

Molecular Disjunctions

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1. NanoTechnoScience

I begin with a thesis that I motivated and explicated elsewhere (Nordmann 2002). Today I am trying to see whether a particular case study can be used to concretize it. To be sure, this isn't anything like testing a hypothesis; at best, it may render the thesis more plausible or substantial.

Nanoscience is not an issue-driven but a site-oriented enterprise. It is neither interested in representations of nature nor in devices that work. Truth/falsity and confirmation/refutation do not serve as its epistemic standards, but epistemic success is also not measured in terms of the functionality of devices. Instead, nanoscience is an exploratory attempt to claim foreign territory and to inhabit a new world or an hitherto unexplored region of the world. Epistemic success is therefore a kind of technical achievement, namely the ability to act on the nanoscale, that is, to see, to move around, move things around, carve your name into a molecule, perhaps initiate productive processes, in other words, to inhabit inner space somewhat as we have begun to inhabit outer space and certainly has we have conquered the wilderness.¹

In this formulation, I speak of nanoscience as opposed to nanotechnology. Roughly speaking, *nanoscale research* concerns molecular architecture, *nanotechnology* aims for the control of this architecture, and *nanoscience* investigates the physical properties that depend on it.² However, if my thesis is correct, it turns out that even nanoscience isn't "science" properly or traditionally speaking, and that even for nanoscience there is no distinction between theoretical representation and technical intervention, between understanding nature and transforming it. More properly I should therefore speak of NanoTechnoScience.³

Let me first present some *prima facie* evidence for my thesis. It comes from the general area of molecular electronics. I will then trace my initial impressions to two culturally distinct research groups that appear to be working on the same problem. Trying to identify in their writings the core commitments of both groups, I find that one of them seeks to identify and solve "fundamental problems" and that this orientation marks a rather fundamental disagreement between the two research groups. However, this disagreement remains entirely implicit and does

¹ The metaphor of inner and outer space was introduced by Sean Howard (2002) in the context of his discussion of military applications of nanotechnology and the need for an "inner space treaty."

² I have adapted this definition from a presentation by Cathy Murphy at the "Reading Nanoscience" workshop, University of South Carolina, August 2002.

³ An "issue-driven" scientific research programme is oriented towards "problems" in the sense discussed by Kuhn (where a paradigm defines the problems of research and where science progresses by solving the outstanding problems or puzzles). It might also be oriented towards a problem like the cure of cancer or the creation of artificial intelligence. As a whole, neither of this holds for Nanoscience (see Nordmann 2002). Instead, the "site-orientation" refers to the claiming and inhabiting of a space. Learning to move around, to act and be productive in this space is no easy task but does not involve "problems" in the previously mentioned senses.

not become subject of debate. Like the lack of scientific discussion on such basic issues as the physical possibility of “molecular assemblers” this lack of debate can be taken as evidence for the thesis of a non-traditional NanoTechnoScience that is not driven by theoretical issues but consists mostly in the settlement and staking of claims on the nanoscale. Nanoscience may be surrounded by controversy but is characterized by its absence.

2. Pressing Problems

In a recent presentation Stan Williams identified a problem that “must be solved”: How are electrons going through molecules? This is a question for molecular electronics. I want to superimpose another question: Why is this such a pressing problem, what makes it so interesting?

There are two kinds of answers to my question. One of these belongs to an issue-driven enterprise, the other characterizes the site-orientation of NanoTechnoScience. According to my thesis, one wouldn’t expect the first of these or one like it offered at all. As we will see, it may not be quite as simple as all that.

This first kind of answer requires for a foil the history of physics at least since the time of Faraday and Maxwell. As Jed Buchwald, in particular, has pointed out, they effected a transformation of physical thinking that prepared the ground also for quantum mechanics (Buchwald 1985). Physical effects were not to be attributed to spatio-temporally localized causes but to space itself which is no longer a mere medium for the transmission of effects as they traverse from their point of origin to a detector, but, in a sense, the space itself can get excited and the change of its state communicated. The propagation of effects therefore does not require particles on which they ride or by which they are transported – all this most famously exemplified in electrodynamics by the propagation of radio waves.

Against this background, the question of how electrons travel through molecules takes on a particular significance. In physical, though not perhaps in chemical terms, it presupposes a curiously old fashioned picture, one according to which a molecule is a discrete kind of body which first is penetrated by and from which then exits another body, namely an electron. Since this electron carries a charge, the passage of the electron through the molecule is associated with a flow of a current and the question by Stan Williams amounts to: Is a molecule qua molecule something like a channel through which current is propagated differently than it is through space on the one hand, through bulk material on the other? If yes, the shape and structure of molecules is physically significant (Woolley 1978). Furthermore, if molecules are conductors of electricity and, so to speak, channel the flow of electrons, do the same laws apply to them as to bulk material? In particular, do they offer a resistance to this flow that serves as a constraint on the amount of current such that too high a voltage would generate so much heat that the molecule ought to melt like a wire that is too thin (compare Ventra et al. 2002, 195)?

All this is terribly crude and simple-minded, quite possibly wrong and certainly does not even begin to reflect the availability of theoretical models that propose answers to Stan Williams’s question. But then Williams was not asking for a theoretical model but for an empirical determination. How *do* electrons move through molecules? Experimental answers to this question can be traced to Mark Reed and Jim Tour's 1997 paper in *Science* on the “Conductance of a Molecular Junction” (Reed et al. 1997). Reed and Tour provide experimental evidence combined with statistical argumentation to distinguish current flow through a single organic molecule from the current that may or may not be propagated in its immediate environment. They use bulk material to create a so-called break junction where two coated gold electrodes are slowly moved together until conductance is achieved (Figure 1).

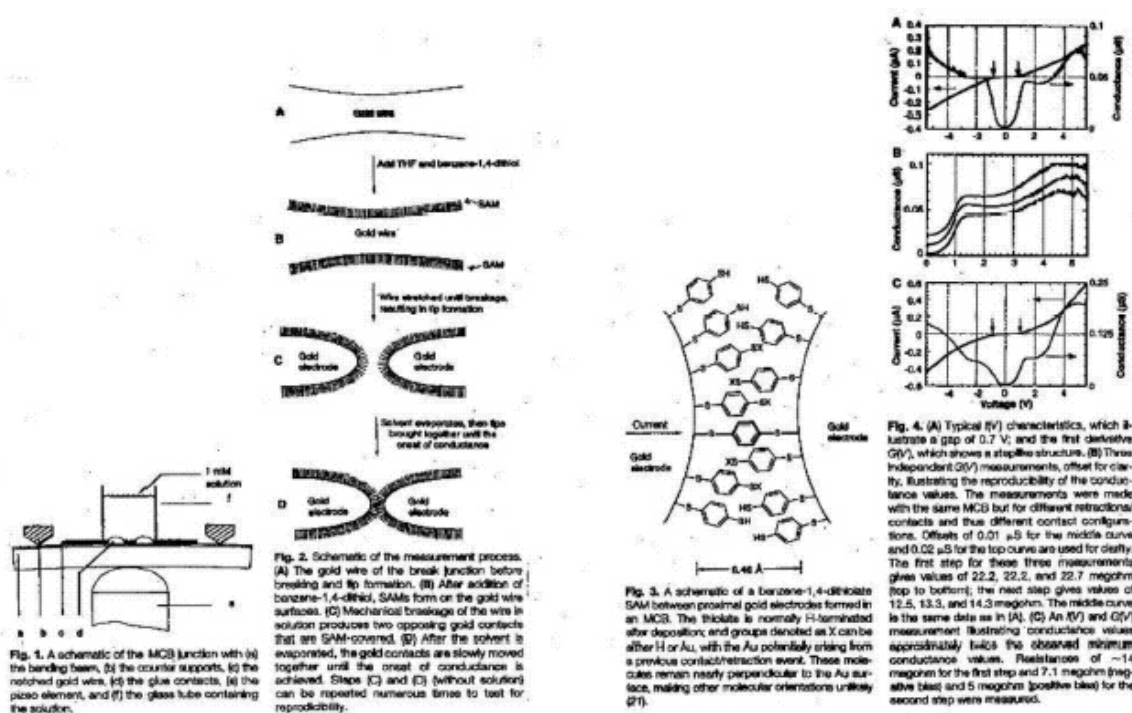


Figure 1: From Reed et al. 1997.

Presumably, this happens exactly when atoms in the coating self-assemble into a first molecular bridge between the parabolically shaped electrodes. Figure 3 illustrates just how a precarious a process this is: There is a lot going on in the space between the electrodes and the experiment is to determine that the measured current is flowing through the single molecule and nowhere else. A first indication of the experiment's success was the fact that the distance of the electrodes when conductance is achieved agreed fairly closely with the calculated length of the single molecule that formed the bridge, namely roughly 8 angstrom. While the authors do not dwell on the amount of current and whether or not it exceeds or agrees with theoretical expectations, they wish to establish that the observed current flows through a single molecule even though they have no direct means of observing the number of molecules that bridge the electrodes. Their paper is based on four measurements, three of which showing very similar values for maximal resistance or minimal conductance (Fig. 4B and A) with the fourth showing approximately half of the resistance and twice the current flow (Fig. 4C). The first three measurements are taken to establish the high reproducibility of the minimum conductance level which, according to Reed and Tour "implies that the number of active molecules could be as few as one." While this is, so to speak, the carefully worded official conclusion of their paper, their statistical interpretation of the fourth measurement suggests a stronger claim.

Figure 4C shows [...] measurements of one singular observation that gave resistances that were approximately half (that is, 0.5) the value of the maximum resistances (using averages, 0.63 and 0.45, respectively). This suggests a configuration of two non-interacting self-assembled molecules in parallel, substantiating the idea that the threshold resistance of a single molecule is ~ 22 megohm [...]. (Reed et al. 1997, 253)

In other words, if one consistently observes a certain amount of current flow and occasionally double that amount, this would seem to confirm the presence of normally one and occasionally two active molecules. In contrast, if there were always anywhere between 5 and 15 active molecules, one would get less consistent results and more smoothly distributed measurements.

What Reed and Tour do not state in this paper, becomes apparent in their June 2000 *Scientific American* article. Here, the caption of an image of a single benzenedithiol molecule acting as a conductor mentions the “relatively large current flow” (Reed and Tour 2000, 90). They elaborate as follows in the body of their text:

It turned out that the resistance of the molecule was in the range of tens of millions of ohms. The Yale researchers also found that the molecule could sustain a current of about 0.2 microampere at 5 volts – which meant that the molecule could channel through itself roughly a million million (10^{12}) electrons per second. The number is impressive – all the more so in light of the fact that the electrons can pass through the molecule only in single file (one at a time). The magnitude of the current was far larger than would be expected from simple calculations of the power dissipated in a molecule [...] (Reed and Tour 2000, 91)

What Reed and Tour call an “impressive” finding generates incredulity among their more skeptical peers. To them, the magnitude of the current would indicate that it is not passing through a single molecule. Such a large current, they might argue, would destroy the molecule just as too large a current will melt a wire. This holds especially for the place of contact where the current is supposed to leave the bulk material and enter a single atom. Reed and Tour recognize this and indicate that their finding is consistent only with a particular account of this process:

The magnitude of the current was far larger than would be expected from simple calculations of the power dissipated in a molecule, leading to the conclusion that the electrons traveled through the molecule without generating heat by interacting or colliding.

We are thus confronted with a classical dilemma – indeed, a text-book dilemma for philosophers of science – where one has to either impeach the integrity of an experimental result or revise one’s theory, for example by adopting Reed and Tour’s somewhat offhanded conjecture.⁴ A dilemma like this may well prompt an urgent call for clarification such as Stan Williams’s insistence that we must solve the problem of how electrons go through molecules.

I have now provided a more or less plausible story for the theoretical interest of Reed and Tour’s researches and their perhaps startling conclusion. Indeed, I have been leading you to expect that their papers in *Science* or *Scientific American* might have appeared under the heading “New finding establishes that electrons travel through molecules without interacting or colliding.” However, this is not how their researches were presented, received, or discussed by the scientific community.

Stan Williams is a senior researcher for Hewlett Packard. He does not distinguish between molecular electronics and molecular computing but confronts major problems in the pursuit of Moore’s law and ever faster, ever smaller computers. He emphasizes that the size-regime of smaller computer chips gives rise to quantum tunneling effects and power leakage, which makes it harder to scale down, leading, for example, to silicon melting. In particular, electron/photon coupling may be responsible for anomalies that need to be understood before he can build the next generation of computers. It is in this context and in view of drastic current changes at low voltages that he calls for an account of how electrons go through molecules (Williams 2002).

⁴ To be sure, Reed and Tour’s conjecture does not answer all of the concerns regarding the flow of current from the electrode into the molecular wire. Compare also Ventra et al. 2002, 195: “This suggests that molecular wires can operate at very large fields without current-induced breakdown. Also, the molecular device at hand [the one from Reed and Tour’s 1997 paper] can carry current densities larger than 10^9 A/cm², i.e., much larger than those allowed in conventional interconnects.”

And this provides the second kind of answer to my about the nature of Williams's problem, this one belonging to NanoTechnoScience as a site-oriented rather than issue-driven enterprise. Along the same lines, when electrical engineer Mark Reed and chemist Jim Tour call "impressive" the number of electrons that pass through their single organic molecule, this is not because of its more or less profound impact on our understanding of nature but because it underwrites their conviction that single molecules can serve as wires in nanoelectronic circuitry. Accordingly, their *Scientific American* article is entitled "Computing with Molecules," their conclusion that electrons pass through molecules without generating heat appears almost as an afterthought or a mere aside, and the previously quoted caption reads in full:

The relatively large current flow bodes well for the ability of molecular devices to work with more conventional electronics. (Reed and Tour 2000, 90)

3. Fundamental Questions

While there has been other work on molecular conductance, there is not to my knowledge anywhere an explicit critical engagement with Reed and Tour's claim that they measured current passing through a single molecule or with their theoretical conclusion that electrons pass through molecules without interacting or colliding. Skepticism does not issue into controversy about the Reed-Tour hypothesis with an aim towards its acceptance or rejection by the scientific community at large. Instead, it is deeply embedded or hidden in investigations that actually build upon their researches as, for example, in statements like these:

Pioneering single-molecule experiments were performed by Reed et al. and later by Kergueris et al. The nonlinear current-voltage characteristics (IV s) found by these groups were attributed to the electronic molecular levels. However, several fundamental questions remain unsolved: Are the IV s really arising from transport through single molecules? Is the electronic flow rather wave-like (coherent transport picture), or is a one-by-one electron transport scenario more suitable (hopping picture)? (Weber et al. 2002, 114)

This statement is taken from the 2002 paper "Electronic Transport through Single Conjugated Molecules" by a research group at the Institute for Nanotechnology of the Forschungszentrum Karlsruhe. The group around physicist Heiko Weber and chemist Marcel Mayor distinguishes itself from its counterparts in the United States by insisting on the fundamental character of these questions, that is, by pursuing molecular electronics as basic research. Their paper therefore begins by duly noting the technological significance of this research as secondary to theoretical considerations.

How does current flow through single organic molecules? This question plays an all-important role in the field of molecular electronics, a field which is not only a fascinating topic of basic research, but may have great potential for future data processing technologies. (Weber et al. 2002, 113)

Clearly, my thesis about NanoTechnoScience as a site-oriented rather than issue-driven enterprise may encounter resistance from this group. [[Indeed, it may turn out to be a thesis not about nanoscience at all, but rather about a cultural difference between the pragmatic orientation of nanoscale research in the United States as opposed to the traditional orientation of publically funded research in Germany - or about the interdisciplinarity of a physics/chemistry and an electrical engineering/chemistry collaboration.]] We should therefore take a closer look also at this paper. In particular, I would like to ask just how it represents basic research in the field of molec-

ular electronics. This will show that the Karlsruhe group conceptualizes its research in theoretical terms. It also indicates, however, that this self-understanding remains largely implicit and that the paper constitutes an explicit nanoscientific advance in that the Karlsruhe group is demonstrating the facility it has achieved at handling molecular break junctions experimentally as well as conceptually. Accordingly, their paper offers two versions of its conclusion, one that explicitly establishes it as a technoscientific contribution to the nanoscale research community, another that implicitly marks its interest for open-ended theoretical discussion.

A first indication of this balancing act appears immediately after the just quoted opening of their article. While the oldest paper cited by Reed and Tour in 1997 was one of Reed's first experimental papers on the topic from 1988, Weber et al. follow a 2000 review article in *Nature* and cite "first theoretical considerations" from 1974. However, the considerations in that 1974-article are "theoretical" only in the sense that the authors provided calculations where measurements were not yet available (Aviram and Ratner 1974).⁵ Just like the 2000 review article, its horizon of interest does not reach beyond electronic circuitry. Indeed, the review article casts the history of these researches in terms of manipulative access to the nanoscale and to the dictates of Moore's laws:

The first proposals for molecular electronics appeared in the 1970s, but it is only the appearance of a number of scientific and economic developments that has allowed the recent resurgence of activity in this field. Crucial are advances in nanoscale science and technology, such as new fabrication methods and probes, which enable individual molecules or small numbers of molecules to be connected in a controlled manner into actual test devices. The driving force behind this research is clearly the need for suitable alternative technologies to Si-based CMOS, which is expected to reach its limitations in 10-20 years. (Joachim et al. 2000, 547)

Just like the Karlsruhe group, this review article adopts a rather diffident view of Reed and Tour's findings, a view that neither criticizes nor endorses them.

Break junctions involve the gentle fracture of a microfabricated electrode in its centre by mechanical deformation while measuring the resistance of the metallic wire junction. Its application to single molecules is difficult because a liquid evaporation step is required after formation of the junction, and the conformation and the exact number of interconnected molecules remain essentially inaccessible. Nevertheless, measurements have provided estimates of $R = 22\text{M}\Omega$ ($T = 5.9 \times 10^{-4}$) for a junction containing molecule **9** shown in Fig. 1a. (Joachim et al. 2000, 542-543)⁶

Weber et al. do not cite any discussion, principled considerations, or empirical evidence to explain why Reed and Tour's "pioneering single-molecule experiments" leave them unpersuaded as to whether they really involved single molecules. Their critique of Reed and Tour is only implicit in their own proposal to "unambiguously identify the *IVs* as current through our sample molecule" (Weber et al. 2002, 114). It will serve as further testimony to the ambivalence of the Karlsruhe approach that its theoretical interests are contained in this largely implicit critique, while their own solution to the problem adopts a similar, albeit more persuasive strategy than did their counterparts in the United States.

⁵ It calculates *I-V* characteristics "of a molecular rectifier including direct electrode to electrode tunneling" which agree rather well with those obtained by Reed and Tour 1997 and by Weber et al. 118, though it does not anticipate voltages nearly as high. This agreement is communicated visually through the likeness of their diagrams. See Aviram and Ratner 1974, 282, Weber et al. 2002, 116 (Figs. 2 and 3), Reed and Tour 1997, 253, also Ventra et al. 2002, 193.

⁶ Here is another example of an elliptic critique of Reed and Tour: "Due to the lack of any specific experimental information, we assume that a single molecule makes contact to both right and left leads as shown in Fig. 1, even though this configuration might not be the actual experimental one" (Ventra et al. 2002, 192). This article goes on to establish a closer fit between (improved) experimental observations and theoretical models.

Weber et al.'s implicit critique of Reed and Tour is that they were guided by the mental model of classical electronic circuitry.⁷ Instead of asking a question about nature, they are already designing a molecular computer. They are less interested in understanding molecules than in forging the smallest possible wire out of a molecule. They were satisfied as soon as they observed an onset of conductance that they could plausibly attribute to the formation of a molecular connection between the electrodes and therefore didn't bother to carefully differentiate their observed current flow from the surrounding and initial conditions, for example by investigating bonding configurations and contact geometry⁸ or by assessing the contribution of the electric field's bias voltage (Weber et al. 2002, 120-123). Moreover, after they satisfied themselves experimentally that they had obtained a molecular wire, Reed and Tour offer an apparently *ad hoc* theoretical model for electron transport.

This implicit critique of Reed and Tour by the Karlsruhe group is contained in its adoption of a more principled theoretical stance. Weber and Mayor set out to remedy these deficiencies and thus to improve and amend Reed and Tour's work. They do so experimentally and by modeling the experimental set up. Both parts of their argument advance the same implicit conclusion: While Reed and Tour were interested to show that current was flowing through an individual, i.e., a single molecule, Weber et al. establish that the individuality of the molecule, i.e., the molecule qua molecule with a particular shape and structure makes a difference to current flow. In effect, they work towards the non-trivial conclusion that "the chemical [rather than physical] nature of the junction is crucial and predominant for the conductance properties of a metal-molecule-metal junction" (Weber et al. 2002, 124).

Experimentally, the Karlsruhe group advances this conclusion by offering an improved variant of Reed and Tour's experiment. The rather limited statistical interpretation of that original experiment did not exclude the possibility that in all the observations, more than one molecule was active,⁹ nor did it offer effective statistical controls (but see Reed and Tour 1997, 253). The paper of the Karlsruhe group is based on a greater number of experimental observations involving two molecules that differ mainly in their spatial symmetries. The symmetric molecules produced symmetric current-voltage curves, the asymmetric molecules asymmetric ones, their peak sometimes offset in a positive and sometimes in a negative direction. This affords a more sophisticated version of Reed and Tour's statistical argument. Weber et al. offer 5 observations that, taken together, still "do not give an unequivocal proof, but strongly indicate that we are indeed sensitive to single molecules." In other words, Weber et al. do not claim that their and, by implication, Reed and Tour's molecular junctions do consist of single molecules. They merely argue that their data is statistically sensitive to the individuality of molecules. This is best exemplified by the fourth of their five observations:

For the asymmetric molecule, the spectrum appears either with a peak at $U \approx -(0.35 \pm 0.1)$ V or a similar peak at positive bias. This discrete asymmetric behaviour indicates that a discrete set of molecules, which is randomly oriented, most probably a single one contributes. A larger set of randomly oriented

⁷ Compare Michael Gorman's discussion of mental models as a means of structuring nanoscale research (Gorman 2003).

⁸ "... it becomes evident that the type of bridging as well as the proximity of gold atoms to the molecular π -system has significant influence on the electronic structure and on electron transport" (Weber et al. 2002, 122).

⁹ In particular, it is hardly credible that their results were based on exactly and no more than four measurements.

asymmetric molecules would average out the asymmetry, a fact that has never been observed. (Weber et al. 2002, 118)¹⁰

The experimental part of the Karlsruhe paper thus appears on the one hand as a mere extension of Reed and Tour's approach. Five years later, one might say, the production and experimental control of molecular junctions has improved. What was once considered a precarious procedure has now been routinized. A greater facility to vary the experiment also provides a regime of improved assessment and control of the experimental observations. This similarity between the two groups of argument and approach tends to disguise the difference of theoretical orientation, however, as exemplified firstly by the comparative diffidence of the Karlsruhe group as to whether or not they are looking at a wire consisting of a single molecule and secondly by their pronounced interest in the chemical nature of the observed conductance patterns.

All this holds also for the theoretical part of the Karlsruhe paper. Here, they model the onset of conductance in purely quantum chemical terms without reference to “elaborate” “theoretical transport models” (Weber et al. 2002, 124, 123). Again, they backtrack Reed and Tour's speculations to a more fundamental theoretical level. While Reed and Tour referred to transport models only to show that their findings are physically consistent with physical and chemical background knowledge, Weber et al. exhibit the chemical sensitivities of the entire experimental set-up which they model as a single super-molecules that includes the electrodes as clusters of gold atoms. These sensitivities concern the architectural features of that super-molecule — spatial symmetry, in particular — and the onset of conductivity which moves the molecule from an insulating to a conducting regime.¹¹ It remains open, however, whether and how this qualitative use of pure quantum chemistry constrains the physical transport models which perhaps are too obviously shaped in the image of electronic circuitry.

There is perhaps a systematic reason for this. Reed and Tour on the one hand, Weber and Mayor on the other hand are using the term “molecule” in very different, Thomas Kuhn might say incommensurable ways. According to the chemical conception, one speaks of complex bonding in the case of a compound of an organic molecule and metal atoms. Depending on whether ionogenic or covalent bonds prevails, these compounds are referred to as complexes or molecules (in the case of purely ionogenic bonds one does not customarily speak of molecules at all anymore). Now, in the case at hand, covalent bonding prevails by definition. Since in the case of the gold atoms and the organic molecule that serves as the wire, the transfer of charge produces an overlapping of their orbitals, the covalent bonding creates a new molecule that includes the gold atoms and the inserted organic molecule. In these chemical terms, then, one can no longer speak of a molecule that does not include the gold atoms but exists as a discrete entity between them even though it shares orbitals with them in the transfer of charge. By treating the “supermolecule,” Weber and Mayor follow chemical usage as opposed to Reed and Tour.¹² Thus,

¹⁰ According to Weber (in conversation), the required discrete set of molecules should be no more than a handful, certainly less than ten.

¹¹ This transition is marked by the breakdown of the theoretical model that was adopted for an analysis of the insulating regime. “In the insulating regime, no current is flowing and the method [an equilibrium method for investigating the electric field in terms of external electric potentials for the two clusters of gold atoms] is justified to a good approximation. In the conducting regime, different things happen in the experiment and in our model: whereas in the experiment a current is flowing, within our computation the molecule will screen the external potential by a static charge transfer from one gold cluster to the opposite. However, both effects are obviously closely related to conductivity” (Weber et al. 2002, 123).

¹² I owe this analysis to Joachim Schummer (in conversation). This case study resonates with Schummer's contention that it is a hallmark of any discipline that it constitutes its objects through its theoretical perspective, its questions, problems and issues. Schummer's observation raises a skeptical doubt regarding the possibility of a truly interdisciplinary nanoscience. If interdisciplinarity consists primarily in the abandonment, loosening, or black-boxing of the theoretical frameworks of the contributing disciplines, how then is an interdisciplinary nanoscience to arrive at “common objects” (Schummer 2003). What would be required is the development of a specifically nanoscientific theoretical perspective. While George Khushf (2003) envisions such a new disciplinary

the considerations of the Karlsruhe group provide a theoretical reframing of the transport models used by Reed, Tour and others. It is yet to be determined whether this reframing is substantially at odds with those transport models, whether it is a linguistic artefact resulting from their different conception of “molecule,” and whether the distinction between “substantial disagreement” vs. “linguistic artefact” can even be accomplished. For now, the only explicit conclusion that can be drawn from the account by Weber et al. is therefore that they are adding to the conceptual toolbox of molecular electronics (see also Tian et al. 1998, Ventra et al. 2002, etc.). Five years after Reed and Tour researchers have expanded their conceptual control of the phenomena just as they did their experimental control. More and more abstract models are available first to represent the phenomenology and to indicate where the phenomenological observations may yet be too crude (Ventra et al. 2002, 192-194, Weber et al. 2002, 122).

Finally, however, one should note that the official or conventional silence about potential disagreements extends beyond the question of molecular assemblers, via the theoretical differences in empirical investigations of the conductance of molecules, even to the expansion of experimental and conceptual control: The case of J.H. Schön whose work on molecular electronics contributed to the researches discussed here (esp. Ventra 2002), though its credibility is now in question.

4. Revisiting the Thesis

I have now identified two distinct attitudes, approaches, perhaps styles of research in molecular electronics. Is it really defensible to claim both for my thesis that nanoscale research is a site-oriented technoscience rather than issue-driven science? I dare not provide an apodictic answer but conclude by offering three considerations.

My contrast of the two research groups is caught up in a fundamental difficulty concerning the very notion of “technoscience.” It was introduced by Donna Haraway and Bruno Latour to mark a new stage in the development of science, namely the technological constitution of the very objects of scientific research such as transgenic mice which are hybrids of nature and technology. However, as soon as that new era of technoscience was proclaimed, it became possible to consider all of experimental science as technoscience. Even a vacuum-pump or certain observational protocols, one might say, technologically constitute the objects of scientific research. While this appears to dissolve the supposed novelty of technoscience, the claim for novelty can be maintained on another level, namely at that of the self-understanding of science. Perhaps, all science has always been technoscience, but only in the contexts of nanoscience, biomedical sciences and genetics, artificial intelligence research and robotics are traditionally trained scientists abandoning the assumption that they understand and explain a given nature and embrace instead the project of overhauling or transforming nature, of “Shaping the World Atom by Atom” (NSTC 1999, Nordmann 2003). Thus, the difference between Reed and Tour on the one hand, the Karlsruhe group on the other hand is not that one has a technoscientific orientation while the other adopts a theoretical stance. Both contribute to NanoTechnoScience. Reed and Tour do so

perspective, there actually appears to be little pressure or movement toward its development.

openly while the Karlsruhe group still represents itself in terms of the traditional opposition between fundamental versus applied science and technology.¹³

Also, as I have shown in this paper the fundamental problem investigated by the Karlsruhe group presented itself not in the development of a quantum-chemical research programme, but in the technoscientific pursuit of electronic circuitry consisting of organic molecules. Similarly, their explicit contribution consists in the enhancement of experimental and conceptual control of molecular break junctions. In contrast, their ultimate interest in the specifically chemical nature of this junction appears as an oblique gesture towards an ongoing and open-ended discussion of a fundamental question that stands in the tradition of natural philosophy.

Finally, this disjunction between the implicit and explicit in the argument of the Karlsruhe group exposes the missing middle ground. It is significant, I believe, that between the finite demonstration of achievement and an obliquely philosophical gesture there is no overt critical engagement of a hypothesis or theory. Reed and Tour did not place the ball in the court of public opinion. Instead, the ball remains in their court and the scientific community adopts a wait-and-see attitude: “If they think they have mono-molecular-wires, let’s see where this gets them; they can win us over by demonstrating a more targeted conceptual, experimental, technical control of the phenomenon. They can present better and better arguments in the form of better and better molecular wires and, ultimately, devices.” To be sure, as in any age of exploration and the claiming of new territory, some won't wait to see where this gets them but instead will themselves attempt instead to get there first. While such efforts build upon Reed and Tour's experiments, they can do so without buying into or bothering to contradict any particulars of their account. The technoscientific occupation and appropriation of the nanoscale thus differs from standard conceptions of theoretical science not only in the orientation towards its subject-matter but also in the interaction among scientists: The critical aspect or “organized skepticism” of public science takes the backseat to the staking-out and entrenchment of private claims.¹⁴

References

- Aviram, Arieh and Mark Ratner (1974) “Molecular Rectifiers,” *Chemical Physics Letter* 29:2 (November 15), 277-283.
- Buchwald, Jed (1985) *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*. Chicago: University of Chicago Press.
- Carrier, Martin (2002) [“Epistemic Standards of Application-Dominated Science”]
- Gorman, Michael (2003) presentation at the conference “Discovering the Nanoscale,” Columbia, SC.
- Howard, Sean (2002) “Nanotechnology and Mass Destruction: The Need for an Inner Space Treaty,” *Disarmament Diplomacy* 65 (July-August),
- Joachim, G., J.K. Gimzewski, A. Aviram (2000) “Electronics Using Hybrid-Molecular and Mono-Molecular Devices,” *Nature* 408 (November 30), 541-548.
- Khushf, George (2003) presentation at the conference “Discovering the Nanoscale,” Columbia, SC.

¹³ This is not to say, of course, that such self-ascriptions are inconsequential for the development of research. To acknowledge this is easy for a Kantian, Peircean, or Wittgensteinian philosopher and a Weberian or Mertonian sociologist of scientific knowledge. It is terribly difficult to acknowledge for all those who are interested in the material culture of science and therefore attempt to deny the historical influence or material efficacy of concepts, ideas, theories, and beliefs.

¹⁴ To be sure, Popper's and Merton's view of science as organized skepticism may have become obsolete even before nanoscience came along. The suppression of theoretical disagreement in the advancement of an application oriented research agenda may characterize many scientific publications (see Carrier 2002). In this case, one might say that nanoscience helps us to foreground this technoscientific development.

- Nordmann, Alfred (2002) "Nanoscale Research: Application Dominated, Finalized or 'Techno'-Science?" (working paper at <www.cla.sc.edu/cpecs/nirt/papers/AN1.html>).
- Nordmann, Alfred (2003) "'Shaping the World Atom by Atom': Eine nanowissenschaftliche WeltBildanalyse," forthcoming in Armin Grunwald (ed.) *Technikgestaltung zwischen Wunsch und Wirklichkeit*, Berlin: Springer (also at <www.cla.sc.edu/cpecs/nirt/papers/AN2.html>).
- NSTC-Report (1999) "Nanotechnology: Shaping the World Atom by Atom," (published at <<http://itri.loyola.edu/nano/IWGN.Public.Brochure>>).
- Reed, M.A., C. Zhou, C.J. Muller, T.P. Burgin, J.M. Tour (1997) "Conductance of a Molecular Junction," *Science* 278 (October 10), 252-254.
- Reed, Mark and James Tour (2000) "Computing with Molecules," *Scientific American* 282:6, 86-93.
- Schummer, Joachim (2003) presentation at the conference "Discovering the Nanoscale," Columbia, SC.
- Tian, W., S. Datta, S. Hong, R. Reifengerger, J. Henderson, C. Kubiak (1998) "Conductance Spectra of Molecular Wires," *Journal of Chemical Physics* 109 (August 15), 2874-2882.
- Ventra, M. Di, N.D. Lang, and S.T. Pantelides (2002) "Electronic Transport in Single Molecules," *Chemical Physics* 281, 189-198.
- Weber, H.B, J. Reichert, F. Weigend, R. Ochs, D. Beckmann, M. Mayor, R. Ahlrichs, H.v. Löhneysen (2002) "Electronic Transport through Single Conjugated Molecules," *Chemical Physics* 281, 113-125.
- Williams, Stan (2002) "Molecular Electronics," presentation at the *Europäische Akademie's* Autumn Conference on Nanotechnology Assessment, Bonn, September 13.
- Woolley, R. G. (1978) "Must a Molecule Have a Shape ?" *Journal of the American Chemical Society* 100, 1073-1078.