

Toward a History of Science from the Perspective of Applied Science

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**Toward a History of Science
from the Perspective of Applied Science**

Toward a History of Science
from the Perspective of Applied Science

Inaugural Lecture

Maastricht University

6 October, 2017

Prof. dr. Cyrus C. M. Mody

Chair in the History of Science, Technology, and Innovation

In memory of Ann Johnson, 1965-2016



Toward a History of Science from the Perspective of Applied Science

Prof. dr. Cyrus C. M. Mody, Maastricht University

Dear colleagues, friends, loved ones: the usual custom in inaugural lectures is to lay out an intellectual program first, and then to thank one's friends and colleagues at the end. But if you will allow me, I would like to break protocol – for one particular reason I will explain below, but also for the more general reason that the personal and professional, the intellectual and emotional are not separable. The intellectual program I describe here is the work of many people, whether they know it or not, and so I am compelled to thank at least some of them at the outset. That means, thank you to my local colleagues for welcoming me to Maastricht, and creating an environment in which I feel useful and happy. Thank you to Karen and Daria, for being willing to try something new, and who have embraced our life here with relish. Thank you to the friends we've made here. And thank you to my wider network of professional friends, whose ideas are visible throughout these remarks.

One person deserves particular mention because for the past decade or so she stood at the center of that network: Ann Johnson.¹ I first got to know Ann through the social studies of nanotechnology community at the University of South Carolina, the “Science in the Context of Application” group at the Center for Interdisciplinary Research in Bielefeld, and through our common membership in the Society for the History of Technology. Our closest collaborations, however, were centered on the Center for Nanotechnology in Society at the University of California, Santa Barbara. We were privileged to work alongside a tight-knit group brought together by Patrick McCray, which included, among others, Matt Eisler, Mara Mills, Hyungsub Choi, David Brock, and Amy Slaton. I fully expected Ann to be at the center of that circle for decades, but instead she passed away from cancer in December, 2016. For many of us in science and technology studies (STS) and history and philosophy of science and technology, she was a friend, mentor, collaborator, inspiration. We were lucky to know her, we are still lucky to work

¹ For more on Ann and the personal and professional obligations I and my colleagues owe her, see Cyrus C.M. Mody, “Ann Johnson, May 28, 1965 – December 11, 2016,” *Technology & Culture* 58 (2017): 545-552.

with her ideas and her precepts for how to be a humane, engaged scholar. But of course we also miss her a great deal.

When I was struggling to come up with a theme for my inaugural lecture, I found myself returning over and over to essays in which Ann posed this question: what if we wrote the history of science from the perspective of applied science?² And just as I came to the conclusion that *that* is the question that I've spent most of my career trying to, Ann's condition worsened. So I would like to dedicate these remarks to her, as an acknowledgement of both personal and professional debt.

Now, I won't claim to answer Ann's question, so this lecture merely points "toward" a history of science from the perspective of applied science. I'll begin by defining some terms, I'll show that we don't have enough answers to Ann's question and I'll speculate as to why, and then I'll tell some stories that show why Ann's question is important for more than academic reasons. But the question itself won't be answered overnight. Indeed, the question is not a new one, though it took Ann's characteristic bluntness to pose it directly. Many colleagues, such as Ernst Homburg and Lissa Roberts, have worked on this topic for some time.³ Indeed, variants of Ann's question have a long history, particularly in the Netherlands. Some of the finest examples we have of histories of science from the perspective of applied science examine Dutch science: from Harold Cook's study of the commercial influences on botany and anatomy in the Golden Age to studies of research at Philips, Unilever, Shell, DSM, etc. by people like Ton van Helvoort, Mila Davids, and David Baneke.⁴ Here in Maastricht, history of science, sociology of technology, and innovation studies all complement each other under the umbrella of science and

² Ann Johnson, "What If We Wrote the History of Science from the Perspective of Applied Science?," *Historical Studies in the Natural Sciences*, 38.4 (2008): 610-620; Ann Johns, "Everything Old Is New Again: What Place Should Applied Science Have in the History of Science?," in *Science in the Context of Application* [Boston Studies in the Philosophy of Science], ed. Martin Carrier and Alfred Nordmann (Dordrecht: Springer, 2010): 455-466.

³ E.g., Ernst Homburg, "Groping for Knowledge: A Historical Perspective on Industrial and Academic Research in the Netherlands," in *Science and Technology Studies at Maastricht University: An Anthology of Inaugural Lectures*, ed. Karin Bijsterveld (Maastricht: Maastricht University, 2010): 67-121; and the essays in Lissa Roberts, Simon Schaffer, and Peter Dear (eds.), *The Mindful Hand: Inquiry and Invention from the late Renaissance to Early Industrialization* (Amsterdam: KNAW, 2007).

⁴ Harold J. Cook, *Masters of Exchange: Commerce, Medicine, and Science in the Dutch Golden Age* (New Haven: Yale University Press, 2007); Ton van Helvoort, Rob van Veen, and Matthijs Senden, *Gas to Liquids: Historical Development of GTL Technology in Shell* (Amsterdam: Shell Global Solutions, 2014); Mila Davids, "Technology as the New Frontier: Unilever and the Rise of Becel Margarine," *Journal of Modern European History*; David Baneke, "Organizing Space: Dutch Space Science between Astronomy, Industry, and the Government," in *Innovation in Science and Organizational Renewal: Historical and Sociological Perspectives*, ed. Thomas Heinze and Richard Münch (London: Palgrave Macmillan, 2016): 183-210.

technology studies – which is why this is the premier place to be the Chair in the History of Science, Technology, and Innovation.

But before we can start to answer the question, “what if we wrote the history of science from the perspective of applied science?” we first need some definitions. What, after all, is applied science? For most of the late Cold War period, the American policymakers, scientists, engineers, and protestors who are the central actors in much of my work routinely used that phrase to characterize certain kinds of knowledge-generating activities, usually in contrast with either “fundamental” or “basic” research. In STS we know, of course, that “applied” and “basic” are social constructions, that different groups draw the boundary differently (or don’t draw it at all), that no rule tells us whether a given piece of research is being carried out to solve a particular problem or to enlarge our understanding of the world around us. Notably, the historical actors who invoked these concepts were quite aware of this point: they keenly felt both the interpretive flexibility and the reality of the distinction between basic and applied. Harvey Brooks, a semiconductor physicist at General Electric and then Harvard, and something of a public intellectual in the US Cold War applied science community, made that point better than I could in a 1967 opinion piece for *Science*:

In institutions whose missions include the application of research results to products or operations, the categorization of research into basic or applied is not too meaningful and has little operational value... [A]ll research in a “mission-oriented” organization contributes or should contribute, however remotely in time, to the general objectives of the organization. On the other hand, there is clearly a spectrum of activities ranging from pure research on the one hand to technological development on the other, and to some extent one can locate research activities within this spectrum according to their “appliedness.”⁵

Here, Brooks invokes a “spectrum” from basic to applied as common sense, if difficult to define precisely or use effectively. He does not note, but we can, that the distinction performs a great deal of work. For example, as Donald Mackenzie, Glen Asner, Benoît Godin, and Phil Mirowski have argued, the people who probably used the distinction between basic and applied most routinely were accountants, economists, and statisticians.⁶ Such people deployed the concept of

⁵ Harvey Brooks, “Applied Science and Technological Progress,” *Science* 156.3783 (30 June, 1967): 1706-1712.

⁶ Benoît Godin, “The Linear Model of Innovation: The Historical Construction of an Analytical Framework,” *Science, Technology and Human Values* 31 (2006): 639–67; Philip Mirowski, *Science-Mart: Privatizing American Science* (Cambridge, MA: Harvard University Press, 2010); Glen Ross Asner, The Cold War and American industrial research, PhD dissertation, Carnegie Mellon University, 2006.; Donald A. Mackenzie, “Economic and Sociological Explanations of Technological Change,” in *Knowing Machines: Essays on Technical Change* (Cambridge, MA: MIT Press, 1998): 49-66.

applied science in, among other places, corporations, advising on how to get tax breaks by shifting funds toward basic research, as well as in the Pentagon, assigning degrees of “appliedness” to procurement contracts, and in science funding agencies keeping track of where their money went. Right there, you can start to see that when historians of science examine the distinction between basic and applied they find a lot to say about bureaucracies, capitalism, and the national security state – things we’d all benefit from understanding better.

So how did the Pentagon’s accountants draw the distinction between basic and applied? What determined where a piece of research was situated on Brooks’ spectrum of “appliedness”? Brooks offered two criteria: “the time scale on which the research is likely to find an application, and the specificity with which the domain of application can be foreseen.” If you do something in the laboratory now, with the expectation that you will see your work in some form on the battlefield or on the market in a year or two, then you’re doing applied research. If you know exactly what form your work will take on the battlefield or market, whatever the timescale, you’re also doing applied research. But Brooks’ definition is hardly uncontested. Many other criteria for distinguishing basic from applied have been offered over the years: basic research was sometimes characterized as “curiosity-driven” and applied as “problem-oriented”; basic as “esoteric” (i.e., of interest only to specialists) and applied as “interdisciplinary;” basic as “foundational,” i.e. what students should learn first or what actually was discovered first, while applied was supposed to come after the foundation had been laid.⁷

All of these criteria are problematic when you look closer. For instance, some people find researching circuits just as “curiosity-driven” a pursuit as researching quarks and quasars. In many historical episodes, such as the laws of thermodynamics, supposedly “foundational” knowledge only emerged after the applied understanding that led to it.⁸ In some eras, science and engineering education privileges “foundational” knowledge, but eventually the pendulum swings back and students are taught to start with “hands-on” techniques first.

⁷ All of these connotations of the basic/applied distinction can be found (with some rather sophisticated nuances) in a paper drafted by the president of Stanford University and some of his fellow university presidents: Richard Lyman, *The Critical Role of Basic Research in the Advancement of Science and Technology*, August 1976, in Stanford University Archives, Richard Lyman Presidential Papers SC 215, Box 43, Folder Research Misc.

⁸ Bruce J. Hunt, *Pursuing Power and Light: Technology and Physics from James Watt to Albert Einstein* (Baltimore: Johns Hopkins, 2010).

Because these criteria are so arguable, I won't offer a fixed definition of applied science. As Robert Bud puts it, applied science is "a phrase in search of a meaning."⁹ It's the kind of concept Nietzsche meant when he said that "only that which has no history is definable."¹⁰ For me, it's easiest to think of applied science as the type of term that Wittgenstein thought was held together by an undefinable family resemblance: all the instances of "applied science" look a bit like each other, though no one feature runs through all of them and no rule allows us to determine whether something belongs to the family or not.¹¹ We do often make that determination, but on the basis of grammars that change as we move from context to context, community to community. And that's one reason why we should ask Ann Johnson's question: many members of the applied science family are members of other families as well, when viewed from a different perspective. Applied science isn't a domain apart from basic research, nor from other domains: technology, art, business, diplomacy, governing, etc. It's precisely because applied science shades into all these domains that we should look at those domains from the perspective of applied science – from the viewpoint of people who say, on some occasions, that they are doing applied science yet who on other occasions might acknowledge that they are doing technology, art, business, government, basic research, etc.

In other words, one of the "family resemblances" that characterizes many, though not all, applied scientists is that they move easily between domains. I noticed this, almost twenty years ago now, in doing interviews for my PhD thesis: many of my interviewees had degrees in a "science," but their current job title contained the word "engineering," or *vice versa*. A few were doing science or engineering but had no degrees in those fields – their backgrounds were in whitewater rafting or psychology or even history. Others were trained as scientists and engineers but in their current practice they were entrepreneurs or grant officers.

It can be difficult to categorize such people and follow them around. Historians of science still divide themselves along lines provided by the scientific disciplines, so people who wander among disciplines fall off our radars. Thus, histories of science that prominently feature these wandering applied scientists are still the exception rather than the rule. I don't want to exaggerate this point, but let me offer one anecdote to show that most histories of science are still

⁹ Robert Bud, "Applied Science: A Phrase in Search of a Meaning," *Isis* 103 (2012): 537-545.

¹⁰ Friedrich Nietzsche, *On the Genealogy of Morals*, ed. Walter Kaufmann, trans. Walter Kaufmann and R. J. Hollingdale (New York: Vintage, 1967), 80.

¹¹ Ludwig Wittgenstein, *Philosophical Investigations*, ed G. E. M. Anscombe (Oxford: Blackwell, 1953), §67.

written from the perspective of basic research rather than from the perspective of applied science. In 2016 a volume was published entitled *A Companion to the History of American Science*.¹² As the title indicates, the volume aims to cover a very broad, even comprehensive, range of topics relating to the history of science in the United States. In many ways, it succeeds: the volume is a very useful reference, with quite a few chapters that go beyond simple literature review to make original arguments. And yet, in its 692 pages, half of one paragraph (in a chapter on the field sciences) is devoted to corporate research. In a volume which includes a chapter on the short-lived “nature study” movement, there is no chapter on the military!

To reiterate, this volume is of a very high quality. It presents an excellent picture of the history of American science, from the perspective of *basic* research. It would be unthinkable to write a history of American science from the perspective of *applied* science without devoting significant space to corporate and military-sponsored research. Yet the editors of this volume did not think that they had to justify excluding those topics, nor did they feel they had to explain that in their volume basic research stands for all of science. That is, for many historians, simply the default.

Why is that the case? Part of the reason is what Joe Martin calls the “prestige asymmetry” between fields which argue for the fundamental nature of their knowledge and fields which gesture to the technological outcomes of their work.¹³ Martin uses the “pub quiz test” to show how prestige accrues to one more than the other. Any pub quiz could ask a question about quarks or Higgs bosons, elementary particles in “basic” high-energy physics; yet no quiz could ask about phonons or “holes,” two of the most useful quasi-particles in applied studies of solids. Hasok Chang draws a similar comparison between the prestige accorded Schrödinger’s equation (from physics) and the utter anonymity of the structural chemistry without which Schrödinger’s equation would be useless.¹⁴

Many reasons for this prestige asymmetry have been advanced. Martin, for one, argues for a “purloined letter effect”: the need to gesture to technological results robs applied scientists of the chance to imbue their work with sacred meaning in the way basic researchers routinely do.

¹² Georgina M. Montgomery and Mark A. Largent (eds.), *A Companion to the History of American Science* (Oxford: Blackwell, 2016).

¹³ Joseph D. Martin, “Prestige Asymmetry in American Physics: Aspirations, Applications, and the Purloined Letter Effect,” *Science in Context* 30.4 (2017): in press.

¹⁴ Hasok Chang, “What History Tells Us about the Nature of Chemistry,” plenary lecture at the 12th International Conference on the History of Chemistry (Trondheim, 29 August, 2017).

Alternatively, Rebecca Press Schwartz shows that the American national security state promoted the visibility of basic research starting in the 1940s precisely because there were no security concerns in doing so – whereas applied science was so valuable it had to be buried.¹⁵ Practically, commercial and national security interests make historical sources relating to applied science hard to access. Conversely, institutions that generate basic science, such as universities, are happy to make documents relating to their discoveries available to historians. Thus, more histories get written about basic science, so that's where the historical conversation focuses, and studies of applied science move to the margins because their findings are hard to fit with established narratives.

Unfortunately, historians haven't adequately acknowledged that we reproduce the sciences' own prestige asymmetries. I'm walking onto thinner ice here, but it's not entirely unfair to say that many historians of science until the 1970s simply didn't consider applied science to be science. "Science" was an intellectual endeavor conducted with no thought of gain or utility, aspiring to universal relevance rather than to the solving of specific problems – except under duress or extreme necessity.¹⁶ After Kuhn's *Structure of Scientific Revolutions* appeared in the early '60s, attitudes slowly changed – but not in a way that gave much more primacy to applied science, at least not as Harvey Brooks would recognize it.

Instead, the consensus emerged among historians that all science was applied science in some way, so there was little need to say anything specifically about Brooksonian applied science: Galileo was trying to work his way up the patronage ladder at the Medici court, Boyle aimed to reconstruct English society after the Civil War, Newton was an alchemist and bloodthirsty master of the mint.¹⁷ Historians of science began to acknowledge the contexts of application which informed their cast of characters, but the cast itself did not really expand to include people like Harvey Brooks. It has expanded in other ways, particularly since the beginning of the current century: to encompass more women, more people from the working class and the Global

¹⁵ Rebecca Press Schwartz, *The making of the history of the atomic bomb: The Smyth Report and the historiography of the Manhattan Project*, PhD dissertation, Princeton University, 2008.

¹⁶ Steve Shapin examines this view, as put forward by Robert Merton and as contested by mid-century managers of industrial laboratories. Steven Shapin, *The Scientific Life: A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008).

¹⁷ Obviously, the "applied" contexts of these figures was a major discovery from which I in no way detract. Mario Biagioli, *Galileo Courtier: The Practice of Science in the Culture of Absolutism* (Chicago: University of Chicago Press, 1993); Steven Shapin and Simon Schaffer, *Leviathan and the Air-Pump: Hobbes, Boyle, and the Experimental Life* (Princeton: Princeton University Press, 1985); Michael White, *Isaac Newton: The Last Sorcerer* (New York: Basic Books, 1997).

South, and so on.¹⁸ And rightly so; my point that we should also pay attention to people like Brooks should complement, and not distract from, the broader diversification of the stories historians of science tell. Brooks and his colleagues were hardly subalterns – they were influential people, so if we want to understand how science works we need to pay more attention to how they used that influence, for good and bad.

In the second half of these remarks I'll show what we might learn by paying attention to people like Brooks. But before doing so, let me summarize what I've covered thus far. To begin, I put forward Ann Johnson's question, what if we told the history of science from the perspective of applied science? I've offered a family-resemblance-type definition of applied science. I've claimed that many historians of science take basic research to represent all of science, and see little need to justify minimal attention to applied fields. And finally, I've given some possible reasons for that. What I'll do next is give you a better idea of what I mean by history of science "from the perspective of applied science" and how that differs from more conventional histories. I'll do that by telling the same story three times – once from the perspective of basic research, and twice from that of applied science.

My case is the discovery of carbon-60, also known as buckminsterfullerene, in 1985, and of the further development of research on C₆₀ and other "fullerenes" including carbon nanotubes and graphene, up to the present. There is a thriving cottage industry of studies of this episode – both popular histories and academic STS research, and I'm particularly indebted to the studies by Matt Eisler, Patrick McCray, and Sarah Kaplan and Joanna Radin.¹⁹ Many of the primary documents relating to this episode are housed at not one but *two* of my former employers (Rice University and the Chemical Heritage Foundation), so I have had access to some sources others have not. But the bare facts of how C₆₀ was discovered are widely known and have mostly been assembled into narratives told from the perspective of basic research, even though – as I'll show – alternative narratives are definitely possible.

¹⁸ E.g., Kapil Raj, *Relocating Modern Science: Circulation and the Construction of Knowledge in South Asia and Europe, 1650-1900* (New York: Palgrave Macmillan, 2007); Londa Schiebinger, *Nature's Body: Gender in the Making of Modern Science* (New Brunswick: Rutgers University Press, 1993).

¹⁹ Sarah Kaplan and Joanna Radin, "Bounding an Emerging Technology: Para-Scientific Media and the Drexler-Smalley Debate about Nanotechnology," *Social Studies of Science* 41.4 (2011): 457-485; Matthew N. Eisler, "The Ennobling Unity of Science and Technology: Materials Science and Engineering, the Department of Energy, and the Nanotechnology Enigma," *Minerva* 51 (2013): 225-251; W. Patrick McCray, *The Visioneers: How a Group of Elite Scientists Pursued Space Colonies, Nanotechnologies, and the Future* (Princeton: Princeton University Press, 2013).

The usual story begins with an enormous experimental set-up, known as the AP2, built by the chemist Richard Smalley and his graduate students in the Space Sciences building at Rice University in the early 1980s.²⁰ The AP2 was composed of a laser which vaporized bits of a rotating disc of material; the bits would then be instantly cooled to near absolute zero by a supersonic flow of helium and a second laser would interrogate the spectroscopic signatures of these small (nanoscale) clusters. Smalley used the AP2 to study the reactivity of small clusters of a series of materials – basically working his way through the periodic table with the help of a theoretical chemist, Bob Curl. When Curl mentioned the AP2 to a British colleague, Harry Kroto, at a conference, Kroto immediately saw the AP2 as simulating the conditions that generate interstellar dust: intense heat (in a star) followed by intense cold (in the void of space). Kroto was one of the proponents of a theory that much interstellar matter is composed of long-chain carbon molecules, and he therefore asked Smalley to put a carbon disc in the AP2 to test this idea.

After some delay, Smalley agreed to do so, Kroto flew to Houston, and Smalley's graduate students ran the experiment. Unexpectedly, they found that the AP2 spit out molecules containing exactly sixty carbon atoms at a much higher rate than molecules of any other size. Sixty carbons must therefore be energetically favorable – but in what configuration? A couple weeks wrestling with the problem yielded a molecule in the shape either of a classic soccer ball or of one of Buckminster Fuller's geodesic domes – hence “fullerene.” The group quickly published their claim to considerable press, yet agreement from the scientific community was slow, largely because the AP2 could only produce minute amounts of C₆₀ – not enough to determine most of its properties. Full acceptance only came a few years later, when two astrophysicists figured out they could create reasonable quantities of C₆₀ by sparking an arc generator in a low-pressure helium environment. Suddenly, fullerene research took off, moving rapidly from spherical C₆₀ to elongated carbon nanotubes and then, a little over a decade later, to flat sheets of graphene. Smalley, Curl, and Kroto won the Nobel Prize in Chemistry in 1996 for the discovery of C₆₀, and Andre Geim and Konstantin Novoselov the Nobel in Physics in 2010 for isolating graphene.

²⁰ I take the general storyline of the C₆₀ discovery from Hugh Aldersey-Williams, *The Most Beautiful Molecule* (London: Aurum Press, 1995); Jim Baggott, *Perfect Symmetry: The Accidental Discovery of Buckminsterfullerene* (Oxford: Oxford University Press, 1994); Philip Ball, *Designing the Molecular World* (Princeton: Princeton University Press, 1994).

Not all, but most tellings of this conventional version emphasize the basic research aspect, particularly the importance of astrochemistry, astrophysics, and space science. It's hard to imagine a more curiosity-driven, seemingly application-less question than what fills the void between the stars. And I think there's great historical – and societal – value in telling the story in that way. We do need to foster research which is motivated by mystery and not by application. I am persuaded by people such as Phil Mirowski, Hans Radder, and Paul Forman that a research system eventually becomes sclerotic, inegalitarian, and dysfunctional if it only prizes application and casts curiosity aside.²¹ Yet we can't have a healthy research system which includes basic research if we don't have an understanding of how basic and applied research interact. If we only have histories of science from the perspective of basic research, we won't actually be able to understand the conditions which foster basic research. And that's where I would like to stake a claim to the relevance – the “valorization” – of my own research. So far, I've presented Ann Johnson's question and my take on it as a basic research preoccupation: Ann identified a “gap in the literature,” and I've been describing how we might fill it, regardless of whether the gap needed to be filled. But telling the history of science from the perspective of applied science is of more than academic interest. Our popular culture increasingly takes for granted that science is equivalent to technology, and our political systems increasingly take for granted that the only science worth supporting is the kind that leads to new technologies. Those are ideas worth contesting. Yet we need to contest those ideas in ways that don't replace them with other fallacies.

Let me make that point concrete by retelling the fullerene story from the perspective of two different applied fields, beginning with microelectronics research. In my recent book *The Long Arm of Moore's Law: Microelectronics and American Science*, I show that the conventional telling misses Rick Smalley's abiding desire to connect his research to the semiconductor industry.²² What was Smalley putting in the AP2 before Kroto asked him to zap carbon? Discs of silicon and gallium arsenide, the two main semiconductor materials used in commercial microelectronics. In fact, one of the three graduate students involved in the C₆₀

²¹ Mirowski, op. cit. note 6; Hans Radder (ed.), *The Commodification of Academic Research: Science and the Modern University* (Pittsburgh: University of Pittsburgh Press, 2010); Paul Forman, “The Primacy of Science in Modernity, of Technology in Postmodernity, and of Ideology in the History of Technology,” *History and Technology* 23.1/2 (2007): 1-152.

²² Cyrus C. M. Mody, *The Long Arm of Moore's Law: Microelectronics and American Science* (Cambridge, MA: MIT Press, 2017).

discovery, Sean O'Brien, went to work in semiconductor research at Texas Instruments. A second, Jim Heath, later became one of the biggest names in the field of molecular electronics, in which single molecules – sometimes carbon nanotubes or graphene – replace solid-state microelectronic components.

In fact, in the five-year period when no one could make significant quantities of C_{60} , Smalley started to abandon fullerenes and move back to research on semiconductors. When fullerene research rebounded in the early '90s, Smalley reversed course – not so much because he saw a future for C_{60} but because of the nearly simultaneous discovery of nanotubes, which he believed could be used in microelectronic circuits (whereas C_{60} could not). Indeed, nanotube research only took off because of advances made at two electronics firms, NEC and IBM. Notably, the social capital Smalley acquired from the C_{60} discovery – including the Nobel Prize given for that discovery – was spent on convincing Rice to hire people in fields related to molecular electronics rather than in more basic fields. Moreover, when Smalley founded a company to manufacture nanotubes, the main market he targeted was electronics firms such as Samsung and Apple. Smalley's imaginary for how and why fullerene research should be done was always oriented to microelectronics. In other words, if you tell the history of fullerenes from the perspective of astrochemistry or astrophysics, you miss the influence of microelectronics. If, on the other hand, you tell the history of science from the perspective of microelectronics, you quickly realize that many fields in the natural sciences – including very basic fields in biology and astronomy – have been shaped, and often facilitated, by the demands of the semiconductor industry.

For several years now, that's the alternative story about C_{60} that I've tried to promote – not to undermine or discredit the usual story that centers on astrochemistry, but to complement it. This one case nicely summarizes my research program of the past decade or so. But let me now look forward, to where my research will go now that I am at Maastricht University. Recently I've become interested in the oil industry's wide-ranging influence over global research – influence which rivals or outpaces that of the microelectronics industry, and which is sometimes pernicious, but also sometimes unexpectedly progressive. I'm particularly intrigued that in the 1970s, firms such as Shell were major sponsors of solar energy R&D and nuclear power, and of a few early biotechnology firms. A few oil executives were even champions of several important environmental organizations, as I've learned from my Maastricht colleagues Ernst Homburg,

Simone Schleper, and Raf de Bont.²³ This isn't exactly secret, yet historians haven't done much with it, partly, I would say, because they aren't used to telling the history of science from the perspective of applied science. For instance, with respect to biotechnology, there's a large literature which tells the history of that industry from the perspective of basic research in the life sciences.²⁴ There's relatively little scholarship relating biotechnology's origins to applied fields, however; and, apart from a chapter in Robert Bud's book on biotechnology, there's almost nothing which could explain why biotechnologists might have been supported by the oil industry.²⁵

The research program I would like to carry out over the next several years would squarely confront that mystery. We need to know why oil firms invested so heavily in alternative energy and in environmentalism in the 1970s. Were they sincere, were they cynically trying to undermine these fields, or were they perhaps hedging their bets in a period of great uncertainty? Given that they did make these investments, what happened to them? Do biotechnology, nuclear power, and solar energy still bear oil's fingerprints? How did some of the leading firms in this industry, such as Exxon and Shell, move from seeing their interests as aligned – at least partially – with environmentalists and alternative energy advocates, to believing by the 1990s that their interests were best served by alignment with climate denialists?

As I've begun to explore these questions, it has recently dawned on me that the fullerene story can also be told from the perspective of petrochemistry. Rick Smalley's connections to microelectronics were almost entirely imagined – he hoped that someday he would be a big player in that industry – but his connections to oil were real, and varied. After college he worked at Shell Research for several years. When he arrived at Rice University, he immediately had access to research funding from both oil firms and philanthropies founded by oil executives, most notably the Welch Foundation. Exxon even paid him to build a replica of the AP2 for use by their researchers – though it seems that that research was oriented more to Exxon's

²³ Primarily in discussions related to Simone Schleper, *Life on earth: Controversies on the science and politics of global nature conservation, 1960-1980*, PhD dissertation, Maastricht University, 2017.

²⁴ Sally Smith Hughes, *Genentech: The Beginnings of Biotech* (Chicago: University of Chicago Press, 2011); Nicolas Rasmussen, *Gene Jockeys: Life Science and the Rise of Biotech Enterprise* (Baltimore: Johns Hopkins, 2014); Doogab Yi, *The Recombinant University: Genetic Engineering and the Emergence of Stanford Biotechnology* (Chicago: University of Chicago Press, 2015).

²⁵ Robert Bud, *The Uses of Life: A History of Biotechnology* (Cambridge: Cambridge University Press, 1993), especially chapter 6.

involvement in nuclear energy than oil.²⁶ In fact, before Curl, Kroto, and Smalley met in 1985, the Exxon team had already published a study of carbon in which they almost saw the C₆₀ anomaly but dismissed it. As it turned out, fullerenes, and especially nanotubes, had been noticed but not commented on for decades by researchers working on soot, for example in the residue of fuel combustion.²⁷ Later, in the 1990s, Smalley's group gained a reputation for making extremely high-quality nanotubes using a synthesis technique borrowed from the petrochemical industry.²⁸ Hence, when Smalley founded his start-up company, he brought in veterans of that industry to run it and invest in it.

That is, the history of fullerene research is shot through with debts to oil firms, even though the fullerene research community has mostly generated quite basic research with little direct relevance to oil. Nor is fullerene research unique – many basic research fields are awash in money, personnel, tools, materials, and ideas borrowed from oil. As we head toward an economy that is less dependent on oil, we should start planning now for a research system in which oil has less influence over the research agenda. Oil's declining influence is salutary, but we also need to note that oil firms currently provide resources that circulate people, tools, materials, and ideas around the research system – resources we have become dependent upon, without even knowing it. My hope is that my research program can contribute to deliberation about oil's role in innovation, innovation's role in the oil industry's failure to confront climate change, and the organization of innovation in the hopefully fast-approaching world that is less dependent on oil.

These aren't hypothetical or esoteric questions, as shown by two news items that appeared within a few weeks of each other in 2017. One was an announcement of a collaboration between Maastricht University and Saudi Aramco, the Saudi national oil company, to research the “sustainability of biobased materials in a circular economy.”²⁹ The other was a story in *De Correspondent* – a newspaper which often engages in dialogue with science and

²⁶ Baggott, op. cit. note 20, 44-48.

²⁷ Sacha Loeve, “Point and Line to Plane: The Ontography of Carbon Nanomaterials,” in *From Bench to Brand and Back: The Co-Shaping of Materials and Chemists in the Twentieth Century*, ed. Pierre Teissier, Cyrus C. M. Mody, and Brigitte van Tiggelen (Nantes: Cahiers François Viète, 2017): 183-216.

²⁸ Sanford L. Moskowitz, *The Advanced Materials Revolution: Technology and Economic Growth in the Age of Globalization* (Hoboken: John Wiley & Sons, 2014).

²⁹ Maastricht University, “Saudi Aramco steunt onderzoek naar biomaterialen van de Universiteit Maastricht,” 27 June, 2017, <https://www.maastrichtuniversity.nl/nl/nieuws/saudi-aramco-steunt-onderzoek-naar-biomaterialen-van-de-universiteit-maastricht>.

technology studies and history of science and technology – about Shell’s much larger and seemingly more questionable sponsorship of a number of programs at Erasmus University Rotterdam.³⁰ The former was celebratory, while the latter was accusatory. The difference in tone should stimulate us to ask: how should we as citizens, as members of a university community, and as academics who study the history, sociology, and ethics of science and technology think about these two, seemingly quite similar, partnerships?

To get some guidance on that question, I want to close the circle and come back to Ann Johnson. Johnson’s articles asking “what if we wrote the history of science from the perspective of applied science” were written with situations like this one very much in mind. Her view was that we can’t yet grapple with the complexities of industrial sponsorship of academic research because most philosophy and history of science is still written from the perspective of basic research. That is, much of the academic debate about the propriety of partnerships like these proceeds – sometimes explicitly but more often almost unconsciously – from the view that applied science isn’t quite science: that it isn’t really motivated by curiosity or wonder, that it isn’t generalizable or rigorous, that it isn’t as open or impartial as science ought to be. No doubt that’s true of much applied science, but certainly not all. For instance, when Ann died she was working on a book about engineers in the Early American Republic; these were among the first people anywhere to put the study of materials on a rigorous scientific footing, but because they did so as part of an effort to build forts and canals and harbors most histories don’t regard them, or almost any 19th century Americans, as scientists at all!³¹

If, however, you write the history of science from the perspective of applied science then it quickly becomes clear that applied scientists are very much driven by curiosity and wonderment, that they are at least as rigorous as their basic research peers, and that much of their job lies, if not in generalizing, then in adapting their ideas from one domain to another. It also very quickly becomes clear that basic research too has interested patrons who are not so different from Shell or Saudi Aramco. We know a great deal about the Higgs boson, for instance, because

³⁰ Jelmer Mommers, “Als de fossiele industrie de onderzoeksagenda op je universiteit bepaalt,” *De Correspondent*, 16 May, 2017.

³¹ Ann Johnson, “Material Experiments: Environment and Engineering Institutions in the Early American Republic,” *Osiris*, 24 (2009): 53-74.

building giant particle accelerators was considered useful for European integration and for the vote-trading which led to the 1968 Fair Housing Act in the United States.³²

So Ann's view is that we need a history of science from the perspective of applied science in order to understand how unusual or problematic partnerships like these are. We also need histories from the perspective of basic research, of course. My point all along has been that we need *both* basic and applied research and we need *histories* that are oriented to both basic and applied research. With such understanding at our fingertips, we might still conclude that arrangements such as these are problematic – I'm by no means seeking to drain the history of science of cynicism. But cynicism must contend with complexity. Critiques such as that in *De Correspondent*, for instance, assume that sponsors determine findings, yet at least two generations of historians of science have failed to achieve consensus on that question, whether the sponsor is the national security state or private enterprise. Sometimes yes, but just as often scientists have, as Dan Kevles puts it, “derived both opportunity and enrichment” from such sponsorship.³³

Moreover, while critiques such as Mommers' are important, sometimes they miss the factors that most affect the research system. As David Kaiser has shown, for instance, the US military's support for physics during the Cold War didn't cripple basic research into things like subatomic particles – just the opposite, since high-energy research flourished. But military patronage did incentivize use of mathematical tools such as Feynman diagrams, on which people could be trained quickly, and discouraged more philosophically-intensive topics such as relativity or quantum entanglement.³⁴ Similarly, Paula Stephan has shown that the economic incentives which have most distorted American science have not been patents or contributions from private firms, but rather changes in pension plans and the retirement age which have hindered younger scholars from attaining tenured positions or winning grant competitions.³⁵

In my own research I've come across the complexity of public and private, basic and applied time and again. For instance, my first book was about a class of microscopes which

³² Thomas J. Misa and Johan Schot, “Inventing Europe: Technology and the Hidden Integration of Europe,” *History and Technology*, 21.1 (2005): 1-19; Lillian Hoddeson, Adrienne W. Kolb, and Catherine Westfall, *Fermilab: Physics, the Frontier, and Megascience* (Chicago: University of Chicago Press, 2008).

³³ Dan Kevles, “Cold War and Hot Physics: Science, Security, and the American State, 1945-56,” *Historical Studies in the Physical and Biological Sciences*, 20.2 (1990): 239-264.

³⁴ David Kaiser, “Cold War Requisitions, Scientific Manpower, and the Production of American Physicists after World War II,” *Historical Studies in the Physical and Biological Sciences*, 33.1 (2002): 131-159.

³⁵ Paula Stephan, *How Economics Shapes Science* (Cambridge, MA: Harvard University Press, 2012).

were invented and initially developed in private firms, particularly IBM and AT&T.³⁶ It was only thanks to those firms that the technology matured to the point where a global community of academic researchers could grow around it. Still, everyone in that community had to build their own microscope until a professor at the University of California decided to commercialize his departmental colleague's design – just the kind of privatization of publicly-funded research which many STS scholars are critical of. And rightly so, sometimes – although in this case, a great deal of publicly available, basic research has only become possible because academic scientists can now buy microscopes instead of building them.

Conversely, my current research is on public sector science in the 1970s. Here, I think you can make a case that at the end of the '70s private interests did curtail the academic and government research into environmental problems, disability technologies, mass transportation, public housing which boomed at the beginning of the decade.³⁷ But that early-'70s boom in public sector research only erupted thanks to a backlash against basic research – a backlash promoted largely by antiwar and left-leaning activists who believed, with some justification, that scientists were using their dedication to basic research to avoid taking responsibility for their complicity in the Vietnam War and in order to discourage their students from becoming politically aware. The politics of basic and applied, public and private can get very complicated indeed.

Thus, there are few reliable benchmarks for judging cases like these. Instead, we can look to perspectives from science and technology studies which allow for complexity, such as actor-network theory or the co-productionist framework most associated with Sheila Jasanoff.³⁸ In this case, both ANT and co-production would encourage us to ask something like, what kind of world comes into being when partnerships like this are allowed, and is that the kind of world that we want? Writing the history of science from both the perspective of applied and basic research helps us to imagine those possible future worlds and to deliberate – alongside

³⁶ Cyrus C. M. Mody, *Instrumental Community: Probe Microscopy and the Path to Nanotechnology* (Cambridge, MA: MIT Press, 2011).

³⁷ Cyrus C.M. Mody, *Responsible Innovation: The 1970s, Today, and the Implications for Equitable Growth*, available at <http://equitablegrowth.org/report/responsible-innovation/> (Washington: Center for Equitable Growth, 2016).

³⁸ Sheila Jasanoff, "The Idiom of Co-Production," in *States of Knowledge: The Co-Production of Science and Social Order*, ed. Jasanoff (London: Routledge, 2004): 1-12; Bruno Latour, *Reassembling the Social: An Introduction to Actor-Network Theory* (Oxford: Oxford University Press, 2005).

sociologists, anthropologists, philosophers, and other STS practitioners – as to whether or not those are the future worlds that we want.

That is, writing histories which run back and forth across Harvey Brooks' spectrum of applied-ness allows us to see aspects of those imagined futures which unsettle reflex responses such as "basic research good" or "oil bad." When I think about the discovery of C₆₀ in the way that Ann Johnson taught me to see, for instance, I don't think she would've been concerned that Rick Smalley was funded by oil companies or that he patented his publicly-funded research or used public funds to seed a start-up company. For her, those were long-standing facets of applied science, without which it would be difficult to sustain basic science. But she would've been concerned that the discovery of a new allotrope of carbon, and the Nobel Prize awarded for it, allowed Rick Smalley and Harry Kroto to build up personal fiefdoms centered on their charismatic authority in which they determined the fates of dozens of students and colleagues.

To conclude: history is about telling stories. Academic historians tell stories from a particular perspective, and try to be as explicit as possible about that perspective. Our vantage points both enable and constrain, reveal and obscure. The history of science as told from the perspective of applied science is no more complete than that told from the history of basic science. But by telling our story from multiple vantages we reveal our assumptions, and foreground factors which were backgrounded in other tellings. Some of those assumptions have important implications for how our stories are taken up in public debate – so clarifying vantage points, and telling stories from multiple perspectives, allows historians to refine the relevance of their stories. But beyond the search for relevance we should treasure such stories for their common, intrinsic value. History itself is both a basic and applied science. Stories when told well are enjoyable, motivating, lesson-conferring, solidarity-inspiring. Telling the history of science from only one perspective leaves a wealth of stories locked away. Researching, telling, reading, listening to stories about science from multiple perspectives has given me great personal enjoyment from an early age, and great personal satisfaction for more than twenty years. I look forward to many more years of both personal and professional engagement with these stories from my new vantage as Chair in the History of Science, Technology, and Innovation at Maastricht University.