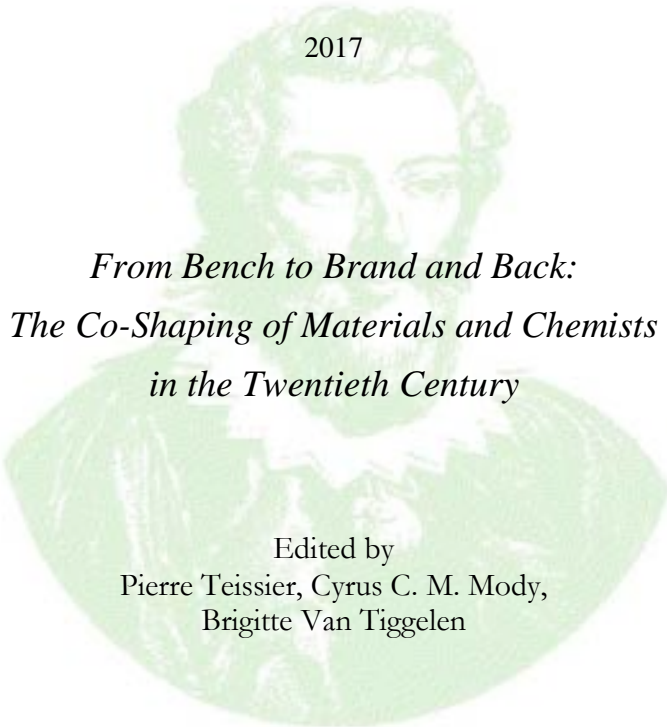
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*From Bench to Brand and Back:  
The Co-Shaping of Materials and Chemists  
in the Twentieth Century*

Edited by  
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Brigitte Van Tiggelen

Centre François Viète  
Épistémologie, histoire des sciences et des techniques  
Université de Nantes - Université de Bretagne Occidentale

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*The Diverse Ecology of Electronic Materials*

## The Diverse Ecology of Electronic Materials

Cyrus C. M. Mody\*

### Abstract

*Silicon has been the dominant material in microelectronics for a half century. Other materials, however, have subsidiary roles in microelectronics manufacturing. A few materials have even been promoted as replacements for silicon. Yet because of silicon's dominance, none of these alternatives has gone from bench to brand; nor could any of them progress from brand to bench. For these reasons, historians have paid little attention to silicon and almost none to other microelectronics materials. I show, however, that we can better understand how the organization of the semiconductor (silicon) industry has changed over time by examining alternative microelectronic materials. I do so by presenting two case studies: one of a superconducting computing program at IBM, the most likely candidate to overthrow silicon in the '70s; the other of carbon fullerenes, the most likely candidates to overthrow silicon today.*

*Keywords: Nanotubes, graphene, Josephson computing, Richard Smalley, IBM, Rolf Landauer, academic entrepreneurship, corporate research, historical alternatives.*

### Résumé

*Le silicium a été le matériau dominant en microélectronique durant un demi-siècle. Cependant, d'autres matériaux ont des rôles complémentaires dans cette filière. Quelques matériaux ont même été promus en remplacement du silicium. Pourtant, en raison de la domination du silicium, aucune de ces alternatives n'est allée de la paillasse à la marque commerciale, et aucune d'entre elles ne pourrait retourner de la marque vers la paillasse. Pour ces raisons, les historiens ont prêté peu d'attention au silicium et presque aucun à d'autres matériaux de la microélectronique. Je montre, cependant, que nous pouvons mieux comprendre comment l'organisation de l'industrie des semi-conducteurs (silicium) a changé au fil du temps en examinant les matériaux microélectroniques alternatifs. Je le fais en présentant deux études de cas : l'un basé sur un programme d'informatique supraconductrice chez IBM, le candidat le plus plausible pour renverser le silicium dans les années 70 ; l'autre portant sur les fullerènes (carbone), les candidats les plus plausibles pour renverser le silicium aujourd'hui.*

*Mots-clés : Nanotubes, graphène, informatique Josephson, Richard Smalley, IBM, Rolf Landauer, entrepreneuriat universitaire, recherche industrielle, alternatives historiques.*

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THE DIGITAL electronic computer has been around for about seventy years. For most of that time, the majority of computer architectures have been built around transistors and other components embedded in integrated circuits composed primarily of silicon and silicon dioxide. Although there is some public awareness of the materials used in microelectronics (e.g. Gorilla Glass used in iPhones), professional historians have largely neglected the materials of computing. Instead, they usually profile individual mathematicians and theoretical physicists (Turing, von Neumann, Shockley, Bardeen) and/or big businesses (Bell Labs, IBM, Fairchild, Intel), neither of which are described as getting their hands dirty messing with chemicals and chemical apparatus. The biggest exceptions, by far, have been Christophe Lécuyer and David Brock (2006), who have consistently reminded us that the transistor would have been a footnote but for the expertise of chemists, metallurgists, and materials scientists who grew and purified crystals, developed novel photoresists, applied sophisticated acids and “dry etches”, invented techniques for cutting and polishing wafers, etc. Brock and Lécuyer have hammered on the point that materials innovation has been indispensable both in the creation of new *types* of gadgets and in the manufacturing of vast *numbers* of those gadgets.

The microelectronics industry both relies on materials innovation and also markets products that are used in materials innovation. Digital computing has been an important tool of chemical and materials research since at least the 1950s: in modeling of molecules (Francoeur, 2002), in more efficient circulation and searching of chemical abstracts (Rayward & Bowden, 2002), in creating a new field of computational chemistry (Johnson, 2006), in operating certain kinds of experimental apparatus (November, 2012). Thus, microelectronics offers a particularly clear example of the continuous circulation of people, materials, and ideas both from brand to bench and from bench to brand.

At first glance, though, that circulation might seem rather narrowly confined to a single material: silicon. Silicon integrated circuit transistors dominate the imaginary of microelectronics – even though little semiconductor manufacturing is done there any more, it is still *Silicon Valley*, not Gallium Arsenide Valley. And that is because silicon also dominates other material configurations of microelectronics in the marketplace. No other material has moved all the way from bench to brand in numbers of products that in any way rival silicon’s numbers. And because materials other than silicon hardly exist in “brand” form, they can’t move from brand to bench either. I will argue, however, that despite silicon’s dominance, the

material basis of modern microelectronics is actually quite diverse, and therefore that the flows of people and materials into and out of the microelectronics industry are quite complex. For all its dominance, silicon is hardly alone. More than half of the elements in the periodic table can be found in today's cell phones. In addition, if we look beyond just microelectronics *products*, and expand our perspective to include manufacturing *processes*, we can see that the production of silicon integrated circuits requires an even more diverse array of materials which draws on a dizzying array of expertise from plasma physics to organic chemistry.

Moreover, silicon integrated circuits have never gone unchallenged – there have always been other materials that have vied to replace silicon. Or, to put it less anthropomorphically, there have always been experts in silicon who have been dissatisfied with its performance and therefore sought out alternative materials. There have also always been experts in materials other than silicon who have sought to bring their materials into the mainstream of microelectronics. This article presents two case studies, one for each of these possibilities. The first looks at IBM's exploration of computer architectures based on superconducting, rather than semiconducting materials; this effort was widely considered the best possibility for the overthrow of silicon in the 1970s. The second case looks at attempts, particularly by the Nobel laureate chemist Richard Smalley, to make carbon the central element of microelectronics – either in the form of pure allotropes or as the main constituent of so-called “molecular electronics”. Molecular electronics and pure-carbon graphene are today widely touted as the best candidates for overthrowing silicon.

Thus, while the microelectronics industry is imagined (by the public, by insiders, by historians) to be a semiconductor industry (and specifically a silicon industry), the reality is that alternatives to and hybrids with silicon have played an important role in silicon's success. I therefore draw on the “historical alternatives” approach from business history, which pays close attention to the presence of alternatives and hybrids in the organization of manufacturing. In particular, the historical alternatives approach emphasizes the role of actors and organizations within an industry in proposing, observing, evaluating, and choosing among a variety of strategies. Although rarely applied to choices among technologies – much less materials – I argue that the historical alternatives approach can help us understand how the different parts of the research system generate and evaluate alternative materialities. By comparing my two case studies – one from the 1970s, one from the 1990s – I also show that we can observe how the roles of different constituents of the research system (corporate laboratories, universities, etc.) have evolved over the past half-century. Alternative materials

serve as a kind of probe to measure how different research institutions have changed in the past half-century.

### **Alternatives and Hybrids versus Epochal Breaks**

By focusing on the materials of microelectronics, I will transgress, though not entirely overturn, the conventional historical narrative of the development of digital computers and of the semiconductor industry. That standard narrative depicts innovation in electronic information-processing as having progressed through a series of discrete material-technological stages going back more than a century: the electromechanical switch yielded to the thermionic valve/vacuum tube, which then gave way to the discrete germanium (and later silicon) transistor, which was superseded by the bipolar (later CMOS) silicon integrated circuit, which in turn will someday surrender to a nanoelectronic architecture based on some other material: graphene, carbon nanotubes, DNA, charge transfer salts, or perhaps an as-yet-undiscovered molecule (Choi & Mody, 2009).

This kind of narrative has a compelling simplicity, yet we should be wary of such all-one-way-or-the-other stories. Among business historians, Jonathan Zeitlin has been particularly critical of narratives in which forms of business organization switch suddenly and completely from one mode to another: from the family firm to the multi-division corporation to the networked venture-labor enterprise. Instead, Zeitlin and Charles Sabel have proposed the “historical alternatives approach” to thicken the temporal boundaries between such transitions and to acknowledge actors’ uncertain and heterogeneous strategies in attempting to choose among different co-existing organizational forms. As Zeitlin (2007, p. 124-128) puts it in a review of his and Sabel’s framework:

the process of strategic reflection and hedging against risk gives rise to a proliferation of hybrid forms.... Hence the predominance of hybrid, mixed, and intermediate forms... over polar types has proved to be the empirical rule rather than the exception.... The interpenetration of strategies and practices within industries and national economies at any one time resulting from actors’ efforts to hedge their organizational and technological bets about future changes in the environment casts inevitable doubt on the possibility of drawing sharp distinctions between epochs.... [I]t seems more useful to distinguish historical epochs according to changing orientations towards [what is] regarded as normal or paradigmatic than to divide history into periods where social life was in fact thoroughly organized according to one or another master principle.



Zeitlin mentions “technological bets” in this passage, but the predominant use of the historical alternatives approach has been in tracing the evolution of organizational forms. As both business historians (including, surely, Zeitlin) and historians of technology would acknowledge, however, the distinction between a technological bet and an organizational one is often fuzzy. Organizations that last continually adjust, and adjust themselves to, the technologies they use.

With that in mind, it should be clear that the historical alternatives approach can help us undermine the stark polarities of the standard narrative of microelectronics. Electromechanical switches continued in use long after the invention of the vacuum tube, and are still one of the dominant and obdurate forms of interface between users and their electrical/electronic devices (Plotnick, 2012). For many years, vacuum tubes possessed decisive advantages over transistors for certain applications. Indeed, in a few niches, such as high-end audio, tubes still live on (Downes, 2009). The shift from tubes to transistors was therefore gradual and still only partial. So was the later shift from the discrete transistor to the integrated circuit. Even though integrated circuits are ubiquitous today, when they were invented in the late 1950s they initially only offered advantages for a few, mostly military, applications. For most consumer electronics products, such as “transistor radios”, the discrete transistor was the reasonable choice for many years. Moreover, the (still incomplete) transition from discrete components to integrated circuits featured a long period in which *hybrids* of the two were at the leading edge – rather similar to the long period of hybrid gas and electric cars in the late 19<sup>th</sup> century (Mom, 2004). Perhaps the most commercially important computer of all time – IBM’s System/360, first sold in 1965 – used a hybrid chip architecture known as Solid Logic Technology, in which various sub-circuits were baked together as discrete components, but the sub-circuits themselves were monolithic integrated circuits (Bassett, 2007, p. 67).

The development of alternatives in parallel with each other, and the emergence of hybrids between alternatives, characterizes the history of electronics and computing from the systems level (whole computers and their peripherals) all the way down to the materials from which those systems are composed. At the systems level, historians of computing are beginning to grapple with the fact that analog computers co-existed with – and in some applications outcompeted – digital computers for much longer than the old celebratory narratives acknowledged (Cohn 2013). At the level of materials, Brock (2009), Lécuyer and Ueyama (2013), and a few others have begun to flesh out the diverse ecology of electronic materials that complemented, competed with, undergirded, and extended the dominance of silicon.

Foregrounding that diversity might, to the uncharitable, simply seem like giving ribbons to the also-rans. The history of microelectronics is filled with side streets branching away from silicon, but so far all of those have turned out to be dead ends or, at best, *culs-de-sac* serving some limited “neighborhood” of applications. For instance, compound semiconductors – semiconductors composed of more than one element, such as gallium arsenide or gallium nitride – have been promoted for decades for their *theoretical* superiority to silicon. So far, though, compound semiconductors have found only niche service in lasers, light emitting diodes (LEDs), and some solar cells. For most microelectronics applications, silicon really has been dominant, in *some* sense, for more than a half century. My intent here is not to romanticize the unsuccessful underdogs.

Still, there are good reasons to pay more attention to the diverse ecology of electronic materials – the mutually supporting tools and materials such as lithographic steppers, advanced cleaning pads, and photochemical resists that combine to allow fabrication of complex chips. One reason is simply that the kinds of actors who pursued quixotic alternatives to silicon are intrinsically interesting. For those not inclined to arguments from intrinsic interest, however, I would point out that the same actors were also often people who made important contributions elsewhere in science, contributions that might not be fully intelligible without examining their interest in alternative electronic materials. David Brock and David Laws (2012), for instance, have examined the history of a superconducting device called the cryotron which competed with the transistor in the late ‘50s and early ‘60s, after which it fell from view entirely. The people who developed the cryotron included Dick Garwin, famous both in gravitational radiation physics and ballistic missile defense policymaking, and Ken Shoulders and Dudley Buck, early pioneers of electron beam lithography. John Bremer (2007) makes the case that the first “integrated circuit” was not the semiconductor-based circuit invented independently by Jack Kilby and Robert Noyce, but rather a superconducting cryotron circuit made by Buck and Shoulders.

Narratives that embrace a larger, more diverse ecology of electronic materials also allow us to see constraints on innovation that would not be apparent from an exclusive focus on silicon. A computer or gadget is more than a chip. The materials that go into user interfaces, such as liquid crystals (Gross, 2011), have often suggested certain applications or innovation pathways and discouraged others. So have the materials used in power systems, especially mobile power supplies incorporated in portable electronic devices (Hintz, 2009; Eisler, 2012). If one looks at microelectronics from the perspective of power supplies, the trajectory of innovation in making transistors draw less power becomes more obvious than the oft-celebrated

trajectory of making transistors smaller and faster. Similarly, if you look at microelectronics from the perspective of the materials in which a chip is packaged (rather than the materials in the chip itself), then the geographic focus of your story moves from *Silicon Valley* to industrial districts in Taiwan and elsewhere (Tinn, 2012).

I could point to other reasons to pursue an historical alternatives approach to electronic materials, and no doubt there are good reasons I've overlooked. For the rest of this article, though, I want to concentrate on applying this framework to the *organizations* that pursued quixotic alternatives to silicon. Acknowledging the diversity of the electronic materials ecology tells us a great deal about the organizations that fostered innovation in electronics in general (including in silicon). It also tells us a great deal about the evolution of R&D organizations in general, both in and out of electronics and at the thick boundaries between the center and periphery of the electronics industry. In microelectronics, new materials co-emerged with new organizational forms throughout the 20<sup>th</sup> century, and will continue to do so in the 21<sup>st</sup>. And, again, many of the individuals involved in promoting (or at least exploring) new materials for microelectronics have also promoted new ways of organizing research on materials for microelectronics.

To give a sense of why organizations matter in the search for new materials, let me juxtapose two extended quotes. The first is from Zeitlin (2007, p. 120-123) again, fleshing out the historical alternatives framework:

the hallmark of this approach is its emphasis on the salience of alternative possibilities, contingency, and strategic choice in the development of modern industry.... [T]echnology and organization should not be taken as fixed, given, or even latent parameters to which economic actors must perforce adjust, but rather as objects of strategic reflection and deliberate experimentation in their own right.... Economic actors ... are often at least as concerned with determining, in the double sense of figuring out and shaping, the context they are in – market, technological, institutional – as with pursuing their advantage within any particular context.... Crucial to this process of strategic reflection is the capacity of economic agents to imagine and weigh up alternative courses of action, connecting the present with both the future and the past through narratives which constitute their identities and interests.

My second quote comes from Rolf Landauer, an IBM physicist probably best known for his work on the theoretical minimum amount of energy required to erase a single bit of information. Within the wider physics community this result is usually remembered as (possibly) disproving the possibility of Maxwell's Demon (Wright, 2016). Within IBM, however,

Landauer is remembered as “an outstanding scientific and technical manager of IBM’s Watson Research Laboratory, guiding it from relative obscurity to become by 1970 one of the world’s two most important and innovative engineering and scientific laboratories” (Bennett & Fowler, 2009, p. 1).

Much of what Landauer did at the Watson Lab (more commonly referred to as IBM Yorktown) was to help determine – in Zeitlin’s double sense – the ultimate limits to the miniaturization of microelectronics in order to aid executives in choosing which material basis for microelectronic circuits would facilitate the firm’s progress toward those ultimate limits ahead of its competitors. One of the critical functions of people like Landauer, therefore, was to provide Zeitlin’s “strategic reflection” by “imagin[ing] and weigh[ing] up alternative courses of action”. After a whole career of this kind of work, Landauer (1993) looked back in this way:

There are many advanced technology proposals which become major thrusts, only to be abandoned again subsequently. An adventurous technological climate has to reward the taking of risk, and must allow failures.... Among the many supposedly broadly applicable logic proposals we have seen come and go, we can find Gunn effect logic, tunnel diodes, ferrite core logic, schemes utilizing combinations of electroluminescent devices and photoconductors, fluid logic, parametric microwave excitation and Josephson junctions. Some technological candidates, such as Josephson junction logic, magnetic bubble storage, or the battery powered automobile, did deserve real examination. When they are discarded, it is done with trepidation, and knowledge that the decision may not last forever.

Landauer’s quote supports my point that we can use the historical alternatives approach to understand the history of microelectronics, particularly in terms of the material basis of microelectronic technologies but also in understanding the function of microelectronics firms’ corporate research laboratories. That is, corporate labs in Landauer’s era served, in part, as places where “advanced technology proposals” could be incubated to the point where they could receive “real examination” and either be pursued or “abandoned”.

Landauer adds his own take, though. If you read the whole article I’ve just quoted from, he argues that these various alternatives often receive more attention than they deserve because their proponents vocally promote their advantages but not their shortcomings, whereas potential detractors (such as Landauer himself) have little motivation to weigh in negatively until very late in the game. It’s a point that goes a long way toward explaining IBM’s rich history of exploring alternative microelectronic materials, devices, and manufacturing processes at enormous cost. Yet the skepticism of

people like Landauer meant IBM only rarely adopted adventurous alternatives.

Indeed, in a few famous cases – most famously CMOS transistors – IBM invented and/or developed alternative microelectronics technologies, only to discard them before being forced by the rest of the industry to re-adopt the technology later on. Other giant corporate research labs experienced similar misadventures in the 1970s and 1980s, such as Xerox PARC’s “losing” the graphical user interface to Apple and Microsoft (Smith & Alexander, 1988). Such mishaps contributed to the long-term reorganization of corporate research since the 1980s in which industry has downsized in-house research and nearly abandoned in-house long-range or fundamental research (Khan, Hounshell, & Fuchs, 2015).

### **A Semiconductor Industry No More**

To give a sense of how the search for novel electronic materials contributed directly to the decline of corporate long-range research, let me focus on an episode that is obliquely alluded to in the Landauer quote above. Note how Landauer assigns a particularly ambiguous status to one “broadly applicable logic proposal”: the Josephson junction. On the one hand, Josephson junctions are thrown in with a pile of other failures. On the other hand, Landauer sets Josephson logic apart as the only such proposal to “deserve real examination”. He even hints that, though Josephson was abandoned, it might – unlike its peers – come back someday.

So what is Josephson logic? In the early 1960s, a young British graduate student, Brian Josephson, made a series of striking predictions regarding the behavior of certain kinds of superconducting circuits – work for which he shared the 1973 Nobel Prize in Physics (when he was just 33). Those ideas were quickly taken up by research labs at several large US firms, including AT&T, IBM, General Electric, and even Ford Motor Company. Interest at IBM focused on ways of applying Josephson’s ideas to constructing circuits with extreme rapid switching times – i.e., circuits that would be useful in high-speed computing.

The Josephson junction’s promise was quickly made evident to IBM management. The early work

culminated in 1966 in demonstration of subnanosecond switching of Josephson tunneling devices, and in 1967 in the operation of a thin-film Josephson device flip-flop, both indicating that Josephson switching devices could indeed be switched very fast and could be competitive with projected semiconductor integrated circuits. On the basis of these encouraging re-

sults, the pros and cons of Josephson devices were assessed and an initially small research program was launched in 1967 with the aim of studying technological and system aspects. (Anacker, 1980, p. 108)

The Josephson project was commissioned partly on the basis of the novel characteristics of superconducting circuits. IBM was committed to leading in basic research, and superconducting materials were attractive on that basis both as a topic of fundamental intrinsic interest, and potentially as key constituents of scientific instruments that could be used in space science, biomedical research, and other areas.

However, IBM was also keen to push into Josephson junctions because of worries that further circuit miniaturization would not be possible for much longer with silicon, and therefore that alternative materials needed to be explored. That view was most vigorously put forward by Robert Keyes, a physicist and close friend of Landauer's (he wrote one of Landauer's National Academies obituaries and is mentioned as a confidante in another). In "Physical Problems and Limits in Computer Logic" and "Physical Limits in Digital Electronics", Keyes (1969 and 1975) warned that power dissipation, in particular, was a rapidly approaching problem, and pointed to a variety of exotic technologies that might provide at least temporary relief.

The stunning success of silicon semiconductor technology for information processing has not completely stifled the search for alternative technological bases for memory and logic. In the first place, although progress in silicon technology seems certain to continue and to provide ever-more-capable and economic general-purpose computers, quantum leaps or revolutions cannot be predicted with confidence; if forthcoming it appears that they must be sought elsewhere.... Thus there has been interest in and research related to logic based on superconducting devices, fluid devices, magnetic bubbles, and even optical devices, in the past decade. Superconducting devices based on the Josephson tunneling cryotron appear to be the most likely candidate for logic that will make a much larger, faster computer possible; a Josephson gate that switches in only picoseconds and has a power dissipation of microwatts has been described. (Keyes, 1975, p. 760)

In other words, Keyes was urging his firm to experiment with – and to engage in "strategic reflection" about – a whole host of "historical alternatives" to the silicon integrated circuit. And of those alternatives, Josephson computing seemed to be the most promising.

IBM heeded that call, and through the early '70s its Josephson program gradually grew, generating know-how and patents that would protect IBM's position if the technology took off. Other firms, especially AT&T

and Sperry, established smaller Josephson groups as, in essence, fast followers behind IBM so that they could catch up if need be – again, exactly what one would expect in a world dominated by uncertain choices among historical alternatives rather than binary transitions from one technological state to another. The US National Security Agency, as well, became interested in Josephson computing as a potential enabler of ultrafast cryptography. A small group at NSA conducted in-house research shadowing IBM and other firms, and at the same time began contributing about a quarter of the IBM project’s funding.

By the late ‘70s, Josephson technology had progressed far enough that upper management deemed it ready to transition from “R” to “D”. The size of the project swelled, to about 125 personnel and a \$20 million annual budget in the early ‘80s. Yet as superconducting chips came tantalizingly close to production, the project became increasingly dependent on exactly the *semiconductor* personnel and expertise that Josephson technology was to supplant. As one of the leaders of the project put it in a review article,

A computer made up of Josephson junctions constitutes a radical departure from a well-established semiconductor technology. The fabrication of Josephson-junction components relies, however, almost entirely on methods learned in the development of semiconductor devices. The substrate material chosen for the Josephson-junction chips is silicon, not because of its conducting properties but because techniques for forming precise microscopic structures on silicon are well established. Circuit patterns are defined photolithographically, as they are in making semiconductor devices. (Matissoo, 1980)

In other words, the Josephson team needed IBM’s silicon manufacturing experts to adapt their technology for mass production, at just the same time that Josephson technology was maturing to the point that its potential could be measured directly against that of silicon, with the possibility that silicon could lose.

In 1980, IBM’s Director of Research, Ralph Gomory, convened an “Extendibility Study” to make that direct comparison between Josephson and silicon technology, with the aim of deciding whether the company would halt the project or continue following the Josephson path in parallel with silicon. In the end, though, the study estimated that with another decade of development Josephson chips *could* be three to six times faster than

silicon bipolar junction chips.<sup>1</sup> That forecast was ambiguous enough that the company continued on with the Josephson project, though its research-oriented leader, Wilhelm Anacker, was replaced by Joe Logue, a manager with deep expertise in semiconductor manufacturing and *product* development. Like most of IBM's silicon establishment, though, Logue was skeptical of Josephson technology's potential, and took the job only on the promise that it would come up for another moment of strategic reflection in two years' time (Logue, 1998).

Accordingly, in 1983 Gomory commissioned another extendibility study, this time to compare Josephson technology, bipolar silicon chips, and gallium arsenide, another of the perennial contenders to unseat silicon. This time, Josephson was found to be even less competitive relative to silicon than just three years earlier. Both Josephson and gallium arsenide circuits possessed *theoretical* advantages over silicon, and in small, simple devices those theoretical advantages had in fact been realized. But the 1983 extendibility study forecast that over the foreseeable future any mass-produced, complex chip based on either Josephson junctions or gallium arsenide would most likely have too small (if any) advantage over silicon to justify the cost of the firm's investment. IBM had reflected strategically, and concluded that it should not follow the alternative path of Josephson computing. And so, on September 23, 1983, the program was canceled (Robinson, 1983).

### **Back to the Bench**

The Josephson program was by no means IBM's only foray into alternatives to silicon, but it grew larger and progressed significantly further than similar efforts. Based on estimates in the trade press at the time and interviews with participants, I believe the whole program probably cost on the order of a quarter billion of today's dollars – not small change, but also not a risky expenditure for a company as dominant as IBM at the time. The firm's return on that investment is hard to specify, but it would have included: a substantial patent portfolio, some key personnel (including several who helped save IBM from bankruptcy in the early '90s), and some admiring press coverage that reinforced the company's image as innovator. Indirectly, the Josephson program helped four IBM scientists win shares in two separate Nobel Prizes for Physics. The first, in 1986 for the scanning tunneling microscope, originated in part as an attempt to characterize ultrathin

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<sup>1</sup> Emerson Pugh *et al.* (1980), Josephson Extendibility Study, in IBM Archives, Box 475, Folder 1 of 8 (# 8 in box), 1-1.



superconducting films used in the Josephson program. The second, in 1987 for high-temperature superconductivity, was inspired by the program's search for better superconductors, and was aided by conversations with the program's superconductivity experts. During the late-'80s frenzy over high-temperature superconductors, the Josephson program gave IBM a leadership position in the field and a stake in reactive policy initiatives such as the Consortium for Superconducting Electronics.

The Josephson program also generated two pieces of critical self-knowledge that IBM slowly absorbed over the next decade. First, it learned that Josephson junctions were *not* the way forward and therefore that – at least in terms of microelectronics – IBM would remain a semiconductor firm and not a superconductor firm. That might seem an expensive lesson, but it allowed IBM (and all of the other firms that had been carefully watching it) to more efficiently allocate resources, particularly during the early '80s semiconductor boom associated with the first personal computer craze. And, obvious as it might seem, Josephson's non-viability was a lesson other organizations – notably the NSA and Japan's Ministry of International Trade and Industry – would continue to pay millions to learn over the ensuing decades. The *theoretical* speed and power advantage of superconducting electronics over semiconductors tempted MITI and the NSA even after the IBM program, and continue to lure smaller research groups even today.

At the same time, IBM learned from the Josephson program that it needed to change the way its research and manufacturing arms worked with each other. Over the course of the '80s, IBM Research would slowly become more product-oriented and less attached to basic, long-range investigation. That lesson would only be fully absorbed, however, after the company's early-'90s brush with bankruptcy. To keep IBM from dissolving, Josephson project veterans such as Carl Anderson, Juri Matisoo, and Mark Ketchen were called in to move IBM from a Cold War business model to a post-Fordist, Third Industrial Revolution (Dosi & Galambos, 2013) model – though, nodding to Zeitlin again, it's important to acknowledge that that transition was not as sudden as the metaphor of “revolutions” implies, and that older and hybrid forms continue to compete with more purely post-Fordist models.

These were exactly the kinds of side-benefits that large Cold War era corporate research laboratories in the US were supposed to accrue from their curiosity-driven exploration of fundamental questions. From the '50s to the early '80s, labs such as IBM Yorktown and Bell Labs were well-resourced and loosely steered, with the expectation that not all – in fact, quite few – of the alternative paths they wandered down would yield viable

products or processes. Basic research allowed firms to hedge their bets by examining alternative technologies that might, potentially, displace their core products. But corporate basic research also helped firms train new generations of managers; it reinforced those firms' reputations for innovation (and, implicitly, a continually improving product line); and it saved firms money in the form of hefty tax incentives favoring basic research (Asner, 2006).

As the Cold War gradually wound down and the global economy became more competitive, however, all of those justifications for basic corporate research diminished. Ironically, that led some of the most vocal supporters of these labs' reflection on alternatives to become prominent skeptics of that strategy. As Landauer's obituarists (Bennett & Fowler, 2009, p. 10) put it, for instance, "he understood what was needed to build a computer very well and along with Robert Keyes tried to pass such knowledge to the promoters of every cockamamie scheme that emerged. As a result he took a dim view of optical computing, [and] logic based on threshold devices, such as Esaki diodes and Josephson junctions". Keyes (1992) made a similar point, somewhat less colorfully:

The differences between the environments in a large [information processing] system and in a laboratory are often not recognized with the result that the essential attributes are missing in device proposals [e.g., single-electron transistors, cellular automata, and molecular electronics]. Thus, although many proposals for devices have been put forward, only three, the relay, the vacuum tube, and the transistor, have proven able to meet the requirements and form the basis of large computing systems.

Here we see the harsh lessons of IBM's Josephson computing foray brought to bear on all non-silicon, non-transistor microelectronics.

And yet, many many varieties of non-silicon, non-transistor microelectronics are still being actively promoted as potential future replacements for silicon transistors. Vast programs run by individual manufacturers, industrial consortia, and government agencies such as DARPA exist to manage research into those alternatives and to incubate them until they might be ready to move into production. However, that research is organized very differently than it was in the era of the IBM Josephson program. Back then, semiconductor manufacturing was still quite vertically integrated, at least at large firms like IBM. That vertical integration included research – once IBM bet big on Josephson in the early '70s, its program depended disproportionately on in-house expertise. Today, semiconductor manufacturing is almost completely dis-integrated. Firms specialize in chip design, chip fabrication, packaging, tool development, even research – but virtually no

firm does it all (and few do more than one of those activities). Thus, manufacturers need to depend on outside actors to elaborate possible alternatives to silicon. Industrial research consortia are one possibility; government laboratories are another. Increasingly, though, firms look to academic researchers – or to consortia and government agencies that manage a portfolio of academic researchers (Khan, Hounshell & Fuchs, 2015) – to bring alternative microelectronics materials closer to the market.

### **C<sub>60</sub> and Fullerene Materials as/and Electronics Research**

To better understand the semiconductor industry's increasing dependence on academic research, I want to finish with a brief outline of a story which is justly famous in the history of recent chemistry – namely, the discovery of buckminsterfullerene (or C<sub>60</sub>), for which Harry Kroto (formerly of the University of Sussex in the UK) and Bob Curl and Rick Smalley (both of Rice University in the US) received the 1996 Nobel Prize in Chemistry. A small cottage industry of popular histories and professional historical, social scientific, philosophical, and literary studies of the fullerene discovery has accumulated in the thirty-plus years since C<sub>60</sub> was announced (Aldersey-Williams, 1995; Baggott, 1994; Ball, 1994; Bueno, 2006; Sparrow, 2007; Kaplan & Radin, 2011; Broadhead & Howard, 2011; Eisler, 2013; McCray, 2013; Moskowitz, 2016). This literature has delineated several contexts which fostered the discovery of C<sub>60</sub> and/or its discoverers' later work – most notably space science and futurist “visioneering”. However, virtually none of these studies has foregrounded fullerene chemistry's more mundane if equally extensive connections with microelectronics.

The standard story usually begins with the fortuitous advent of the Kroto-Curl-Smalley collaboration. As a postdoc at the University of Chicago, Smalley had invented an apparatus for making spectroscopic measurements of very small, very cold clusters of atoms. A version of that device which he and his students built at Rice – known as the AP2 – was the center of his research program and formed the basis for collaborations with Curl. When Kroto encountered Curl at a conference in the early 1980s, the AP2 came up in conversation and Kroto seized on it as the means to investigate the chemical makeup of matter in interstellar space – an environment he believed contained a variety of very cold, very small clusters of carbon atoms which he thought could be simulated in the environment of the AP2. In the late summer/early fall of 1985, Smalley finally agreed to generate carbon clusters with the AP2, Kroto flew to Houston, and over the next few weeks Curl, Kroto, Smalley, and the latter's graduate students stumbled on, and struggled to interpret, data indicating the presence of a molecule

made up of sixty carbon atoms forming a closed cage –  $C_{60}$ , the third known allotrope of pure carbon after diamond and graphite.

This narrative of heroic serendipity amidst curiosity-driven research leaves a great deal obscure. Elsewhere in this volume, Sacha Loeve suggests how we might situate this episode in the long history of carbon allotropes. What I'll do here, instead, is to situate Smalley's contribution to this episode within the long history of research on electronic materials. At the time Kroto first suggested examining carbon, Smalley was studying the reactivity of small clusters (today we would call them nanoclusters) of *semiconductor* materials. The AP2 was originally built to study metal clusters, but in the months before  $C_{60}$  was discovered Smalley's group (in collaboration with Curl and a Rice electrical engineering professor, Frank Tittel) had moved on to examining whether semiconductor clusters differ significantly from metal ones (they do). Because semiconductor properties can vary wildly in the presence of even minute amounts of impurities, Smalley was afraid that vaporizing a carbon disc in the AP2 would contaminate any future experiments with silicon, germanium, or gallium arsenide in the same apparatus. Therefore, he refused to do Kroto's carbon experiment until after he had finished working on semiconductor clusters with the AP2.

The AP2 semiconductor cluster experiments were, in a sense, the kind of incremental basic research that scientists sometimes describe as “picking the low-hanging fruit”. Smalley had an experimental apparatus, and the periodic table provided a menu of elements and simple compounds to put in it. Semiconductor clusters, in that light, were epistemically no different from the metal clusters they followed or the carbon clusters they preceded. But epistemology is one thing; gaining the resources to do an actual experiment, and then using that experiment to gain further resources – what Bruno Latour and Steve Woolgar (1986, p. 231) referred to as the cycle of credit – is another.

Semiconductor research, of course, has a large, well-resourced audience with specific technological aims in mind which Smalley, *et al.* played to in setting up their experiments with semiconductor nanoclusters – as seen in the introduction to one of their articles:

Driven by the extreme technological importance of new breeds of semiconducting materials, there has been quite an active interest in theoretical models of III-V semiconductors.... Virtually all theoretical approaches to semiconductor surfaces and interfaces start with a relatively small cluster of atoms and... compare to bulk surface measurements... [T]here is still a potentially severe mismatch between the essentially microscopic theory and essentially macroscopic experiments. One appealing way out... is... by developing techniques for generating and probing the very clusters the theory

is best able to handle. Certainly this will not be a universal solution. Particularly for semiconductors (where the major device-driven interest often focuses on such intrinsically macroscopic phenomena as depletion layers, etc.), not all properties of bulk interfaces will be accessible through the study of microscopic clusters. But the crucial short-range phenomena... occur in the small clusters as well. (O'Brien *et al.*, 1986)

Notably, the large discs of silicon, germanium, and gallium arsenide used in these experiments came from Texas Instruments, and one of the Ph.D. students working on the project, Sean O'Brien, went on from Rice to work for TI. Indeed, TI probably assisted Smalley more because they hoped to recruit his graduate students than because they thought his research findings would help them manufacture circuits (which they didn't).

With the (initially contested) discovery of  $C_{60}$ , Smalley's work on semiconductor clusters was put on a back burner, and has been forgotten in the popular and scholarly historiography. No wonder – Smalley's most-cited semiconductor cluster article has received less than 1% of the citations of the article announcing  $C_{60}$ . Yet that disparity was still somewhat contingent. By the late 1980s, Smalley was satisfied that he had overcome all possible objections to the  $C_{60}$  model he, Kroto, and Curl had proposed, but he still could not make enough  $C_{60}$  to analyze using bulk characterization tools. The amount that could be learned about  $C_{60}$  seemed to be disappointingly constrained.

So Smalley began to wind down his fullerene work and returned to the semiconductor cluster research he had been working on before  $C_{60}$ . As the title of a talk by a Smalley student in 1989 put it (perhaps in deliberate contrast to  $C_{60}$  research), "Silicon Is Never Boring".<sup>2</sup> In the early 1990s, however, three discoveries convinced Smalley to return to carbon-cage materials (generically known as "fullerenes"). First, in 1990 Donald Huffman and Wolfgang Krätschmer discovered an astonishingly simple process for making larger quantities of buckyballs. Simply by running an electric arc across two graphite rods in a helium atmosphere at reduced pressure, they could make enough  $C_{60}$  to analyze with an infrared spectrometer. Suddenly, a lot more became known about buckyballs very quickly. Smalley (1991) referred to this as " $C_{60}$ , Chapter 2".

Chapter 2, as it turned out, moved quickly away from buckyballs and toward carbon nanotubes. Both were closed cages of pure carbon, but

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<sup>2</sup> Poster for RQI Informal Seminar/Discussion Series talk by Mike Alford (1989), "Silicon Is Never Boring: Some New Results of Silicon Cluster Ion Reactivity", September 22, Rice Quantum Institute information file, Woodson Research Center, Fondren Library, Rice University.

Smalley could foresee much more interesting electrical and mechanical properties for the elongated nanotubes than for their spherical cousins. The year 1991 saw the first production of macroscopic quantities of multiwalled carbon nanotubes. Then in 1993 came the discovery of single-walled nanotubes (or SWNTs) – notably, by groups at microelectronics giants NEC and IBM. Smalley dubbed single-walled tubes “the world’s most perfect material” and dedicated the rest of his career to making, understanding, and applying them.

But perfect for what? Most scholarly attention to Smalley has focused on the rhetorical connections he spun from nanotube research to futuristic applications of nanotechnology such as Eric Drexler’s “molecular assemblers” or a “space elevator” lifting people and goods from earth’s surface to geosynchronous orbit. Indeed, Smalley used both Drexler and the space elevator in his attempts to persuade federal policymakers and university administrators and donors to support nanotechnology research. Yet even a cursory glance at Smalley’s public and private writings shows that the microelectronics applications of nanotubes and other nanomaterials pervaded his outlook. Smalley used the social capital that he accrued from the buckyball discovery largely to persuade Rice to hire a cohort of physicists, chemists, and electrical engineers with expertise in exotic electronic materials such as quantum dots and so-called “molecular electronics”. In the wake of the discovery of nanotubes, he re-oriented his own research to figuring how to make large quantities of high-quality nanotubes for *any* application, yet he consistently maintained that it was the microelectronics applications of nanotubes which were most achievable and would be most profitable. Smalley was incredibly eager to form collaborations with groups both inside and outside Rice to develop microelectronics applications of nanotubes, to the point of submitting a proposal to the NSF in 1998 for a multi-sited Center for Carbon Nanotechnology with “nano-electronics” as its top objective.

By 1998, Smalley was also preparing to found a company to manufacture tubes. In anticipation of that move, he spoke with *Business Week* about nanotubes’ commercial potential, focusing almost exclusively on their microelectronics applications:

Q: What makes buckytubes compelling to industry?

A: Take a look at the preface and introductory sections of Sematech’s *National Technology Road Map for the Semiconductor Industry*, 1997.... The notion that they will have to leave silicon was discussed in depth. They see so many problems on the horizon that they can’t get around. So now they are ready to think about things like carbon.

Q: That’s a big departure.

A: Yes. And this gets back to the old dreams of “molecular electronics”.... There is a huge electronics industry, well in excess of \$200 billion a year, with a great desire to maintain Moore’s Law for another 50 years. It’s likely that tens of billions of dollars will be spent on breaking the 100-nanometer barrier. And the only thing on the other side of that barrier is molecular electronics.... In the 1970s, there was much discussion of molecular electronics, but nothing came of it, mostly because people didn’t have good molecular metallic wires. But now it looks like we do, and the name is “buckytube”. (Anonymous, 1998)

Nor was Smalley alone in predicting that nanotubes would soon lead to the overthrow of silicon by molecular electronics – IBM and other big companies were also pursuing that goal, as were many academic researchers.

In fact, the main customers for Smalley’s company, Carbon Nanotechnologies Inc., were microelectronics firms. Samsung (Anonymous, 2008), for instance, repeatedly said it was close to marketing a display system incorporating nanotube emitters (some bought from CNI). Mobile phone makers, too, reportedly experimented with using nanotubes as additives in their glass touch screens (Hecht, 2009). In 2007, CNI merged with another firm (Unidym), which was then sold in 2011 to another Korean electronics company (Wisepower), “a leading supplier of Li-polymer batteries for mobile appliances” (Anonymous, 2011). Today, Unidym’s corporate tag-line is “carbon for electronics”. Its one market success seems to have come from Entegris, a maker of the trays on which silicon wafers are carried in semiconductor fabs – an application where even the tiny decrease in dust and flaking caused by incorporating nanotubes into the tray’s plastic matrix could justify the enormous expense of using one of the world’s most exotic materials (Anonymous, 2014). That is, the microelectronics industry is so vast and depends on so many different high-performance technologies, that any time a new wonder material such as the carbon nanotube is invented, the microelectronics industry can probably find multiple ways of using it.

The Smalley-fullerene case gives a good sense of how academic chemists, materials scientists, and allied researchers increasingly spin their work as relevant to – perhaps even as a panacea for – the microelectronics industry, to great effect. At every step, no matter what experimental material or apparatus or research institution he was working with, Smalley could successfully siphon resources from firms, donors, and government agencies by constructing a plausible narrative about how that apparatus, material, and/or institution was going to extend Moore’s Law and revolutionize electronics. But Smalley’s case also points to the parallelism and hybridity of

research on electronic materials. He helped discover  $C_{60}$  in the midst of work on semiconductor clusters – work he returned to when  $C_{60}$  research stagnated, then abandoned with the nanotube boom of the early '90s. Yet he used that boom to bring resources not just to nanotube microelectronics research but also to people studying other electronic materials such as quinones and quantum dots.

As Sacha Loeve's chapter explains, the fullerene research community Smalley helped found has continued to spit out new candidates for electronic wonder-material, most recently graphene (which yielded a Nobel Prize in 2010). And yet, all that activity has displaced silicon only a little bit symbolically, and not at all commercially. In fact, silicon is indispensable to research on graphene and molecular electronics, whether as the substrate on which those alternative materials rest, or as the material for which nanolithographic methods were first developed but which are now applied in graphene and molecular electronics research. Moreover, as the Unidym example shows, even when silicon outcompetes an alternative material such as carbon nanotubes, that material may still find an application in the larger ecology of materials used in the manufacture of silicon integrated circuits. The relationship between bench and brand is complicated indeed.

## Conclusion

What I've tried to get across in these two case studies is some sense of how the task of considering alternative materials for microelectronics was re-organized in the late Cold War and post-Cold War periods. During the Cold War, firms invented their own alternatives to silicon, observed alternatives to silicon that originated in competing firms, and pursued parallel lines of research in-house to aid in the process of strategic reflection as to whether to abandon silicon in favor of something else. Academic research on alternatives to silicon existed, of course, but firms prized that research primarily as a source of personnel who could be recruited into corporate labs, rather than as a source of *knowledge* that they could directly put to use.

Curiously, though, academic training was often only indirectly relevant to the future careers of corporate researchers. Many of the people who participated in the IBM Josephson program had virtually no experience with superconductivity – or even microelectronics – before joining IBM. If they did have a background in microelectronics, it was more likely to be in semiconductors than superconductors. Even previous corporate experience was poorly predictive of an individual's future industrial research. People like Richard Garwin bounced merrily from superconducting electronics to



meson decay to gravitational radiation research (Collins, 2010) to ballistic missile defense (Slayton, 2013). A few of the Josephson project personnel stayed in superconductivity, but most floated into other areas, often tacking among basic research, applied research, and technology management for the rest of their careers.

By the 1980s, that style of work was becoming more common in academia as well (Mowery *et al.*, 2004). Particular end-products (“brands”) became compelling imaginaries to motivate a wide variety of activity at the academic bench. The ultrafast computer, in particular, was an imaginary that stimulated bench-work on a tremendous range of materials. Increasingly, that imaginary shaped the work of individual researchers more than their expertise in any given material. Rick Smalley, for instance, moved easily from semiconductor nanoclusters to buckyballs to nanotubes. The importation of this industrial template for shaping the identity of the academic scientist has been a source of vociferous protest by some historians, philosophers, and even scientists themselves (e.g., Forman, 2007). Yet it is hard to see how, in our current moment, it could be otherwise (Mirowski, 2011). The increasingly competitive post-Cold War global economy has led many firms in the US and elsewhere to believe that they cannot sustain *any* activity that spans all the way from bench to brand, much less the Cold War model of an in-house R&D portfolio consisting of multiple arcs leading from bench toward brand *and back* at the same time.

Pathways from bench to brand within industry are now supposed to be short, singular, and have a high probability of success. Research that may or may not lead to a product over a long time horizon has therefore been increasingly outsourced to universities and to industrial research consortia. Meanwhile, research within academia is increasingly supposed to delineate a plausible path from bench to brand. When that pathway becomes *very* plausible, academic researchers such as Smalley increasingly take it upon themselves to patent their work (and license the patent to someone who will turn it into a brand), and/or found their own company to shepherd their ideas to the marketplace themselves. Whether this increasing emphasis on “translation” from bench to brand (and decreasing tolerance for moves from brand to bench) will prove a sustainable innovation model over the longer term is very much unclear.

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