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Christian Kehrt:

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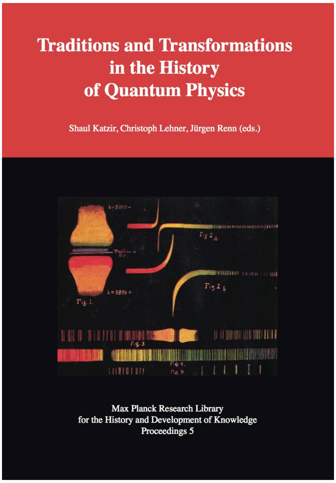

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Chapter 13
From Do-it-yourself Quantum Mechanics to Nanotechnology? The History of Experimental Semiconductor Physics, 1970–2000

Christian Kehrt

> Given the hype surrounding nano-technology (NT), few people realize that some of us have been practicing NT for over 30 years—we just didn't call it NT. (Kroemer 2005, 959)

Herbert Kroemer has been influencing the field of semiconductor physics, surface science and quantum electronics from the 1960s to the present day.[1] From his perspective, nanotechnology is mainly a re-labeling of the well-established and highly dynamic field of experimental semiconductor physics that traditionally stands between science and technology. He denies the claim of novelty by arguing that recent developments of so-called nanotechnology are rooted in the experimental practices of semiconductor physics from the early 1970s, when do-it-yourself quantum mechanics was made possible by new research technologies, such as Molecular Beam Epitaxy (MBE).[2] Nevertheless, the aim of this paper is not to follow Kroemer's defensive and rather skeptical argument and to reduce nanotechnology to the traditions of semiconductor physics or surface science (Kehrt and Schüßler 2010). Instead, I propose to carefully contextualize the discourse of nanotechnology in the 1990s from a historical perspective and to look for continuities and changes in specific scientific practices within a wider societal and political framework.[3]

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[1] In 2000, Kroemer received the Nobel Prize together with Zhores I. Alferov and Jack Kilby in physics for his work on semiconductor heterostructures and optoelectronics.

[2] A similar observation is made by the pioneer of MBE, John Arthur, who heard a radio broadcast on nanotechnology that left him "a bit impatient because of the heavy emphasis on the more flamboyant future possibilities that research may provide" (Arthur 2002, 190). Nevertheless, he was fascinated by the discussion since obviously he himself had been practicing nanotechnology for over thirty years: "It struck me that for more than thirty years, some of us have been doing this, in one dimension at least, by the process known as molecular beam epitaxy" (Arthur 2002, 190).

[3] These are results of an interdisciplinary case study, funded by the Volkswagen Stiftung, that I conducted together with Peter Schüßler on the practices and knowledge production of nanotechnology in Munich at the Deutsches Museum. It was based on oral history interviews and bibliometrics of local

My approach within the recent or contemporary history of science starts in the present, but tries to avoid the pitfall of constructing ex post a linear genealogy of nanotechnology (Rheinberger 2006; Söderqvist 1997). From my perspective, a "history of nanotechnology" is not possible in the sense of inventing milestones or traditions of nanotechnology, as its proponents intend to do, or by critically showing that scientific work at the nanoscale had been practiced in many fields already in the course of the twentieth century. Nanotechnology is a boundary object that has different social relevance for different groups of actors (Gieryn 1999, 5–6; Star and Griesemer 1989, 70; Kehrt and Schüßler 2010). Therefore, a closer look at specific scientific communities, their strategies, and their research traditions is necessary to explain why the rather vague and often stereotypical—but highly popular—futuristic discourse has been actively shaped by semiconductor physicists. These scientists have been working, as Kroemer noted, since the early 1970s at the nanoscale but only identified themselves as "nanoscientists" at the turn of the twenty-first century. So basically, I will tell a story of experimental or "do-it-yourself quantum mechanics" (Esaki 1992) that starts in the 1970s, culminates in the 1980s and looks for new orientations in the 1990s.

How did this dynamic field between science and technology evolve in this period? Is experimental semiconductor physics at the quantum level a case of so-called technoscience (Latour 1998; Nordmann 2006), mode II science (Gibbons, Limoges, and Nowotny 1994), finalized science (Böhme 1978), or—to cite Paul Forman—postmodern science (Forman 2007; Carson, Kojevnikov, and Trischler 2008)? As philosopher of science Joachim Schummer and many others have pointed out, nanotechnology is an umbrella term that encompasses almost every branch of science and thus is not helpful in specifying new fields of research (Schummer 2009; Decker 2006, 42). However, the wide use and active participation of scientists in the visionary nanotechnology discourse has real impact on the formation of local networks, research agendas, and careers. In the case of solid-state science, the reference to the rather vague idea of "nanotechnology" helps scientists to cross disciplinary boundaries and work with new experimental systems and methods from the life sciences. Besides this intra-scientific, transdisciplinary dimension, the participation in the public nanodiscourse highlights the extra-scientific, social and technological significance of this research that appears to be related to a future key technology. This seems to cohere with Forman's claim that the downgrading of science and the upgrading of technology indicates an epochal change that took place in the 1980s:

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nanotech networks. All interviews and translations in this paper are done by the author. I also want to thank Michael Eckert and Paul Forman for their helpful comments and critical remarks and Fred Koch for his careful reading of the Klaus von Klitzing story.

> Indeed, the transition from modernity to postmodernity, whatever else it involves, involves an upward revaluation of technology and a downward revaluation of science, leading to a far-reaching change in the culturally presupposed relationship between science and technology. (Forman 2010, 160)

More particularly, my argument is that nanotechnology is a funding and media strategy scientists use to pursue undirected free research at universities with the intent of emphasizing the technological relevance of their research and to still be able to freely play with molecules (Kehrt 2011). Therefore, the reorientation of semiconductor physics after the end of the Cold War reflects a general ideological shift from science to technology, without necessarily abandoning basic research or aiming merely to realize technological goals (McCray 2005; Johnson 2004).

This paper is based on a case study of local nanotech networks in the city of Munich, Germany. The high-tech region of Munich—with Siemens as a major employer for physics students, two high-ranking physics departments at the Ludwig Maximilians University (LMU) and the Technical University Munich (TUM), the resulting Center for Nanoscience (CeNS), and the national excellence network Nanoinitiative Munich (NIM)—is a good place to study nanoscience networks. While much work has been done on the discourses and futuristic background of nanotechnology, there are few studies that explicitly deal with the scientific networks, practices, and historical dimensions involved. The 1970s recently gained attention in the general history community (Jarausch 2008; Doering-Manteuffel and Raphael 2010; Trischler 1999; 2001). Unfortunately, there are almost no studies in the history of science about developments in microelectronics, semiconductor physics or experimental quantum mechanics in Germany that deal with developments in the period from the 1970s to the present.

13.1 New Research Technologies at the Quantum Level

A closer look at the research practices of nanoscientists at TUM and LMU shows that scientists in the field of semiconductor physics conduct experiments with quantum effects in semiconducting materials, such as quantum wires and dots, that confine the movement of electrons in two, one and zero dimensions. These nanostructures provide the opportunity to investigate new physical phenomena and promise new technological possibilities. Quantum phenomena of electron transport in two dimensional electron gases were first predicted theoretically by John Robert Schrieffer, who “pointed out that for high electric fields in surfaces of high perfection it would be necessary to consider quantum effects” (Landwehr

1975, 50). In 1966, researchers at Bell Labs proved quantum behavior of electrons in two-dimensional electron gases (Fowler et al. 1966, 901). This was the beginning of a new research field that dealt with electron transport at the quantum level.[4] Quantum states were realized in experimental systems with ultra-pure silicon samples or heterostructures of molecular thin layers of gallium and arsenide with a high electron mobility in vacuum conditions at low temperatures, but also at room temperatures.

> The intense contemporary interest in the physics and technology of thin films, surfaces and ultra-thin multilayer heterostructures has been motivated, at least in part, by the remarkable development of the solid-state electronics industry in the past thirty years. These areas are intriguing because, apart from their obvious technological importance, they offer the possibility of new effects that are not present in the bulk of a solid. (Dingle 1975, 21)

These new experimental possibilities at the quantum level were based on advances in materials (Mönch 1973, 242). The production of high-quality silicon wafers demanded new and extremely costly silicon growth and production techniques that were only realizable in large-scale industries. The aim was to build electronic devices with better qualities and performance. Especially the metal oxide semiconductor field effect transistors (MOSFETs) stimulated new research about electron transport in semiconductor surfaces and interfaces (Eckert and Schubert 1986, 200).[5] MOSFETs were developed in the 1960s and allowed an increase in performance through the ability to integrate more transistors and connections on a chip (Bassett 2002; Eckert and Schubert 1986, 200).

> With the beginning of the seventies, a new era became visible that has been directly connected with extreme demands concerning high packing densities (in integrated circuits) or homogeneity (in high power devices). (H. Hermann, Herzer, and Sirtl 1975, 281)

In contrast to the invention of the transistor, the development of the silicon MOSFET is based on technological advances and the control of surface phenomena and not so much on theory (Ernest Braun and MacDonald 1978, 101; Morris 1990, 85;

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[4] The fact that these quantum effects depend on the size of the devices and materials involved corresponds to the formal but vague definition of nanotechnology that assumes new effects at the nanoscale.

[5] The MOSFET is a sandwich-device built with layered materials of semiconducting silicon, conducting metal and non-conducting silicondioxyde. It can control the flow of electrons in the surfaces of silicon layers with the help of a thin metal film (or more recently, polysilicon). This gate electrode steers the flow of electrons by inducing a conducting channel between two electrodes called "source" and "drain." This principle allows for amplifying signals and constructing electronic switches.

Handel 1999, 208). Nevertheless, practical questions about the purity of the materials involved and the resulting "considerable renaissance in materials research and particularly diagnostic techniques" (H. Hermann, Herzer, and Sirtl 1975, 281) also led to research that went far beyond the daily business of industrial research labs:

> Quite apart from its technological importance in the form of the metal oxide semiconductor field effect transistor (MOSFET), the space charge layer on a semiconductor surface is a fascinating physical system. Under the influence of a surface electrical field, electrical charge is accumulated on the surface in a narrow channel typically 10 lattice constants in depth [...]. The electrons in the surface space charge layer are bound in their motion normal to the surface in discrete quantum mechanical states. They are free with respect to their motion parallel to the surface, and electron states thus form a two-dimensional band—the electric subband, as it is called. (Koch 1975, 79)

The early 1970s can be seen as a period with an experimental breakthrough in quantum mechanics and the beginning of a new and highly dynamic field of research. At that time, IBM researcher Leo Esaki proposed the so-called artificial superlattice where electron tunneling determines electron transport:

> It should be possible to obtain a novel class of man-made semiconductor materials, at least as far as electronic properties are concerned, and one expects the properties to depend not only on band parameters of the host crystal, but also on the characteristics of the superlattice. (Esaki and Tsu 1979, 61)

According to Esaki, new instrumental practices and technological equipment were crucial for this kind of experimental work at the quantum level that allowed one to operate with theoretical assumptions formulated in the early 1930s:

> A general tendency in those early days of quantum mechanics existed to try to explain any unusual effects in terms of tunneling. In many cases, however, conclusive experimental evidence of tunneling was lacking, primarily because of the rudimentary stage of material science. (Esaki 1974, 1149)

With the development of new research technologies such as MBE, it became possible for theories, models, and concepts of quantum mechanics from the 1930s to be realized in experimental physics:

> Weimann: I mean, quantum wells, quantum films […] or potential pots as we called them back then. They were calculated in the 1930s, when quantization was introduced and we had gotten used to it. There were models, but only conceptual models. Now [in the 1970s], for the first time, we really could use and create it in components and in semiconductors and really see that the qualities in the components improved.[6]

Besides silicon, which was favored by industry due to its stable surface properties and cleanliness, gallium arsenide compounds were also of interest since they promised future devices with superior performance in comparison to silicon MOSFETs. Especially scientists at universities, those with a greater interest in basic physical processes, moved to experimental systems with III–V element semiconductors (Ernest Braun and MacDonald 1978, 138).

One key research technology that made quantum experiments possible was MBE. It enables the precise tailoring of material structures at the nanometer or Angstrom level, so that quantum phenomena determine the transport of electrons. This research technology was developed at AT&T Bell Laboratories by Alfred Y. Cho and John R. Arthur in 1970 (Cho 2004, 199); both were interested in surface phenomena.[7] At the center of MBE is an ultra-high vacuum chamber with several heating pots that contain semiconducting materials, such as gallium and arsenide, that evaporate and finally condensate in ultra-thin "nanolayers":

> Thus, it has been possible to produce a large range of unique structures including quantum well devices, superlattices, lasers etc., all of which benefit from the precise control of composition during growth. Because of the cleanliness of the growth environment and because of the precise control over composition, MBE structures closely approximate the idealized models used in solid state theory. (Arthur 2002, 189)

Scientists spoke of "band gap engineering" and "artificial atoms" that are created by new research technologies and simultaneously promise new high-speed electronic devices as well as new, rather fundamental scientific discoveries and principles (Esaki 1985, 27; Capasso 1987). According to Terry Shinn and Bernward

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[6] "Weimann: Ich meine, Quantenfilm oder Quantenbrunnen oder Quantentröge oder Potenzialtöpfe haben wir es eigentlich früher genannt. Die hat man in den 30er-Jahren schon gerechnet. Das kam auf, nachdem man die Quantisierung eingeführt und sich an die gewöhnt hatte. Das gab ja die Modelle, aber immer nur als Gedankenmodelle. Hier konnten wir das jetzt wirklich im Bauelement, im Halbleiter ausnutzen, herstellen und tatsächlich auch sehen, dass man sehr viel bessere Bauelementeeigenschaften bekommen hat." (Interview with Weimann, 20 February 2008).

[7] First attempts to grow III–V element heterostructures go back to Siemens laboratories in the 1950s (Günther 1958).

Joerges, a research technology brings different actors from science and industry, electronics, semiconductor physics and also theoretical physics together (Joerges and Shinn 2001). In Germany, it was Klaus Ploog who pioneered MBE in the 1970s at the Max Planck Institute for Solid State Research in Stuttgart.[8] Kroemer also worked with MBE, as well as Gerhard Abstreiter and Günter Weimann at the Walter Schottky Institute (WSI) in Munich. In contrast to the tunneling microscope or electron microscope, this widespread research technology did not gain much attention beyond the realm of involved experts. Only with the new interest in the origins of nanotechnology was it identified as a precursor of today's nanotechnology (McCray 2007).

13.2 The 1970s: A New Quantum Generation

The biographies of leading Munich scientists like Jörg Kotthaus, who founded Munich's CeNS in 1998, or Abstreiter, director of the WSI, point at the origins of today's nanotechnology research projects in the early 1970s. Both belong to the generation that studied and worked in Munich in the 1970s and then actively shaped local nanotechnology networks and research projects in the 1990s. In an interview, Kotthaus stated:

> Esaki […] started to work with Molecular Beam Epitaxy at IBM in the early 1970s. And that is what fascinated me completely. I have to say, for me, […] the beginning was when people started to build artificial semiconductors by layering materials. That was essentially the beginning of experimental nanoscience, if you leave Feynman out.[9]

In this passage, the Munich scientist distances himself from the official storyline of nanotechnology that starts with a thought experiment by Richard Feynman. In an after-dinner speech in 1959, the pioneer of quantum electrodynamics came up with the idea that it should be possible to build electronic structures with single atoms and electrons. The ex post reference to Feynman's long-unnoticed talk is an invention of traditions by which nanoscientists try to emphasize the credibility of their research. Feynman's slogan "there is plenty of room at the bottom" then became the official headline of the US nanotechnology initiative at the turn of the

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[8] Interview with Klaus Ploog, 1 July 2008.

[9] "Kotthaus: Ich meine Esaki hat bei IBM die Molekularstrahlepitaxie angefangen Anfang der 70er-Jahre. Und das ist auch das, was mich völlig fasziniert hat. Da muss ich sagen, für mich fing es an, als Leute künstliche Festkörper gebaut haben durch Schichtung von Materialien. Und das war auch im Grunde genommen der Beginn der Nanowissenschaften im experimentellen Bereich, also wenn man Feynman mal weglässt." (Interview with Kotthaus, 19 January 2006).

millennium (Nordmann 2006; Junk and Riess 2006). Nevertheless, Kotthaus does not locate the origins of today's nanotechnology endeavors in Feyman's speech but in the research traditions and experimental practices of semiconductor physics in the early 1970s.

This new experimental work at the quantum level was enthusiastically pursued at TUM, especially in Koch's research group.[10] At the University of Maryland, Koch was already experimenting successfully with electrons that were bound in metal surfaces by magnetic fields (Doezema and Koch 1972). He was fascinated by the idea of applying this approach to semiconductors to study quantum behavior. In an interview on 15 June 2009, Koch explained:

> Koch: Epitaxy. When you build layered structures. That goes back to important things that Esaki had done. I was there, in the USA. Leo Esaki was one of the first who dreamed of growing semiconductors in such dimensions that something would happen [if you built in electrons], because he also took the slow electrons into account. And if you build such electrons into nanostructures, […] if they are confined to certain dimensions so to say, then their properties will change.
>
> Kehrt: And that's exciting?
>
> Koch: That's absolutely exciting. That is completely fundamental physics. That is the wave mechanics of the 1930s; that is where it was recognized. That is Heisenberg and Sommerfeld. Sommerfeld not so much, but Heisenberg and Max Planck and so forth. So the whole quantum physics of the electron is involved.[11]

Then in 1972, Koch took the chance to start a new branch of semiconductor physics at TUM. The appointment of distinguished American scientists was meant to help close the knowledge gap between the United States and Germany; the latter had lost ground in cutting-edge fields like semiconductor physics and

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[10] Kotthaus was Koch's assistant in 1973. Abstreiter was his first PhD candidate.

[11] "Koch: Epitaxie. Dass man eine Schichtstruktur aufbaute. Und das geht jetzt auch einher mit wichtigen Dingen, die Esaki gemacht hat. Ich war in den USA dabei. Also Leo Esaki war einer der ersten, der davon träumte, Halbleiter in solchen Dimensionen zu wachsen, dass sich was tun würde, weil er auch die langwelligen Elektronen erkannte. Und wenn ich solche Elektronen in Nanostrukturen einbaue oder habe oder die Elektronen erscheinen dadurch, dass man sie injiziert oder irgendwas macht, dass ein Elektron da ist […] und wenn dann ein Elektron in solchen Dimensionen sozusagen beherbergt ist, eingesperrt ist, dann ändern sich seine Eigenschaften.

Kehrt: Und das ist spannend?

Koch: Das ist absolut spannend. Das ist ganz grundlegende Physik. Das ist die Wellenmechanik der 30er-Jahre, da hat man das erkannt. Das sind Heisenberg und Sommerfeld, Sommerfeld nicht so richtig, aber Heisenberg und Max Planck usw. Also die ganze Quantenphysik der Elektronen kommt da zum Tragen." (Interview with Koch, 15 June 2009).

electronics after World War II. TUM introduced the American department structure to create better and supposedly more successful learning and research conditions (W. Hermann 2006, 505). But so far, the research focus at TUM's physics department was on nuclear physics due to the strong influence of Heinz Maier-Leibnitz (1911–2000) and Rudolf Mößbauer.[12] In the early 1970s, Koch had the chance to start a completely new direction of experimental physics. At TUM he had a distinguished position where he could basically build everything up from the very beginning and cooperate closely with Siemens:

> Well, that really opened up my eyes, when I saw that here [in Munich] you have Siemens just around the corner. I met the Siemens people. [They said:] we will provide you with samples. In the US I couldn't compete with IBM or Bell Labs. They had their own research teams. And here in Munich I saw the chance, since there was nothing going on in semiconductor physics at all. The whole physics department was based on nuclear physics, nuclear methods, Mößbauer, Maier-Leibnitz and the research reactor on campus over there [directly opposite Koch's office].[13]

Abstreiter, one of the five most-cited authors in semiconductor physics (Tsay, Jou, and Ma 2000, 505), also identifies Koch's group at TUM as the starting point of nanoscience in Munich: "I was the first doctoral student at the SFB (*Sonderforschungsbereich*, collaborative research center), also the first doctoral students of Prof. Koch's professorship that was newly established in 1973. You could roughly say that it was a kind of precursor to nanoscience, this special research field."[14] This so-called *Sonderforschungsbereich* investigated quantum phenom-

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[12] Maier-Leibnitz had a strong influence in the realm of nuclear physics in Munich as well as on the German nuclear research. At TUM, he held a chair in Technical Physics. He founded the first research reactor, the so-called atomic-egg (*Atomei*) that was the nucleus of the Garching research campus. He also motivated Mößbauer to return to Munich from CalTech and was a key figure in establishing the physics department structure at TUM (Eckert 1988). Mößbauer studied physics at TUM under Maier-Leibnitz. In 1961, he received the Nobel Prize for the discovery of the so-called Mößbauer Effekt—based on the investigation of recoil-free emission and absorption of gamma ray photons by atoms bound in solids.

[13] "Also mir sind die Augen aufgegangen. Dass ich sah, Siemens hier vor der Haustür. Ich habe die Siemens-Leute getroffen gehabt. Wir beschaffen euch die Proben. In den USA konnte ich ja nicht mit IBM und Bell Labs konkurrieren. Die hatten ihre eigenen Forscherteams. Und ich sah diese Chance hier in München, wo es Null Komma Nichts an Halbleiterphysik gab. Das ganze Department war auf Kernphysik, kernphysikalische Methoden, Mößbauer, Maier-Leibnitz, der Reaktor da drüben." (Interview with Koch, 15 June 2009).

[14] "Ich war da in dem SFB der erste Doktorand, auch der erste Doktorand im Lehrstuhl von Professor Koch, der '73 da neu aufgebaut wurde und man könnte grob sagen, das war so eine Art Vorläufer für Nanowissenschaften, dieser Sonderforschungsbereich." (Interview with Abstreiter, 22 November 2007).

ena in the surfaces of semiconductors. It was a highly successful research effort started in 1978, in which two future Nobel laureates, von Klitzing and chemist Gerhard Ertl, were working and also where the first scanning tunneling microscope (STM) was introduced in Munich.[15] In the 1990s, these approaches would probably not have been called surface chemistry or surface science but nanotechnology (Mody 2004, 364).

13.3 Von Klitzing's Nobel Prize: The Discovery of the Quantum Hall Effect

> The discovery of the Quantized Hall Effect (QHE) was the result of systematic measurements on silicon field effect transistors—the most important device in microelectronics. Such devices are not only important for application but also for basic research. (von Klitzing 1985, 316)

The discovery of the QHE by von Klitzing in 1980 is a milestone in the field of experimental semiconductor physics. Its origins go back to the early 1970s with the intensifying experimental work on quantum effects in two-dimensional electron gases: "The first indications for the QHE were already obtained by von Klitzing in 1974, when he measured the magnetoresistance of a MOS Hall bar between the current contacts and observed a plateau" (Landwehr 2003, 2). Von Klitzing was appointed to be professor at TUM while he still was at the high magnetic field facility of the Max Planck Institute for Solid State Research in Grenoble, doing the decisive Hall measurements.[16] But the discovery of the QHE was not just a Munich or Bavarian story that then resulted in later discoveries in Grenoble. At that time, many research groups worldwide, especially in Japan, were interested in localization phenomena and conducted Hall resistance and magneto transport measurements.[17] In 1977, Japanese scientist and theoretician Tsuneya Ando from the department of physics of the Tokyo Institute of Technology was a

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[15] Interview with Behm, 16 December 2008.

[16] Koch strongly supported the appointment of von Klitzing as professor to be able to conduct experiments like Gottfried Landwehr in Würzburg.

[17] In 1879, Edwin Hall discovered that if an electric current in a conductor flows through a magnetic field, that field exerts a separating force on the charge carriers so that an electrical field builds up perpendicular to the magnetic field and to the current's direction. If the Hall Effect is produced in a two-dimensional semiconductor at low temperatures, a series of steps appear in the Hall resistance as a function of magnetic field instead of a monotonic increase. Von Klitzing realized that the Hall conductivity of a two-dimensional electron system is quantized in whole fractions of e^2/h (Thouless 1984, 147; Landwehr 2003, 9). This Quantized Hall Effect is taken as a natural constant to define the Ohm resistance with an uncertainty better than 10^{-6}; it does not depend on the material of the samples.

visiting scholar at TUM. In Munich, he introduced the possibility of constructing MOS Si probes with a multicontact geometry as they were produced in Japan. He also had data from Shinji Kawaji, a semiconductor physicist from Gakushuin University Tokyo, who measured—besides the normal longitudinal resistance—the so-called transversal Hall resistance with the now-famous von Klitzing steps. According to Koch, Tsuneya Ando pointed to the relations between these steps and the phenomenon of localization.[18] Koch remembers clearly that his Japanese colleague interpreted these Hall resistance steps as a mathematical artifact that was founded in the phenomenon of localization of electrons and thus saw these only as approximate quantum measurements. This was the general tendency of the early discussions about the Hall steps before von Klitzing's discovery. Research groups in Japan that conducted Hall measurements in semiconductors could also show plateaus in the Hall resistance values. But it was von Klitzing who realized in the early 1980s in Grenoble that these energy plateaus are quantized stepwise with very high precision.

The silicon MOS-structure that was later used by von Klitzing in the high magnetic field facility of the Max Planck Institute for Solid State Research in Grenoble was conceived and designed at TUM and then produced by Siemens. This so-called MOS Hall bar, a high-quality MOSFET with high electron mobility could only be provided by industrial research labs.[19] Koch's research group designed the masks for the lithography process of such multicontact probes at Siemens. The probes that then were used for the measurements in Grenoble resulted from these. In an interview, Koch stated:

> That structure was built for us, the way Hitachi did it for their researchers and neighboring universities. And with that we gained a basic insight. Von Klitzing's true merit was not in the steps in the diagram—the Japanese scientists had them already and I had Japanese visitors here who showed me this data and so on. It wasn't the insight that there were steps in it. One of our theoreticians was sitting in the room next door. Back then he said: forget about the steps, that is a mathematical artifact. But von Klitzing realized: wait a second, there is a natural constant in there. And the real meaning of his discovery was to point that out to an infinite number of places behind the decimal point—no one has shown yet, how many places it is. Or

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[18] Personal communication between Koch and the author, 6 June 2011.

[19] The other samples and control measurements were conducted by Michael Pepper at Cambridge's Cavendish laboratories with samples produced by the Plessey company (the Munich group worked with Siemens). The Hall bar structure is a sample configuration that measures the different components of the conductivity tensor. So in a MOS probe, there are four additional contacts besides the usual source and drain contacts to measure electrical resistance and the Hall voltage.

> it's so many places that you don't even have to ask. It's a natural constant that emerges from this experiment. That's von Klitzing's true merit.[20]

It is the merit of von Klitzing to have realized that:

> [T]he Hall resistance at particular, experimentally well-defined surface carrier concentrations has fixed values which depend only on the fine-structure constant and speed of light, and is insensitive to the geometry of the device.[21] (von Klitzing, Dorda, and Pepper 1980, 494)

Gerhard Dorda from Siemens, who wrote the decisive paper together with von Klitzing, did not just provide the samples; he himself conducted research on quantized phenomena in the early 1970s. Dorda was confronted with measurements in MOSFETs that could not be understood with the band structure model. He had to assume quantum states to explain the transport behavior in inversion layers underneath the surface of semiconductors:

> The rapid development of MOS devices with traceable surface characteristics has led to measurements of the physical properties of semiconductor inversion layers. In almost all considerations it was supposed that the band structure of the bulk is also applicable to the surface. Schrieffer has pointed out that in the interpretation of transport properties of inversion layers a quantization of carrier motion perpendicular to the surface has to be considered. (Dorda 1971, 2053)

In 1972, Dorda first presented his results at an international conference in Hawaii, where he also met Koch, who went to Munich within a year.[22] In contrast to Dorda, Koch and his team at TUM, as well as von Klitzing, had more freedom to

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[20] "Koch: Diese Struktur wurde für uns geschaffen, genau so wie Hitachi für ihre Forscher das machten und benachbarte Hochschulen. Und daraus wurde dann die Grunderkenntnis gewonnen. Der wahre Verdienst von von Klitzing sind nicht die Stufen, die haben die Japaner vorher gehabt und ich hatte japanische Besucher hier, die mir diese Daten zeigten und so was. Es war nicht die Erkenntnis, dass da Stufen drin sind. Wir hatten einen Theoretiker, der saß im Nebenraum. Der sagte mir damals: Vergiss die Stufen, das ist mathematisches Artefakt. Von Klitzing hat da erkannt: Augenblick mal, eine Naturkonstante steckt darin. Und die Bedeutung seiner Entdeckung war wirklich darauf hinzuweisen, dass bis auf unendlich viele Stellen hinterm Komma, noch niemand gezeigt hat, wie viele Stellen es sind. Oder es sind so viele Stellen, dass man gar nicht danach fragen muss, ist das eine Naturkonstante, die aus diesem Experiment raus kommt. Das ist der wahre Verdienst von von Klitzing." (Interview with Koch, 15 June 2009).

[21] Koch explained these details in written form to the Nobel committee and also requested the inclusion of the Japanese colleagues.

[22] Koch was appointed to TUM on 1 December 1972.

deal with quantum phenomena that were not the focus at Siemens. Dorda himself said this kind of research was tolerated but not really motivated by Siemens, a kind of "submarine science" (*"U-Boot-Tätigkeit"*) that takes place unobserved and then suddenly pops up to the surface with a new discovery. Dorda explains:

> That was always my motivation: to deal with fundamental questions. I pursued that along the way and also within the universities. And they [at Siemens] acknowledged that in so far as they said, he is a typical scientist/researcher. We called that submarine work; it remained underground, nobody knew that this was happening, because it was not condoned. And then, when I was successful, I resurfaced, so to say […]. They [the managers at Siemens] said I was a typical scientist of this kind. After the Nobel Prize, I of course got absolute freedom to do whatever I wanted. I was the last one. They said I was the last Mohican at Siemens, because before this time, before they started working with semiconductors at Siemens, they had also discovered the III-V semiconductor at Siemens. That was in Erlangen.[23]

Von Klitzing's success was seen as a triumph of experimental physics (Landwehr 2003, 11; Thouless 1984, 147) and also as a result of basic research: "Not applied, but basic research led to a very substantial improvement of the accuracy of the resistance standard" (Landwehr 2003, 12). Interestingly, the first jury member for *Physical Review Letters* initially refused von Klitzing's decisive paper for publication since it did not contain enough theory (Landwehr 2003, 15). In fact, von Klitzing's discovery was possible without direct theoretical prediction and was based on experimental laboratory work with refined methods and measurement techniques. Nevertheless, this kind of experiment with quantized phenomena is based on quantum theory and a creative interaction between experiment and quantum theories about the behavior of electrons in semiconductors.

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[23] "Dorda: Das ist mein inneres Bestreben, immer so Grundlagenfragen zu erörtern, ich habe das nebenbei weiter getrieben und eben über die Universitäten. Und sie haben das dann anerkannt, insofern dass sie sagten, na ja, ich bin der typische Forscher. Man nennt das U-Boot-Tätigkeit, also im Untergrund, ohne dass jemand was, weil es nicht gebilligt war, wusste. Und wenn ich dann erfolgreich war, bin ich wieder aufgetaucht sozusagen. Und die sagen, wenn man das so macht, ist es auch okay. Es ist also tolerierbar. Und ich bin ein typischer Forscher dieser Art, haben sie gesagt. Und dann, also nach dem Nobelpreis selbstverständlich, habe ich dann absolute Freiheit bekommen. Ich konnte dann quasi machen, was ich wollte. Ich war der Letzte. Sie sagen mir, ich bin der letzte Mohikaner bei Siemens, weil noch vor dieser Zeit, also als sie angefangen haben bei Siemens, mit Halbleitern zu arbeiten, da haben sie bei Siemens ja auch die III–V-Halbleiter entdeckt. Das war doch in Erlangen." (Interview with Dorda, 17 June 2008).

13.4 The 1980s: Founding the Walter Schottky Institute

The founding of the WSI in 1988 is closely related to the prestigious event of von Klitzing's Nobel Prize in 1985. It has to be seen in the context of an increasing competition for the best scientists in a global microelectronics race. The idea of such an interdisciplinary center to facilitate knowledge flow between universities and industry was formulated by Abstreiter and Ploog after a visit to Japanese research facilities that were equipped with MBE systems.[24] In comparison to industrial research labs, the WSI pursues rather basic and long-term perspectives:

> Kehrt: But what you do here is science, not engineering science?
>
> Abstreiter: That's in-between. We also have engineering, but not in the sense of classical engineering, we rather look for new principles.[25]

The WSI holds a strategic middle position between basic science and technology development that did not exist previously. However, in the 1990s, following growing competition in the globalized semiconductor industry, Siemens—like many other big companies—cut down its research department and focused on shorter innovation cycles. That was the time when basic research in semiconductor physics lost contact with industry, and nanotechnology was entering the focus of such scientists as Kotthaus, Koch or Abstreiter, who had been working at the quantum level with semiconductor "nanostructures" since the early 1970s:

> Kehrt: There was a move away from microelectronics as a key technology?
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> Koch: Yes.
>
> Kehrt: In these research fields that were previously closer to microelectronics?
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> Koch: That's it. Right. That's what Abstreiter and I and Kotthaus did in the early 1970s until the 1980s, but in the middle of the 1980s that began to diverge. And then in the 1990s, when the companies also withdrew; that's when such nano-institutes did things that were far from real applications.[26]

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[24] Interview with Abstreiter, 22 November 2007.

[25] "Kehrt: Aber was sie hier machen, das ist Naturwissenschaft, keine "engineering science"?
Abstreiter: Das ist zwischendrin. Wir haben auch "engineering", aber im Sinn nicht das klassische "engineering", sondern wirklich neue Prinzipien." (Interview with Abstreiter, 22 November 2007).

[26] "Kehrt: D.h. es gibt so eine Wegorientierung von der Mikroelektronik als Schlüsseltechnologie?
Koch: Ja.

In the 1990s, undirected basic research in semiconductor physics lost ground and made new orientations and strategies necessary (Angel 1994, 3; Gerybadze, Meyer-Krahmer, and Reger 1997, 20; Hack 1998, 102). Obviously, the general consensus for basic research as it was practiced in the Cold War—especially in fields related to the military, such as semiconductor physics—vanished, and the rise of the life sciences forced semiconductor physics to reorient its research strategies. Furthermore, there was a general crisis in the German innovation system after reunification (Caspar 2007, 76; Nusser 2006, 66–67; Cuhls, Uhlborn, and Grupp 1996, 53; Bundesbericht Forschung). The need for a new visionary technology seemed to be fulfilled by the promises of nanotechnology (Bachmann 1998). The German Ministry for Science and Education (BMBF) was well aware that the Clinton presidential administration in the United States was creating a new nanotechnology strategy and started its own German initiative. The main reason was not to fall behind at the beginning of radical new technological developments but to support the possibility of future key innovations.[27] Von Klitzing predicted a blossoming of nanoelectronics based on future quantum devices before the ultimate physical limits of miniaturization were reached. He criticized the reduction of basic research and the dominance of economic restraints, and he argued for long-term perspectives in—and basic research on—quantized phenomena in semiconductors (von Klitzing 1995, 26).

13.5 Munich Nanoscience Networks

The perception of nanotechnology as a new scientific trend began in the late 1980s and early 1990s when new developments in the field of semiconductor physics allowed for designing nanostructures for basic science as well as for future technologies:

> It is anticipated that the independent technologies will be married in the next decade, with consequent production of structures that are atomically engineered in all three dimensions to nanometer design rules. (Kelly 1987, 264)

Due to the advancement of research and materials processing technologies, it became possible to artificially design structures that confined the movement of elec-

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Kehrt: In diesen Forschungsfeldern, die vorher näher an der Mikroelektronik dran waren.

Koch: So ist es. Ganz richtig. Also das, was Abstreiter und ich und Kotthaus in den frühen 70er-Jahren machten und in die 80er hinein […]. Mitte der 80er fing sich an, das zu divergieren. Und dann in den 90ern, als die Firmen sich zurückziehen, dann haben solche Nanoforschungsinstitute ganz andere Dinge getan, die weit weg sind von der wirklichen Anwendung." (Interview with Koch, 15 June).

[27] Interview with Secretary of BMBF, Wolf-Michael Catenhusen, January 2007.

trons in one and zero dimensions (Kuchar, Heinrich, and Bauer 1990; Reed 1993, 118). “Top-down” techniques, such as electron beam lithography, were coupled with “bottom-up” approaches from chemistry and the life sciences to artificially design new materials, such as nanotubes and quantum dots, that do not exist in nature: “The study of quantum dots is the result of tremendous advances in molecular beam epitaxy, dry processing, and advanced lithography” (Smith 1990, 10). Also, the STM (invented in the early 1980s by Heinrich Rohrer and Gerd Binnig at IBM) was identified as a key instrument of nanotechnology. It allowed experiments with self-organizing processes of molecular clusters in very different fields of research (Hennig 2011). Already in 1988, an article in *Nature* assumed the possibility of atomic engineering with the help of the STM (Pethica 1988, 301). In a special section of *Science* entitled “Engineering a Small World” in 1991 (Science. Special Section 1991), all topoi that constitute the future nanodiscourse were formulated: the idea of engineering atoms and molecules, the visions of Feynman and Eric Drexler, the processes of self-assembly, the use of biological materials, as well as the key role of the STM. More specifically in the field of semiconductor physics, nanotechnology was associated with the possibility to conduct experiments with quantum dots, nanowires and nanotubes (Corcoran 1991, 78). However, the relabeling of these well-established research fields under the heading and hype of nanotechnology was motivated primarily by science policy considerations, when the Clinton administration started its National Nanoinitiative in 1998, and thus research at the nanoscale became very attractive because of its association with a future key technology.

The founding of the Munich CeNS in 1998 was related to the emerging public nanohype. At the time when the national nanotechnology strategies were formulated, Kotthaus, together with colleagues from the experimental physics department of LMU, quickly realized the potentials of research at the nanoscale and came up with the idea of a center for nanoscience. This local nanoscience network tries to meet the new transdisciplinary, media and economic challenges of science at the turn of the twenty-first century. Obviously, the freedom of scientists to play with molecules beyond established disciplinary boundaries requires other, more flexible forms of interaction and strategies.

Publication statistics show that, at an international and a national level, Munich has a leading position and is a good example for studying general trends in nanotechnology. Research in nanotechnology is mainly taking place at universities (Kostoff, Koytcheff, and Lau 2007, 576) and basic research dominates (Heinze 2006, 113). Also in Munich, the two major universities—LMU and TUM—dominate nanopublications, while only 6% can be located in industrial research labs.[28] The fact that local nanotech endeavors are rooted in semicon-

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[28] Result of a bibliometric study of Munich nanotech networks (Kehrt 2011).

ductor physics can be explained by the high density of microelectronics in the Munich area. Siemens especially was the main employer for physics students and had a strong influence on the field and career patterns of semiconductor physics. The main protagonists of the new nanotechnology networks stemmed from the semiconductor physics community. The twenty founding members of CeNS belong to the field of experimental physics, and 65% of the involved professors are located in semiconductor physics or biophysics. This orientation of semiconductor physics toward nanotechnology at the turn of the century is confirmed by an analysis of the leading German journal *Advances in Solid State Physics*.[29]

In the context of quantum mechanics, it is interesting that the idea for such a bottom-up nanoscience network is related to quantum electronics and its organizational structures in the United States. The founding father of CeNS, experimental physicist Kotthaus, refers to the US Center for Quantized Electronic Structures (QUEST) that he knew through his long contacts at the University of California in Santa Barbara, where he had studied in the 1970s and where Kroemer has been working since the late 1970s. In an interview with the author on 19 January 2006, Kotthaus remembers:

> The idea for such a center, frankly speaking, is something that had moved me since the beginning of the 1980s, when I saw how such centers were created in the USA. QUEST was certainly a role model. QUEST meant "Quantum Electronic Structures" and was a close cooperation among scientists at UC Santa Barbara that was truly based on common interests. Back then, I was in Santa Barbara almost every summer for a month or two.[30]

Such problem-oriented, interdisciplinary centers were pushed in the 1980s to facilitate cooperation between disciplines (and between universities and industry) (Thompson Klein 1992, 36). Nevertheless, the strategies of successful and influential scientists like Kotthaus and Abstreiter changed in the 1990s. Semiconductor physics lost its immediate relationship to industry and had to look for new alliances and visions. Now basic research—even at universities—needs a stronger legitimacy in utility. The university itself has turned into a place for

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[29] A database search of all nano composites in titles or abstracts shows that 35% were written in the years 1990–1999 and 65% during 2000–2008. The word "nanotechnology" appears only since the year 1999, while word composita with "nano" appear earlier.

[30] "Kotthaus: […] das heißt die Idee so was zu machen, ehrlich gesagt, hat mich an sich bewegt seit Anfang der 80er-Jahre, als ich gesehen habe, wie in den USA solche Zentren entstanden; Vorbildfunktion hat für mich das QUEST gehabt. Das QUEST hieß eben 'quantum-electronic structures' und war eine Zusammenarbeit von Wissenschaftlern in Santa Barbara, die eben wirklich auf gemeinsamen Interessen beruhte und ich war damals praktisch, ja, jeden Sommer ein bis zwei Monate in Santa Barbara […]." (Interview with Kotthaus, 19 January 2006).

entrepreneurial science. This strategy began with Wolfgang Hermann's appointment as president of TUM in 1995. According to Hermann's new entrepreneurial philosophy, such traditional disciplines as physics, biology or medicine should have a closer relationship to technology (W. Hermann 2006, 931). This new orientation can be observed within Munich nanotech networks. Doctoral students learn to address the media, defend the usefulness of their endeavors and are encouraged to found spin-off companies, such as Attocube, Nanion or Nanotools. These Munich nanotech firms often directly result from PhD work in semiconductor physics.

Despite this new entrepreneurial spirit promoted by nanotech spin-off companies, the research conducted at universities has no direct link to the market, is far away from direct application and follows rather long-term perspectives. Indeed, there are few chances for direct technological development stemming from nanoscience research. This current state of affairs was already realized when nanotechnology was identified as a new research field in the early 1990s. Nanotechnology provides "wonderful tools for science," but it does not offer clear economic or technological perspectives (Ball and Garwin 1992, 766). For example, Don Eigler, who gained public attention through his first manipulation of single atoms by writing "IBM" with xenon atoms, is rather critical of overrated hopes of utility and application:

> However, on the nanometer scale, we simply do not have a robust, practical method for mass production. […] Nanotechnology is now in the single device invention stage, and there is no clear vision about how one could practically integrate devices in a second stage. (Brus and Eigler 1994, 273–274)

This situation, that nanotechnology was rather in the stage of basic research, did not change, although in Munich a dozen small university spin-off companies such as Attocube, Nanion or Nanotools were founded. A closer look reveals that these "nanotechnology" enterprises still do produce high precision scientific instruments and analytic tools to enable basic research at the nanoscale.[31]

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[31] There are a dozen firms founded by students of LMU that are conducting research in experimental semiconductor physics and biophysics. These firms use the "nano" label to promote their equipment for scientific research. Nanotools was founded in 1997 by students of Kotthaus. Using the atomic force microscope, they realized that scanning required much stronger tips and thus constructed these special tips to improve research with the instrument (Interview with Bernd Irmer, Founder of Nanotools, 10 March 2009). Attocube was founded in 2001, also by scientists of the Kotthaus group. Attocube produces high precision piezo-engines for scientific instruments working in high magnetic fields or in ultra-high vacuum conditions (Interview with Attocube—Prof. Karrai and Dr. Haft—28 January 2009). Nanion, founded in 2002 by Niels Fertig, also a former doctoral student of Kotthaus, uses the patch clamp method to develop labs-on-a-chip. In 1991, Bert Sakmann received the Nobel

Therefore, the reference to university spin-offs and future key technologies is a sign of a new scientific culture that now already positions economic thinking within the realm of the university. But this does not imply that basic research and traditional modes of knowledge production are completely abandoned. In regards to Forman, I argue that the promotion of spin-off companies and the reference to entrepreneurial PhD students is a sign of an ideological shift. While in former times students of semiconductor physics went directly to Siemens, these big companies no longer offer career opportunities. In this context, the formation of nanotech-networks has real effects on career patterns and the strengthening of local science clusters. The nanohype allocates money from the government and supports projects that identify themselves as being related to nanotechnology. In the beginning, CeNS was an informal network to bring scientists together and exchange ideas.[32] Then, with the resulting success of the excellence initiative NIM, money from the government was turned into new careers, professorships, and infrastructures.[33]

13.6 Nano-biotechnologies. New Forms of Interdisciplinary Cooperation?

In previous sections, Munich nanoscientists were located in the field of experimental semiconductor physics. There are clear continuities from the 1970s to the 1990s related to quantum phenomena of electron transport in low dimensional physical systems. Yet, there are also significant new transdisciplinary developments and changes that cannot be explained by these research traditions. In this regard, Munich nanoscientists emphasize their close cooperation between semi-

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Prize for this method to measure electric currents in ion channels between cells to understand the communication between cells.

[32] At this point, one could critically ask why or if this rather normal science communication and exchange of new ideas was not possible within the traditional disciplinary and institutional setting of the university.

[33] Three professorships, those of Alexander Holleitner, Thorsten Hugel and Christina Scheu, were fully sponsored by money from the NIM. Also the following research projects received funds from NIM: Prof. Philip: Tinnefeld Biophysics (LMU); Prof. Lukas Schmidt-Mende: Hybrid/ Colloidal Nanosystems (LMU); Prof. Dieter Braun: Physical Aspects of Hybrid Nano-Bio Systems (LMU); Prof. Don Lamb: Live Cell Imaging (LMU); Prof. Scheu: Transmission Electron Microscopy of Nanostructures (LMU); Prof. Alexander Högele: Nanophysics (LMU); Prof. Ulrich Gerland: Theoretical Nanophysics (LMU); Prof. Bettina Lotsch: Synthetic Chemistry (LMU); Prof. Ulrich Schollwöck: Theoretical Physics (LMU); Prof. Holleitner: Nano-technology and -materials (TUM); Prof. Friedrich Simmel: Bioelectronics (TUM); Prof. Hugel: Molecular Machines (TUM); Prof. Tim Liedl: Bio Interfaces (LMU). Also the WSI was able to enlarge its research facilities and build a new "nanoscience building" with money from the NIM (Peter Sonntag, general manager of NIM, email communication, 23 October 2009).

conductor physics and the life sciences. In an interview on 20 November 2006, Heckl stated:

> In fact that would not have been possible twenty years ago, that someone like Kotthaus—a semiconductor physicist working in clean rooms—suddenly started to touch DNA. He would have never done that in the past. He would have said: "My lab will get contaminated by that kind of organic stuff." But a lot happened back then, and especially here in Munich with its research environment, because obviously [. . .] or maybe I'll put it the other way around [. . .] that is certainly a reason why we have now, for example, become an "Exzellenzuniversität" (Excellence University).[34] Because in many fields things have changed, moved forward.[35]

According to Munich nanoscientists, a characteristic trait of the Munich nanoscience landscape seems to be the close cooperation between life sciences and semiconductor physics. In fact, at TUM, Erich Sackmann established a school of biophysics and his pupils introduced the STM and atomic force microscope (AFM) to study processes of molecular self-assembly that were then identified as being an integral part of so-called nanotechnology (Mody 2004; Hennig 2011). In the 1990s in Munich, a large biotechnology cluster also emerged near the village of Martinsried (Heßler 2007, 167–187). However, it is not clear in what sense there are direct interdisciplinary cooperations between biotechnology, biochemistry and genetics on the one hand and semiconductor physics and surface science on the other. Do semiconductor physicists really cooperate closely with scientists from the life sciences in concrete interdisciplinary nanoscience research projects?

While early bibliometric studies (Meyer and Person 1998, 203) seem to confirm the interdisciplinary nature of nanotechnology, others rather doubt this claim (Heinze 2006, 111; Schummer 2004, 461). Indeed, a closer look at the Munich

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[34] The German University Excellence Initiative was a national competition between universities for the prestigious title "Excellence University." This official campaign started in 2005 and aimed at funding cutting-edge research. LMU received the title Excellence University, and the local nanoscience network became the excellence cluster known as Nanosystems Initiative Munich (NIM). LMU Presseinformation 13 October 2006, Entscheidung im Exzellenz-Wettbewerb. "LMU ist Spitzenuniversität", http://www.nano-initiative-munich.de, accessed 15 October 2007.

[35] "Heckl: Also das hat es eben vor 20 Jahren nicht gegeben, dass jemand wie der Kotthaus, der also ein Halbleiterphysiker mit Reinraumlabors ist, plötzlich eine DNA anlangt, ja. Das hätte der nie gemacht früher. Der hätte gesagt: "Meine Kammer wird verunreinigt durch so ein organisches Gezeugse." Also, da ist schon viel passiert auch, aber gerade auch bei uns natürlich auch in München in dem Umfeld, weil natürlich, oder ich sage es jetzt mal andersrum und das ist mit Sicherheit auch ein Grund, warum wir jetzt, zum Beispiel, eine Exzellenzuniversität geworden sind. Weil in vielen Feldern sich etwas bewegt hat, was vorwärts gegangen ist." (Interview with Heckl, 20 November 2006).

nanoscience landscape and their transdisciplinary cooperations show that experimental physics is open to life sciences methods and approaches, but there is no real interdisciplinary cooperation between different disciplines.[36] Therefore, I argue that the boundary object of nanotechnology—with its rather vague and indefinite character—opens up new venues and spaces for research beyond disciplinary boundaries, but it does not necessarily lead to strong interdisciplinary interaction or the emergence of nanotechnology as a distinct scientific discipline.

Simmel's experimental work with DNA is an example for new transdisciplinary approaches in the direction of nanobiotechnology or synthetic biology. In his PhD, Simmel analyzed quantum dots in Kotthaus's research group (Simmel 1999). Then as a postdoctoral researcher, he went to Bell Labs in New York, in a period when cutting-edge basic research was still promoted there. In Bernward Yurke's research group, they developed a so-called nanotweezer, based on DNA strings, that can open and close and thus possibly lead to the foundation of new principles for future molecular scale devices (Yurke et al. 2000). In the beginning of this new research, the hopes were high to be able to "construct simple, machine-like nanomechanical devices" (Simmel and Dittmer 2005, 285). They used DNA to create new artificial nanosystems that do not exist in nature. Characteristic of Simmel's work is the radical change of experimental systems. Simmel explains:

> I have to say, frankly speaking, that the production of semiconductor chips was no longer any fun after a couple of years. I think I don't like that clean room work very much. And then in 1998, for example, new work was published by Uri Sivan[37] who proposed for the first time to use radically new methods of production based on the principle of molecular self-organization and biological material. And that fascinated me somehow and I thought, if I want to stay in this field at all, then I want to work in this biological self-organization direction.[38]

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[36] I distinguish interdisciplinarity from transdisciplinarity. While transdisciplinarity implies the transcending of disciplinary boundaries, interdisciplinarity involves a stronger form of cooperation, where scientists from different disciplines work together on the basis that each partner has to learn the presupposition of the other's discipline to come up with a new project, idea or technological device (Thompson Klein 2001; Schummer 2004, 11; Kehrt and Schüßler 2010, 38).

[37] In 1998, Sivan and his colleagues from the University of Haifa used DNA as a template to attach a silver wire to construct an electric circuit (Erez Braun et al. 1998).

[38] "Simmel: Und da muss ich aber sagen, dass mir ehrlich gesagt diese ganze Produktion der Halbleiterchips nach ein paar Jahren keinen Spaß mehr gemacht hat. Ich mag diese Reinraumarbeit nicht besonders, glaube ich. Und da kamen dann im Jahr 98 Arbeiten raus von Uri Sivan z.B., wo die Idee vorgebracht wurde, dass man vielleicht ganz neue Produktionsmethoden nutzen könnte, die auf Selbstorganisation und biologischem Material basieren. Und das hat mich irgendwie fasziniert

Simmel, who holds a chair of bioelectronics at TUM, no longer works with quantum dots and computer chips, but with DNA and methods from the life sciences. That is a radical step beyond his original field of research. He does not operate in the clean room any more. His laboratory, which moved into the new nanoscience building at the WSI looks more like a biotechnology lab. The aim is to find new ways of handling and using DNA as a building block for future "DNA machines," DNA computers as a template for materials synthesis or intelligent drug delivery systems. For Simmel, DNA is not just a carrier of information and a basic unit of life that scientists try to understand; it also has interesting physical, electrical and mechanical properties, it is something to "play around" with and to see how artificial molecular machines behave with their abilities to host other molecules or to act as semiconductors.

Biochemist Nadrian Seeman has influenced this nanobiotechnological research field since the 1980s (Seeman 1999, 11; 2002, 53–84; 2003, 33–37). He is interested in the functional properties of DNA to create radically new systems and DNA structures, so-called Nano-Origami, with potential technological applications:

> For the past half-century, DNA has been almost exclusively the province of biologists and biologically-oriented physical scientists, who have studied its biological impact and molecular properties. During the next 50 years, it is likely they will be joined by materials scientists, nanotechnologists, and computer engineers, who will exploit DNA's chemical properties in a non-biological context. (Seeman 2003, 431)

But experts doubt that DNA will ever be able to compete directly with silicon-based technology. Therefore, such far-reaching technological visions of DNA as a building block for future computers has no direct meaning for technology development and is more a question of basic research practiced by university-based scientists. As Simmel points out, these questions are rather basic and a DNA computer is not realistic so far:

> And now they want to bring these two worlds together. That is incredibly difficult in a technological sense, and maybe even unrealistic. So we have to ask in what direction that should go. On the other hand, I still think that for some kinds of things this is useful, if you want to solve some basic questions. But basically when you say you want to combine semiconductor technology and biotechnology

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und ich habe mir gedacht, also wenn ich überhaupt in dem Feld bleibe, dann möchte ich in diese Bioselbstorganisations-Richtung." (Interview with Simmel, 30 September 2008).

> for example with a lab on a chip or biosensors or such things. I see perspectives there, because you are interested exactly in the interface of the two worlds, so to speak. But if the question is, whether it is realistic to build a Pentium Processor out of DNA, then I would say this isn't realistic.[39]

In the early 1990s, researchers hoped that DNA would one day replace silicon as the basis for a new generation of computers,"scientists have realized that there are numerous problems inherent in DNA computing and that they would have to live with their silicon-based computers for quite a while yet" (Parker 2003, 7).

Despite these new experimental practices that combine new methods from the life sciences with approaches and research questions from experimental physics, there are only few signs of close interdisciplinary cooperation. In most of the cases, scientists from life science departments are not really interested in what their physics colleagues try to do with DNA:

> Simmel: I think that the influence of biophysics was very important in Munich because biophysics is interdisciplinary in its roots. And that was also an important influence in CeNS and then later NIM concerning research topics that were chosen. Because ultimately biophysics works at the border to biochemistry. But in contrast, there were almost no direct influence from biochemistry or biology on the nano-developments here in Munich, as far as I can see.
>
> Kehrt: So strongly oriented towards physics?
>
> Simmel: Yes.
>
> Kehrt: Physics is opening up, while chemistry remains within its classical structures?
>
> Simmel: Exactly. Here with CeNS and NIM there is almost no participation with chemistry and almost none with biology […]. Sometimes they [the biochemists] say we are really dealing with the important biological questions while what you are doing is simply playing around. So in the end, in their view, what I do is of

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[39] "Simmel: Und jetzt will man diese zwei Welten zusammen bringen. Das ist an manchen Punkten einfach technisch wahnsinnig schwierig und vielleicht auch unrealistisch. Also da muss man sich fragen, in welche Richtung das gehen soll. Umgekehrt glaube ich aber schon, dass man es für manche Dinge brauchen kann, zumindest als einerseits um Grundlagenfragen zu beantworten. Aber eben dann wenn man sagt, die Verknüpfung aus Halbleitertechnologie und DNA oder Biotechnologie findet meinetwegen lab-on-a-chip oder im Biosensorikbereich oder solche Sachen. Da sehe ich durchaus Perspektiven, weil da ist man ja genau an diesem Interface sozusagen interessiert zwischen den beiden Welten. Wenn es aber jetzt darum geht, ist es realistisch, mit DNA einen Pentium-Prozessor zu bauen, dann würde ich sagen, dass es nicht realistisch ist." (Interview with Simmel, 30 September 2008).

> course purely playing around. In this sense nanoscience in Munich remained within physics, perhaps because in physics it is more easily accepted that scientists play around without any clear goals. But I have to say that is different in the US. There people like me almost always work in interdisciplinary centers with a strong participation of biochemistry and chemistry, which is quite remarkable.[40]

Only a few experimental physicists like Simmel adopt methods from biochemistry and leave their discipline far behind without really closely cooperating with their neighboring disciplines from the life sciences. There is no direct cooperation or interdisciplinary exchange with scientists from the life sciences. Doctoral students from the life sciences also hesitate to work in physics departments because of their strict career patterns. So if we want to identify the trading zones between physics and the life sciences, it is the laboratories of experimental physicists like Simmel in which knowledge is transferred from the life sciences in order to use DNA as an experimental system to build artificial devices and lay the foundations of future DNA computing. This is one of the rather seldom cases in the history of physics where physicists adopt and incorporate approaches from other disciplines (Kragh 1999, 445). In this instance, the boundary object of nanotechnology facilitates knowledge transfer and the sometimes radically new methods beyond disciplinary boundaries that obviously would have been difficult to pursue within the framework of semiconductor physics.

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[40] "Simmel: Ich glaube, dass in München der Einfluss der Biophysik sehr wichtig war, weil die Biophysik von der Anlage her interdisziplinär ist und das war ja auch bei CeNS und dann später NIM ein sehr wichtiger Einfluss bei Themen, die dann letztlich auch gewählt wurden. Weil die Biophysik zwangsläufig an der Grenze zur Biochemie arbeitet. Im Gegensatz dazu ist aber aus der Biochemie selber oder auch der Biologie kaum ein Einfluss auf die ganze Nano-Entwicklung hier in München gewesen, soweit ich das sehen kann.

Kehrt: Stark physikorientiert?

Simmel: Ja.

Kehrt: Die Physik öffnet sich, während die Chemie in ihren klassischen Strukturen drin bleibt?

Simmel: Genau. Also man hat auch im NIM und im CeNS und was es da alles gibt fast keine Beteiligung von Seiten der Chemie und so gut wie gar keine von der Biologie. […] Manchmal bekommt man auch mitgeteilt, mehr oder weniger, wir kümmern uns um die wirklich wichtigen biologischen Fragestellungen und das andere ist halt Spielerei. Also letztlich, das was ich mache ist auch, aus deren Sicht natürlich, pure Spielerei. Insofern ist es gerade hier in München relativ physiklastig geblieben, vielleicht weil das eben in der Physik eher akzeptiert wird, dass man so ein bisschen rumspielt, ohne ganz klare Zielrichtung. Ich muss aber sagen, das es im Gegensatz dazu in den USA anders ist. Also da ist meine Konkurrenz fast immer in interdisziplinären Zentren, in denen die Biochemie und die Chemie sehr stark beteiligt ist, was ganz kurios ist." (Interview with Simmel, 30 September 2008).

13.7 Conclusion: The Reinvention of Semiconductor Physics

The Munich case study shows that nanotechnology is deeply embedded in the history of semiconductor physics (Choi and Mody 2009; McCray 2007). The story of experimental physicists dealing with the confinement of electrons in two, one and zero dimensional systems—so-called quantum wells, wires and dots—started in the early 1970s with new instrumental practices at the quantum level. This can be shown by looking at the careers of that generation of physicists who finished their doctorates in the 1970s, were of political interest in the 1980s chip war, and then reoriented their research efforts in the direction of nanotechnology at the end of the 1990s. The relabeling of semiconductor physics' research traditions was mainly stimulated by science policy and motivated by extra-scientific interests, such as the necessity to present research in the media, emphasize its economic potential and receive funding from partners outside academia. Therefore, nanotechnology is more a rhetorical tool and ideologically motivated science policy strategy which has emerged to cope with new challenges that university-based research had to face at the turn of the twenty-first century. Scientists have to legitimate their research by referring to the potential utility and innovations that might result from that research without necessarily being directly involved in innovation processes. In this sense, the Munich case study confirms Forman's thesis that there is a primacy of technology in so-called postmodernity. But the story of Munich nanotechnology networks differs from Forman's diagnosis, which dates the changes and shifts toward postmodern science to the 1980s. While the research practices of this field started in the early 1970s, the new and explicit orientation toward nanotechnology appeared in the 1990s—exactly at a point when that field lost its crucial contact to the semiconductor industry. At that time, new developments within the life sciences stimulated new approaches in experimental physics. The boundary object of nanotechnology helped physicists leave the traditions of semiconductor physics behind and adopt new methods and experimental systems from the life sciences. Therefore, nanotechnology—with its strong rhetoric of innovation, its dizzying transgressions and redefinitions of existing institutional frameworks, and its presence in the public sphere—is rather typical for science at the end of the 20th century.

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