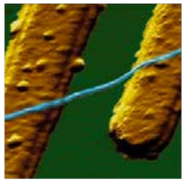
QT

the story of a research group at the origin of nano and quantum science in Delft 1971-2013

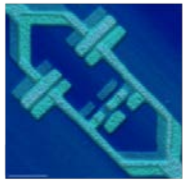
Hans Mooij

2022

quantum point contact
1987



carbon nanotube
1997



flux qubit
2003

Introduction

Delft, at this time, is a very strong center of research in nanoscience and quantum information technology. How that came about is an intriguing question. The group called Quantum Transport (QT in abbreviation) played a crucial role. Its origin can be traced back to 1971, when a small research activity started in a small corner of the Department of Technical Physics. It grew by itself, discouraged by many of the professors of the Department because they felt it was not "technical" enough. Nevertheless, it survived and started to flourish. The group stopped to exist in 2013 when it was absorbed into the large institute QuTech.

The name Quantum Transport appeared first in 1998, but the group went under other names before. It started as a project within a section called Molecular Analysis, headed by professor Jan-Berend Westerdijk. When he retired in 1980 the new activity, as a self-laid cuckoo's egg, had taken over the research program, now fully focused on superconducting electronics. In 1981 the name was changed to Superconductivity. Responding to a university call for larger research units the group became part of a "vakgroep" Solid State Physics. In 1998 the vakgroep units were abandoned, a good time to choose a new name. Going back to Superconductivity was not an option because in the meantime more than half the group addressed other materials. The common interest was clearly in new quantum effects for electrons in very small fabricated structures. "Quantum electronics" would have been perfect, but the term was already claimed by laser physics. We settled on Quantum Transport and that name survived for fifteen years. In this story the name QT is used over the full period 1971-2013.

This story aims to record the development of QT over the years and to explore the factors that contributed to its success. Specific individual people in the university and in the international scientific community were important. Several times the group made conscious choices, starting new directions of research and abandoning others. Group members came and went, some came back. I myself was there all the time and played a central role. My aim is to tell the story of the group, but I can hardly be an objective historian.

Three rather distinct phases occurred in the evolution of QT, which could be labeled as "learning", "growing" and "focusing". From 1971 to about 1985, the group was evolving from a set of one-student projects to a small research group with international embedding. We learned how to communicate with leading scientists around the world, relevant research questions were distilled and a technical arsenal was built up that made us competitive. Something significant happened: more and more the engineer's wish to build a useful system was combined with the scientist's wish to understand unexplored aspects of nature. In the "growing" phase from about 1985 to 2000, the group expanded strongly. New projects on semiconductor nanostructures were started, electron transport in single molecules was chosen as a research subject. Projects were started (but after a few years stopped) on high temperature superconductors and charge density waves. The numbers of PhD students and postdocs increased significantly. No longer were group meetings held in Dutch. After 2000 ("focusing"), quantum information became the main theme. This started in superconducting circuits, later semiconductor quantum bits were addressed with 3-5 materials, with silicon, and with individual defects in diamond.

Over the full lifetime of QT, the group studied and applied electronic quantum effects that are connected with small dimensions. Controlled lithographic fabrication of submicron structures (later called nanostructures) by its own students in its own clean room of the time was always essential. In the early seventies, the minimum feature size for state-of-the-art commercial silicon transistors was around 10 μm. In QT, with opportunistic methods, individual structures as small as 0.5 μm could be fabricated. In later years, electron beam lithography provided the highest resolution, down to 10 or 20 nm. Too slow for industrial chip fabrication, this technique is perfect for single objects and small circuits. Deposition of thin films and lithography are never standard when applied to research materials. The interpretation of results always requires insight with respect to the definition of the physical sample. The same is true for the measuring electronics. Quantum effects in solid state circuits are fragile and they can easily be destroyed by the electronic circuitry that is used for control and read-out. That circuitry on and near the chip is always an essential element of the experiment.

Over the years, the group QT attracted many good people, each with their specific talent. Students and staff were strongly motivated. Many of the young people leaving the group are now leading their own research teams.

Part 1. Learning, 1971-1985

The birth of QT

QT started in the Molecular Analysis group of professor Westerdijk in the Department of Technical Physics of Delft University. The group studied physical methods for chemical analysis. Many different topics had been addressed, but at that time the focus was on submillimeter spectroscopy (wavelength 50 to 500 µm, frequency around 1 THz). Equipment could not yet be bought but was fabricated in the laboratory. In the large workshops in Delft gratings were mechanically cut out of soft metal on steel and parabolic mirrors were fabricated from glass with reflective coatings.

Jan-Berend Westerdijk 1917-1982

The Netherlands Space Research organization was strongly interested in starting astronomical observations in the submillimeter range, but the sensitivity of available detectors was insufficient. In the decade before 1971 the Josephson effect had been predicted and confirmed experimentally. Here two superconductors are weakly coupled and a weak voltage between them leads to coherent oscillations in the frequency range corresponding to submillimeter wavelengths. It was believed that so-called Josephson junctions could become very sensitive detectors. The people in space research had heard about these junctions. They hired Kees Andriesse (previously in nuclear physics), who investigated how much progress had been made by means of an extended study trip to the United States and in Europe. In addition, they approached Westerdijk to start a collaborative project for developing a Josephson device. The group in Delft had assigned this task to an undergraduate student named Teun Klapwijk.

It is necessary to describe the scientific atmosphere in the laboratory at that time. Delft University was primarily an educational institute, training engineers for industry. Research was very limited. Nevertheless, it was a well-known institute of technology at the highest level in Europe. In Applied Physics, more than 100 new "ingenieurs" graduated each year, many of them joining the large research labs of Shell, Philips, AKZO or Unilever. Because applied physics combined a focus on analytical thinking with a broad technical background, a relatively large fraction ended up in high management positions in industry. The curriculum spanned five years, where in the fourth year about 50% and in the fifth year about 90% of the time was spent in a research group. Both years required an extensive thesis report. Molecular Analysis had around 15 undergraduate students who worked on more than 10 different projects. Progress in each project was discussed in a weekly group meeting. The budget from the Department amply covered all expenses.

Jan-Berend Westerdijk was 55 years old in 1971. He came from a respected family, his aunt Johanna Westerdijk was the first female professor in The Netherlands. He studied physics at ETH Zurich, doing his "diplom" work with the more than famous physicist Pauli. He liked to tell stories about the atmosphere in Pauli's institute where all the legendary physicists of the twenties and thirties came by regularly. Einstein in particular was a frequent guest. He often brought his violin and volunteered to play for the group. Westerdijk played that instrument himself and according to him Einstein's musical sense was awful. The students had to sit through the performances and be polite.

Westerdijk returned to Holland in 1940 and became an assistant in Delft. During the second world war he was detached to the Kamerlingh Onnes Lab in Leiden but he came back to Delft in 1947. In 1971 he had been a professor for 20 years. He had a very keen insight in physics, technology and people. He understood everything but had no strong drive to do something new. He was a natural teacher and he always gave the physics lectures for large groups of students in mechanical engineering and similar studies. He looked very formal but was an independent thinker.

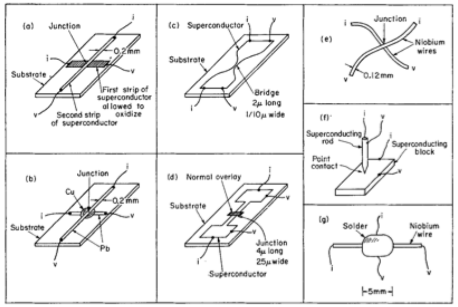
In November 1971 I rejoined Molecular Analysis. I had obtained my ingenieur's degree in that group and became its first PhD student, finishing

my experiments in 1968. After military service I went to work with Shell, but missed the curiosity aspect of scientific research. I asked Westerdijk's advice, he immediately offered me a position in his group. At his suggestion I would focus on the Josephson junction project. Teun Klapwijk was on holiday following his marriage, and found me added to his project when he returned. I would claim that QT started when Teun and I first talked with each other. Our ages and experiences were different, but our ambition and approach were very similar. Until Teun left in 1985 to become a professor in Groningen our interaction was crucial for the basis of the research team that turned into QT. We developed a common vision on what we wanted to study and what we needed. Too often, this vision grew in long discussions at the end of the day, with our families waiting at home.

The first years, finding our feet

I have a very clear memory of these early times. However, before starting to write I spoke with several people and I consulted official reports. I discovered that my brain had not stored all events in quite the right order. I have tried to use the "external" information as well as possible. The student reports are an invaluable resource. In contrast to present-day PhD theses or papers that are published in journals, they describe the equipment and the methods that were used, and they report on failed experiments. The full sets of reports for the "kandidaats" (4th year) and the "ingenieurs" (5th and final year) exams are still available. 34 students obtained their "ingenieurs" degree as members of the group between 1971 and 1985, typically after two and a half years. As for PhD students, the number was very limited. Teun Klapwijk defended his PhD thesis in 1977, followed by Marianne Stuivinga and Victor de Waal in 1983. The bulk of the actual experimental work, fabricating samples, developing equipment and performing measurements, came from the hands of the undergraduate diploma students. These students remember that they worked very hard during long days to achieve their results. They felt involved and were strongly self-motivated.

The interaction of Josephson junctions and microwaves was in the forefront of our minds. Early in 1972, we were facing the challenge to develop our own Josephson junctions. A Josephson junction consists of two superconductors that are "weakly coupled". The Bardeen, Cooper, Schrieffer (BCS) theory of 1957 gave the explanation for superconductivity, the mysterious phenomenon that was discovered in Leiden by Kamerlingh



Types of Josephson junctions. (a) Thin-film junction with oxide barrier; (b) thin-film junction with metallic barrier; (c) Dayem bridge (microbridge); (d) Notarys-Mercereau bridge; (e) crossed-wire weak link; (f) point contact junction; (g) slug.
from J. Clarke, American J. Phys. **38**, 1071 (1970)

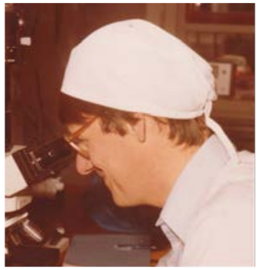
Onnes in 1911. Superconductivity is the result of quantum condensation of electron pairs. In the condensate the electrons are no longer distinguishable, collectively the pairs are described by an order parameter with a magnitude and a phase. At rest in a superconducting body the phase has the same value everywhere but is mostly irrelevant. Josephson, as a 22-year-old student, showed in 1962 that, as a consequence of the BCS theory, the phase difference between two superconductors that are weakly coupled should be a relevant quantity that can be observed in experiment. Here "weakly coupled" means that each superconductor still has its own phase, but the two phases are coupled by a small energy term that depends on the cosine of the phase difference. A small DC current without voltage can be present, its maximum value is called the critical current. This critical current is directly linked to the Josephson coupling strength.

In addition to the DC current, there should be an AC Josephson effect when a voltage difference is applied between the two superconductors. Because the energy difference for a pair that crosses from one superconductor to the other is $2eV$, an internal quantum oscillation at a frequency $2eV/h$ should occur. Here $2e$ is the electrical charge of the Cooper pair, V is the voltage difference between the electrodes and h is Planck's constant. A typical voltage difference would be 0.1 to 1 mV, which corresponds to frequencies from 50 to 500 GHz, and to wavelengths from 6 to 0.6 mm. One

expects special features when the internal frequency is equal to the frequency ν of external radiation (or a multiple thereof). This leads to so-called Shapiro steps at fixed values of the voltage $V_n = nh\nu/2e$, with n an integer number. The presence of good Shapiro steps is a quality indicator for good Josephson junctions.

The figure is a summary of the types of Josephson junctions that were known in 1970. The original tunnel junction consisted of two thin films, separated by the natural oxide of the bottom film. The oxide barrier had to be pinhole-free and thin enough to allow tunneling. Their lateral size was typically 0.1 mm. With such junctions, the Josephson effects had been confirmed. They were not well suited for work at high frequencies. The electrical capacitance of the superconductor-insulator-superconducting sandwich was around 1 nF, the impedance at 10 GHz was consequently below 0.1 Ω and the coupling to radiation very weak. When the oxide was replaced by normal metal, the impedance was even much lower. For high-frequency applications, two types were used or considered: point contacts and microbridges. The point contact consisted of two bulk metal superconductors (usually niobium), with a sharp point that was mechanically screwed down to just touch the other electrode. Obviously, these were not very stable when the set-up was cooled down to helium temperatures; in situ mechanical adjustment with retracting screwdrivers was mostly used.

When we started to work on Josephson junctions, most of what we did was new for us. However, Delft was a place with a large collection of technical people in many fields. For our work we needed thin metal films. Evaporation systems were present, mainly used to fabricate mirrors for optics. There was a low temperature group in the lab, where helium was liquified and where expertise on cryostats and also on microwave equipment could be found. One of their main research subjects was magnetic resonance. The mechanical workshop was large and the quality of their staff excellent. We did not have to pay for work-hours. The glass instrument makers could fabricate cylindric glass cryostats with concentric vacuum-nitrogen-vacuum-helium spaces. The electronics workshop built the driving electronics on specification (of course very primitive in the beginning). In the Molecular Analysis group there was one technician, Chris Gorter, with an unusual background. He had been studying physics, finished his 4th year in the theory group and then started but never finished his 5th



Teun Klapwijk in our "clean" room

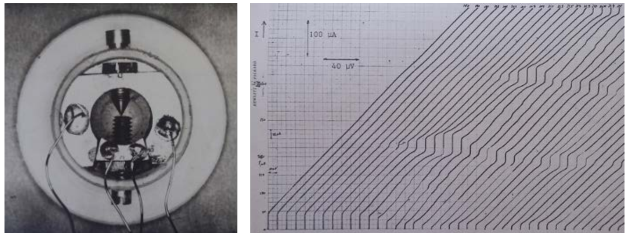
Chris Gorter

year in Molecular Analysis. After a few years, he was appointed in a technician's position and stayed in QT until his retirement. In the first period of QT, Chris Gorter played an essential role. Teun and I discussed our physics problems, resulting in things that we would like to have or that we wanted to be able to do. He then made it or had it made in the workshops. He tended to a slight overkill relative to the original requirements, which often was very useful a few years later.

Within an hour from our lab in Delft was the Kamerlingh Onnes Laboratory in Leiden. In that place helium was first liquified and in that place superconductivity was first observed. However, that was 60 years before. When I went to visit in Leiden, my impression was not of a vibrant research atmosphere but mainly of an old building, old facilities and old people. The general attitude of the Leiden community towards our work in the first ten years of our existence was rather condescending, which was partly due to the appreciation of a real university for an engineering school. What I say here is certainly not fair towards Rudolf de Bruyn Ouboter who was extremely helpful. Working with him was a PhD student named Fons de Waele, experimenting with Josephson point contacts. We learned much from them about fabrication techniques for these bulk niobium systems. We fabricated point contacts with the help of our Delft glass instrument makers, using the advice from Leiden. For the electrical insulation of the two niobium superconductors a special glass was used that had the same thermal expansion coefficient as niobium. Our technicians could learn the tricks and all went well. Teun's final report for his ingenieur's exam in June 1972 describes attempts to see the influence of microwaves, but these attempts had failed and the report focused on theoretical aspects. Exactly a year later

the report of Tjerk Veenstra contained beautiful Shapiro steps observed on tantalum and niobium point contacts with 35 GHz microwave radiation.

We did not want to rely on bulky and unstable point contacts and most of the efforts of the group in the first years were directed at fabrication of microbridges. A microbridge is a thin film structure with two super-conductors in the same plane, connected by a narrow and short connecting bridge. The relevant length scale is the superconducting coherence length. It is short for materials with a high critical temperature. Typical values range from 0.1 to 1 μm. The minimum feature size of transistors in state-of-the-art electronic circuits at that time was 10 μm. That presented a serious technological challenge. Moreover, it was not clear from theory what the ideal bridge should look like. Theoretical descriptions were based on rigid boundary conditions, the bridge being enclosed between two completely undisturbed banks. Of course, that was not realistic because the current passing through the bridge only gradually spreads out. The ideal materials

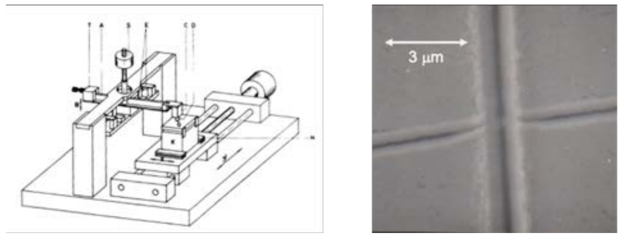


Point contact and Shapiro steps, Tjerk Veenstra (1973)
left: tantalum point contact. The contact itself is in the center of the picture made with an adjustable screw. The two superconductors are connected with two soldered wires each; they are separated by fused glass. The tantalum ring is about 1 cm in diameter.
right: measured current-voltage characteristics with application of 35 GHz microwave radiation. The characteristics are recorded on a Hewlett-Packard x-y recorder, consecutive traces are shifted horizontally. The microwave power increases from left to right. The critical current (at zero voltage) is seen to decrease, gradually the first Shapiro step grows at a voltage of 70 μV followed by the second step at 140 μV.

parameters were not known. Niobium has a critical temperature of 9 K, comfortably above the operating temperature of the simplest cryostats with liquid helium (4 K). Niobium is not easy for deposition in thin films.

Various groups in the world worked on microbridges. The usual photolithographic techniques could not make small enough structures. At IBM, where electron beam lithography was pioneered, the set-up was used to make small bridges in niobium. We tried the same in the lab in Delft with the help of the group of professor Jan Le Poole, who had been a central person in the development of Philips electron microscopes. In Delft he built a system for electron beam lithography, intended to fabricate high resolution optical zone plates for astronomers. That system was now out of use and we could play with it to make bridges that were 100 times smaller than the transistors of that time. However, Shapiro steps were not seen.

In Copenhagen, a group headed by Paul Gregers-Hansen (later called Lindelof), developed a two-scratch method. In a glass substrate, a light scratch was first made using a hand-held razorblade. The surface was lightly etched to make a smooth groove. Metal was evaporated to create a thin film that followed the bottom of the scratch. Afterwards a second scratch was made perpendicular to the first one. It cut the film in two parts (the two superconductors), except at the crossing of the two scratches. We went to Copenhagen where the group was very helpful. The technology for shaving beard hairs had reached the double-edged stage and blades from the drugstore were no good. The Copenhagen group had bought the remaining supply of the old-fashioned variety with one cutting edge; they gave us ten of those to work with. We used them to make microbridges in soft materials such as tin (critical temperature 3.7 K) and indium (3.4 K). Unfortunately, these materials were very unstable when cooled and reheated. Beautiful but very unwelcome whiskers grew spontaneously. We decided that aluminum was a better choice (T_c 1.2 K, relatively large coherence length). Razor-blade scratching by hand did not work so well there and we decided that a better cutting tool was needed. We talked with fine-mechanics professor De Jong of the Department of Mechanical Engineering. His group specialized in constructions with blade springs. We discussed the principles needed and afterwards Chris Gorter designed our own scratching apparatus. The scratching element was a diamond, polished for us in the optimal cutting shape by the diamond firm Drukker in Amsterdam.



diamond scratching apparatus for submicron fabrication, tin microbridge

Our choice for aluminum made it necessary to measure below its transition temperature of 1.2 K. In the group a Roots vacuum pump turned out to be available, having a very high pumping speed. The glass cryostats with their good thermal insulation and with their very smooth surface allowed us to reach temperatures down to almost 1.0 K, just pumping on helium-4. To have a long coherence length, it was advantageous not to go further below T_c (anyway, we could not). The cryostats were large thermos flasks with four concentric cylinders. From the outside, first came vacuum, then a shield of liquid nitrogen, then vacuum again and centrally the helium vessel. All inner surfaces were coated with silver to reduce radiation transfer. Narrow vertical slits were left open, so that one could look inside. Nowadays, with the all-metal cryostats, students do not know the comfort and pleasure of actually seeing the helium liquid entering, first totally evaporating when the vessel was still warmer than 4 K, but then forming a meniscus to show the height of the liquid column. Pumping down from 4 to 1 K, one would see the sudden transition at 2.2 K from a wildly boiling liquid to a completely still superfluid.

In the begin of 1973 we had fabricated microbridges with several methods, in aluminum, tin and niobium, that according to the vague existing theory were small enough to be Josephson junctions. They had a critical current that varied when microwaves were applied and several other reproducible features in the current-voltage characteristics. However, the Shapiro steps that were seen so easily with point contacts were absent. In publications, measurement results on bridges were reported on a very positive tone but the actual disappointing lack of good Josephson behavior was simply ignored. Fake news by omission, I would now say.

Building our infrastructure remained a high priority. We realized that in Delft we had strong advantages in this respect. We had excellent workshops and did not have to pay the work-hours, a situation that was unthinkable for our colleagues in the US. In our group we had Chris Gorter who took charge when it came to thin film deposition, lithography, cryogenic installations (in collaboration with the technicians of the Low Temperature group), and electronic measuring equipment. We installed a primitive clean room for our lithography, and we built a shielded room for our most sensitive measurements. In retrospect, both were useless for their direct purposes but valuable as learning exercises. To extend our temperature range we made our own He-3 cryostat with an internal sorption pump, that took us down to 0.3 K. In this respect we could compete with every laboratory in our field.

In the seventies, organizational change was in the air for universities in the world and in the country. Even in our generally unpolitical technical university the administration building was occupied by protesting students. The national parliament passed a new law for the universities (Wet Universitaire Bestuurshervorming WUB, 1970) that gave the decision power to elected councils rather than to the faculty of full professors. The council for the Department of Technical Physics had 12 members, of which 6 were elected by the scientific staff, 3 by the support staff and 3 by the students. Political polarization was strong, one side wanting to keep everything completely as it was and the other wanting to change absolutely everything. In Technical Physics every decision was voted on with a 7 to 5 distribution in favor of the conservatives. I became a member of the council when one of the "7" left the department. I tried to keep an open mind and experienced the strange leverage factor of a swing voter.

The Josephson project in Delft had started from a collaboration with the Space Research organization in Groningen. In 1973, that organization hired two PhD students for work on superconducting detectors, one in Groningen (Harm Tolner) and one in Delft (Briël Daalmans). Tolner and his successor in Groningen worked with niobium point contacts. Daalmans was selected by Groningen and reported to Groningen, single-mindedly devoting himself to the development of a practical submillimeter detector. He would have one or two of the group's undergraduate students working with him, we would share equipment, but otherwise the interaction was limited. He did an excellent job in developing small Josephson tunnel junctions of niobium, of

quite reasonable quality by the end of his project. He never took his PhD, more because of his perfectionism than for a lack of results. He left Delft in 1979 to go to Siemens, continuing his research on junction fabrication.

Connecting to the outside scientific world

The Westerdijk group had no tradition in international scientific exchanges. In the period that I knew him, from 1962 till his death in 1982, Westerdijk himself never went to any international meeting. He did have excellent contacts with everybody that counted in The Netherlands. Also, every summer he went for an extended holiday in the mountains in Switzerland and on his way there he visited with old colleagues at ETH Zurich. It would be impossible for the QT students of later periods to imagine how isolated we started. I was thirty years old and had not given a scientific presentation outside Delft. Nevertheless, from the beginning it was clear to us that we wanted to make use of all experience available and that we wanted to contribute to the field at the international level. How that worked in practice, we had to learn by doing.

In September 1973 Teun and I attended a conference in Perros-Guirec in Bretagne. The title was "Detection and emission of electromagnetic waves by Josephson junctions" and it was organized by the French Telecommunications Research Institute. The program had many talks where the Josephson junction was used as a high-frequency nonlinear element and where local oscillators, intermediate amplifiers and three-wave mixing excited the electrical engineers. The point contact was the ideal element for this, apart from its instability and non-reproducibility. The need for a thin-film solid-state alternative was strongly felt and a significant fraction of the program was devoted to the physics of practical Josephson weak links, to fabrication methods and to materials properties. The conference made very clear that no clear recipe existed yet for the ideal superconducting Josephson element, mostly through personal contacts after the talks. Laibowitz from IBM showed beautiful niobium microbridges that were fabricated with the electron beam lithography that had been pioneered in the Watson Laboratory. However, those bridges only showed Shapiro steps at high temperatures close to the critical one, not at 4 K or lower where they were intended to be used. The latter fact was not mentioned in the talk and only at the end of a long French lunch did we manage to extract this information from Laibowitz.

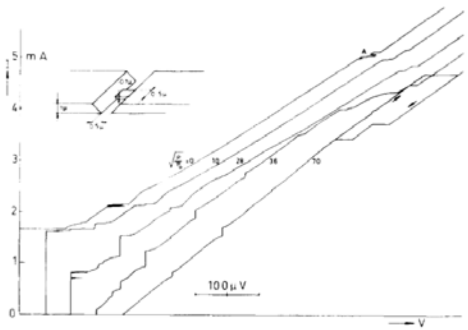
Village of Perros-Guirec

At the conference, I presented a talk on the fabrication of microbridges and of arrays of microbridges. It featured an aluminum bridge with 0.3 μm width, made with our e-beam projection system. It also demonstrated a two-dimensional array of microbridges, fabricated with our scratching apparatus. Our tool allowed us to interrupt the scratch 30 times at mutual distances of 4 μm, and to repeat this 18 times, with the scratches 10 μm apart. Making an array of 540 identical bridges within an area of 120x180 μm^2 was intended to demonstrate our microfabrication technology. We also hoped that we could synchronize the high frequency dynamics and achieve a sensitive detector or a superradiant source of microwave radiation.

One particular talk at Perros-Guirec had a lasting influence on the QT program and on the scientific careers of both Teun and myself. This talk was by Malcolm Beasley on behalf of the Tinkham group at Harvard. The title was "The electrical behavior of superconducting thin-film microbridges: self-heating and superconducting quantum processes". The Harvard researchers had found that the current-voltage characteristics of their small hand-carved bridges were never of the Josephson type seen with point contacts. They concluded that over a wide range of currents and of bath temperatures the behavior was dominated by heating. Locally in the constriction the temperature rises to the critical temperature and superconductivity is lost. Only at bath temperatures near the critical temperature where the current levels are smaller and for limited voltage levels is the dissipated power small enough for superconductivity to survive. Even then the response was not of the pure Josephson type. The high-frequency dynamics led to the generation of nonequilibrium quasiparticles that diffused over long distances away from the center region and led to an additional voltage drop.

For us this was exciting. We heard a talk where problems were not swept under the rug but actually were faced straight-on. The current-voltage characteristics were not the same as ours, but we saw clear similarities. At the end of the session we went to talk with Mac Beasley, in particular to discuss how such unwanted effects could be suppressed. There was no easy answer to that, but it was a very good discussion. Much later Mac told us that also for him this was a memorable occasion. He was traveling for the first time in an environment where people did not speak English and were not very helpful. After the conference, Teun and I talked about the new insights but still many questions remained. We wrote a long letter to Mac in which we explained what we had observed and why we could not understand our results. We also put down our thoughts about the choice of material and the optimal geometry and we asked his opinion. A letter in those days was typed on paper by the group secretary and sent in an envelope by postal services. We anxiously waited for an answer, but a very long time passed. We started to think our questions were too naive and did not merit much thought. Then, suddenly, a five-page answering letter arrived from Mac Beasley. He told us that we asked the right questions, that he did not know the definite answer to them, but that our questions had stimulated him to think in more depth. He gave us his considerations and some preliminary conclusions. This was a wonderful moment for Teun and me. Here we were, taken seriously by someone who clearly belonged to the established top in the field. From there a strong interaction grew that resulted in sabbatical periods for me in Mac's group at Stanford and for Teun in Tinkham's group at Harvard.

From all these discussions, Teun and I concluded that it should help to make our microbridges more three-dimensional. We started to fabricate thick-film, thin-bridge structures. Starting point was a very thick (1 μm) film of aluminum. We used two procedures, one being the old double scratch which worked well with soft materials. For aluminum we started with one scratch across the whole film that almost (but not quite) cut through the film. Our apparatus allowed systematic tuning of the vertical cutting force to achieve this. Next, the diamond was moved to the beginning, lowered into the V-groove and this time the cut came all the way through. However, in



Current-voltage characteristics of a variable-thickness microbridge.
Clear Shapiro steps are observed, induced by 35 GHz radiation.

the middle the scratch was interrupted, the diamond lifted and moved by about 0.5 μm, and then the other half was cut through. The result was a thin bridge. It worked amazingly well and it did lead to microbridges with good Shapiro steps. We published this in Physics Letters, with Teun and Tjerk Veenstra as the authors. The earlier Perros-Guirec conference paper had Chris Gorter, Jan Noordam and myself as authors. Clearly Teun should have been a co-author, and I should have been a co-author on the thick-film-thin-bridge paper. I think that we just made sure that all five of us, including the two masters' students, had a publication. Not very logical but we had to find the right balance in such matters.

In 1974 I went to the Applied Superconductivity Conference, held in Chicago. I presented a paper on our variable thickness bridges. I extended the trip to about three weeks, in which I first went to the West Coast (Berkeley, Stanford, Caltech) and then crossed over to the East Coast where I spent a few days at Harvard and visited IBM. For me this was a strong experience. I enjoyed the scientific atmosphere in these high-level labs and I grew convinced that we should try to create a similar open interaction in Delft. I discovered that the technical facilities of these top universities were certainly not as good as ours. It should give us the chance to compete at the world level if we kept investing in our group's infrastructure. We have done that over the years.

Starting in our new direction, our theory background was below standard. Until around 1970 Delft had only one professor of theoretical physics, Ralph Kronig, who retired in 1969. In the first half of his life he worked with Bohr and Pauli and narrowly missed a Nobel prize for the discovery of electron spin. Amazingly, after his arrival in Delft in 1939 Kronig hardly published anything significant. He had strong opinions. He himself had contributed to the birth of quantum mechanics and its application to solid state physics with the Kronig-Penney model. He decreed that quantum mechanics was not needed for ingenieurs. In his lectures he did not go much further than the hydrogen atom. Two new young theory professors came to the department around 1970: Jaap Kokkedee with a background in high energy physics and Hans van Leeuwen from statistical mechanics. They brought the theory education up to date, but of course that took time. Jaap Kokkedee chose to study the Josephson effects. He helped us understand the BCS theory of superconductivity in a series of discussion meetings. Junior collaborator in the theory group Rini Renne was very interested in the non-

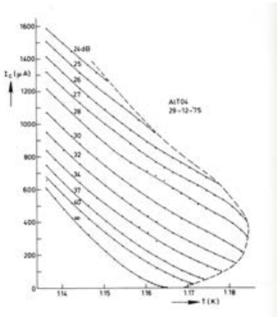

Jaap Kokkedee


Dick Polder

linear equations that describe the junction when it is shunted with a resistance. In this work he collaborated with Dick Polder, who was a part-time professor with his main job in Philips Research Labs. Dick Polder was the one who in a series of lectures introduced modern solid-state physics in Delft. For me he became a wise older brother.

In common with other groups working on superconducting microbridges, we observed what was called the Dayem effect. From theory and even more from common sense, one expects that the critical current of a Josephson junction goes down when microwaves are applied. However, it was often seen that the critical current went up. There were two possible explanations.

One relied on the junction character, and presumed that nonlinear effects in the high current region between the two superconductors were responsible. The other explanation by Eliashberg was in essence a bulk effect. The microwaves would pump electrons to higher energy states where they are less efficient in blocking the pair formation process. Teun and I discussed this and designed a good test. We would make a long narrow strip between two superconductors. The microwave intensity in that strip would be high, but the banks would be decoupled from each other. With our scratching apparatus it was very easy to make a narrow line with a length of 100 μm. The day after our discussion we had the sample, the day after we did the measurement. The critical current of the strip, and even the critical temperature, went up much more than was seen in short microbridges. Not a junction effect, but non-equilibrium superconductivity as predicted by Eliashberg. No room for discussion left.



Microwave enhancement of the critical current.
Radiation increases from bottom to top (number indicates attenuation).

Teun performed further measurements and eventually we had material for a publication. Our lack of international experience made us decide to submit the manuscript to Physica, a Dutch physics journal. The editor in Leiden told us that they had started a new 'Letters' segment with faster publication. Of course, we should have gone to Physical Review Letters, the leading physics journal. In those prehistoric times, transfer of knowledge was exclusively through face-to-face meetings of people or by moving paper over the globe.

In 1975, Teun and I went to the Low Temperature Conference in Helsinki. This turned out to be another turning point in our development. The results on stimulated superconductivity came too late to submit them for presentation, but an improvised session was established on non-equilibrium effects. Teun presented his data there, which led to strong discussions. Russian scientists could not travel freely at that time, but as Finland was still strongly under Russian influence the number of Russians at the Helsinki meeting was higher than usual. We experienced their confrontational discussion style, Teun survived well. Our work was noticed by the community. In his letter to us, Mac Beasley had drawn our attention to a theorist in Germany, Albert Schmid. His student, Gerd Schön, had performed new calculations on microbridges. One morning, directly behind us in the line for breakfast we noticed two participants with 'Schmid' and 'Schön' on their nametags. Over our scrambled eggs, we immediately engaged in an intense discussion and a few months we visited them in Dortmund. Shortly afterwards they moved to Karlsruhe. Now, Gerd Schön is a close friend.

The Lorentz guest chair for theoretical physics in Leiden in 1975 was occupied by John Bardeen. He was invited to come to Delft to give a colloquium. For Delft it was an important event. He had twice received the Nobel Prize in physics, the first for the invention of the transistor, the second for providing the theoretical explanation for superconductivity fifty years after its experimental discovery. The Bardeen-Cooper-Schrieffer (BCS) theory is in many respects a highlight in physics. This visit was a very interesting but also very confusing experience. The colloquium was hopeless in terms of presentation (as rumors had predicted). In the program for Bardeen's visit some time had been reserved for Teun and me. We told him what we had seen with our microbridges. In particular we spoke about the extremely high local current density and about the observation of Shapiro steps at very high voltages which seemed to be out of line with the BCS theory. He listened but did not react. We could not help feeling that our questions were too trivial and we tried to expand. No reaction for a very long time. Then he started to answer everything we had said. It turned out that he had been thinking about our results, was intrigued and had no direct explanation. What more could we want? We met him a week later in Leiden for more discussions. In 1976 at the Applied Superconductivity Conference held at Stanford, Bardeen gave a talk with the broad title "Progress in Superconductivity". Imagine my

John Bardeen 1908-1991

surprise and delight when he singled out our Delft work as an interesting new direction.

John Bardeen was a very remarkable man, but a non-scientist normal person would never have noticed. Many years later, when I was entering the United States on a work visa, an immigration officer asked me whom I thought was the greatest physicist ever. I am sure he expected Einstein to be the answer. When I gave the name Bardeen, I had to explain but he accepted my sincerity.

In the following years, we more and more developed the contacts with the people in our scientific field. The attitude in this branch of research on superconductivity was very open and helpful. Leaders, such as Michael Tinkham at Harvard and John Clarke at Berkeley were generous by nature. They would share techniques and would answer our questions in a straight-forward way. They were also interested in what we did. Surely it helped very much that we did have interesting new results such as the variable thickness bridges and the Eliashberg measurements. We profited much from the exchange. We went regularly to conferences and workshops and became part of the family.

Both Teun and I went on an extended sabbatical period. I was the first in 1978-1979, spending 8 months at Stanford with Mac Beasley. I worked on fabrication of thin films of alloys (niobium-tin with a transition temperature of 15 K), but also on the superconducting properties of films with a high normal state resistivity. I learned about vortices in such films. Mac, his PhD student Terry Orlando and I wrote a paper predicting a Kosterlitz-Thouless phase transition in these films which was indeed observed later. The paper

is well-known and still relevant. Teun went to Harvard for a year in 1979-80 and worked with Mike Tinkham and Gregg Blonder on superconductor-normal metal-superconductor sandwiches with Andreev reflection at both interfaces. They were very successful in explaining known experimental observations such as excess current and subharmonic gap structure. This work is still cited very highly and "BTK theory" is a common buzzword. For both of us it was a very important experience. Spending a period with one's family in an attractive environment, experiencing the high-level scientific culture. Also, realizing that the students of these top-level institutes were not necessarily smarter than ours had a long-lasting stimulating influence.

FOM, external support

For the first few years, the group budget coming from the Department of Applied Physics was enough for our needs. Gradually, our needs expanded and we became interested in external resources. Professors at Delft University often came from industry and relied on their private connections to run their research. For us the only source for external money was the national funding agency ZWO (translated: pure scientific research). Our professor Westerdijk was on the board of that organization. Specifically for physics there was FOM (translated: fundamental research on matter). FOM was set up after the second world war mainly to coordinate nuclear and high-energy research. FOM was extremely well organized with an office in Utrecht. Although its budget came from ZWO, FOM behaved very independently with its own informal connections to the ministry. FOM ran its own institutes for nuclear physics and high-energy physics. In addition, FOM supported fundamental research at universities through so-called werkgemeenschappen (literal translation: work communities). Until around 1970 there was enough money to support practically every physics professor at the non-technical universities without competitive procedures. New people were funded through automatic increases of the budget. However, around 1970 the budget was frozen. New people got nothing. The en-trenched professors hang on to what they had.

In FOM, it was the young co-director Kees Le Pair who designed a proce-dure for change. A fraction of the budget of all werkgemeenschappen was taken to create the 'beleidsruimte' ('policy space'). Any physicist with a permanent appointment at a university could apply. Competition was across the full range of physics. A selected jury decided, with written input from anonymous experts. If a position was awarded, it was provisionally attached to a werkgemeenschap. If awarded again after two years it was added to the structural formation. In this way a werkgemeenschap could gradually grow relative to the others. Within the werkgemeenschappen a system of internal evaluation based on external referee reports was initiated. These new rules had just been in place for a few years when we entered the scene.

We submitted a beleidsruimte proposal in 1975, asking for a PhD student position and some working money. It went well, our project was awarded. I became a member of the committee of the werkgemeenschap Solid State. It consisted of about 20 professors from all Dutch universities, complemented with three members from the Philips Physics Labs (all also part-time professors at a university). The scientific program of the werkgemeenschap was the sum of the work of all committee members, excluding Philips. I had no frame of reference, but looking back I see how eccentrical the program was relative to the international community of solid-state physics. There was very much emphasis on magnetic materials and on magnetic resonance methods. In the nineteen fifties and sixties, Gorter as the director of the Kamerlingh Onnes Lab in Leiden established a strong program in these fields. His pupils were now professors in many places and continued along the same lines. High quality, but with a tendency to focus on details. There were two main exceptions: Isaac Silvera in Amsterdam and Peter Wyder in Nijmegen who came from outside The Netherlands with a fresh view.

The turnaround that had been initiated in FOM had effect. Procedures with anonymous grading made a strong difference. The Solid-State committee consisted of good scientists with a critical mind who were willing to respect the outcome of an agreed evaluation. Significant shifts occurred where Silvera, Wyder and others could grow at the cost of the more traditional groups. Also, researchers that had their main basis in other branches of physics or chemistry, such as Van der Waals and Sawatsky, could become important members of Solid-State.

Once we had understood how the system worked, we made full use of its possibilities. We submitted a series of Beleidsruimte proposals that were well received, and we gradually, over the years, built up a werkgroep with about five structural positions that were funded by the Werkgemeenschap.

Looking back, I think this was crucial for our development. With those semi-permanent FOM positions we did not have to sell a project as with a Beleidsruimte proposal. We could take risks, as long as the total group scored high enough. We could have a PhD student explore a new direction that we did not yet understand well enough to write a convincing story. Throughout the history of the Quantum Transport group we have used the credit that we had built up to start something new that our intuition told us was promising, but that a referee could blow away with good arguments. Sometimes these new ideas did not work out, but we were right often enough.

FOM started a new program in 1978 on technical physics. In the first round of that program we submitted a proposal for SQUID fabrication which was awarded. In 1981 a new funding agent for all applied sciences was started, known as STW (Foundation for Technical Sciences). Within FOM a new werkgemeenschap for semiconductor physics was started in 1981. There, we also submitted projects that were accepted.

Around 1985, at the end of our “learning” period, we were firmly present in FOM. I was secretary and later chairman of FOM Solid-State. Without FOM we would not have been able to build the group that QT became. A different group might have resulted without the Q in its name, focusing on sub-millimeter spectroscopy as a tool. We would certainly have missed great opportunities.

An established group 1980-1985

Around 1980, the superconductivity team within Molecular Analysis had become a young but established research group. The initial years had brought international contacts and opened the connection to outside funding. After that, we gradually expanded. Our superconductivity project took up a larger and larger fraction of the Molecular Analysis group. The group leader Jan Berend Westerdijk was always very supportive. His health deteriorated and he had to stop coming to the laboratory. In 1980 he retired in silence, without the usual academic event. He also refused to accept an informal farewell party with the many ex-students, but we could persuade him to come one final time to a group coffee with cake. The photograph taken there shows him in the middle, bravely suffering our efforts. Westerdijk died in February 1982. I fondly remember him with great respect.

farewell coffee with professor Westerdijk, 1981
Teun Klapwijk is standing behind Westerdijk, our main technician Chris Gorter is seated left of him. I am on the ground in front, flanked by the two PhD students Marianne Stuivinga and Victor de Waal.

I was appointed “personal professor” in 1980. After the retirement of Westerdijk the status of our group in the department was not quite clear. In practice, not belonging to an official chair gave few problems. We could deflect the suggestion to incorporate us in the large Low Temperature group. In 1980 I gave an official speech as a new professor with the title: “Superconductivity on a small scale”. This title not only referred to the submicron structures that we used, but also to the position of a small university group relative to large efforts in industry and national labs in the US. In particular, at IBM a major program had been started to develop a fast digital computer. Small Josephson junctions, primarily of lead but later also niobium, were integrated into large electronic circuits. A special issue of the IBM Journal of Research and Development in March 1980 gives both overviews and details. Close to 200 people were active at the high point, just before the Josephson project was abandoned altogether in 1983.

Our chosen field was superconducting electronics, where our goals became the study of fundamental processes as well as applications of Josephson junctions in devices and circuits. We used microfabrication techniques to achieve thin film devices. In The Netherlands there were no other groups active in this field. “Bulk” Josephson junctions, niobium point contacts, were used in Groningen for experiments on submillimeter detection and in Leiden for quantum interference. We were convinced that sustained development

of Josephson devices was only possible through thin film techniques. Our results on variable thickness microbridges and microwave enhancement in long strips had brought us international recognition but demonstrated that microbridges were not the easy substitute for point contacts that we hoped for. Clearly the high current densities led to non-equilibrium processes of various nature that were not yet understood. We became very interested in these physical processes and our first PhD student (apart from Teun), Marianne Stuivinga, had the task to study so-called phase slip centers in long aluminum bridges. She started in 1976, funded by FOM.

The first two PhD students Marianne Stuivinga and Victor de Waal, who worked on phase slip centers in long aluminum strips and niobium SQUIDs, respectively.
They both finished in 1983.

We also wanted to keep working on applications of Josephson junctions. Development of detectors for submillimeter radiation, at the origin of the group, was pursued by the national space organisation SRON in Groningen with their student Daalmans in Delft. The latter left in 1979 and we decided to leave the subject to SRON. Instead we took up research on SQUIDs, superconducting sensors of magnetic flux. In many places SQUID sensors had been developed and applied in many areas such as geological surveying and cardiography. Almost exclusively, bulk point contacts were used.

We hired a PhD student, Victor de Waal, to develop SQUIDs and SQUID arrays using thin film techniques. The results of Daalmans on niobium junctions gave a good start. De Waal had excellent results, for example he was the first to fabricate a gradiometer system with thin film junctions and loops. Coming from a family of entrepeneurs, be it in concrete foundations, he seriously developed a plan for a small company to sell SQUIDs commercially. He made a business plan under the name SQUICTOR and started on finding sponsors. The concept of a high-tech start-up was certainly not so common as it is today. We took the plan very seriously and started a new PhD student with the task to develop a small closed-cycle refrigerator for SQUID-use to decouple the commercial product from helium liquifiers. Niek Lambert started in 1986 with a partial home base in the Low Temperature group. By the time he finished, the SQUICTOR project had died. In the De Waal family a new director was needed unexpectedly and Victor spent his further professional life providing existing buildings with in situ concrete support. We did not initiate a new application project.

Marianne studied the non-equilibrium region around a constriction in an aluminum strip by placing small tunnel junctions at several distances. She could measure the decay of the quasiparticle potential over tens of microns. This confirmed the theoretical model of the Tinkham group. I cannot help relating the following story from her Delft period. We went to the Low Temperature Conference in Los Angeles in 1978. Marianne was scheduled to give a talk on her measurements. Early that morning, her husband Victor Goldman (from the Silvera group in Amsterdam) was looking for me at the conference site. It turned out that Marianne that night had given birth to a son. Being premature, the baby needed isolation and special care, but otherwise everything was fine. Victor gave me her overhead sheets and a short time later I gave her talk. My introduction certainly drew attention.

We continued working on non-equilibrium superconductivity in aluminum. We started a PhD student (Peter van den Hamer) who analyzed the enhancement effects in a quantitative manner, comparing with theory. The relaxation of the non-equilibrium involved inelastic scattering processes, both electron-electron and electron-phonon. The numbers that we obtained connected with results from the field of weak and strong localization that was very popular in that period.

Center for Submicron Technology

Submicron fabrication of the samples that the group investigated was opportunistic and pragmatic. The diamond scratcher could make narrow straight cuts in the surface of a substrate and it could cut a line in a polymer resist film. Students experimented with electron beam lithography with left-over tools of the Electron Optics group, with surprisingly good results. Hans Romijn, who became a PhD student in 1983, converted an old Philips

SEM500 electron microscope to a writing tool with an electronic box that switched the beam on and off. Because we spent a large fraction of our group's effort on fabrication, we were in this respect certainly as good as the best physics groups internationally. There was a considerable gap between those physics groups looking for new effects and the electrical engineers in microelectronics, who used optical lithography with a resolution of several micrometers in line with the industry standard at that time.

But then, a Deus ex Machina manifested itself. Maybe, in this particular case, one should speak of a Machina ex Deus. A very advanced electron beam lithography machine was placed in our Applied Physics lab in Delft. It was a new product that the Philips electron microscope division had developed and was taking to the market. It was called the Electron Beam Pattern Generator (EBPG) and it had a nominal resolution of around 20 nanometers over an area of $(0.5\ \mathrm{mm})^2$. Its price for industry was several millions of dollars. It came to Delft on the initiative of the university together with TNO (government organization for applied research, their TPD branch was closely connected to our Department of Applied Physics). The imminent retirement of the electron optics professor Jan Le Poole also played a role, him lobbying to give the remaining group a new goal. The ministry in The Hague came forward with a special grant in the context of promoting advanced technology. This kind of machine was far out of proportion with university practice. It needed a clean room of class 100 and very high standards of vibration isolation and temperature control. Just to run it and keep it up to standards was too expensive for a regular university group. A separate unit was created, called the Center for Submicron Technology (CST), with a dedicated budget. A staff of two people was attached, one being the business manager who should promote the industrial use and the other a technician who was very good in setting up and running electron microscopes. They had no experience with lithography, the Center had no equipment for deposition or etching of thin films.

The decision to place this machine in Delft was certainly ill-conceived. There was no scientific program, there was no team, there was no infrastructure. One should never allow a retiring professor to influence the program of his or her successor. In the electron optics group the successor, when he came, had no personal interest in submicron fabrication. Nevertheless, for us the new Center created new possibilities. In the Department we made the case that there should be a scientific director and a scientific staff with its own program on nanofabrication technology. This was taken up and in 1982 Sieb Radelaar was appointed as the director and as a professor in the Department. In the following years he collected an extensive staff and succeeded in having the Center recognized as a FOM institute.

Sieb Radelaar

It took many years before our group had publications based on samples that were fabricated with the EBPG. Our students had their own ways to make their samples and had no strong incentive to change. In the beginning they were not allowed to operate the EBPG themselves and as a consequence were dependent on the kindness of the small CST staff. The strong point of the EBPG was that it provided a high resolution in a large field. For me it was a reason to start a research line on two-dimensional arrays of small area Josephson junctions. The junctions had submicron dimensions and a sample could contain thousands of junctions with nearly identical parameters. For this, the EBPG was ideal. For a project like that to come to fruition, the time constant is around five years. Cutting edge lithographic fabrication is not a matter of simply following recipes, but of achieving systematic control of all the different steps that are necessary for a particular sample. From around 1987 a continuous stream of samples and measurements resulted. After 1990, the EBPG was the main nanofabrication tool for the group.

New research lines

As a superconductivity group we wanted to have access to a wider range of materials than the pure metals such as aluminum and niobium. During my time at Stanford, I had participated in research on niobium-tin alloy films

that were made by simultaneously evaporating the two constituents. One needed to measure and adjust the evaporation rates with feedback control. We decided to build a similar facility.

In 1983 we installed a new ultra-high vacuum deposition system for metal films, known in the group as the UTS. It had two independent sources with individual programmable rate control. The system could be used to deposit films of alloys with fixed composition, but the rates could also be varied in time to achieve a continuously varying superlattice. The project on superlattices was run jointly with the Crystallography group who had a strong research line on ‘natural’ modulated crystals. Our two groups were joined at that time in a new organizational unit, the ‘vakgroep’ Vaste Stof (Solid State). We attracted a PhD student, Roland van der Leur, who belonged to both the section VS-FK (solid state physical crystallography) and our section VS-SG (solid state superconductivity). Another PhD student (Jacques Schellingerhout) was found to fabricate and investigate films of metallic alloys. We wanted to study and use metals that were superconducting with high critical temperature and/or with very high normal state resistivity. Both students started in 1983 and took control of the UTS, which was a job by itself.

Another PhD student, Wim van der Wel, started in 1983 on a program to develop a practical fundamental standard of resistance for use in the Van Swinden Laboratory. VSL is the official metrology institute in The Netherlands. It already had a fundamental standard of voltage based on the Josephson effect. In this context, fundamental is used to indicate a standard that derives its value from Planck’s constant h and the electronic charge e. The Josephson voltage standard defines the Volt by using the relation between the voltage V_n of the n^{th} Shapiro step and the frequency of the generating AC signal, assuming the frequency is known very accurately. Similarly, the quantum Hall effect, discovered by Von Klitzing in 1980, connects plateaus of resistance with the fundamental quantity h/e^2. This project was not directly in line with the rest of our program, but it was meant as a longer-term investment. The main goal was to establish a foothold in the semiconductor world, where our submicron lithography and low temperature measurement techniques might bring in new possibilities. It gave me a good reason to go to conferences such as the EP2DS conference in Oxford (1983) to meet personally with people from the leading groups and to learn about the materials.

The real basis for our later work on semiconducting systems was laid at the Low Temperature Conference in Karlsruhe in 1984. During a break I had a meeting with Joachim Wolter who was the leader of a group at the Philips Research Laboratory in Eindhoven. In his group, among other subjects, electron transport was studied in clean III-V semiconductor systems at low temperatures. In the Philips lab in Redhill, England, heterostructures were grown epitaxially of two materials with different gaps. At the atomically flat interface between the two materials a potential well was formed and with the right density the electrons only moved in that flat well. They were strictly two-dimensional with an extremely long mean free path. So far, they had only been studied as “bulk” 2D systems. It turned out that at the lab in Eindhoven there was no suitable lithography available to fabricate narrow lines and narrow diaphragms. They were interested in our lithography and we were interested in their ultraclean heterostructures. In the park in Karlsruhe we agreed to start a joint project on submicron patterning of their heterostructures. We agreed that the transport measurements would be made both in Delft and Eindhoven.

Looking back, this meeting was of crucial importance for QT. It led to the discovery of the quantized conductance four years later. However, I should not run ahead of the story. I see great irony in the fact that Philips Research needed us because we could do submicron lithography with an old Philips scanning electron microscope and a new Philips electron beam pattern generator and they could not.

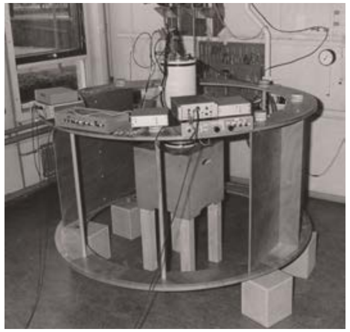
QT at the end of its learning period

In 1985 Teun Klapwijk left QT to become a professor in Groningen. There he started a new research group that focused on mesoscopic superconductor-semiconductor systems. His new environment stimulated him to develop high mobility silicon MOS-FETs for transport studies at low electron density and, in line with his original masters’ project, niobium junctions for astronomical submillimeter detection. In all fields he did very well.

Teun was very important for the start of QT. Within the group Teun and I formed an informal two-person management team. “Learning” was done by him and me together, without him the group would not have been the same. We had different roles, as I had more outside obligations. I was teaching lectures, had to be on various committees and I interacted with the other faculty and with FOM where we found our solid support basis.

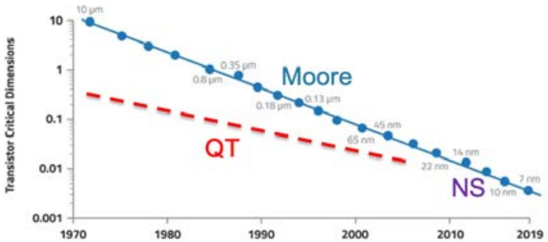
After Teun’s departure the group, still called Vaste Stof–Supergeleiding (solid state–superconductivity) consisted of one professor, five PhD students and around twelve undergraduate students. We had a half-time secretary and two full time technicians. In addition, a number of people from departmental services were formally attached. As a group in the Department of Technical Physics we were small.

So, what did we learn in our “learning” period? We had found a focus point for our research: investigating new effects in electronic transport associated with lateral dimensions on the submicron scale. We had developed techniques to fabricate such structures and had achieved a collective expertise that could be aimed at new projects. We had established good personal relations with the top researchers in the world on Josephson junctions and small superconducting structures, both in theory and experiment. We had learned how and where to present our results, at international conferences and in the respected international journals. We had found and developed a sound financial basis for the group, even when our primary interest was on new phenomena with no direct applications within 10 years.



test arrangement for SQUIDs

Fabrication



The figure shows the smallest dimensions in fabrication of electronic elements over the years. The logarithmic vertical scale is in micrometers. The blue dots and the blue line refer to commercial integrated circuits. The line follows Moore's law. The red dotted line is a rough indication of the smallest dimensions realized in QT. Before 1980 we used single object opportunistic methods. After 1985 we used the electron beam pattern generator. For years we were one of a few university groups with controlled lithography down to 20 nm. Now, lithography with scanning probe techniques permits nanoscale dimensions (NS).

Part 2. Growing, 1985-2000

Introduction

For the emergence of a strong nano/quantum focus within Delft University, this second period of QT history was crucial. In the years between 1985 and 2000, the scope of activities grew dramatically. In a widening range of materials new phenomena were investigated that occur in fabricated structures with submicron dimensions. With Philips Research Labs, QT started fabrication of small structures at the interface of two different III-V semiconductor materials. Electrons in these epitaxial materials have a very long mean free path. The investigations led to the discovery of quantized conductance in narrow two-dimensional contacts. The research was expanded to confined electron states in so-called quantum dots. Together with the Tarucha group in Japan, artificial two-dimensional atoms were developed. Similarly, spectacular results were achieved in new research on electronic conduction through carbon nanotubes, which can be viewed as single molecules with well-defined atomic structure. These nanotubes were grown by the Smalley group in Houston. Both with the semiconductor heterostructures and the nanotubes, our group was attractive for a collaboration to study transport properties because we could fabricate the submicron contacts and we had the equipment for low-temperature measurements including the electronic filtering at various temperature stages. That experience we developed in the international community studying fabricated superconducting elements with circuits of small Josephson junctions. Quantum behavior of a new kind was discovered. The same samples, with superconductivity suppressed by a strong magnetic field, exhibited tunneling of single electrons. In these years, the QT group in Delft became very visible on the international scale.

Not everything worked so well. We also started new research on charge density wave structures where we thought that submicron fabrication and mesoscopic measurement techniques could bring new effects. We never got this under control well enough and we abandoned it after the first round. As from 1987, the global physics community was shaken up by the discovery of new superconductors at much higher temperatures. A technological breakthrough was predicted and governments all over the world initiated highly funded research and development programs to be part of that wave. We in Delft participated in the Dutch High-Tc Superconductivity program and had a team of several postdocs and PhD students active in the field. Good solid work came out, but we could not use our special skills. In submicron structures, the materials of that time lost their good properties. We abandoned this field as well.

The group changed in character. It expanded in size as well as in scope. The "permanent" QT scientific staff grew from one (Mooij) in 1985 to four in 1990 (van der Marel, Harmans and Hadley added). Van der Marel left in 1993 but Dekker and Kouwenhoven came at about the same time. The dominance of the large group of undergraduate students was reduced when their time for research, originally almost two full-time years, became little more than one year. The number of PhD students grew (almost all Dutch at first) and the new category of post-docs arrived (all non-Dutch). QT was distinctly different in 2000: larger, more ambitious, self-confident (outsiders said arrogant), vibrant. By 2000 Cees Dekker left QT to start a new nano-bio activity. At the end of the second period, the QT permanent scientific staff consisted of Mooij, Harmans, Hadley and Kouwenhoven.

Infrastructure

Advanced research on condensed matter physics inevitably requires a considerable infrastructure. One needs ideas, certainly, but they remain fruitless if they cannot be checked in experiments. The right sample has to be prepared and measurements of well-defined properties have to be performed under the right conditions. One has to invest in tools, both in terms of equipment and of expertise. Equipment can be bought if a sponsor is willing to provide the funds, but expertise must be developed and maintained over many years, more than one student generation. In practice, the choice for new experiments is determined as much by what is possible as by the curiosity-driven wishes of the researchers. Building the tools is expensive and slow, but once they exist they may inspire new research questions. The history of QT, in particular in this second 15-year period, is a good illustration.

Around 1985, I was the only faculty member in the group. I did make a conscious choice which I can illustrate with the following three wonderful but very different physicists in the field of superconductivity. Mike Tinkham at Harvard was our great example where it came to understanding phenomena in small structures and tunnel juncions. He understood BCS-theory, did not get lost in complicated calculations but could think up the key experi-

Mike Tinkham Mac Beasley Ted Geballe

ment. He had a small group with no long-term collaborators and did not invest in infrastructure. Mac Beasley worked with him at Harvard as a non-tenured associate professor before moving on to Stanford where as a new tenured professor he joined up with Ted Geballe to form a single group that shared equipment and funding. Later Aaron Kapitulnik came to Stanford and the three professors formed the famous and very successful KGB group. Ted Geballe came originally from Bell Labs where the solid-state physics of our textbooks was co-invented. Geballe and Beasley set up supporting labs for the production and characterization of alloy materials, deposition of thin films and measurements in strong fields. They kept in mind what they wanted to achieve and did not get lost in the techniques. Mac looked for the physics effects in new superconducting materials and understood that the material itself needed to be well-defined to get anywhere. He, about the same age as I, became a close personal friend.

I had been visiting the Tinkham group several times. I was a great admirer, but when I went on my sabbatical I did not choose to go there. I felt that in our field the fabrication and measuring techniques would become more and more important. I went to Stanford (1978-1979) where I learned much about superconducting thin films. All this had a profound influence on me and therefore on the Delft group. My strong belief that one has to invest in long term development of supporting technology, even as a university group, was born at that time. The larger group with multiple principal investigators was my model for QT. Joint group meetings, sharing of facilities, shifting funds to help the start-up of new activities are very valuable elements.

Our group in 1985 had the name Superconductivity but our original technical aim to develop radiation detectors had been abandoned. We had developed tricks to fabricate small microbridges, narrow lines and small tunnel junctions with dimensions significantly below 100 nm. Such techniques were not common, Moore's law prescribed a minimum feature size of 1 μm for transistors in 1988. We saw opportunities and started research on small tunnel junctions in the "normal" (non-superconducting) state. We also wanted to aim our submicron techniques at semiconductors and possibly other materials. We had built up credit in the university and in FOM (the physics funding agency), and we were given the chance to invest in major equipment.

The special infrastructure needed for our type of research had several facets: thin films with good characterization, patterning at submicron scale, and electrical transport measurements at very low temperatures with extreme control of the back-action from the measuring apparatus and connections.

Submicron fabrication involved complications far beyond our group. From our side we were very eager to profit optimally from the unexpected windfall when the Philips EBPG was dropped on our doorstep. However, in many ways this apparatus was a ticking time bomb. The service contract to keep it running amounted to a yearly sum far beyond any regular budget. It was covered for the first years, but it had been assumed that industrial contracts would come in soon. Clearly this was too naïve. The technology first needed to be developed and kept up to date by a considerable team of dedicated people. When Sieb Radelaar came over from Materials Science and a special FOM Institute was established for submicron technology, the personnel issue was solved. Radelaar collected a group of about 10 engineer/scientists who were devoted to the technology but also could initiate their own science projects.

We managed to establish a good working relation with the Center for Submicron Technology (CST) where our own young people could run the machines. In this way the students knew all details of their fabrication process with many steps. On the other hand, the CST staff had to keep the machines on specification and did not always feel this was sufficiently appreciated.

The Department of Electrical Engineering ran a successful institute called DIME (Delft Institute of MicroElectronics). They had chosen not to try and keep pace with the mainstream industrial chip laboratories, but focused on sensors and actuators where requirements on small dimensions were more relaxed. The institute had its own staff that fabricated samples designed by

the groups in Electrical Engineering. Simon Middelhoek and Jan Davidse were the more senior principal investigators. A national Program for the stimulation of microelectronics was initiated to help DIME and the corresponding centers at the other technical universities to modernize and run their facilities. Sieb Radelaar saw a chance to find a stable funding base for the Submicron Center and applied for funds from this program. The DIME people felt that this physics-related activity only would take away money and would not strengthen the program as conceived by them. Strong words were exchanged, in particular Middelhoek and Radelaar were not afraid of expressing their opinions of the political situation and of each other.

After evaluation of the proposals The Hague decided to award stimulation money for the support of DIME but also for CST. It was made a condition that the two elements in Delft would form a single institute and that there would be one clean room to house their equipment. Both the EE side and the Technical Physics side were not happy, but had no choice. A new institute DIMES (Delft Institute for MicroElectronics and Submicron Technology) was created and the University Board (College van Bestuur) took personal charge to implement it. A search was started for the new director for the joint institute, and it was stipulated that the directors Middelhoek and Radelaar could not be candidates. Interesting times, I had the honor to be on the search committee with Davidse, university oficials, and representatives for the other technical universities, industry and FOM. Our choice was Piet Balk, an almost retired Dutchman at Aachen University. He had a good reputation in the field of silicon oxides and a quiet personality. He did not set up significant new research in Delft but kept the institute in the air. The DIMES institute had a good working budget and the submicron technology remained available for us. Disadvantage was that our equipment moved to the new large clean room on the Electrical Engineering site and our students had to cross a road and pass through long corridors to get there. I sat on the board and spent many hours in meetings. That was a small price to pay for a lithographic facility that we could use freely and which was better than almost any other university set-up in the world.

CST and DIMES did not have deposition equipment that could deliver the superconducting films that we wanted. In our group we could deposit aluminum or niobium films. Aluminum had many advantages but one big drawback: its critical temperature is not much more than 1 K. Using a superconductor is best done at half its critical temperature or below, because otherwise there are too many non-superconducting electrons around. Easy cryogenics was in the range of 1 to 4 K and for that reason niobium with a T_c of 9 K was much more attractive. Good, well-defined niobium films are not so easy to make and even more difficult to pattern. There is no good insulating natural oxide and it is therefore not easy to make good quality tunnel junctions. At Stanford I had been involved with films of superconducting alloys such as niobium-tin, with a critical temperature of 18 K and in many respects a better-defined material in the category of the so-called dirty superconductors. We wanted to develop means to fabricate films and Josephson junctions of niobium-tin. As in Stanford we would co-evaporate with controlled rates to get the right composition, with the rates controlled by fast feedback. Our special idea was also to develop a thin tunnel barrier by changing the composition in a very thin layer to “insulating” and back again to metal.

Jacques Schellingerhout at work on The Balzers UTS deposition system

In 1983 we bought a Balzers deposition system with two electron beam guns. In an e-beam gun an amount of material can be locally molten and evaporated. Heating is done with a beam of high-energy electrons. The rate of evaporation is controlled with the current of the beam. The special feature of our system was the rate control. A mass spectrometer, near the substrate where the film was to be deposited, measured the local pressure of each material and the two signals could be fed back to the appropriate gun controls to adjust the rates. On paper the rates could be adjusted on the fast time scale we needed, but it turned out not to be so easy as I thought. Two new PhD students were put on the task but they had a hard time. Jacques

Schellingerhout applied all his technical ingenuity and managed to get results, but hardly could touch the nice physics that we had in mind when he started. The other student, Roland van der Leur, was appointed half-time in the crystallography group to study modulated crystals which he could make by periodically varying the composition. Commissioning this advanced apparatus was too much of a burden for these two students, but our group could afterwards dispose of a versatile tool that allowed, with sufficient effort and attention, to fabricate interesting films. It has been used intensively over many years, and still is in active use.

Another significant addition to our arsenal was an Oxford S200 dilution refrigerator. Until 1985 the lowest temperature that we could reach was 300 mK in our ^{3}He cryostat, for a few hours at a time. ^{3}He-^{4}He dilution refrigerators offered much lower temperatures, down to 20 mK, in continuous operation. In these systems the two helium isotopes ^{3}He and ^{4}He are used to establish a closed cycle where (in simplified terms) ^{3}He is pumped around, mixing with ^{4}He in the mixing chamber where heat is extracted from the environment. The S200 was what now is called a “wet” refrigerator. The critical area was shielded from room temperature by two concentric shields, the outer one at 77 K as cooled by a bath of liquid nitrogen and the inner shield at 4 K as cooled by a bath of liquid helium. The two liquids had to be refilled at least once a day, otherwise the system could run for weeks on end. The present-day QuTech Lab has a multitude of “dry” fridges, where a cryocooler keeps the shielding cold so that students can stay in bed on Sunday mornings. The disadvantage is a penetrating huffing and puffing sound that gets to some people’s nerves.

Not too long before we bought our machine, dilution refrigerators were not tools for general use but rather research projects for low temperature physicists. The Oxford machine represented a breakthrough in this respect. However, that was not the full story for a group that wanted to perform electronic measurements on real samples. A bare piece of aluminum or niobium attached to the mixing chamber and surrounded by a well-cooled radiation shield would assume the low temperature. However, connecting wires for a transport measurement or microwave connections for high frequency electronics bring in noise from room temperature that will heat up the sample, in particular the electrons. At very low temperatures, electrons are only weakly coupled to phonons and can have effective temperatures

Bart Geerligs, start-up of our S200 dilution refrigerator

significantly above the bath temperature. Every connection between the room temperature electronics and the sample has to be carefully guarded by filters that are firmly anchored to intermediate low temperatures. A single filter at the temperature of the mixing chamber would itself heat up by the incoming noise. We had to learn all this, the criterion being the quality of our measurement results. We had close contacts with the Quantronics group in Saclay (before it had that name) and learned much from discussions with them. We achieved a good standard, but it always remained an issue that required much detailed attention. It is easy to spoil an experiment, even if almost everything is done correctly. The quality of a research group is as much determined by control of practical experimental details as by deep insights.

Care for our methods and tools was, from the beginning, a matter of attention and initiative of students even more than staff and technicians. For our new types of ultrasensitive electronic measurements, sample fabrication, and data processing one could not buy adequate equipment. QT had a long tradition of home-developed programs, boxes and standardized tricks. Our masters' and PhD students were capable and willing to invest time in finding solutions for practical use that were not (yet) for sale. There was a text editor in QT long before Word Perfect. SGPlot was a home-developed software program where data from experiments could be plotted and compared with theoretical models. The hopelessly complicated problem of connecting voltage, current and other meters to samples without ground

loops was solved with the QT "meetkast" (measuring box) that contained plug-in modules with separating amplifiers. Hundreds of such boxes were put together by the electronic workshop in the laboratory for friendly groups in our field, all over the world (often requested by former students or postdocs). The first e-beam lithography was performed by means of an old scanning electron microscope for which Hans Romijn developed a pattern generator. The atmosphere in the group encouraged such time investments beyond one-time pragmatic actions. The staff of course welcomed such actions and found money for components, but the initiative usually came from students. Remarkably, the postdocs that later came from elsewhere in large numbers did not have that same attitude.

Mesoscopic semiconductors

Starting in 1980, research on fundamental semiconductor physics went through an exciting period. New quantum effects were discovered for electrons moving in a two-dimensional plane, placed in a strong perpendicular magnetic field. The field exerts a force on the electron perpendicular to its motion. If a current between two contacts runs in the x-direction, a voltage develops in the y-direction. This is the Hall effect, dating back to 1879. The ratio voltage/current (current/voltage) is called the Hall resistance (conductance). For electrons that are restricted to a two-dimensional plane such as a semiconductor surface or interface, the Hall effect is strong and unusual. At low temperatures, for strictly 2D electrons, the Hall conductance was found to be exactly quantized in fundamental units e^2/h (e is electron charge, h is Planck's constant). Von Klitzing discovered the integer quantum Hall effect in 1980. He recognized that the conductance remained exactly on the fundamental values when parameters (electron density or magnetic field) were changed. These first measurements were performed on silicon MOSFET devices, but later focused on epitaxially grown heterostructures of III-V semiconductors. Electrons at the interface of two different semiconductor materials can have an extremely long mean free path. In such ultraclean samples the fractional quantum Hall effect was discovered in 1982 by Tsui and Störmer. Laughlin and others provided the theoretical explanation for both integer and fractional quantum Hall effects. It turned out that these explanations are very different, for the fractional quantum Hall effect the electron-electron interactions are important.

Clearly these were exciting times in the research field. The Nobel prize for physics went to Von Klitzing in 1982. One understood that the effect could be applied as a standard of conductance (or resistance), on the same footing as the Josephson voltage standard. In several countries such standards were developed. The Van Swinden Laboratory (Netherlands Bureau of Standards) was also interested. We submitted a joint proposal to the new Foundation for Technical Sciences STW and in 1983 our PhD student Wim van der Wel started. I hoped that we could use this project to make a connection to fundamental semiconductor physics in the non-diffusive ballistic regime. It seemed obvious that a next step there would be the study of small fabricated structures, in the same way as in superconductivity the study of thin films was followed by narrow lines, microbridges and tunnel junctions.

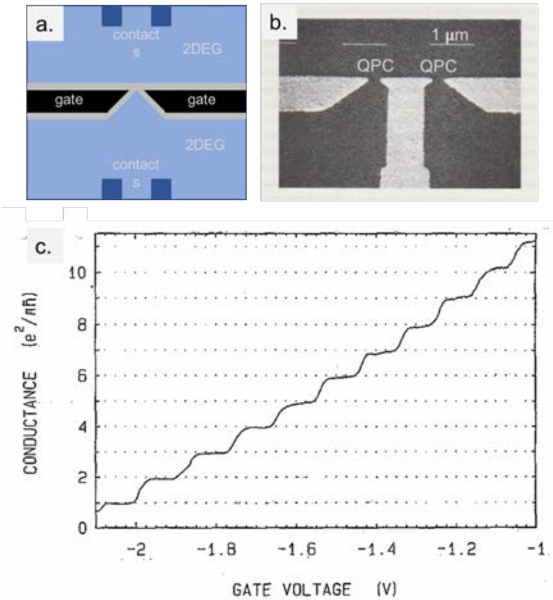
Following discussions at the Karlsruhe LT conference in 1984, between Philips group leader Joachim Wolther, Philips scientist Pierre Woerlee and myself, we agreed to start a joint project. Philips would provide extremely high-quality two-dimensional electron gas (2DEG) wafers, grown at their Redhill Laboratories. Pierre Woerlee had developed chemical etching techniques to make patterns in the 2DEG without cutting through the delicate interface where the electrons moved. Delft brought in the nanoscale lithography. The two main persons to do the actual work became young Henk van Houten who had just received his PhD in Leiden at the end of 1984 before joining the Philips NatLab, and our new PhD student Bart van Wees. Bart finished his Masters' thesis on Josephson junction arrays in our group in June 1985 and had good experience with nanolithography. Carlo Beenakker had also recently joined Philips after finishing his Leiden PhD as a theoretical physicist, and focused his natural enthusiasm on the new subject.

The first series of samples was made using Woerlee's etching technique. It proved impossible to get a low enough scattering rate in the remaining conducting regions. The Pepper group at Oxford University had obtained better quality in-plane definition by using a gate on top of the 2DEG material. A negative voltage applied to that gate pushes away electrons from underneath and patterns in the 2DEG can be defined. The width of a narrow line or the size of a contact can be varied continuously. When this technique was applied using our lift-off lithography for the definition of the gate structure, very good results were obtained. In fact, it started a new field.

This may be the time to say something about the main characters. On the Philips side, John Williamson was added to the team. Joachim Wolter was appointed a professor at Eindhoven University and left Philips. The project was placed in a group led by Martin Schuurmans, a theoretician with no history or special interest in this subject. Henk van Houten was the one who gave out the Redhill material, with strict instructions what could and could not be done with it. Henk, besides being a smart scientist and a hard worker, was a natural manager who tried to formalize proceedings on behalf of Philips. He and Carlo Beenakker had intensive discussions. On our side, Bart van Wees was equally smart and hard-working, but by nature an anarchist who in initial joint discussions figured out what needed to be done but from there went his own way. Leo Kouwenhoven joined Bart for his masters' thesis work.

Several samples were designed, fabricated and studied. The Eindhoven people worked out a system where electrons were injected into an open half-plane through a narrow opening. They planned to study the ballistic (scattering-free) trajectories, using a second point contact as a detector. In parallel, experiments were set up to study electron transport through narrow one-dimensional lines and narrow constrictions. Papers about ballistic transport in confined structures by Sharvin were studied. I felt a strong association with our work on superconducting wires and microbridges, but of course the physical description for single electrons with spin is different from the condensate of paired electrons in a superconductor. Bart made a sample where both experiments could be accommodated, as shown in the figure. At that time the old SEM500 electron microscope was used for the lithography, as adapted by Hans Romijn.

A great surprise came from measuring the resistance/conductance of one single contact. A clear step pattern as a function of the gate voltage presented itself, unexpectedly. Obviously, one had hoped to see some structure as the quantum wave length of the electrons approached the size of the contact opening, but so many regular features were very unusual. It turned out that plotted as the conductance, the steps had a mutual distance of $2e^2/h$. It was immediately clear that here something new and very special presented itself. In retrospect it is very remarkable that nobody predicted this phenomenon of quantized conductance before it was observed. It took less than a week to provide a theoretical explanation. The conductance plateaus were clearly visible but not exactly flat, as could be explained from scattering and other influences. This quantized conductance could not be the basis for a new metrological standard such as the quantum Hall effect.



Quantum point contacts.
a. schematic layout, viewed perpendicular to the GaAs-AlGaAs interface where the two-dimensional electron gas (2DEG) is located. Two gate electrodes in black are shown on top of the 2DEG in blue. A negative voltage on the gates leads to depletion under the gates and in an area around them (grey), adjustable by the voltage. Current and voltage contacts to the 2DEG are shown at top and bottom. b. SEM picture of actual sample, central region with two point-contacts. c. Conductance as a function of gate voltage, as measured. The quantized steps are clearly visible.

Not much later the steps were also observed in Cambridge.

Within a short time, a manuscript was written in which the results were reported. It was sent to the most prominent journal for condensed matter physics at that time, Physical Review Letters. Because the paper was submitted around Christmas, we were anxious about the refereeing process in

which bad luck could easily lead to a delay of several months. I called the editor Gene Wells, to ask for his special care. I explained that we considered this paper as an important breakthrough, in a very competitive field. He promised to keep a close eye on the responsivity of the referees. This worked: the paper was received officially on 31 December 1987 and published in the issue of 29 February 1988, amazingly fast.

The discovery of the quantum point-contact (QPC) initiated a tsunami of new experiments in Delft/Philips and elsewhere. In a strong magnetic field, available in our new dilution refrigerator, the contributions to the conductance of the two spin directions could be pulled apart, resulting in steps at half the distance, e^2/h. In the new samples one could also study the quantum Hall effect with very new results and new insights. A deeper level of understanding resulted from the description that Rolf Landauer and his IBM colleagues Markus Büttiker and Joe Imry had developed about quantum channels in very general. Joe Imry came over to Delft to discuss our experiments, both he and we were very excited about all implications. That was as well, because as I remember the visit it was in a weekend and the lab was not heated. In those years the university management assumed that research stopped at the same time as the administration.

In our samples, each quantum channel for each spin yields the quantum e^2/h of conductance. In a QPC the gate voltage can be used to control the number of quantum channels passing through. For the description of the quantum Hall effect, the concept of edge channels was introduced. For free two-dimensional electrons in a strong perpendicular magnetic field, the electron states are the Landau levels at fixed separation. The Fermi level determines which Landau levels are filled. The levels are spatially flat in the bulk 2DEG, but at the edges the electrostatic potential makes the levels curl up. Where they cut through the Fermi level, an edge channel is formed. The number of edge channels depends on the electron density and the magnetic field. Each edge channel contributes a conductance quantum in transport. Experiments were designed where edge channels were passing through QPCs and could be cut. A beautiful, consistent picture emerged from all this. Other groups also participated in this new field.

Bart van Wees received his PhD in 1989 and stayed on in Delft until 1991. He moved to Groningen where he is now a Spinoza-prize-winning professor.

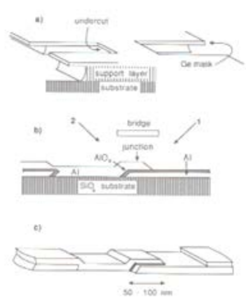
Leo Kouwenhoven finished his masters' thesis in May 1988, staying on in the group as a PhD student. His PhD exam was in June 1992. He received a fellowship from the Academy of Sciences and went to Berkeley from 1992 to 1994 (with Paul McEuen). Since then he is in Delft, in various capacities of growing importance. Kees Harmans joined us in 1987. He came from the Van Swinden Laboratory, the Dutch bureau of standards, where we had collaborated with him for the resistance standard. He joined the semiconductor work, later switching to the superconducting quantum bit experiments. He had an extended experience with optimizing cryogenic experimental arrangements for precise measurements. His presence brought a much-improved balance to the staff. He had a very good eye for the practical aspects that were connected with the deeper questions addressed by the group.

Metallic tunnel junction systems

We had learned to make small aluminum tunnel junctions with a shadow evaporation method. The principle is shown in the figure. Two aluminum films are evaporated under two different angles through a mask that is fabricated on the substrate. The first film is oxidized in situ to form a very thin barrier. The small overlap of two shifted patterns defines the junction. Junction fabrication involves many steps, all of which can go wrong. Complete understanding of the oxidation step of the granular aluminum was never achieved. Reproducibility of a process that worked was the highest aim in practice.

Two different lines developed with metallic junctions. Both could only take off when our dilution refrigerator, the Oxford S200, was operational. One was the study of Coulomb blockade effects, both in the superconducting and in the normal state. The typical small junction was around 100 nm square and had a capacitance of around 1 fF, the corresponding charging energy was around 1 K in temperature or 20 GHz in frequency. The other research line was the study of vortex dynamics in two-dimensional junction arrays. We found that vortices in arrays could behave as quantum particles.

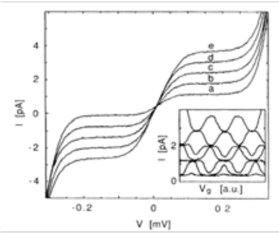
Two visitors were of importance. Kostya Likharev spent several months in Delft before he settled in Stony Brook in 1989. He was a visionary pioneer in predicting special quantum effects in small junction circuits. Terry Orlando from MIT spent a sabbatical period in our group in 1990. With him we discussed the results of measurements on quantum vortices in arrays.



Junction fabrication by shadow evaporation.
a. The mask through which the aluminum films are evaporated. The mask is patterned by e-beam lithography in a top layer on a support layer. The mask contains slits that are interrupted by small bridges. The junctions are formed below those bridges. The support layer is etched through the openings in the top layer with isotropic under-etching.
b. The substrate is placed in the junction fabrication chamber and the bottom film of the junction is evaporated through the mask. The substrate is tilted so that the metal pattern is shifted with respect to the mask. The film is oxidized at low pressure and for a short time. Then the substrate is tilted in opposite direction and the counter electrode is evaporated. The substrate is taken out of the chamber and the mask is lifted off.
c. The resulting junction.

It was not obvious that single electron or single Cooper pair physics made sense. Most theorists told us that the number of metallic electrons in a piece of granular aluminum with around a billion atoms, with all kinds of chemical bindings and a very complicated Fermi surface, could not really be counted. However, the experiments made clear that the particle tunneling through the oxide barrier is a true electron and not a complicated quasiparticle state. Samples could be studied in the superconducting state and in the normal state by means of a strong magnetic field. Measurements were done in transport, for instance through two tunnel junctions linking a small island to two electrodes. Clearly the normal state was simpler, superconducting samples exhibited more features that were not easy to classify.

The first PhD student on single charge tunneling was Bart Geerligs, starting in 1986. His first task was to commission the newly arrived S200 dilution refrigerator. He put in and tested wiring and filters, he tested the best operating conditions. When it was ready to be used, Bart van Wees' quantum point contacts were given priority because of the exciting new result of the quantized conduction. After that Bart Geerligs could start measurements on his own samples. He first looked in the normal state and found good agreement with relatively simple theoretical predictions. In these first years, understanding the influence of the electromagnetic environment was probably the key issue. Gradually, clear novel results came out. One of them was the turnstile, the first single electron quantized current source. One electron was shuttled through in each cycle of an applied AC gate voltage.



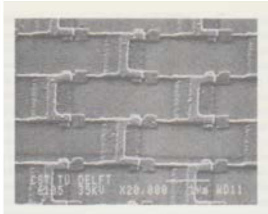
Single electron turnstile, yielding quantized current plateaus.
One electron is passed through for each cycle of an applied AC field.
From a to e the frequency increases from 4 to 20 MHz

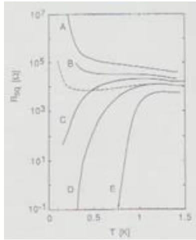
Switching off the magnetic field yielded a superconducting circuit. Our lithography machine could write small nanostructures in a large area. We could fabricate arrays with thousands of small junctions, modeling two-dimensional granular materials with well-defined parameters. Coulomb blockade effects came out very clearly, mainly because the influence of the environment was smaller. The inner regions were shielded by the outer areas.

After his PhD in 1990, Bart moved to the Center for Submicron Technology. Work on superconducting arrays was pushed forward by Herre van der Zant (PhD in 1991). He studied the motion of vortices, induced by a small perpendicular magnetic field. Clearly the friction they experienced at low temperatures was very small. Ballistic-like behavior was observed.

The work on superconducting arrays with high-quality tunnel junction links was continued for many years, extremely interesting results came out.

For us in Delft, they demonstrated the intrinsic quantum nature of the circuitry in general and in particular of vortices in the 2D system. However, there was very little competition from other labs. Others had the equipment to make high quality circuits on a small scale, but very few could make large arrays. Scientific work is only fully appreciated by colleagues who have performed or tried to perform the same measurements. For students it is a





Phase transition in arrays of small Josephson junctions. Superconducting islands are coupled to four neighbors through junctions with area 100x100 nm^2 and different oxide thickness. Above 1 K the resistance of the array is equal to the junction normal-state resistance. Cooling to below the superconducting transition temperature, samples C, D and E with thin oxide (strong Josephson coupling) behave as a superconducting film. In samples A and B with thicker oxide, the Coulomb charging energy dominates, leading to insulating behavior at the lowest temperatures. Note the exponential scale.

great pleasure to interact with other teams that do very similar things. Friendly competition, as was customary among mesoscopic superconductivity groups, was largely absent with the arrays.

Superconducting electronics and high temperature superconductors

The professed research goal of our group named Superconductivity was to develop superconducting electronics. It needed to develop expertise in fabrication of suitable thin films for junctions and other circuit elements. The work on nonequilibrium effects in aluminum samples close to 1 K could hardly be expected to contribute directly to this practical goal. In particular after IBM stopped its large superconducting computer project our aim tended to become more long-term. With our new dual-material deposition system we started to develop alloy materials and metallic superlattices. We obtained a new permanent-staff position and appointed Dirk van der Marel. He came originally from the Sawatsky group in Groningen, specializing in optical spectroscopy of surface states in metals. After his PhD he had moved to Philips where he was in the same group as Henk van Houten. In Delft he took charge of the efforts to create new special materials, modifying the program to include the study of electronic surface states.

In 1986 Bednorz and Müller at the IBM Zurich Lab published data on superconductivity in a new class of materials, ceramic perovskites. The transition took place around 35 K, far above the highest known so far (niobium germanium 23 K). When this result was reproduced by other labs, great excitement ensued. Many institutes started to explore the properties of these materials. Powders were made, mixed and fired in an oxygen atmosphere. Magnetic properties were measured to determine T_c. No one had the slightest idea why these layered materials were such good superconductors (even now, more than 30 years later, there is no generally accepted theory).

Within a year lanthanum-barium-copper-oxide with 35 K was succeeded by yttrium-barium-copper-oxide, T_c at 93 K. Other materials were found. An absolutely crazy reaction followed, not only among scientists. Bednorz and Muller were awarded the Nobel Prize in 1987, a very short time after their discovery. Time magazine ran a cover and inside story that suggested everything electric would soon be superconducting. Scientists lost their moderation. Some labs had hundreds of students sleeping inside, grinding powders to explore all possible combinations of metal oxides without any leading idea why one should be better than the other. Each self-respecting solid-state theorist came up with his specific mechanism for the high T_c. Little respect or even attention was given to the products of colleagues. In 1987 there was a Low Temperature conference in Kyoto. The lecture rooms were overcrowded, for us scientists used to serene gatherings the whole event was an unbelievable circus. At any time at least ten TV crews were in action, not to be interviewed was reason to feel offended.

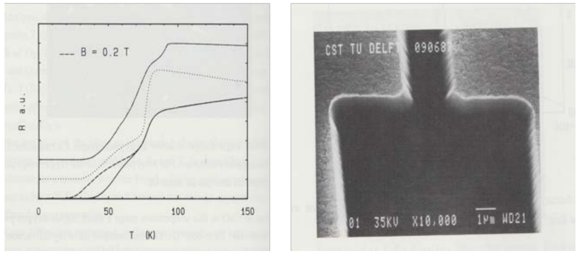
This was extreme, but also on other occasions I have noted how the interactive process between good scientists, who like to be honest but also smell fame and research money, can run out of hand. For me, it was a very

cover Time magazine, May 11 1987

sobering experience to participate in a meeting that the Philips Lab called with a table full of Dutch superconductivity researchers and Philips group leaders. The question on that table was: suppose a room temperature superconductor is discovered next week, how would the world change, how should Philips anticipate? One should realize that full superconductivity is only present below half the critical temperature. To have an application at room temperature one needs a T_c of at least 500 K. “High temperature” superconductivity at 100 K is a long way off that goal. The main question that Philips asked was: are there any applications that are fundamentally new and might now come within range? We could not think of one. Thirty years later high temperature superconductors are used in the electric power grids to reduce losses, and the big magnets of the Large Hadron Collider at CERN are similarly equipped with YBCO at the temperature of liquid nitrogen. Quantum computing might count as a fundamentally new electronic application but coherence would still require low temperatures there.

Circumstances put us in a delicate position. We had our own research plans and we did not feel that joining the stampede of high T_c would be very valuable for us or for the world. On the other hand, there was this extremely interesting development that we wanted to know more about by hands-on experience. As one of the few groups in the Netherlands with super-conductivity central in its focus, it seemed almost impossible not to do anything.

We compromised. We had the whole group drop their regular research for two weeks and together we made a concentrated effort to produce YBCO films (instead of bulk material). In this period the Balzers UTS system was modified under the direction of its main users, Jacques Schellingerhout and Roland van der Leur, to evaporate three metals simultaneously with a local oxygen pressure at the substrate. Reasonable control of the film composition was obtained, on the basis of EPMA analysis. Other group members performed supporting tasks such as measurements. As deposited, the films did not show superconductivity but after post-annealing, signs appeared at



YBCO films from 2-week campaign. Superconductivity sets in around 90 or 80 K but the transition extends to 40 K. The fabricated structure on the right was one of the very first in the world but no measurements were performed on it.

around 70 K. At the end of our limited period, we had a clear onset of a transition at 85 K, but a resistive tail remained down to 40 K. We also performed lithography with quite good definition. Our films were very early relative to other groups, so they were interesting enough for a high-level journal. However, two weeks were not enough for a good analysis. We published in Zeitschrift fur Physik where Bednorz and Müller had published their first data. It took extremely long to appear, but we did not really care.

All countries initiated special support schemes for high temperature superconductivity. In the Netherlands and in Europe funds were made available at very short notice. Our group in Delft received the possibility to buy a new dedicated deposition system for high T_c films. Dirk van der Marel, who had arrived just before our short campaign took charge of these developments. He also installed a surface spectroscopy chamber. In the years

1990-1995 we had three PhD students working on films of $YBa_2Cu_3O_x$, with good results. These were Huub Appelboom (co-evaporation in the Balzers machine, finishing 1992), Neng Chen (laser ablation, 1995) and Zi-Wen Dong (making devices from laser-ablated and MBE deposited films, 1995). After the first few years of unbounded global optimism it was becoming clear that the materials were difficult to understand and to control. We had two PhD students who worked on high-Tc components for existing instruments in joint projects with other groups. Jan-Peter Adriaanse developed superconducting components for electron microscopes and Sven Wallage developed microwave filters.

Kijkduin meeting

In 1992 Dirk van der Marel received the offer of a full professorship in Groningen and decided to accept it. We suddenly had to choose. We could continue the group as it was, maintain superconducting electronics as the core activity, look among known colleagues in the world for a good replacement and use our access to national and European funds for the financial basis. However, the success of our venture into semiconductor physics gave room for thought. Submicron fabrication techniques (not yet called nanofabrication) had given us a splendid opportunity to start some-thing new, where the attitude and the experimental measurement tech-niques were in fact close to the type of research that we had developed for non-equilibrium and submicron superconductivity. We decided to open more options.

A discussion meeting was arranged where we would discuss a number of relevant topics, in particular topics where small dimensions and submicron fabrication would open new prospects. The meeting was held on Friday/ Saturday March 7/8 1993 at the Atlantic Hotel on the seaside in The Hague. On purpose we chose a location away from the university and two days to promote informal interactions on the beach for refreshing intermissions. Unfortunately, I have not kept the program and the list of participants. As I recall it, we were with 8 people mostly from Delft. We had beforehand made up a list of possible research options and asked each participant to reflect on one or two of those in a short presentation. The intention was that this choice would be final and that we would look for suitable candidates accordingly. These candidates would not need previous experience in the particular new field as we were primarily interested in unexplored territory. Funding would not be considered to be a problem, we were confident that FOM and other sponsors would allow our group enough leeway.

We started Friday at lunch time, with presentations and discussions in the afternoon and evening. At the end of the day we had covered all subjects. The next day we started to narrow down, ending after lunch. It worked extremely well. The preparation was thorough and the discussions open and intensive. We concluded that two subjects were worth adding to the group. The first was transport through a single molecule. The idea would be to attach contacts to a conducting molecule. Electronic transport should have the quantum nature of the underlying quantum structure of the molecule. No such measurements had been performed so far. The other subject was

Hotel Atlantic, Kijkduin, The Hague

mesoscopic charge density waves. In certain crystals charge density waves occur, there were ideas that these could behave similarly to the condensate in a superconductor. A weak connection between two charge density wave crystals might perhaps form a Josephson junction-like object. No such small structures had been made.

Back in Delft we started to implement our choice. We advertised the position and the subjects and we sent notifications to many colleagues in and outside the country. We received many applications and interviewed several persons. We chose Cees Dekker who at that time was at Utrecht University working on noise in mesoscopic semiconducting systems. He had previous experience in spin glasses and had spent a sabbatical at IBM on vortex dynamics in high-Tc superconductors. When he came, Cees took on the single molecules as well as the charge density waves.

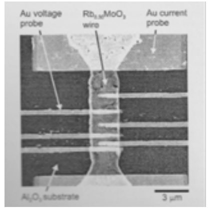
Charge density waves

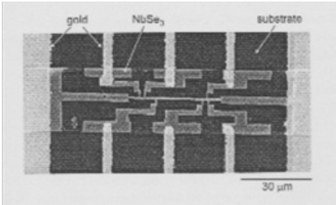
The kind of charge density waves (CDW) that were of interest occurred in strongly anisotropic, practically one-dimensional materials. The Fermi surface consists of two points at plus and minus the Fermi wave vector. By a periodic distortion of the electron density a so-called Peierls gap can be opened at these Fermi points, leading to a lowering of the total electronic energy. The Peierls gap is temperature-dependent, going to zero at the critical temperature. When the Fermi wavelength is not commensurate with the crystal lattice, the electronic system can slide through the crystal, supporting a current. With the current, a periodic variation is associated. That period is coupled to the current. Due to pinning, a critical minimal voltage is required to set the CDW into motion. Overall, there is a strong analogy to Josephson junction physics with the roles of current and voltage interchanged.

The CDW has a fundamental coherence length that is very similar to the superconducting coherence length, proportional to the inverse gap. It is of order 1 nm at very low temperatures but diverging near the critical temperature. The hope was that studying CDWs in small structures should give more insight, and might possibly lead to interesting devices.

The PhD student that was attracted for this project was Onno Mantel, coming from Utrecht. Also joining the charge density waves was Herre van der Zant. He had spent three years at MIT after his PhD in Delft on junction arrays. He was awarded an Academy position for 3 years in a joint project with Peter Kes in Leiden. The subject was mesoscopic charge density wave systems. Herre acted as the de facto PI on the subject.

The first task was to make CDW films, for which laser-ablation was used. We had experience with this technique from our high-Tc superconducting work. Onno made films of so-called blue bronze ($Rb_{0.30}MoO_3$), and made fabricated structures using a 0.35 µm thick $NbSe_3$ crystal on a substrate. By measuring on samples with different length, the additional voltage at the contact ends was clearly found, independent of length. However, for very short wires this voltage was reduced. For the blue bronze films, the depinning voltage was very high relative to crystals, demonstrating that fabrication induced defects. For the crystals of $NbSe_3$ this was not true. Overall, from this first round no strong conclusions could be drawn. The project was continued with a second PhD student, Erwin Slot, who finished in 2005. In the end, it seemed clear that the control of the materials was not sufficient to fabricate mesoscopic structures in a well-defined way. It was therefore decided not to continue.



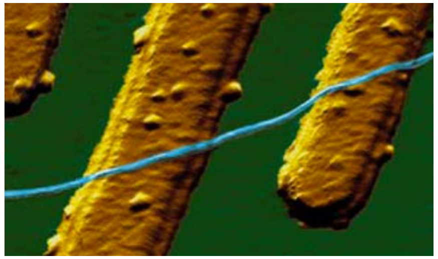


Structured charge density wave samples with multiple probes on top.
Left a film of $Rb_{0.30}MoO_3$, right a super-thin crystal of $NbSe_3$.

Carbon nanotubes

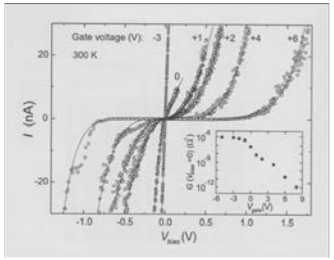
Was it possible to place a chemically defined object between electrodes and measure the electrical conductance? If it could be done, it might be significant for the understanding of the atomic or molecular quantum states, it might be a first step in molecular nanoelectronics and might lead to whole circuits that could be grown by chemical means. Given the limits of lithography the molecules needed to be rather large and stiff, these were hard to find. The discovery of carbon nanotubes brought a wonderful solution. In the wake of the carbon-60 spheres ("bucky balls"), chemists discovered the possibility to grow long objects. In the beginning these objects were not too well defined, but later so-called single-walled nanotubes were made. The result is best visualized by thinking of a monolayer graphene sheet that is rolled up to create a tube. As with rolled sheets, one can obtain different diameters and one can do the rolling in different directions and along two crystallographic directions simultaneously as long as the end result yields a crystallographic fit.

Cees made contact with Smalley from Houston. The fact that our group in Delft had clearly established credentials in making small contacts and in measurements on small quantum systems helped to convince Smalley to start a collaboration. The Texas group provided the tubes and Cees with his team did the transport measurements. Sander Tans as a PhD student was the central actor. Michel Devoret from Saclay who was in Delft for a short sabbatical took a very active part as well. The first paper was published in

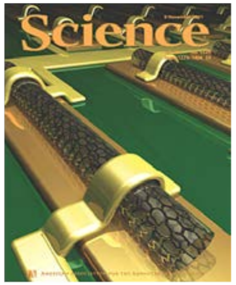


STM picture of a nanotube on top of gold contacts, with PR coloring.

Nature in 1997. Just before, Smalley received the Nobel Prize for chemistry for his work on the buckyballs. In the Nature paper, it was demonstrated that the transport through the tubes was consistent with coherent electronic states extending from contact to contact. This contrasted with the many diffusive or hopping-like observations in previous molecular electronics experiments. The paper and the conference presentations of the work drew extreme attention, well-deservedly. Cees and the team established themselves as players in the first league. More papers followed. One of these described a room temperature transistor consisting of a nanotube on contacts with a capacitive gate attached. By varying the induced charge on the tube, the conduction could be changed. This transistor blew all fuses of



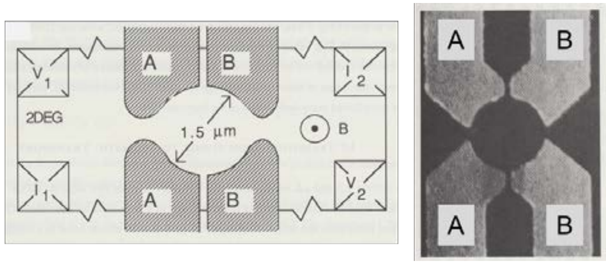
nanotube transistor: current-voltage characteristics for various values of the gate voltage



Science cover 9 Nov 2001 nanotube circuit science reporters and had almost 5000 citations. Implications for information processing with nanotube circuits were taken for granted. However, not every tunable switch is a useful circuit component. From the point of view of an electrical engineer this transistor had many undesired properties as the journalists should have noted. No practical follow-up ensued. In a later paper with Peter Hadley, the team assembled two or three nanotubes to form logic gates. This required clever manipulation and was quite an achievement.

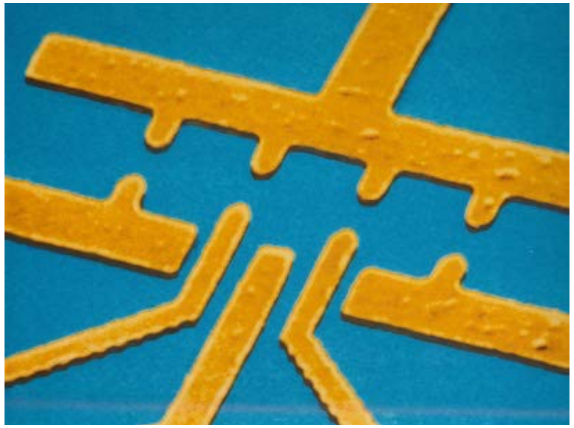
Theoretical predictions on these single-walled carbon nanotubes were that some would be semiconducting and some would have metallic conduction, depending on the winding numbers. In the production of nanotubes always a variety of different nanotubes was produced without identifying labels. Our PhD student Liesbeth Venema used low-temperature scanning tunneling microscopy to determine the winding speed of specific nanotubes. She worked with Jeroen Wildoer, in interaction with the group of Herman van Kempen in Nijmegen. The correct correlation was found with semiconducting or metallic behavior. She also observed standing wave resonances of the wave function in tubes that she had cut short with the STM. It all supported the notion of well-defined, structure-determined, clean, extended electron states in the macromolecule.

semiconductor quantum dots

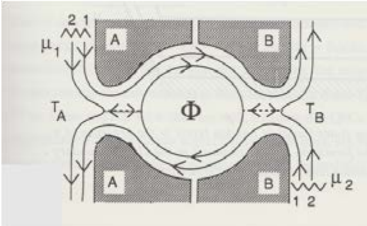


Quantum dot. Two quantum point-contacts A and B are placed in series. Transport through the two-dimensional electron gas (2DEG) from left to right is measured, a perpendicular magnetic field is applied. The slit between the two QPCs is narrow and fully depleted. The enclosed region is the quantum dot.

A quantum dot is formed with two quantum point contacts in series. In later designs an additional electrode was placed between the QPCs to change the electrical potential and with that the electron number. The quantum dot became an important tool. In its simplest form it is very similar to the metallic single electron transistor. In zero magnetic field the dot contains a certain number of electrons, determined by the chemical potential that is controlled by the gate. The QPCs can be tuned close to cut-off, where they behave as tunnel junctions. An interesting feature is the possibility to change the tunnel barrier on a fast time scale. A turnstile for single electrons was created in this way, with the dot biased at a small voltage and the gate tuned so that a single electron level was between the chemical potentials on both sides. Lowering the left barrier lets in one electron from the left, increasing that barrier followed by lowering the barrier on the right lets that electron exit on the right. The cycle can be repeated, in practice at a rate of 10 MHz. This corresponds to a quantized current of 2 pA. For a practical current-standard the current was too small and the accuracy too low (about 1%, limited by leakage).



Three quantum dots (four QPCs) in series, with a gate electrode attached to the middle dot. This picture was widely used for PR purposes

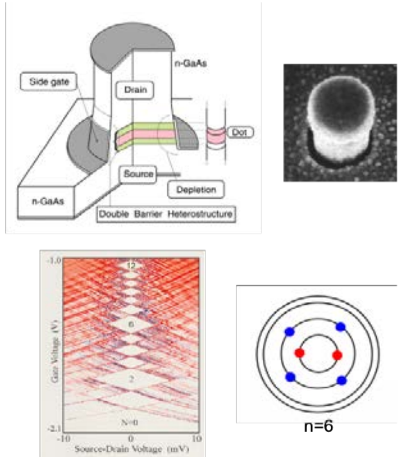


Quantum dot in strong magnetic field.
At the edges of the 2DEG edge channels are formed. Here the second level is cut off by the point contacts. The inside of the dot is connected to the outside by tunneling. Coulomb blockade as well as zero-dimensional quantization play a role

More interesting features occurred when a magnetic field was applied. The fermionic character of the electrons came into play. Zero-dimensional states within the dots could be analyzed through tunneling through the dot from source-2DEG to drain-2DEG. With microwave spectroscopy the levels could be determined. The team in this period consisted of Kees Harmans, Leo Kouwenhoven, postdoc Charlie Johnson, a younger PhD student Nijs van der Vaart and a number of enthusiastic masters' students.

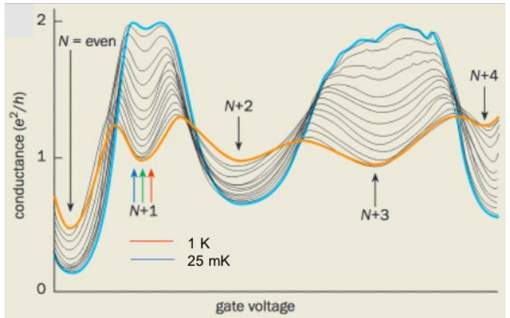
The most beautiful results on the zero-dimensional states were obtained on a sample that was fabricated in Japan by the Tarucha group at NTT and University of Tokyo. These were 'vertical' quantum dots as shown in the figure. The dot area was circular. In practice this was a two-dimensional atom with the electron confinement not provided by an attractive nucleus but a repelling ring gate around it. Electrons could be loaded one by one. The two-dimensional atom was a student's theory exercise in the early days of quantum mechanics, but now it was realized. Shell structure emerged with fewer quantum numbers. Consecutive shells had 2, 4, 6 electrons. Atoms with 2, 6, 12 electrons were more stable and could be called 2D noble gases. This was all very elegant in its simplicity, once the sample came available.

Another set of experiments that drew strong attention from the outside world was the study of the Kondo effect in quantum dots. Sarah Cronenwett (a student visiting from Stanford) and PhD students Tjerk Oosterkamp and Wilfred van der Wiel were, with Leo Kouwenhoven, the main actors. The



vertical quantum dot, artificial two-dimensional atom
from top left to bottom right: design and realization of circular quantum dot, 0.5 μm across; source-drain current (indicated with color) as a function of source-drain bias voltage and gate voltage. Near zero bias voltage, diamond-shaped blockaded regions are seen. With higher gate voltage (less negative) the number of electrons n on the dot increases one by one, starting from zero. One can see that the diamonds with n=2, n=6 and n=12 are relatively wide. These are filled two-dimensional shells; on the right a schematic representation of two filled shells with n=6

Kondo effect was known for many decades in bulk samples that contained magnetic impurities. The electrical resistance of Kondo metals drops with decreasing temperature until a certain temperature and then increases. Kondo gave the explanation in 1964, spin exchange processes turn out to be responsible. A so-called Kondo resonance can occur where a trapped spin couples with the spins of free electrons, creating new states at the Fermi level. Not so easy to understand, but theory predicts that the resistance scales with temperature divided by a reference value (Kondo temperature) specific for that sample. This was confirmed by experiment on bulk samples. In quantum dots the electron occupation can be manipulated. To see the

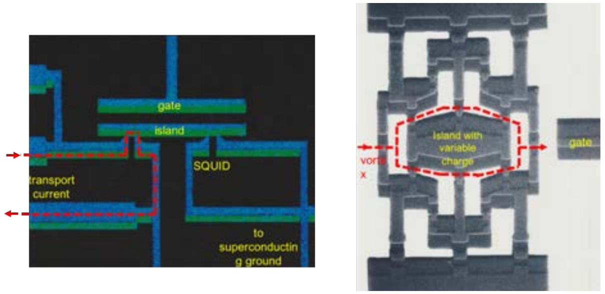


Kondo effect in quantum dot.
The conductance is plotted versus gate voltage, showing the Coulomb peaks. The number of electrons at a specific low gate voltage N is found to be even from analysis. Clearly there is a systematic difference in the temperature dependence between even and odd regions.

Kondo effect, one needs a net magnetic spin which means that it is only seen for odd numbers of electrons in the dot. The figure shows the result of a measurement of the conductance at different temperatures. Half a year earlier a similar quantum dot was studied at MIT. The Delft results expanded and supported these earlier data, with a consistent picture when analyzed on the basis of theory.

quantum effects in superconducting junction circuits

The techniques to produce metallic tunnel junction circuits where the Coulomb charging energy exceeded the temperature was now applied to superconducting circuits. The switch was easy, it needed only to turn off the strong magnetic field. Now there were two competing energies, both higher than the temperature. If the Josephson coupling energy of the junctions exceeded the charging energy, the circuit behaved primarily as a superconductor. If the charging energy was higher, the primary response was as an insulator. The numbers were such that by changing the oxide thickness of the junctions both regimes were accessible. Moreover, replacing one junction with two parallel junctions gave the possibility to tune the effective Josephson coupling by varying the enclosed magnetic flux. With

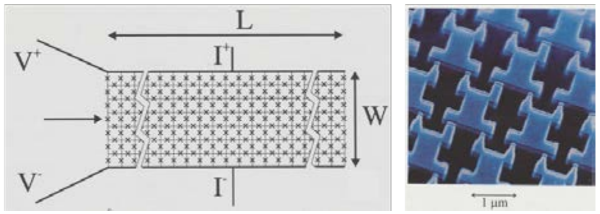


Superconducting circuits demonstrating emergent quantum properties. Left: the Heisenberg sample. Suppression of phase fluctuations leads to increased charge transport. Right: interference of quantum vortices moving around a charge (Aharonov Casher effect).

this toolbox we went to work. In parallel, Harvard, Saclay and Gothenburg moved in the same directions with open and close interaction. To develop our understanding of quantum effects in superconducting circuits, many discussions with Kostya Likharev were of great value. When he moved from Moscow to Stony Brook in the US, on his way he spent a few months in Delft. In these circuits one can distinguish islands, small metallic regions connected by tunnel junctions on which the charge is well-defined. There are also loops where the phase of the superconducting order parameter can be followed around and needs to be the same or an integer number times 2π returning to the same island. Phase and charge behave as conjugate quantum variables in the same way as position and momentum in mechanical systems. If the charging energy dominates, the charge is well-defined but the phase is uncertain. Conversely, for high Josephson coupling energy the phase is well-defined but the charge fluctuating. We ran directly into this Heisenberg uncertainty when Wiveka Elion and Marco Matters studied charge transport in the Coulomb blockade regime through an island that was connected to a large superconducting body, by means of a tunable Josephson junction. When its Josephson energy was high, phase fluctuations were suppressed. As a consequence, charge fluctuations increased which made transport possible even in the Coulomb blockade regime. Without the notion of the Heisenberg uncertainty this would be difficult to understand.

Our EBPG-based nanofabrication allowed us to fabricate large two-dimensional arrays of small Josephson junctions, with small variation of parameters. We could study such arrays with junctions near the quantum transition from the phase regime (Josephson energy larger) to the charge regime (Coulomb energy dominates). First Bart Geerligs, later Herre van der Zant fabricated two-dimensional arrays which were very homogeneous. Vortices are dual particles to charges, they are driven by a current, a flow of vortices induces a voltage difference. In an array in the charge regime, the mobile single charges see a strong variation in the background potential due to charged defects. In contrast, nature has no magnetic monopoles and the background potential for vortices is very smooth. Quantum vortices move very freely if the variation in junction properties is small. Herre clearly demonstrated the quantum mobility of vortices in 2D arrays.

PhD student Alexander van Oudenaarden fabricated long, narrow arrays where vortices could move only in the length direction. Their potential energy in the array could be modulated by changing the Josephson energy (junction area) for a row of junctions across the array. Local barriers at fixed mutual distances of 10 cells in the length direction did not block vortex motion, but putting in a random variation of the distances did lead to Anderson localization. The vortex density could be varied with the applied



Long 1D arrays for the study of one-dimensional motion of quantum vortices. Arrays were up to 1000 cells long and typically 9 cells wide. Along the length, on top and bottom, were busbars for application of a homogeneous cross current to drive vortices. Vortices entered and exited at the left and right ends. The voltage difference between the busbars is the rate of passing vortices. Vortex density is determined by an applied perpendicular magnetic field. Vortices are repelled by the busbars and consequently move along the middle of the array, experiencing a periodic potential because of cell structure. A row of stronger junctions across the array presents a high potential.

field. If that density was commensurate with the barrier lattice, Mott localization was the consequence. In addition, Bloch oscillations of the vortices could be seen but not studied in detail. A later student, Hannes Majer, also managed to create a ratchet potential for the vortices and saw clear asymmetry in transport.

Together with Michel Devoret, Alexander designed an experiment where electrons moved in a closed loop containing tunnel junctions. A transport current was applied to two contacts opposite each other on the loop. The electrostatic potential of one half of the loop was raised by means of an electric gate. This induced a phase difference and induced an interference between the two branches. This electrostatic Aharonov-Bohm effect had been predicted but not clearly observed until that time. After publication it even made it to the TV show The Big Bang Theory, by mouth of non-physicist character Penny.

By 1996, we had seen many quantum effects in superconducting circuits, in accordance with calculations where phase and charge were treated as conjugate quantum variables. In other groups similar data came available. It was possible to fabricate on-chip superconducting circuits in the quantum regime, and it was possible to perform weak measurements that did not kill the effects outright. Most experimental work elsewhere focused on the charge regime, creating a superposition of two charge states on a single island. Clear progress was made, but charge noise was very strong due to charged defects in the junction barriers or on the surfaces.

quantum information

At the same time, the concepts of quantum information processing with quantum bits started to be a real area of research. People started to develop quantum algorithms, assuming the availability of interacting quantum bits. Apart from quantum games, Grover's search algorithm and Shor's factorization protocol showed that practical advantages could be gained. Quantum gates such as the controlled-not gate started their life on paper, but they were also realized in hardware. Experimental lines for quantum computation were started with nonlinear optics using single photons, and with trapped single atoms or ions driven by photons. It worked, but it was not clear whether the concept could be scaled up to large numbers. At IBM San Jose lab, Ike Chuang with his student Lieven Vandersypen performed the factorization of the number 15 with Shor's algorithm, applying nuclear magnetic resonance on the spins of atoms in a molecule. Sufficient signal was obtained by using a large number of identical molecules, in fact executing the algorithm simultaneously in many parallel quantum computers.

Beginning 1996 there was no expectation in the quantum information community that fabricated solid state objects could be made into a quantum bit. The large number of degrees of electronic quantum states, not to speak of all other excitations, seemed to make this impossible. However, the results achieved with our mesoscopic superconducting quantum systems made us think. I went to the ITP in Santa Barbara for the extended workshop on Quantum computing and quantum coherence organized by Wojciech Zurek and David Divincenzo in summer and fall of 1996. Before going there, I spent two months in Japan at the NTT lab to study on quantum information, and went to Curacao for a NATO conference where I talked about superconducting qubits with a big question mark. The Santa Barbara workshop was very exciting. I learned what a quantum computer was and had very long discussions with Seth Lloyd and Ike Chuang, translating the quantum information concepts to the simplest superconducting qubit: two charge states on a Cooper pair transistor. I was the only one from condensed matter among the participants. I gave a talk explaining what had been achieved and could possibly be done. Murray Gell-Mann was one of the participants and posed the first question: "Why in the world would you try to do this?" When I returned to Delft at the end of August the plan was to work out more details and see whether we could write a paper. I saw that a qubit could work but not how it could be fitted into an architecture. When I was back in Delft Ike Chuang wrote a concept paper that, looking back, might have been good enough to start a serious discussion. However, I wanted to take it a step further first and nothing published came out in the end. I also did not give a talk at the concluding conference in December.

Personally, I had a complicated period around that time. A kidney was removed in 1995, and during recovery I realized I was not so happy with the combination of fast-growing organizational responsibilities and my wish to do original physics. I applied for a position as institute director outside Delft and was selected for the job. In the end I did not take it and focused on the design of our qubit. Coming back from Santa Barbara, I had to step in unexpectedly as Department chairman. In that year the university was restructured and I became dean. Also, I was confronted with a delegation of

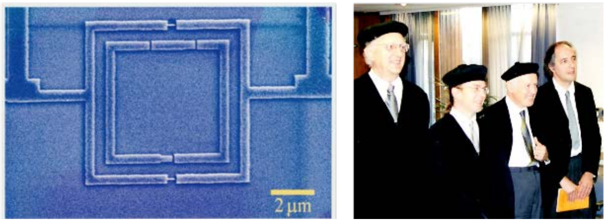
my colleague-professors who claimed that Technical Physics was not the place for "solid state physics that could be done in Leiden". They had held a meeting behind my back and suspected that I was abusing my position as dean to advance our wrong type of research. This was all very unpleasant. Nevertheless, it ended well. Nico de Voogd, our university president, heard that I had considered to leave. He offered me the possibility to spend 3 months per year at MIT, to focus on my own research. That worked very well for three years. I used only 2 months not to be away so long.

The groups that worked on mesoscopic superconducting quantum circuits zoomed in on possible qubits. Saclay convincingly demonstrated a quantum superposition of charge states in a Cooper pair box. Dynamic experiments were started as well. At the end of 1998 we were all completely surprised by a paper from Yasu Nakamura and Shen Tsai, submitted to Nature. Fantastic, they had coherent quantum dynamics and had made the first superconducting qubits. Of course, that started the field that is now so blooming.

My own attention was more aimed at flux states than on charge states, because we had seen in our array experiments how homogeneous the background for vortices was. During an extended visit to MIT in fall 1998, Terry Orlando, Seth Lloyd and I worked out the design of the flux qubit. In Delft, PhD student Caspar van der Wal started to fabricate the sample. It consisted of a loop with three junctions, with parameters such that a vortex could tunnel in and out of the loop. Biasing the loop at half a flux quantum, the state with no vortex and the state with one vortex had equal energy, but the persistent currents in the loop had opposite sign. A quantum superposition of those two states was observed.

Of course, Caspar's object was not really a qubit yet. It did not show coherent transitions and in fact it took several years to develop an improved model that was sufficiently isolated from the environment. Nevertheless, it was a big step and the outside world perceived it as such. This experiment and a related experiment at Stony Brook showed that a macroscopic object could be placed in a quantum superposition. In our case the persistent current had a value of around 0.5 μA, the loop was many μm long and a billion electrons had to reverse direction to cross over to the opposite state. The outside world immediately invoked Schrödinger's cat. However, that was not correct. Only two quantum states participated, each of them macroscopic.

I had been told repeatedly by very high-ranking theorists that of course quantum mechanics did not apply on our loop's scale. The great exception had been Tony Leggett, who always believed in macroscopic quantum effects. It was a great pleasure to have him in Delft for Caspar's PhD exam.



Left: Flux qubit made by Caspar van der Wal 1998. The inner square loop contains three Josephson juntions. Biased at half a flux quantum, a superposition state is created of two persistent current of opposite sign.
Right: PhD committee members for exam of Caspar van der Wal: John Clarke, Seth Lloyd, Tony Leggett and Daniel Esteve.

Relations outside the group, in Delft and beyond

As discussed in the early section on infrastructure, in Delft we had the Center for Submicron Technology (CST), part of the Delft Institute for Microelectronics and Submicron Technology. The investment as well as the major running costs were covered by a national grant for the stimulation of electronics. This allowed us to do the fabrication without continuously running bankrupt. Most of the equipment there was shared between the users, but specific tools for deposition of our films remained in our control. One cannot easily evaporate one material today and another tomorrow without contamination and without loss of reproducibility. This arrangement worked well, the staff was professional and maintained the high quality.

CST and its director Sieb Radelaar had more ambition than merely to run the facility. As a FOM institute they had one position for a more senior person who should lead a fundamental physics project or program. Radelaar was a good materials scientist but had no ideas in mesoscopic or quantum physics. He did not want to operate in our shadow. We did not want a parallel set-up in our field with independent planning. I hoped something or

someone would turn up where we all felt comfortable. The first appointment was Gerd Schoen (at my suggestion). As a theorist he kept working with us, but also initiated projects together with CST staff. However, he was appointed as a full professor in the Department after a short time and the FOM position came available again. The next occupant was Heinrich Jaeger, who did nice work on materials-related transport properties of superconducting films and other similar subjects. We had a good relationship with him. In 1999, Sieb Radelaar left CST to become director of a new national "top" institute on metal science, with close interaction between industry and universities.

A very ambitious new project was set up in our building starting 1997, with the goal of nanofabrication on almost atomic level, combined with analysis, device fabrication and measurement. The project was called NEXT. It was mainly run by the CST, but several of the groups in the Technical Physics Department participated. The idea was to have a set of units with different functionality, all at ultrahigh vacuum (UHV), all interconnected. A film could be fabricated in one chamber, be analyzed in various ways in other chambers, be patterned with scanning tunneling microscopic tools, contacted and subjected to various experiments, all without ever leaving the UHV environment. Cees Dekker was the main participant for QT, hoping to perform measurements on single molecules other than the stiff and long nanotubes. Good ideas were generated from other groups as well, and implementation started. It ran over several years. This project made it possible to set up conceptually new experiments from scratch. However, these

NEXT Lab

projects were difficult enough in each chamber and in practice proceeded without requiring the transfer to other team's chambers. NEXT remained a set of separate experiments. When Cees left QT, no new projects were started from our side.

Theory

For our research, it was important to interact with theoreticians. In our new unexplored field, they could not give immediate clear-cut answers. As their approach was different, it was very fruitful to discuss our physics with them. We had a useful interaction with the small theory group in Delft, but in these earlier years these people had other interests for themselves. We mostly interacted with friendly theorists at other universities. The semiconductor team was in very close contact with Carlo Beenakker. Carlo was a member of the Philips team on the ballistic devices, and later in Leiden was continuously active in our mesoscopic and quantum field.

With the superconducting systems the interaction was mainly with Gerd Schoen. He was in Julich from 1984 to 1986, then came to Delft on the scientist position of the FOM institute CST. In 1987 he became a full professor. Unfortunately for us, in 1991 he got an offer from his home town Karslruhe and he left. In spite of the distance, we kept close contact. In Delft, we also received visitors for longer or shorter periods. Leonid Glazman was a very welcome guest several times. While I worked at MIT on the design of our flux qubit, Leonid Levitov was closely involved.

New professors

The traditional Dutch university system knew professors and other members of the scientific staff, together responsible for teaching and research. Non-professors were allowed to call themselves assistant or associate professors in English, but not in Dutch. They were not addressed as professor and did not have full responsibility as PhD advisor. In QT it meant that for each PhD graduation, also those under the full responsibility of a staff member, my name had to be attached. In 1999, Delft University found a way to circumvent this by giving seventeen 'excellent' staff members the title 'Antoni van Leeuwenhoek professor'. Two of those were in QT: Cees Dekker and Leo Kouwenhoven.

Cees Dekker simultaneously decided to change his field and to switch to nanobiology, making the Van Leeuwenhoek title even more appropriate. He started a new group outside QT, called Molecular Biophysics. Later this

provided the basis for a new Department of Bionanoscience. The university welcomed these developments and generously made room for them.

Evaluation, QT 1985-2000

In the introduction for this second part of the story of QT it was indicated that many changes were to occur between approximately 1985 and 2000. QT at the edge of the 21st century was larger, more diverse in its science, more international. It was recognized in the outside world as a strong center for mesoscopic physics studying electronic properties of superconducting, semiconducting and molecular nanostructures. This was the period where QT took flight. How did that happen? In my recollection it mainly went by itself, but that is oversimplified. We believed in our common direction, looking for new physics in fabricated nanostructures. We agreed that we needed to invest a significant part of our thoughts, time and manpower in developing our fabrication methods and our measurement tools. That investment needed to be made at all levels, from all the staff members to all the PhD students. It would not have worked if we had established a separate sub-section of fabricators and another of measurers. The design of the sample and of the experiment was an evolving balance of what one wanted ideally, what could be made in practice and how one could extract information without spoiling the game. This was where the roots of the group were found and where we competed with our friends/colleagues in the world. There was little friction about resources. The group kept moving and could adapt.

I think nobody will deny that QT in this period was very successful. By 2000 there were three main research lines, all dealing with new mesoscopic and quantum effects in fabricated structures. One, based on circuits of small Josephson junctions, was in many ways a logical continuation of the earlier research program of the group. The work on ballistic semiconductors together with the Philips group had been prepared and initiated before 1985 but came to strength in this second phase. The third line, on carbon nanotubes, came falling out of the air after 1992. All three had a very high international standing in 2000.

This is the positive side, but not everything came out so splendidly. It is only fair to list the projects that were started but after some time abandoned because they did not reach a sufficiently promising outcome. Years of dedicated work by young people and a considerable sum of research money were involved. Not wasted, because we learned much. The abandoned projects for us were:

- Controlled fabrication of alloy superconductor films, tunnel barriers and superlattices gave very limited results and was too demanding on the students. We used the deposition but not the fancy parallel control.
- High Tc superconductivity. We participated in the national program but stopped when Dirk van der Marel moved to Groningen.
- Charge density waves. When Cees Dekker came, he started work on charge density waves as well as on single molecules. The charge density wave materials could not be produced in a form that allowed nanofabrication of device structures. We stopped.
- The NEXT project. The idea of an interconnected ultra-high vacuum multi-station system where samples were deposited, patterned, analyzed, contacted, and measured without ever leaving the vacuum was not based on an urgent need of one or more users. The separate stations remained, but the grand scheme failed.
- Carbon nanotubes. When Cees Dekker switched to molecular nanobiology we did not seriously continue the nanotube work in QT. In some experiments nanotubes were still used.

Looking back, I think our decisions to stop research lines were good. In experimental physics it is a serious danger to continue too long on a certain track where the balance between needed effort and potential reward tilts in the wrong direction. As advisor to PhD students, I often felt that it was my most relevant contribution to say: stop trying this.

In terms of internal research management, the Kijkduin meeting was our star performance. We had an open staff position after Dirk van der Marel's departure which could easily have been used to strengthen our existing work on mesoscopic superconducting or semiconducting systems. We decided to opt for something new, knowing that the start-up costs would have to be borne by the group. We had an honest and open meeting that came up with two possible subjects where our technology could bring something essentially new. We advertised and found Cees Dekker as the person to drive the effort. Ten years later both subjects had been addressed and both had been abandoned. One (the charge-density waves) presented too many materials problems to proceed well. The other (single molecules in the form of carbon nanotubes) was spectacularly successful but was stopped at its high tide when Cees opted for nanobiology. That "Kijkduin" worked out so well involved a certain amount of luck. However, I will claim that the careful preparation by organizers and all participants was certainly also responsible. Later the single molecule work was successfully continued by Herre van der Zant and others in Delft outside QT.

Around 2000, after Cees' departure, we had four staff members in the group. Leo Kouwenhoven had developed the quantum dot systems and was ready to start manipulating the states and to look at coupled dots. With the superconductor team I myself was now fully focused on creating quantum bits and circuits for quantum information processing. Kees Harmans had worked on hybrid superconductor-semiconductor systems and was an important factor in all advanced measurement set-ups in the group. Around 2000 he joined the superconductor team. Peter Hadley developed nano-electronic devices and circuits but now took on more and more teaching and scaled down his projects.

Why did we not have a new Kijkduin meeting when Cees left? Why did we not continue the nanotube work in QT? Why did we not choose a new subject where our technology gave us a strong advantage? There were many reasons, I think. In the first place we did not have an open position that we could fill in. The Department had already gone out of its way to set up Cees Dekker's new group. The outside world had changed too. Our excellent fabrication techniques were no longer unique, many other physics labs had recognized the potential of the nanofabricated structures and devices. But apart from these negative considerations, the main reason was that we smelled the new world of quantum information processing and the role that nanofabricated solid state devices could play. Exciting new physics and conceptually new applications were waiting just beyond the horizon.

14 November 1998, reunion, retirement Chris Gorter, 10 papers in PRL, Nature and Science within 12 months

Part 3. Focusing, 2000-2013

Introduction, look ahead

In 2013 the group Quantum Transport ceased to exist when the new institute QuTech was established. QuTech, much larger, is a joint operation between two Faculties of Delft University and TNO, the Government Organization for Applied Research. QuTech is fully focused on quantum information processing. By increasing the scale, attractive opportunities were created to participate in national and European programs. Also, intensive collaboration projects could start with industry giants Microsoft and Intel. QT as a home base was dissolved in the wide range of people, projects and technology that constitute and surround QuTech. The role of the research group QT as an explorer of new science through nanofabricated structures and nanoscale techniques was taken over by other groups in two new Departments of Quantum Nanoscience and Bio Nanoscience.

In this Part 3 it will be described how the group QT came to be fully concentrated on quantum information. By 2013, QT had developed quantum bits with four different technologies: superconducting junction circuits, III-V semiconductor quantum dots, silicon quantum dots and spins in diamond. In the world, the field of quantum information processing with fabricated solid-state quantum objects had taken off. Delft found itself in a prominent international position.

The context in which QT operated changed markedly in this period. The Department of Technical Physics was split and a new Department of Nanoscience was created, encompassing all physics groups that were involved with matter at the nanoscale. A new nanofabrication facility was built, funded by a large national nanotechnology program. People came and people left; there was significant growth overall. This written story describes these developments. A discussion of the physics research will be limited to the programs in the Quantum Transport group and it will be short. The last five years of QT are so clearly connected to the present work in QuTech that it is too early to write the history.

In the year 2000 Cees Dekker left the group QT to start a new nano-biophysics group. Given his previous field of experience this was a brave step. It worked out brilliantly. QT could have chosen to continue activities on molecules, nanotubes and charge density waves but did not. The two remaining principal research lines in QT were superconducting quantum circuits (Mooij, Harmans) and semiconducting quantum dots (Kouwenhoven). Both had a good number of excellent young people and sufficient funding to be competitive on the world scale.

Peter Hadley became involved more and more with molecular electronics. The main researcher in that subject was Herre van der Zant who after his PhD in QT had gone to MIT to work in the group of Terry Orlando. He came back in 1994, was awarded an Academy scholarship based on the subject of charge density waves. We could not offer him the prospect of a permanent position, he therefore associated with Peter Kes in Leiden. In 2004 Herre and Peter formed a Molecular Electronics group with Alberto Morpurgo who originally came from the group Nanophysics.

In 2006, Peter Hadley accepted a position as professor at the university of Graz in Austria and he left QT. He was the staff member who more than the others looked for direct electronic applications of our nanodevices. Within QT, all eyes were now directed at quantum information processing with the implication of a very long time constant.

Fabrication, NanoNed

The fabrication of samples with submicron patterning was always a crucial aspect of our research and of the training of our students. For cutting-edge experiments, hands-on experience is needed to know what can be done and cannot be done and what could be the influence of the various processes on the quality of the sample. This engineering quality complements the analytical thinking required for our type of fundamental physics. The students graduating from QT have always been able to find good jobs in industry as well as in university laboratories.

Setting up a high-quality clean room is expensive and requires expertise. The investments are beyond the means of a group, a department or even the university. It is necessary to be aware of special programs at the national or European level in the earliest stages of their inception. QT started with an improvised group-built simple facility, opportunistically profited from the launch of a submicron fabrication center, hitch-hiked on a national program for the stimulation of microelectronics and by 2000 needed a new basis for the fabrication. The collaboration with the Electrical Engineering people drifted apart as they shifted their main interest from self-fabricated sensors to conceptual circuit design. As before, a solution came externally from political moves at the government level. Of course, these were influenced by signals from the universities but even more by international trends that policy makers in the ministries pick up.

In The Netherlands it had been decided that a small fraction of the income from natural gas exploitation should be spent on innovative research. Around 2000, the so-called BSIK program was created with a budget of 800 M€. The Academy of Sciences was invited to coordinate applications from fundamental sciences. One of the subjects was nanotechnology, a buzzword of the time. The BSIK program explicitly aimed at large projects in concentrated places. David Reinhoudt (nanochemistry, Twente), George Robillard (nanobiology, Groningen) and I (nanophysics, Delft) were asked to formulate proposals for mainly our three universities. George and I came up with a request for about 20 M€ each, which would nicely allow us to set up and maintain the facilities we needed for the scale of our teams. David, in contrast, wanted to ask for a total amount of 100 M€ and claimed 40 M€ for Twente. He encouraged us to ask for more as well. This, in retrospect, was an interesting dilemma. Money seemed to be available, but should one ask for more than “needed”? I strongly felt it was better to think of the type of facility we ideally would like to have, and to ask for the corresponding amount. A capital investment always needs follow-up in maintenance costs, to be found in the local budget. Also, the receiving institute has to provide matching funds at the start. In the end we received just what we needed and were happy with it. The new clean room was shared with TNO, and located on “our” side of the street. It is still running fine. NanoNed was followed up by NanoNext and the operating costs were covered until QuTech came into being. NanoNed started in 2004, running until 2010.

Another interesting consequence of the major share for Twente was that they were the leading partner and spokesman to the government. They exploited that in the press. Our university president Nico de Voogd was extremely unhappy as in his eyes Delft was bigger and better in the field. He wanted us to step out of the consortium and start our own program. We refused that and De Voogd gave in. I personally was very happy about the administrative leadership of Twente. In the first years David Reinhoudt had practically no time left for his own science. The scientific results that came out of Groningen and Delft were sufficient to create excellent visibility. President De Voogd gave his full support once we were under way.

Department of Nanoscience

We had a complicated relation with our Department of Technical Physics. That department was too large and too diverse for the faculty to know what developments in other groups implied. In our field we had built an excellent international reputation which in diverse ways became known to the condensed matter colleagues in The Netherlands. As a result, we scored high in national evaluations. That mechanism did not work with local colleagues in pattern recognition or fluid mechanics. The long-term department chairman Jaap Kokkedee definitely helped us, but in general the other groups (in particular the professors) were of the opinion that our field was not “technical” enough and therefore could only be marginal in a Department of Technical Physics. In meetings I tried in vain to point out that our advanced nanofabrication techniques provided excellent technical training for our students. I also indicated the prominent place that our type of research had in internationally established technical universities. Our funding position was very healthy due to external projects, but we needed lab space and we wanted to initiate tenure-track appointments for young faculty.

Technical Physics had become part of the large Faculty of Applied Sciences that included Chemical Technology and Materials Science. The new dean Karel Luyben favored smaller departments. This made it attractive to establish a new Department of Nanoscience in 2002. I became the first chairman. The department existed of five “sections”: our Quantum Transport group, the new Molecular Biophysics group of Cees Dekker, a group Nanophysics headed by Teun Klapwijk, who had come back to Delft from Groningen as the successor to Radelaar, the Theory group with Bauer and Nazarov and a section Electronic Materials. We also had the responsibility for the physics part of the DIMES fabrication facility. We were fortunate to find an enthusiastic young department secretary, Bart van Leijen. He took care of all formal arrangements. Elly Pauw was our Human Resources officer, who had new ideas and was eager to facilitate us in procedures for selection and appointment of new young faculty. I think the new department was a success. The other departments in the Faculty of Applied Sciences gradually adapted similar procedures as we had started.

A special case was the Nanophysics group, established by Sieb Radelaar when he took charge of the nanofabrication facility in 1988. He attracted a considerable staff, eight faculty members, two of them directly running the nanofabrication with the help of technicians. The others were mostly into materials-related research with surfaces and very thin films. When Radelaar

left in 1999, a search was started for a successor. Although QT was a prominent user of the facility, our Department of Technical Physics of that time was critical of our “fundamental” physics interests and gave us little influence. After several attempts to attract a purely technical nanotechnologist, the search came to Teun Klapwijk who accepted the position. Teun reduced the staff of permanent researchers considerably and redirected the program. In the longer run, one very interesting new line was the fabrication and optimization of detectors of submillimeter radiation for astronomy. Their “hot electron bolometers” are not based on Josephson junctions but on voltage generation in superconducting films with very high normal state resistance. They are now used in many telescopes.

Kavli Institute

In 2003, out of the blue, I received a mail from a certain David Auston of a certain Kavli Foundation. He said that they were considering Delft as a possible site where they would “assist the university” in establishing a research institute on nanoscience. They had selected nanoscience, neuroscience and astrophysics as focus areas. Both Auston and Kavli were completely unknown to me, but we did enquire and entered into discussions. It turned out that Fred Kavli, a businessman of Norwegian origin living in California, was setting up a foundation to which he was planning to donate half a billion dollars. After prolonged interactions ending with a visit of Kavli to Delft, it

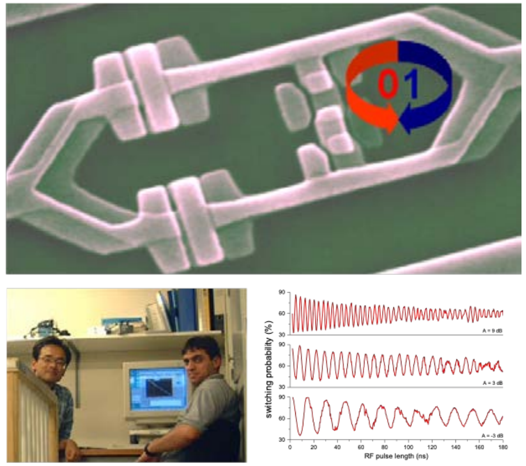
Left: Fred Kavli 1927-2013. Right: signing of the pledge agreement, New York 2004
David Auston is on the lower right, Delft president Hans van Luijk next to him
while Hans Mooij as the director of the Delft Kavli Institute looks on.

was agreed that Delft would have a Kavli Institute. We received 7.5 million dollars.

For us, this kind of support was new. We were to conserve the capital “in perpetuity” and could only use the net generated income. Major American universities have an endowment fund with a full-time staff that acts as a sharp investment company and generates 10 to 15% on donated capital. Private universities such as Harvard and Stanford have funds of order 30 billion dollars and half their income is from the yearly profits. In Holland we only have rules that forbid universities to make risky investments. We could at that time spend about 250 thousand euro per year, a small fraction of the total budget of all the nanoscience groups. Still, the institute director could use the money at very short notice without any procedure if a relevant need or opportunity arose. In later years significantly more capital was received. Being a Kavli Institute along with nanoscience institutes at CalTech, Cornell, Berkeley and Oxford renders prestige but is not a major factor. A good thing is that it unites all nanoscience in Delft.

Superconducting quantum circuits

In 2003 our first superconducting flux qubit demonstrated coherence. As discussed in part 2, in 2000 we had developed a superconducting object with two quantum states that could be brought into a quantum superposition. It was almost a quantum bit, the ability to perform coherent operations was still missing. That object was a superconducting ring biased with half a flux quantum. There were two macroscopic persistent current states that had opposite currents of around 0.5 μA. It was up to us to prove that this huge object could be made to behave as an atom or an electron spin. It took us several years. It was needed to analyze all the ways in which the “environment” could influence the qubit, in fact creating a weak coupling with a large number of uncontrolled variables. Variations of the flux in the loop were important, due to current noise in the driving and measuring circuits. Also, charge fluctuations on islands between Josephson junctions due to charged surface states that could absorb or emit an electron were relevant. Success came when Yasunobu Nakamura spent a sabbatical period with us and collaborated with our postdoc Irinel Chiorescu. A new design with symmetric bias leads for the measuring SQUID made the difference. In summer 2003 the first coherent oscillations were observed. After some optimization,



First flux qubit.
top: the qubit is the closed loop with three small junctions on the right.
It is integrated with the measuring SQUID (two big junctions on the left).
bottom: Yasu Nakamura and Irinel Chiorescu; Rabi oscillations.

decoherence times were of order 1 μs, similar to the times for other superconducting qubits.

In the following years the flux qubit was put to the test in various ways. The energy levels of the two qubit states were well separated from the next higher levels, which allowed for strong driving without mixing. Coupling to the flux loop could be strong, a two-qubit controlled-not gate could relatively easily be operated (published in 2007). The coupling to a harmonic oscillator could be pushed into the strong and the ultra-strong regimes, which was interesting from a fundamental as well as a practical point of view. Pol Forn-Diaz demonstrated the Bloch-Siegert shift for very high intensity and very strong coupling. That had been seen before in nuclear magnetic resonance, but not on fabricated structures. After his PhD, Pol moved to Barcelona and continues in this direction.

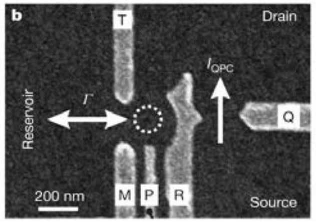
The main line in superconducting quantum information processing was driven by the introduction of very high quality electromagnetic striplines. Resonators were developed in the microwave frequency range with extremely high quality-factors. The qualification microwave quantum optics is fully justified for these techniques. Schoelkopf and collaborators at Yale University coupled their transmon qubits to the striplines and operated them with pulses through them. Lower frequency 1/f type noise is very effectively filtered. The technology was subsequently adapted by Martinis in Santa Barbara, IBM and others. It is now the standard technology. Coherence times are now far above 100 µs.

After 2006 when I reached the statutory retirement age our research on flux qubits gradually was stopped. We could not any more introduce the microwave quantum optics techniques, but in other labs that was realized and similar coherence times were obtained. In Delft, Floor Paauw and Arkady Federov showed that flux qubits could be operated in a few nanoseconds without leakage to higher levels.

In 2010 Leo DiCarlo arrived in Delft. He came from the Schoelkopf group in Yale and had ample experience with transmon ciruits and microwave quantum optics techniques. He immediately started to set up fabrication and operation tools for the transmon-based technology. He set up new experiments in a very short time. As an example, with his team he published a Nature paper in 2013 on deterministic entanglement of two qubits by continuous measurement and feedback. In Qutech, the superconducting quantum information branch is one of the main pillars. A large European program is running with many laboratories involved.

Semiconductor quantum dots

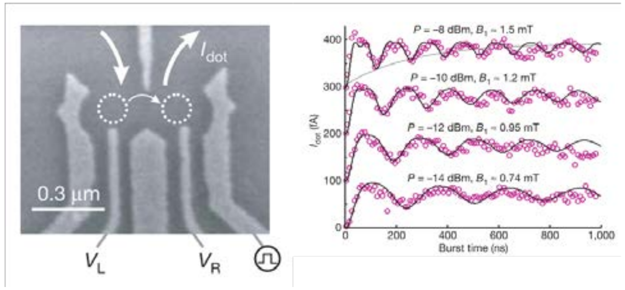
The semiconductor quantum dot team of Leo Kouwenhoven gradually improved their control of single electrons and single spins in quantum dots. This went so far that not only the charge but also the spin of one electron could be measured in an elegant way. In the presence of a magnetic field, the energy levels of the spin-up and spin-down states are different (Zeeman splitting). From the higher energy state, leaving the dot is easier. The presence or absence of the electron could be measured with a quantum point contact next to the dot system, acting as an electrometer. The team that reported on this spin readout (Nature 2004) is also of interest in the context of the present leading scientists of QuTech. The names Ronald Hanson and Lieven Vandersypen appear, the first being a PhD student at the time. Lieven



Sample for single-shot spin read-out, Nature **430**, 432 (22 July 2004)
Metal gates define the quantum dot in the middle. With a tunable contact on the left and a central gate P, the electron states are controlled. The quantum point contact on the right is sensitive to the electrical potential of the dot.

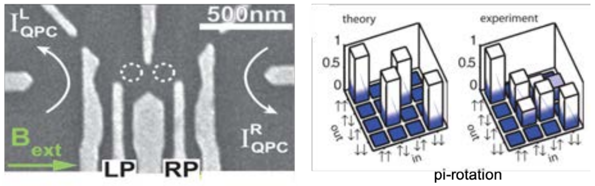
had come to Delft in 2003 after his PhD at Stanford, to work with Leo. The first author of this paper was Jeroen Elzerman.

After it had become possible to measure the spin state of an electron in a dot, the road was open to create the first Delft qubit with a quantum dot. Now two coupled quantum dots were created with each one electron. After driving the spin in one dot, escape was through the other dot and was better controlled. Clear Rabi oscillations were observed. Remarkably, oscillations persisted up to a microsecond (modified by interactions with the slow fluctuations of the nuclear spins). Frank Koppens was the lead author here.



Driven coherent oscillations of a single electron spin, Nature **442**, 766 (17 August 2006)
Left: sample with two connected quantum dots. The dots are loaded with one electron each, the spin on one dot is rotated with an RF magnetic field (ESR). Depending on that spin state, a charge can or cannot move from left to right through both dots. The process is repeated fast so that a measurable current is created. Right: observed Rabi oscillations.

In these experiments at the edge of the (im)possible, very systematic improvement of understanding and control of many small factors is needed to make progress. It took two years (2004 to 2006) to go from single spin readout to observation of coherent oscillations although adding RF excitation might seem straightforward. The next "obvious" step was to achieve independent readout of the two electron spins in the dots and to create a two-qubit gate. The report on this came in 2011 with Katja Nowack as first author. The interaction between the two spins was through the exchange interaction.

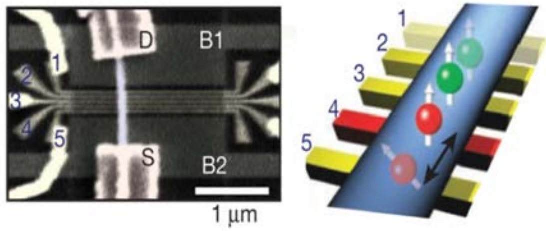


Two-qubit gate, Science **333**, 1271 (2 September 2011)
Left: sample, the presence of an electron in one dot is observed with a quantum point Contact, on each side. Manipulation is with two RF gates LP and RP.
Right: truth table for a pi-rotation of the exchange oscillation.

The qubits that were discussed so far were defined by means of gates on top of a two-dimensional electron gas. Such gates have a relatively long range of influence. This type of architecture is difficult to extend to a large number of qubits. A new direction was initiated by creating quantum dots in grown narrow nanowires. InAs wires were grown by Erik Bakkers and his team in Eindhoven.

For InAs the spin-orbit coupling is so strong that spin and orbital number cannot be defined separately. Spin-orbit doublets can be created in a magnetic field and can be driven similar to spin doublets. The orbital part of the wavefunction can be used for qubit manipulation. In these first experiments, the T_2* dephasing time was 8 ns, the echo time 50 ns. The great advantage of these wire-defined dots is that the in-plane definition is provided by the wire.

In the following years the nanowire qubits were improved and completely new structures with multiple wires were created, as well as with interfaces to superconductors.



Double quantum dot system in InAs nanowire, Nature **468**, 184 (23 December 2010)
Five gates below the wire define the dots and are used for manipulation.

Further developments

Lieven Vandersypen was appointed as Van Leeuwenhoek professor in 2007. After his arrival in Delft in 2001 he had first worked with Leo Kouwenhoven's team on quantum dots in the GaAs two-dimensional electron gases and was involved in the previously described experiments. Later he directed himself to different material systems, such as graphene, silicon and silicon-germanium. His silicon qubits are clearly a very attractive new line of research, beyond the time-scope of this story.

Ronald Hanson took his PhD in 2005 and went for a postdoc period in Santa Barbara with the group of David Awschalom. He became familiar with NV centers in diamond, basically single isolated electrons with spin, that could be addressed with optical pulses. When he came back to Delft in 2007, he set up similar experiments in Delft. This was new for QT, dark rooms full of lasers and mirrors. Nanofabrication was still used. Ronald was appointed as Van Leeuwenhoek professor in 2012. He was to become famous with his loophole-free Bell experiment in 2015. His main line is quantum communication.

At the start of Qutech in 2013, the faculty input from the side of QT consisted of Leo Kouwenhoven (nanowire qubit systems), Lieven Vandersypen (silicon quantum dots), Ronald Hanson (spin/photon quantum communication) and Leo DiCarlo (superconducting quantum information processing).

QT on the steps of the Zürich Opernhaus

4. Social side

When I retired in 2006, a fantastic booklet was made with one-page contributions by very many (ex) students and staff members. When I look at it now, what strikes me is the recurring description of a group that was ambitious, worked hard without keeping office hours but also had a very active social life. I am convinced that this was a very important factor for the functioning of the group. The social side was informal and run by the group members themselves. It was not broken up along the sub-teams.

The most informal meeting place was the coffee table. People came and left there when it suited them, but when birthdays and accepted papers were celebrated with cake the timing was more relevant. On Friday afternoons usually small groups descended to the student pub Tee Pee kafee in the basement.

From the start, our group members were very active in sports. One or more teams participated in the soccer competition and with the yearly Physics sports days the QT team scored very high. Many trophies were brought to the coffee table.

1975

2005

PhD students defending their thesis or staff members leaving were a good reason for a party where all QT members were welcome. Inevitable highlight was the presentation of a group present, which started with a song. A well-known tune was supplied with new words, special characteristics of the victim were amplified and highlighted. Usually also an experiment had to be performed, for which complicated fake equipment was created. It involved quite an effort by the group members closest to the victim, without exception at the very last moment. A practice session for the group song was held between the official ceremony and the party. The language of the song

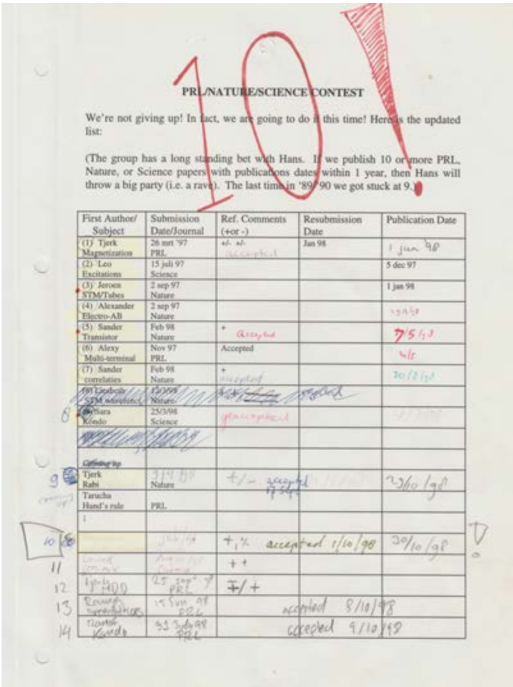
gradually shifted from exclusively Dutch to almost exclusively English. Group members who played instruments constituted the accompanying band, with innovative combinations.

Excursions and group weekend outings (“QT uitje”) were organized by an undemocratically appointed student committee. In the years around 1990 the masters’ students (who still spent almost two years in the group) could profit from a special fund set up by industry to promote study trips. This resulted in 3- or 4-day trips to Belgium, France, England, south Germany or Switzerland with visits to friendly labs, industry and big institutes such as CERN.

When industry stopped their subsidy, the “uitje” became the replacement. In a weekend a large fraction of the group moved to the Ardennes in Belgium, to a sailing boat on the Waddenzee or a similar activity.

Over the years, the composition of the QT team changed very significantly. Between 1971 and 2000 one sees a dramatic shift. In the beginning the masters’ students dominated, all from Dutch origin. Talks were standard in Dutch, as were student reports. In 2000, QT was populated by a wide range of nationalities. All talks were held in English, reports about research were written for the international community. Dutch was the language for teaching to undergraduate students (not officially a QT task, but QT people were involved), and for reports on organizational matters to the university. As mentioned, about half the PhD students and practically all postdocs were non-Dutch. Another noticeable difference was the number of female students and postdocs. Gender balance was certainly not achieved yet, but an individual female person would not feel out of place (I sincerely hope). The female PhD students and postdocs were equally strong in science as the male ones, more diversity is a definitive improvement.

In the later 1990s the group became very productive in producing attention-getting papers. Physical Review Letters was the most prestigious journal for solid state physics until that time, but with our increased range of subjects Science and Nature also came into view. One Friday afternoon, in the Tee Pee kafee, the idea was born that the group should throw a big party if we were to publish 10 papers in these three journals in one calendar year. Later it was relaxed to any period between identical dates in two consecutive years. I had mixed feelings, but I was persuaded. A running list was put on the coffee table, it had to be replaced twice but in 1998 the goal was reached. The party was held on the 14th November 1998. It became a multi-purpose party because we celebrated the 10 super-papers, we said goodbye to our retiring chief technician Chris Gorter, we held a reunion of all QT members, and my recent Shell Oeuvre Prize paid the bill.


10!

PRL/NATURE/SCIENCE CONTEST

We're not giving up! In fact, we are going to do it this time! Here is the updated list:

(The group has a long standing bet with Hans. If we publish 10 or more PRL, Nature, or Science papers with publications dates within 1 year, then Hans will throw a big party (i.e. a rave). The last time in '89/'90 we got stuck at 9.)

	First Author/ Subject	Submission Date/Journal	Ref. Comments (+or -)	Resubmission Date	Publication Date
	(1) Tjerk Magnetization	26 mrt '97 PRL	+/- +/- accepted	Jan 98	1 jun 98
	(2) Leo Excitations	15 juli 97 Science			5 dec 97
	(3) Jeroen STM/Tubes	2 sep 97 Nature			1 jan 98
	(4) Alexander Electro-AB	2 sep 97 Nature			[illegible]
	(5) Sander Transistor	Feb 98 Nature	+ accepted		7/5/98
	(6) Alexy Multi-terminal	Nov 97 PRL	Accepted		4/5
	(7) Sander correlaties	Feb 98 Nature	+ accepted		[illegible]
	~~(8) Liesbeth STM~~	~~13/3/98 Nature~~	~~[illegible]~~		
8	(9) Sara Kondo	25/3/98 Science	geaccepteerd		
	[illegible]				
	Coming up				
9	Tjerk Rabi	Nature	+/- accepted 17 Sept		23/10/98
	Tarucha Hund's rule	PRL			
10			+, ½ accepted 1/10/98		30/10/98
11			+ +		
12		25 sept 98 PRL	∓/+		
13		15 Jun 98 PRL	accepted 8/10/98		
14	Kondo	31 July 98 PRL	accepted 9/10/98		

Part 5. Final remarks

"This story aims to record the development of QT over the years and to explore the factors that contributed to its success. Specific individual people in the university and in the international scientific community were important. Several times the group made conscious choices, starting new directions of research and abandoning others. Group members came and went, some came back. I myself was there all the time and played a central role. My aim is to tell the story of the group, but I can hardly be an objective historian." (in the introduction).

I noted this down before I seriously started writing. The story has become a booklet and it has taken me much more time than I anticipated. With the last part I felt restricted because I felt that history can only be written if the future is known. At this stage, not even a quantum computer can predict what quantum information with our nanofabricated tools will bring us.

This booklet is only about the group QT. When I started to write, I thought to write about all the developments concerning nanoscience and quantum physics in Delft. It turned out to be too much for me to do justice to all that happened. For self-protection I drew a narrow line around our limited endeavors. A real science writer should pick up this subject and make a balanced and objective analysis. I think it is worth doing. Whatever happens in the coming years, this is an interesting and relevant part of the history of Delft University.

The heritage of QT goes beyond its specific contribution to QuTech and the QuTech-related efforts. Technical Physics in Delft has changed, through our new existence and through catalytic influence on other physics activities. The new Departments of Quantum Nanoscience and Bio Nanoscience have their own reasons of existence and their own people, but they would not be there without the QT adventure. Some of "QT" has been exported to universities and laboratories around the country and the world.

Could it have ended differently? To me the year 1997 jumps to mind when I was told by my colleagues that our activity was not wanted in the department. I could have left Delft at that time, which would have changed the balance in QT. Instead, our president Nico de Voogd offered me time at MIT where I could really explore the potential of superconducting circuits for quantum information processing. This led directly to our first qubit and it helped to pull all of QT in the quantum direction.

faculty members of QT over the years, main program
Dirk went to Geneva, Peter to Graz. Teun went to Groningen, but came back in Delft outside QT. Cees created his own bio-nano environment in Delft. Herre is in Quantum Nanoscience in Delft. Hans and Kees are retired.

Many people, more than could be mentioned in the story, contributed to the building and running of QT. The faculty members are listed above. We also had an excellent support staff, a few of them are shown below. Chris Gorter was the chief technician for the first part of QT, Bram van der Enden succeeded him. The part of their job they said they did not like so much was to discipline students, postdocs and faculty who forgot to close helium valves or failed to return special tools. Hanneke Hartgring was the group

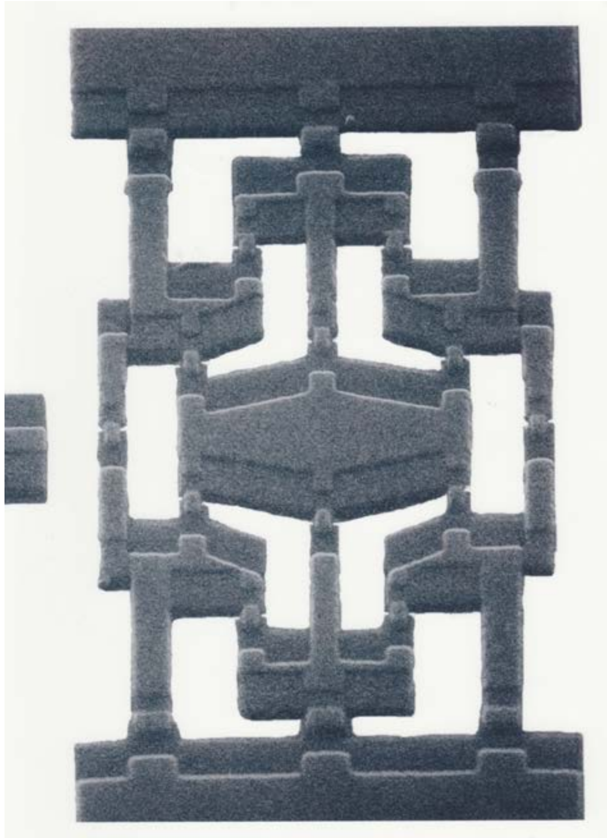
some of the support staff

secretary from even before 1971 when she worked for professor Westerdijk. Ria van Heeren kept order in the paperwork for many later years; Yuki Nakagawa kept order in the budgets. Raymond Schouten was (still is) the electronic magician.

We did make choices on the way. Our route turned out to connect with several scientific/technological hypes. The first was high temperature superconductivity. That certainly was a scientific breakthrough that deserved strong attention. However, the reaction in the world was out of all proportions. Reasonable people could foresee what the likely outcome would be: no breakthrough technology in the short run. In The Netherlands and Europe special research funds were set up at short notice. Our main funding agent FOM took the attitude that the extra money would be accepted but used for purposes that remained valuable after a possible hype might collapse. We in Delft used extra money to set up surface characterization techniques that could be used for many materials. These moved to Groningen when Dirk van der Marel left us.

The next buzz word was nanotechnology. All over the world, suddenly nanoparticles, nano biosystems and nanoelectronics became fashionable although they had little to do with each other. Nanophysics was our subject and we fully participated when extra funds were made available. As described earlier, it gave us the chance to find a solid financial base for our nanofabrication facility. I am convinced this is well-spent research money, but of course I have a strong bias.

Quantum information is also a subject of high recent attention in the press and in politics. The outcome will determine whether in this round it is a hype or an important breakthrough. I am convinced that quantum information is a subject with extremely interesting and important science and an extremely high potential for practical applications. However, it is difficult to get it right and it may take a longer time than the attention span of politics and industry.

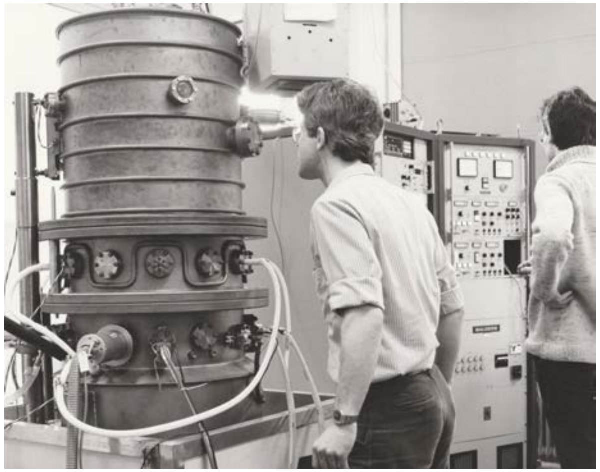


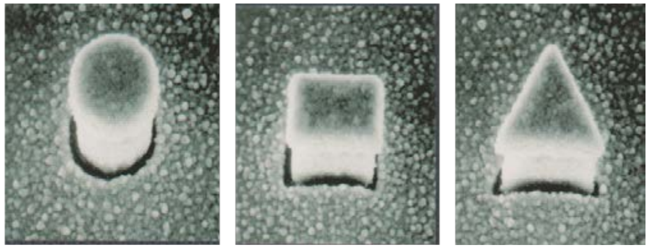
quantum interference of vortices (Wiveka Elion 1993)

I think that scientists should have a good medium-term plan of their own that they keep in mind when they apply for short-term support. They should also have a good instinct about where they want to go in the long run. At all times they should be honest to themselves and to others about what they expect.

In physics, there is a complex relation between theory and experiment. Good theoreticians are very smart and they have the great ideas. Still, In the end only the experiment decides what is possible and what is true. Experiments usually take much longer than the time in which theoretical ideas and models are generated. If some first experimental results look promising, it is tempting to think the predictions are right. However, no model contains all aspects of nature and disappointments can occur in the longer run. Too great expectations are unfair towards the young people that do the real work and as principal investigators we have to be careful.

I do believe in experimental intuition, which in a way is the opposite. It can be very smart to be naïve. Sometimes theoreticians clearly explain why a certain thing is impossible. If your experimental results nevertheless keep indicating that something "impossible" works, do not hesitate to try a new bolder step. This is how we realized the flux qubit.


Roland van der Leur and Jacques Schellingerhout running the UTS 1986


artificial atoms Tjerk Oosterkamp 1999