



J. Peter Towns

SERENDIPITOUS MEANDERINGS AND ADVENTURES WITH MOLECULAR BEAMS

J. Peter Toennies

*Max-Planck-Institut für Strömungsforschung, 37073 Göttingen, Germany;
email: jtoenni@gwdg.de*

■ **Abstract** This is the story of a native-born American who came as a postdoc to the country of his parents, Germany. There, by good fortune, he could participate in the revival and the rebuilding of the physical sciences following the ravishments of the Second World War, becoming at the age of 38, the director of a Max-Planck-Institut in Göttingen. Working under nearly ideal conditions, he carried out basic research using molecular beams. Aided by many active, youthfully impulsive, yet perceptive and imaginative, students and experienced knowledgeable guest scientists from many countries, he enjoyed exciting adventures into unknown landscapes in the fields of molecular gas-phase interactions and solid-surface phenomena and, most recently, in the realms of quantum liquids and solids.

INTRODUCTION

When I received the email from Steve Leone asking me to write a prefatory chapter to the *Annual Review of Physical Chemistry*, I felt greatly honored and quickly accepted. Like so many times in the past, I realized months afterwards, as I began to cope with the work, that I had made a terrible mistake. What should I write about? Certainly, there is plenty, but who might be interested? Then I started reading the prefatory chapters of my mentors and friends and finally realized the great value of such articles that emphasize the human aspect of scientific endeavor. After all, science is created by individuals in the laboratory and not, as some would like us to think, by bureaucrats, anonymous funding agencies, or political institutions. Often it seems that the human aspect of scientific endeavor is forgotten behind the facade of objective, factual, scientific articles. In my life, many people have served as mentors and have guided and accompanied me and contributed to my early development. Later in my career, many young students have joined our scientific projects and enterprises. These students and more senior, experienced, and knowledgeable scientists have all contributed to this story, and by telling it here, I wish to pay homage and express my gratitude to all of those and others, who, for lack of space, could not be mentioned.

The other message I hope to bring across, as did many others before me in this series, is that doing scientific research is immensely rewarding and can bring great satisfaction. In this respect, it is quite like the arts, music, literature, and

most sports. But, in addition, scientific research is adventure. It takes courage and boldness to pursue controversial, often daring, and apparently naive ideas, especially when they involve large commitments of material resources and personal sacrifices. But when these ideas are confirmed by hard scientific evidence or, less frequently, when completely unexpected new concepts, ideas, or even paradigms emerge, then science becomes really exuberantly exciting. We have been fortunate that, thanks to the unbureaucratic and steady funding by the Max Planck Society for more than 30 years, we have had the freedom to partake in these adventures without being subjected to restrictive peer review. We could, guided by many mostly foreign colleagues, define our own goals and evaluate our own work. It is indeed unfortunate that today these freedoms are being increasingly constrained.

FORMATIVE YEARS: 1930–1952

During World War I, my German father was drafted at the age of 18 and sent to the western front, where he was shortly afterwards taken prisoner by the French. The following four years were spent in various prison camps where he also worked in a gas plant. This, he later told me, aroused his interest in chemistry and determined his (and presumably my own) career. Upon his release in 1920, my father studied chemistry in Kiel and received his Ph.D. in 1924 for a project directed by Otto Paul Hermann Diels (who later received the 1950 Nobel Prize in Chemistry). The chaotic political conditions in Germany, which lead to hyperinflation culminating in 1923, nurtured his desire to emigrate to the New World in 1924. My mother, who met my father while she was working as a household trainee in his father's house, left Husum, a small seaport on the western coast of northern Germany, a year later to join him in 1925. Their marriage on Ellis Island allowed my mother to enter the United States.

Soon after I was born in Philadelphia, my parents were able to afford to move to the comfortable Main Line suburbs of the city. I was fortunate in the choice of my parents. My mother taught me by her example to love nature, work hard, and to stand up for my own ideas. My father aroused my interest in the outside world and the sciences. Because of my parents' German origins, I was often confronted with animosity from my playmates, who were influenced by their parents' attitudes left over from World War I.

Getting accepted into college in 1948 was not easy because of competition from returning World War II veterans. Without knowing what I wanted to study, I applied to the little Ivy League colleges Amherst and Wesleyan as well as to Princeton and several other institutions. I was fortunate to be accepted by Amherst with a \$350 per semester scholarship. Amherst College is in a small New England village set in rolling countryside. The beautiful campus is on a hill overlooking the distant Mt. Holyoke range.

My father had connections to the owners of the Sun Oil Company, and this got me well-paid (\$209/month) summer jobs to help defray college expenses.

I was employed on one of their tankers, which commuted between their refinery at Marcus Hook, Pennsylvania, and various oil terminals in Louisiana and Texas. Usually my job was in the engine room, where I served as an ordinary wiper. These tankers were powered by huge, opposed-piston diesel engines with four or five cylinders each with a diameter of about 80 cm; the entire engine was about three stories high. The wipers were entrusted with regularly cleaning up spilled oil and scrubbing or painting bulkheads. I enjoyed many adventures in the engine room, despite the heat, which rose from about 40°C on the ground floor to higher temperatures in the upper levels, especially on sunny days in the Gulf of Mexico. One experience that I will never forget started by being pulled out of bed in the middle of the night to enter the dark, hot, cavernous crankshaft casing just after the engines had been shut off. With hot oil dripping on me from above, I was entrusted with holding a nearly 1-m-long heavy wrench upon which the engineer, standing somewhere above me in the murky darkness, whacked with a huge sledgehammer to loosen a bearing-casing nut. The fascination with these huge engines, the trips to new places like romantic New Orleans, and the stark beauty of the oceans in different moods, all tempted me to go to sea, but several of my shipmates, who kindly fathered me, warned me of the hardships of this restless homeless life.

During my junior year, I took a course in physical chemistry given by David C. Grahame. Although I had somewhat hesitantly signed up for a major in physics, Grahame's inspiring lectures had a big impact on my future. Not only were his lectures meticulously organized and enthusiastically presented, but he also made me and the other students aware, for the first time in our studies, of the role of molecules in determining chemical phenomena. At that time, the emphasis in physics courses, aside from atomic spectroscopy and nuclear physics, was mostly on macroscopic phenomena.

In our senior year at Amherst, the physics majors were given the opportunity to choose an adviser for an honors research project. As I recall there were six of us altogether: Rudy Bauer (a German guest student), Lang Crane, Don Neal, Bob Romer, and Bill Zimmermann. Bauer, Romer, and Zimmermann became professors of physics at Berkeley, Amherst, and Minnesota, respectively. I was fortunate in my choice of William Fairbank as my adviser. He suggested I study paramagnetic resonance in a simple salt using a war-surplus microwave apparatus. To calibrate the magnetic field he showed me how to build a 6-ft-long wooden lever with a small coil at one end and how to yank it out of the field by a sudden jerk at the other end. The current pulse was measured with a homemade self-compensating ballistic galvanometer. Fairbank was always full of enthusiasm, and often when we met he would inform me rather hastily about some new results from the outside world of research physics. Although I really could not grasp or fully understand what he told me, I remember being flattered and inspired by his sharing his excitement with me.

Now that my interest in molecules had been aroused by Grahame's physical chemistry course, I applied to Princeton Graduate School, hoping to earn a Ph.D. with C.P. Smythe on microwave studies of molecular structures. But because the

Princeton department insisted that I would have to make up the courses in organic chemistry, which as a physics student I had not taken, I was advised by Ralph Beeby, the chairman at Amherst, to apply to nearby Brown University because they had a more physics-oriented physical chemistry program. I was relieved by the reply to my letter of application from the Brown Chairman Robert Cole, who wrote, "We are not so interested in the courses you have taken, but more in your future potential."

GRADUATE SCHOOL AT BROWN: 1952–1953

The east side of Providence, Rhode Island, where Brown University is located, was at one time the wealthy residential part of this old New England port and was still characterized by many imposing old mansions. They overlook both the downtown area as well as the old fishing harbor. I especially remember the beautiful fall foliage that greeted me upon arrival in the fall of 1952. At that time, graduate students started on their research project immediately upon arrival. I liked Ned Greene's project and became his first graduate student. Ned had arrived a few years earlier, a graduate of George Kistiakowsky's renowned Gibb's Laboratory at Harvard. As a postdoc at Brown with Don Hornig, Ned had been involved in shock-wave experiments. Ned proposed that I should study chemical reactions in the suddenly heated gas behind a shock front. I was fascinated by the idea of triggering a chemical reaction by a one-dimensional temperature jump and the novelty of this experiment.

Because we were expected to build our own equipment from scratch, I had to learn glassblowing, how to work in the machine shop, soft and hard soldering (welding was not common in those days), etc., skills that have proven useful ever since. Ned was a very supportive adviser: When guests came to see our experiments, he would invariably give me credit for all the results, even those that he had obtained in my absence.

During that first year, I heard of a new scholarship program to support one-year studies in Germany, which had been proposed by Senator Fulbright to rebuild German-American cultural exchanges. Several trips to Germany as a child had instilled in me a romantic fondness for Germany. I had even often thought that maybe someday I might want to live in Germany, and perhaps this would be a way to find out if this was really what I wanted. I was fortunate to receive a Fulbright fellowship and despite concerns about interrupting my graduate studies I accepted. This must have been quite a blow for Ned: his first Ph.D. student leaving for a year just after he had broken him in.

FULBRIGHT YEAR IN GÖTTINGEN: 1953–1954

I had been accepted by Professor Erwin Meyer at the Drittes Physikalisches Institut in Göttingen. Upon arrival, I quickly realized that the project did not interest me because no chemistry was involved. At the nearby Institut für Physikalische

Chemie, Arnold Eucken had passed away in 1950 and Ewald Wicke was the interim director. He arranged for an interview with the newly appointed Professor of Physical Chemistry, Wilhelm Jost, who was still closing down his activities in Darmstadt. Jost was very kind to me despite my unruly red beard (beards were by no means in style at the time) and lack of experience. I had the impression he was happy to have a chance to talk to an American after the good reception he had received during his visit to Harvard some years earlier. He suggested I consider one of three possible projects, which he briefly described. I had understood little, so at our next appointment the following week, I was at a loss which to choose. He apparently fully understood my embarrassment and encouraged me to consider studying spinning detonations in CO/O₂ mixtures with a new high-speed drum camera that had been ordered from a local centrifuge company. A few days later, when I met my laboratory mate, Gerhard Spener, I had second thoughts about the project after learning from him that the green speckles on his face had come from an accidental explosion that nearly cost him his eyesight during the course of his detonation studies.

Heinz Georg Wagner (later a codirector at the Max-Planck-Institut für Strömungsforschung and a university colleague), who was Jost's right-hand man, had designed the drum camera and helped me to get started. Until that time, drum cameras were operated with the film moving at right angles to the direction of the shock wave or detonation front. It occurred to me that by allowing the film to move in the same direction as the shock wave one might be able to freeze the motion so that the image moves with the same speed as the film. Because any transverse motion of a localized hot spot in the detonation front would still smear out the picture, I installed a series of small narrow mirrors that provided a series of spatially resolved sections at ten different positions and, in this way, a complete x-, y-, and z-resolved image of the detonation. Because I was aware of the dangers involved in the operation of the detonation apparatus, I chose to carry out my experiments during the night, when I would not be disturbed. Surprisingly, the neighbors never complained about the hourly detonations that occurred all night long. The experiments were a success, and Wagner wrote up the work, which appeared after my departure in the *Zeitschrift für Elektrochemie* (1).

Life in the Institute, which was close to the old town, was very *gemütlich*. Once a week, I think it was on Thursday afternoons, a big long table was set up in the library, and one member of the group was responsible for *Kaffee und Kuchen*. At 4.15 P.M., all the members of the department would take their seats at this long table until at 5.15 P.M., the weekly seminar began. As a Fulbright student, I received a \$100 monthly stipend, and by local standards, I was very well-to-do. Thus, I could afford a Volkswagen Beetle, which with Professor Jost's Opel Kapitän were the only cars in the Institute's parking lot.

Because Göttingen had not been significantly damaged during the war, the Max Planck Administration and several Max Planck Institutes, including the Institute for Physics had been evacuated to Göttingen from Berlin. Thus, in addition to lectures by Richard Becker, Werner Buckel, and Rudolph Hilsch at the University Physics Institutes, I was also able to attend seminars given by Werner Heisenberg

and Carl Friedrich von Weizsäcker. Max von Laue and the emeritus professors Max Born and Richard Courant were also associated with the physics department at that time.

BACK TO BROWN: 1954–1957

Upon my return to Brown, I profited from John Ross's (now at Stanford) course on statistical mechanics, even though it was held at 8.00 A.M., which even today is very early for my taste. Unfortunately, I never seemed to be able to make it on time, and I invariably arrived 10 minutes late. I also took several advanced physical chemistry courses from Gene Carpenter. During the semester, many famous scientists, including Isador Amdur, Lou Branscomb, Abe Kantrowitz, Hans Kuhn, and Bernard Pullmann, gave lectures on Monday evenings. Even though I was usually quite drowsy after dinner, surprisingly, I recall many of these talks, perhaps because the speakers were so relaxed after cocktails and a good dinner.

Instead of the usual preliminary examinations, we were expected to pass one-half of the required 16 exams, which were offered during the second and third years. These 3-h-long exams, which covered all aspects of physical chemistry, including recent publications, were offered from 9:00 A.M. to 12:00 NOON on the first Saturday in the month. After having slept through one exam, and having had bad luck with several others, my future at one point looked bleak because I had to pass all the remaining three. Fortunately I did!

At the end of my Fulbright year in Göttingen, Professor Jost had entrusted me with asking Ned Greene if he would be willing to write a monograph on shock waves for a series Jost was editing. When I passed the message on to Ned, he hesitated and this prompted me, somewhat presumptuously, to offer to help with the project. Our monograph, written in English, was the first in the field, but because it appeared in 1959 after having been translated into German as *Chemische Reaktionen in Stosswellen* (2) it was ignored in the United States. By the time we published an updated English version in 1964 (3), we had been scooped by several other books in English.

At Brown, I came into contact with a number of postdocs who became lifelong friends. In 1956, Gian Gualberto Volpi (now professor of a very successful molecular-beam department of chemistry in Perugia, Italy) came to Harvard as a postdoc from Rome and frequently would drive down to Brown to visit his friend Mario Azzarri, an Italian postdoc with whom I was sharing an apartment. Volpi was very dapper, well-dressed and, as Mario had warned me, knew everything. And indeed I learned much from Volpi, including what was going on at the Gibb's laboratory at Harvard. Howard Palmer, who was a postdoc from Madison, Wisconsin, with Ned Greene, shared the same laboratory with me, and I am grateful to him for many patient explanations. Karl Heinz Hansen, from Frankfurt, spent some time at Brown before moving to Princeton with Hornig around 1956, and he also became a good friend. Later we were colleagues at Bonn before his untimely death in 1970.

Crispin Simpson from Cambridge, who later settled down in Oxford, also became a close friend. He was always in excellent physical shape, and I remember long runs (now called jogging) where he would always leave me far behind. And there was George Turrell, who had earned his Ph.D. with Decius at Oregon, and was an expert spectroscopist and a connoisseur of good food and fine wines.

At the end of 1956, I had finally passed the required Ph.D. exams, and the end of the book and the Ph.D. thesis were in sight. Being afraid of the regular schedule and the routine life of an industrial scientist, and despite several offers of jobs at Los Alamos and General Electric research laboratories, I decided that I would postpone making a decision and instead look for a postdoctoral position.

THE IMPACT OF THE TAYLOR AND DATZ EXPERIMENT: 1955

In the course of my Ph.D. research on the determination of the dissociation energies of N_2 and CO from measurements of the velocities of reflected shock waves (4, 4a), I was confronted with questions of the efficiencies of collisions in transforming translational energy into internal energy. At that time most of the understanding of these processes came from relaxation measurements such as from the dispersion of sound with frequency. There had been some discussion in the department about collisional studies. John Ross had worked with Isidor Amdur, who had developed an ingenious technique of accelerating atomic beams to energies of 200 to 2000 eV (5). At these energies, the scattered atoms are easy to detect by a simple-to-build bolometer. As a young graduate student, I was not aware of the pioneering experiments on atomic and molecular scattering conducted in Hamburg during the period 1926 to 1933 in Otto Stern's laboratory (6). But Kantrowitz's & Grey's 1951 paper (7), suggesting the use of nozzle beams, and the subsequent unsuccessful attempt by Kistiakowsky & Slichter (8) to implement this new technique at Harvard were well known in the nearby Brown chemistry department.

Also, the assessment by the famous Kistiakowsky that molecular beams are "a graveyard for good chemists" (E.F. Greene, private communication) had made the rounds. Thus, the 1955 paper (10) by Ellison Taylor and Sheldon Datz entitled *Study of Chemical Reactions with Molecular Beams. The Reaction of K with HBr* came as a big surprise. Taylor was unknown in the New England Ivy League physical chemistry circles because he had been at far-away Oak Ridge since 1945, and Datz was still a young Ph.D. student. Interestingly, before joining the navy, Datz, during the war at the age of 16, had already worked as a technician with Taylor at Columbia on research related to the Manhattan Project. Their publication especially electrified Ned Greene and John Ross, and shortly after the Taylor and Datz paper had appeared, they jointly gave an informal seminar about these new experiments. I recall being fascinated by the directness of this new approach. Greene and Ross immediately set up a small apparatus, and a new student Dick Roberts, who had started his Ph.D. research on shock waves, was entrusted with

constructing a molecular-beam machine. As Greene later told me, they had already thought about doing something new in addition to shock waves (Greene) and studying transport processes (Ross). Roberts was an amazingly well-organized and technically gifted student. He rapidly designed and managed to put together a crossed molecular-beam apparatus with a velocity selector during my final year at Brown. At the time, I was most impressed by the fact that Roberts had been successful in persuading his mentors to provide him with a key to the shop. When after hours I had needed something out of the shop I had to pick the lock.

Inspired by the excitement at Brown, I resolved to seek a postdoc in a laboratory where I could learn all about molecular beams. It was a difficult decision because, aside from the Taylor and Datz experiment and the new apparatus at Brown, there were no new reports of successful experiments. Rather naively I wrote to Rabi at Columbia and Trischka at Syracuse University asking for a postdoc position. Neither answered, which in hindsight is perhaps not so surprising. Both were in physics departments, probably had no positions available, and certainly did not need a physical chemist with no experience with molecular beams. In the spring of 1957, I happened to meet Hans Gerhard Bennewitz at the physics department in Princeton. Bennewitz, who at that time had not heard of the Taylor and Datz experiment, was interested in learning about the possibilities of doing collision studies with molecular beams. He promised to write to his professor at Bonn, Wolfgang Paul, to get his support. I left Princeton quite intrigued by the idea of going to Bonn, except for the facts that I had decided after my Fulbright year in Göttingen to stay in the United States and, moreover, I had never heard of Paul's laboratory in Bonn. Encouraged by my friend Paul Fjelstad who was working on his Ph.D. with Norman Ramsey, I decided to ask Ramsey for his advice. Ramsey very kindly received me. He encouraged me to go to Bonn and gave me the following advice that has since served me well: "Since working with molecular beams is, in itself, perhaps the most demanding in experimental physics, I should in planning my experiments avoid the temptation of introducing more than one new technology." In his opinion, this was why many earlier experiments had failed.

In the ensuing months I realized that electrostatic quadrupole fields, which had been developed in Bonn for focusing and state-selecting polar molecules (11) and for carrying out Rabi-type spectroscopy on fine-structure transitions, were ideally suited for inelastic collision experiments with molecules. It was only necessary to replace the high-frequency plates between the two quadrupoles with a collision cell. I informed Bennewitz of my ideas, and we had a good time inventing new *Gedanken* experiments along these lines. Unfortunately, Paul never replied to Bennewitz' letters, and as the summer progressed, I became increasingly worried about my future. In a final effort, I sent a telegram to Bonn with a prepaid reply. Even then I received no answer, until a week later I was called to the only student phone, which was in the organic chemistry laboratory, and the operator started to read a telegram to me. At first, I had trouble deciphering the text until I realized it was in German: "Biete Ihnen Arbeitsplatz und Stipendium an, W. Paul." I was completely bewildered by this unexpected turn of events. In the preceding days,

I had decided that the people in Bonn (like Rabi and Trischka) also did not want me. I finally accepted the invitation and departed by ship from Hoboken, New Jersey, on the *M.S. Vollendam* for Antwerp to arrive in Bonn on the evening of November 7, 1957.

POSTDOC IN BONN: 1957–1965

In October of 1957, the Russians surprised the West by successfully launching their first Sputnik into outer space. Little did I realize then what a great impact this would have on U.S. science and the further development of molecular-beam technology. During the voyage to Europe, we were informed on November 3 that the Russians had succeeded in launching a second satellite, this time carrying a dog. Upon arrival in Antwerp, the newspapers were full of speculations about Russian manned space flight and even Russian colonies on the moon.

Bennewitz greeted me at the railroad station on a dark, dreary, cold, wet evening and took me directly to the I. Physikalisches Institut der Universität Bonn, Nussallee 12, where he proudly showed me his bewilderingly complicated molecular-beam machine, the electronics of which he had adapted to receive and analyze the radio-frequency signals from Sputnik II. Bennewitz had immediately realized that by carefully analyzing the Sputnik signals one could not only determine its height from the time it took to circle the earth, but also, from the gradual change in period, estimate the friction and from this the density in the atmosphere. As far as I am aware, Bennewitz and his colleague Wolfgang Priester in the astronomy department were the first persons outside Russia to carry out such an analysis.

I had a second shock, a cultural one, the next evening when I was introduced to laboratory social life. Bennewitz and his colleague and friend Karl Heinz Althoff, who was another *Assistent* working on the 500-MeV electron synchrotron (the first in Germany), were wondering what to do when they came upon the idea to play chess. Their partner was the beginning Ph.D. student Peter Toschek (now a professor in Hamburg), who I gathered was well-known for his prowess at chess. To my surprise, Toschek left the room where the game board had been set up and took a seat in the adjacent laboratory. Toschek won the match, and I suddenly realized that students in Germany must be so extraordinarily gifted that they can easily beat two assistants at chess without even looking at the board. This little episode illustrates how in those years the lives of young students and university assistants centered around the institute because living conditions were still quite primitive in the aftermath of World War II. The institute provided a place not only of work, but also for social life and, in addition, the only shower available to most of us.

In moving to Bonn, I initially had thought I would learn everything about molecular beams by joining an ongoing project and then, after a year, return to the United States to take up a job where I could build my own machine. But Bennewitz insisted that we build an apparatus along the lines I had suggested that previous

summer. Recklessly, I finally agreed, naively thinking that this way, after all, I would learn much while achieving what I wanted to do.

Our first experiment was designed to measure the anisotropy of a molecule in collision with an atom. Ramsey, at that time, had published a short communication in which he predicted that for angular momentum-oriented molecules the $m_j = 0$ and $m_j = \pm j$ states should differ in their scattering cross sections by factors of two or more (12).

I remember how Paul was horrified when I suggested that we invest in a 500-l/sec oil diffusion pump, since, previously, the 50-l/sec Hg50 mercury pumps had always been large enough. At the time, vacuum apparatuses were made of brass, and the flanges had to be hard soldered in place. Because of the strains from bolting the flanges, in the course of time, leaks developed that had to be resealed by epoxy, which also continually cracked open. Finally, we had to resort to dousing the critical joints with alcohol every 8 to 12 h to get a satisfactory vacuum. Alcohol not only served to dissolve small amounts of grease or epoxy that temporarily plugged the leaks, but also improved the vacuum because it was very effectively pumped by the liquid-nitrogen traps. I learned these and many other simple ingenious procedures from Bennewitz, who was a gifted experimentalist. For example, Bennewitz had earlier designed the Fizeau-type velocity selector, which we also used in our apparatus. To reduce demands on the shop and because of the need to balance the rotor, we suspended the ball bearings on little rubber tubes used in bicycle inner-tube valves that cost a few *Pfennig*. With these velocity selectors, we could easily achieve 300 Hz (18,000 rpm). After three years of hard work and considerable frustration, we finally found that the factor-of-two effect predicted by Ramsey was only a 1% effect (13). Along the way, we had to learn all about collision theory and intermolecular potentials.

In Bonn, we were not the only ones interested in molecular collisions. The Institut für Angewandte Physik under the directorship of Professor Rudolph Jaeckel was in the same building. To better understand the physical processes in diffusion pumps, Jaeckel had started a research project in his department with the goal of understanding the scattering of simple atoms and molecules from organic (oil) molecules (14). This effort, which started around 1950, was quite independent of the earlier work in Stern's Hamburg laboratory and the contemporary work in Erwin Becker's laboratory in Marburg (15). Hans Pauly (who later became a codirector at the Max-Planck-Institut für Strömungsforschung) was Jaeckel's second student to work on these problems. When I arrived in Bonn, he had just completed the construction of a small, brass vacuum apparatus, with which he later succeeded in measuring the velocity dependence of atom-atom integral cross sections and experimentally confirmed the 1935 Mott-Massey $v^{-2/5}$ formula (16). This was the first experiment to demonstrate convincingly the importance of the long-range dispersion potential for understanding the unexpectedly large scattering cross sections of atoms and molecules.

Christoph Schlier, another young assistant on Paul's staff, (later a professor in Freiburg), had also become interested in molecular collisions. On his return from

his postdoctoral year at CERN in 1958, he set up as his first independent project an experiment to measure the anisotropy of polarized atoms in collisions with other atoms. This apparatus, which was built by the Ph.D. students Klaus Berkling and Peter Toscheck, was in the same room as ours. We had innumerable good times but also many little altercations because money, floor space, and equipment were always scarce.

Also in 1958, several of us enrolled in a summer course on quantum chemistry organized by Per Olaf Löwdin's Uppsala group in the middle of Sweden in a far-away village called Vålådalen. The group included Otto Osberghaus (later a professor in Freiburg), Hans von Weissenhof (later a professor in Hannover), Gernot Gräff (later a professor in Mainz), Bennewitz (later a professor in Bonn), and me. It was a very intensive course with George Hall, Laurens Jansen, Roy McWeeny, Rudolph Paunz, Hanswerner Preuss, Harrison Shull, and many others, in addition to Löwdin, giving the lectures and assigning plenty of homework! During the coffee breaks, we played Frisbee with the other American participants. In the final week, several notables including Robert Mulliken and Linus Pauling came for special seminars. It was amusing to witness Linus Pauling at the blackboard estimating molecular-bond distances and bond angles with uncanny accuracy, while Mulliken, puffing on his cigarette, would try to find the corresponding calculations in a big pile in front of him. During one of



Figure 1 Reunion of the Bonn colleagues (from *left to right*) Hans Pauly, Christoph Schlier, Dieter Beck, and J.P.T. in his office in Göttingen (around 1975). Ludwig Prandtl, the first director of the institute, is seen in the photograph on the wall.

the breaks, I asked Pauling if he knew how to throw a Frisbee. He caught on immediately.

In the Bonn institute, we had regular seminars where the Vålådalen participants and other senior members including Hans Pauly, Christoph Schlier, and Dieter Beck, upon his return from a postdoc year at Brown, frequently gave the talks. These activities were directly and indirectly stimulated and fostered by the Director of the Institute, Professor Wolfgang Paul (who won the 1989 Nobel Prize in physics). Paul had received his Ph.D. with Hans Kopfermann in 1939. In his dissertation *Eine Atomstrahlapparatur zur lichtstarken Anregung schwer verdampfbarer Elemente*, he used atomic beams for high-resolution spectroscopy. During World War II, he was first drafted but soon after called back so that he could continue his university career. As part of the German nuclear bomb project, a few young scientists were exempted from military service to develop mass spectrometers for isotope separation, and Paul was among the lucky few. Paul came to Bonn as a full professor in 1952, and when I arrived in 1957, the institute had grown considerably because Paul had initiated research in a large number of loosely related fields. Of course, he is best known for the Paul ion trap, which together with the quadrupole mass spectrometer, developed out of the famous Paul-Steinwedel paper (17). In the late 1950s, many experiments were devoted to exploring new uses of the quadrupole mass spectrometers, e.g., for molecular-beam detectors and even for plasma research. There were many more projects going on in a wide diversity of fields, many of which were centered around high-energy research using the 500-MeV electron synchrotron, the first particle accelerator in postwar Germany. The Monday Evening Institute seminars were devoted to a wide range of subjects, and because the institute was the central point in the lives of most of us, the seminars were well attended. Paul would frequently start off the seminars with an amusing story or a report of a recent breakthrough either in the institute or elsewhere.

In those days, the department system, which was developed after the 1968 student revolts, had not been installed, and Paul was in sole charge of the entire institute. Of course, he was helped by the *Assistenten*. For example, my colleague Bennewitz was invariably in charge of supervising the construction or renovations of new buildings, the most prominent of which was a large lecture hall. Althoff was in charge of the construction and operation of the synchrotron electron accelerator (the first in postwar Germany) and later entrusted with supervising the construction of new accelerators. My job as a native American was to host foreign guests. In this capacity, I got to meet Robert Frisch, Marie Goeppert and Joe Mayer, Hans Frauenfelder, Vernon Hughes, and many others.

As Paul proudly liked to emphasize, the one-professor system had many advantages since the institutes were self-governing and virtually completely free of outside state control. The *Assistenten* had, aside from their organizational responsibilities and supervision of lab courses, no teaching responsibilities and could concentrate on their research. Paul would, for his part, then make sure that the project would be funded. Extensive reviewing was not required, and all that was often needed was a one-page project description. The system also had disadvantages.

From the 1960s onwards, Paul was given other duties at CERN, DESY, and at the nearby KFA in Jülich; consequently, he was often out of town, and difficult decisions frequently had to be made without him. This situation in the institute was called *gelenkte Anarchie* (guided chaos) by some. It functioned because Paul had competent coworkers. Moreover, he was also a father figure and altogether highly respected.

By the spring of 1961, we had succeeded in using tandem electrostatic quadrupoles to measure collision-induced, state-to-state, rotational, inelastic-scattering cross sections for the $\Delta j = 1$ transitions such as $(j, m) = (1, 0) \rightarrow (2, 0)$ and even for $\Delta j = 2$ transitions of TIF molecules in collisions with 14 different target atoms and molecules (18, 18a). These were the first measurements of quantum state-resolved molecular-collision processes, which today are being extensively studied using laser methods.

This seemed to be the right time for me finally to return to the United States. Previously, Professor Paul had offered me a full-salaried *Assistant* position in Bonn, which I was reluctant to accept because of the low standard of living and the uncertainties for the future. But after a disappointing reception in the United States, where no one seemed interested in state-selected collisions, I accepted since it gave me the opportunity to have my own research group and the freedom to continue the scattering experiments, which still fascinated me.

HABILITATION, PRIVAT DOZENT, AND DOZENT: 1965–1969

In the following years, we constructed our first stainless-steel apparatus, which was an improved version of the tandem quadrupole arrangement. Inspired by our contacts with the high-energy group, we also set up a second apparatus designed to measure vibrationally inelastic cross sections by a time-of-flight technique. The idea was to use Li^+ ions, which, having a closed outer shell, have a short-range potential similar to an He atom but, being charged, are much easier to detect. We had no grants for this project, so the electronics had to be borrowed from the synchrotron group over the weekend by my student Jürgen Schöttler. These experiments, although they were at the time incorrectly interpreted, were the first in the field of molecular collisions to investigate inelastic cross sections via the associated energy transfer and the first to employ the time-of-flight technique.

I also was fortunate to attract several theory students who were well versed in distorted-wave and close-coupling techniques through Horst Rollnick's excellent lecture course. In several publications, we pointed out the importance of orbiting (19) and Feshbach resonances in inelastic and three-body recombination collisions (20). Feshbach resonances are now, 40 years later, in vogue again in connection with Bose-Einstein condensates.

My first postdoc was Roger Grice (later a professor at Manchester), who came to us after earning his Ph.D. with Dudley Herschbach at Harvard. During his

stay and with the help of his successor Sanford Safron, also from Harvard (later a Professor at Florida State), we succeeded during 1969–1970 in measuring the distribution over rotational states of the products of a crossed-beam reaction (21) using electrostatic quadrupole fields. Because laser-induced fluorescence had not been introduced yet, these were the first direct measurements of internal quantum-state distributions in reactive scattering.

In the late fall of 1964, during one of Paul's evening *Spaziergänge* through the institute, which he undertook regularly with his wife after having had dinner at his nearby apartment, he informed me that he had decided that I was ready for my *Habilitation*. Habilitation involved (a) submitting a second dissertation-like manuscript, (b) presenting a one-half-hour lecture on one of three subjects chosen only a week earlier to a closed session of an assembly of all the natural sciences professors, and (c) presenting a public lecture. My public lecture on field emission got considerable publicity because the local newspaper had been alerted in advance that Einstein would appear in person. Einstein was a graduate-student friend, Peter Meyer, in disguise, complete with a violin.

Having passed the Habilitation, I was, finally at age 34, authorized and entitled to give my own lecture courses, and in fact, according to the rules, I was required to give a certain number of lectures weekly to maintain the status of a university *Dozent*. In the past, there had not been any lectures on solid-state physics in the Bonn department, so the suggestion was made that I should give such a course. I was somewhat dismayed because I had never taken a course in solid-state physics. To my surprise, my lectures were well attended even toward the end despite the usual exponential decrease in attendance. My official title at the time was *Privatdozent*, the *Privat* meaning that I was not paid for my teaching.

In 1965, Bennewitz and I were awarded a prize for young physicists by the Academy of Sciences in Göttingen for our experiments on rotationally state-selected collisions. To our surprise, we received a letter from Max Born expressing his regrets for not being able to attend the ceremony. He was very interested in our work but because of another commitment he had to stay home in nearby Bad Pyrmont. We replied that we were honored by his interest and suggested that, if it would suit him, we would gladly visit him on the day after the ceremonies. He graciously received us and explained to us that when he was in Frankfurt (Born was professor of physics at the University of Frankfurt from 1919 to 1921) he had a young colleague, Elisabeth Bormann, who already in 1920 had had some success in observing the scattering of silver atoms (22, 23). At the time, however, his Frankfurt colleague Otto Stern was so much more skilled at experiments that upon accepting the chair in Göttingen in 1921 he decided that he would not try to compete with Stern and that instead he would from then on concentrate on theory!

In the fall of 1968, I gave a lecture course on molecular-beam scattering at the chemistry department of the University of Illinois at Urbana. Now, finally, molecular beams had come into vogue in U.S. chemistry departments. They had been popularized by the stellar experiments of the Herschbach and Bernstein groups, who had extended the early experiments of Taylor and Datz to many different

bimolecular reactions; all of which, however, at the time involved alkali metals because of their easy detectability. Dudley Herschbach has called this “the alkali metal age” in the evolution of molecular-beam chemistry (24). Thus my colleagues at Urbana Bill Flygare, Rudy Marcus, and Don Secrest were interested in hiring me, but by then other opportunities had opened up in Germany.

Another aspect that had emerged largely from several experiments in Bonn was the awareness that the molecular beam scattering technique made it possible to investigate intermolecular potentials experimentally with great precision. In the past, most of what was known about potentials came from an analysis of the equation-of-state or transport processes. For example, in the then famous “green bible” of physical chemistry, *The Molecular Theory of Gases and Liquids*, by Hirschfelder, Curtiss, and Bird, which appeared in 1954, there had been no mention of beam-scattering experiments. Certainly the Sputnik-induced renaissance in U.S. science and the sudden interest in rocket-related space technology helped to foster this rapid development. Nevertheless, as Pauly and I pointed out in our first comprehensive review of the subject entitled *The Study of Intermolecular Potentials with Molecular Beams of Thermal Energies* (25) until 1964 only six small groups were active in the field: These were located at the Oak Ridge National Laboratory (Taylor and Datz), University of Bonn (Beck, Bennewitz, Pauly, Schlier, and myself), Brown University (Greene and Ross), University of Wisconsin (Bernstein), General Dynamics in San Diego (Fite and later Rothe) and Harvard University (Herschbach and Karplus).

In early 1968 the Max Planck Society had been searching for new directors for the Max-Planck-Institut für Strömungsforschung (fluid dynamics). The institute was established in 1924 as the *Kaiser-Wilhelm-Institut für Hydrodynamik* for Ludwig Prandtl to ward off a call to the technical university of Munich. Prandtl’s Kaiser Wilhelm Institute later became world famous for pioneering experiments in fluid dynamics and aerodynamics. Prandtl retired after World War II, and the institute became part of the Max Planck Society, which is the postwar successor to the *Kaiser Wilhelm Gesellschaft*.

Unknown to us, Manfred Eigen, a director at the nearby Max Planck Institute for Physical Chemistry (now part of the Max Planck Institute for Biophysical Chemistry), who in 1967 had received the Nobel Prize in Chemistry (together with George Porter and Ronald Norrish), had proposed a new direction of research for the institute. The idea was to create an interdisciplinary research establishment modeled after the Russian Academy of Sciences Institute of N.N. Semenov and N.V. Kondratjew in Moskow. The new institute was to be focused on investigations of the microscopic interactions of atoms and molecules underlying flows, flames, and combustion processes. This reorientation of the scientific direction illustrates the tradition of continual recreation within the Max Planck Society. In July 1968, both Hans Pauly and I received official letters from the president of the Max Planck Society, Adolf Butenandt, (who received the 1939 Nobel Prize in Chemistry) offering us positions as Members of the Max Planck Society and Directors at the Max-Planck-Institut für Strömungsforschung in Göttingen. The negotiations with



Figure 2 Max Planck is depicted as bestowing upon J.P.T. the *Max-Planck-Institut für Strömungsforschung*. Drawn by Zdenek Herman (1970).

the Max Planck Society went very smoothly. By December 10, 1968, everything was settled, and we started work on January 1, 1969. Today, similar negotiations may take years, but we were young and the Max Planck Society could afford to be generous. The postwar *Wirtschaftswunder* boom was still in full swing, and Germany was in the final phase of rebuilding its research establishments even though 25 years had passed since World War II.

BACK TO GÖTTINGEN AS A DIRECTOR OF A MAX PLANCK INSTITUTE

As Members of the Max Planck Society and Directors of the Institute, which are the equivalent to a full professor's position at a German university, we were promised a regular yearly budget far beyond my wildest dreams. Suddenly we were independent of detailed applications to the Deutsche Forschungsgemeinschaft, which in the past had been our sole source of research funding. It took me a while to realize in those early years in the Max Planck Society that we were under no direct scrutiny by review panels or other peer review processes unknown to us.

As one of the central themes of the new institute, I had envisaged research in the area of surface physics and chemistry. Surfaces had interested me ever since my graduate-student days at Brown. Several of my student friends were working in Farnsworth's surface-science group in the basement of an old New England house that belonged to the Brown physics department. But because I had grown up with glass apparatuses, their limitations were well appreciated. Moreover, the experiments were considered especially difficult and tedious. In Bonn, I had assisted Bennewitz in introducing ultrahigh vacuum (UHV) stainless steel technology, which involved using homemade gold rings as seals. Around 1970, the Varian Company had introduced CONFLAT technology, and UHV components were becoming commercially available. Ekkehard Hulpke, who joined my group after completing his postdoc in Amdur's MIT lab, had had some experience with surface physics while conducting his Ph.D. research at Freiburg, so I suggested that he should initiate work in this area. On one of my regular visits to Gothenburg, Sweden, where from 1965 to about 1975 I supervised the establishment of a molecular-beam group in the chemistry department, I heard about an experiment by Probst and Pieper at Illinois from a Swedish colleague Jan Christer Ericsson, who had just returned from the United States. Probst and Pieper had succeeded in demonstrating the first electron energy-loss spectroscopy (EELS) experiment on W(001) and had been able to measure the vibrational frequencies of several adsorbates (26). In view of our successes with Li^+ -ion scattering from molecules, I suggested that Hulpke should repeat the EELS experiment with ions. As Hulpke found out, the Doppler effect resulting from the vibrational motion of the individual surface atoms smeared out the energy losses thereby preventing their resolution. But Hulpke discovered rainbows in surface scattering (27) and contributed much to our present understanding of the interactions of ions with solid surfaces, processes of major importance for fusion reactors.

In the fall of 1969, Christoph Ottinger, also joined our new group after two consecutive postdoc appointments with Dudley Herschbach and Dick Zare. After his Habilitation in 1976, Ottinger received the equivalent to an associate professor position. He has been a pioneer in spectroscopic studies of reactively or laser-excited species in beams of molecules and ions (28). The experience gained by Andrej Vilesov, who was a postdoc with Ottinger from 1990 to 1996, later played an important role in our studies of superfluid helium droplets.

Pauly and I were extremely fortunate to have had an excellent highly motivated scientist Wolfgang Sattler as laboratory manager. He supervised the various building projects and was responsible for hiring new technical staff and organizing the engineering activities, the drawing office, and the other service facilities. Sattler took an active interest in our scientific projects and shielded us as much as possible from excessive paperwork and bureaucratic chores, such as the weekly meetings with the workers council (*Betriebsrat*) required by German law. Since 1990, Rudolf Düren has continued in Sattler's footsteps and has fostered our research in many ways. Düren has played an important role in steering the institute into its new research direction, which has only in recent months been finalized.

STORIES OF YOUTHFUL SERENDIPITY

As I describe our research activities in Göttingen and their progress over the years, it would be laudable and certainly very satisfying for me if I could today claim that everything unfolded and evolved according to a grand plan. But I soon learned that my plans were frequently poorly orchestrated, or based on unjustified prejudices, simply not original, utterly unrealistic, or far too ambitious. Instead, imponderables, chance events, good fortune, and also bad luck and accidents all played a much greater role than I had expected. Also, I had to learn that as a supervisor I no longer had the same control over the course of an experiment as I had when I was the doer. Thus the course of an experiment now depended on the researchers involved. Their enthusiasm and motivation, their instinct, and in some cases, their naiveté played a decisive role, and these qualities were especially pronounced among the young students.

For this reason, some of the research developments in the following years can best be described by recounting fortunate events, which in hindsight determined in actual fact some of the important directions of our research. There are many of these stories, as I call them, of which I describe only a few.

High Speed-Ratio Helium Atom Beams

Manfred Faubel joined our group as one of the first students to take up work in the Max Planck Institute. Together with another student from Bonn Rudolph David, he was to build a new and improved crossed-beam machine to repeat the $\text{Li}^+\text{-H}_2$ scattering experiments with a much higher resolution. By chance, Ulf Bossel joined one of the neighboring Deutsche Luft und Raumfahrt Institutes, (DLR) after having completed his Ph.D. with Frank Hurlbut and Fred Sherman at the University of California, Berkeley, Department of Aeronautical Sciences, and from him we learned about a new type of skimmer that was longer and more pointed than those used at that time by the established groups. The nozzle-beam expansions, which had been prematurely condemned nearly 20 years earlier by Kistiakowsky, were now gradually being accepted. In 1970, Yuan Lee and his students reported at an early Molecular Dynamics Conference in Oak Ridge, Tennessee, on their remarkable measurements of high-resolution angular distributions for rare gas-scattering partners, which utilized two free-jet expansions, and this led to their acceptance by even the staunchest skeptics.

Beams produced in free-jet expansions are characterized by the speed ratio S , which for atom beams is related to the relative velocity half width Δv by the relationship $S = 1.65 v/\Delta v$. For the target beam in his new apparatus, Faubel chose the new skimmers developed by Bossel (29). Whereas Bossel and others, including the Fenn group, had not achieved speed ratios much beyond 20, Faubel one day out of the blue proclaimed that there should be no problem in achieving the unheard of value of $S \geq 100$. Because he could neither measure this in his apparatus nor explain to any of us why this should be possible, we quickly dismissed his

idea as preposterous. But in the back of my mind the idea haunted me, and when a few years later I had to offer a research project to a new Diplom student, I suggested that he put together a small apparatus to measure speed ratios of nozzle beams of rare gases. I recall that on one of his visits John Fenn (who received the 2002 Noble Prize in Chemistry) was puzzled by our efforts because he felt that everything was known about jet expansions and surely this project was a waste of time. Klaus Winkelmann continued with his measurements, and much to our surprise he observed anomalously sharp velocity distributions in He expansions with speed ratios in excess of 100 (30). Fortunately, David Miller was visiting as a postdoc from the University of California, San Diego. Dave, who was very helpful in teaching me about surface scattering (an area in which he had then been active) also had brought a program for solving the Boltzmann equation in free-jet expansions. To calculate the transport integrals required as input, it was customary to use a classical scattering program, which was not available in our group. Instead, I suggested to Winkelmann that he use an available quantum-mechanical program, which turned out to be a serendipitous decision. At low energies below 10 K the quantum mechanical He-He cross section rises precipitously from about 30 \AA^2 to what is now estimated to be about 259.000 \AA^2 at $T \rightarrow 0$ (31), whereas the classical cross section is several orders of magnitude smaller. The large quantum cross section is caused by a quantum effect related to the extremely weak binding energy $\sim 1.1 \cdot 10^{-3} \text{ K}$ of the helium dimer. Recently, we have been able to measure the binding energy in an experiment in which the dimers are identified and analyzed by nondestructive diffraction from transmission nanogratings (32). Shortly afterward, Guido Brusdeylins (a member of our group) achieved $S = 550$ (G. Brusdeylins, unpublished data). Then a few years later, Ron Gentry, Clayton Giese, and their students achieved $S = 1000$ by using a pulsed nozzle and a 10-m-long flight tube (33). The discovery of high-speed-ratio He jet beams had a great impact on the further course of our research and was the beginning of our fascination with the many peculiar quantum-mechanical properties of this the simplest of all elements.

Several years later, with the high-speed-ratio He atom beam in another apparatus, which some claim is the largest of its kind, and using time-of-flight spectroscopy, Faubel was able to resolve the quantized rotational energy losses for collisions of He atoms with O_2 , N_2 , CO , and CH_4 (34). In our earlier experiments, we had resolved rotational transitions in H_2 using Li^+ -ions. The inelastic scattering experiments on the important ubiquitous molecules O_2 , N_2 , CO , and CH_4 are landmark experiments, which even with modern laser techniques have not yet been repeated or extended.

Time-of-Flight Spectroscopy of Surface Phonons

Winkelmann's experiments based on Faubel's hunch justified the construction of a new surface-scattering apparatus to measure inelastic scattering from surface phonons. Before the new machine was finished, I received a letter from an MIT graduate student who announced that he had applied for a scholarship to join our



Figure 3 Discussions with visiting theoreticians (*left to right*): J.P.T., Gabriel Balint-Kurti, Michael Baer, Don Kouri, Danko Bosanac, and Don Secrest in J.P.T.'s office in Göttingen (around 1981).

group. His research proposal described a gigantic surface-scattering machine with several quadrupole fields for state selection combined with time-of-flight capabilities and many more accessories. His proposal struck me as being so grandiose that I was not sure that he was serious and, certainly, he would not get the scholarship. But he did, and when he finally arrived, he turned out to be a polite, modest, reasonable young man. I explained to him that his experiment was far beyond even our capabilities and asked if he might settle for something simpler. Within a year, Bruce Doak (now a professor of physics at Arizona State University), who turned out to be a very hard-working student with considerable engineering skills, put together the first high-resolution high-speed-ratio He atom inelastic surface-scattering apparatus. This apparatus differed from all previous surface-scattering machines in two important respects: The mass spectrometer detector was no longer inside the target chamber but, to have sufficient time-of-flight resolution, was 1 m away from the target. To make up for the loss of signal, the He atom background was reduced by many orders of magnitude by inserting four differential pumping stages between the target and the detector. To make the apparatus simple, the angle between incident and outgoing beams was fixed, and only by rotating the crystal could the scattering angles be varied. Soon afterward, Bruce succeeded in measuring the first full-surface phonon-dispersion curve out to the zone boundary (35, 35a). Surface phonons differ from the well-known bulk phonons, which have been studied using neutrons since around 1940, because of the two-dimensional

boundary conditions imposed by the surface. Because the corresponding surface waves are localized in the outermost layers of a solid, the dispersion curves provide important information about the interatomic forces at the surfaces of single crystals. Several dozen machines have subsequently been built along the lines of the machine that Bruce put together. In our group, not only were we able to measure the surface phonon-dispersion curves of more than 100 different insulators, semiconductors, and metals, but we also observed a number of new phenomena such as low-frequency ($\hbar\omega \leq 10$ meV) adsorbate vibrations, not previously accessible (36), and the dynamics of diffusing atoms and molecules for a large number of adsorbate-surface combinations (37).

Helium Clusters

These successes prompted us to consider new applications of He atom beams. By reducing the source temperatures below the limitations of liquid N₂ cooling, we should be able to produce very slow cold beams that, because of the large increase in the He-He cross sections at low temperatures, might possibly exhibit even narrower velocity distributions. The problem was assigned to a new Diplom student, Rudy Minuth, whom I instructed to keep the source pressure sufficiently low to avoid cluster formation. Minuth, however, soon realized that only by raising the source pressure would the signal be intense enough to do any reasonable measurements. One evening when I was about to reprimand Minuth for using too high a source pressure, I noticed a highly structured time-of-flight spectrum on his oscilloscope screen. This unusual unexpected result fascinated me so much that instead of admonishing Minuth I congratulated him on his remarkable discovery!

This event, in fact, marked the start of our own program of He cluster research. Some years later, Adi Scheidemann agreed to a rather daring project that required rebuilding another apparatus to attempt to deflect the helium clusters by directed collisions with heavy rare-gas atoms such as Kr or Xe from a secondary beam. We had little idea what would happen in such collisions. Would the rare-gas atoms go through the helium clusters because of their superfluidity and come out at the other end as proposed earlier by E.W. Becker (38)? Or would they be merely reflected from the cluster, which might act as a quantum-coherent collective mass? The experiment was a success in that we discovered that the rare gases fulfilled neither of these two possibilities, but instead, were simply trapped in the interior of the clusters. Of course, then it became clear to us that since helium remains liquid down to 0 K the much heavier rare gas can plunge into helium clusters to become trapped by the strong collective potential exerted by the many helium atoms. The Ph.D. students Bernhard Schilling and Jan Harms were able to measure the angular distribution of the deflected clusters and determine both the mean sizes and establish that the sizes obeyed a log-normal distribution (39). Because these clusters were much larger than expected, with thousands of atoms, we call them droplets.

Even more importantly, this 1990 experiment revealed that molecules could be easily embedded in the interiors of these liquid droplets (40). Then it occurred to



Figure 4 Cartoon depicting the situation in the molecular-beam community in 1983. Drawn by J.P.T. as part of his closing summary of the 1983 International Molecular Beams Symposium in Freiburg.

us that perhaps the spectra of chromophore molecules might help us to gain more insight into the properties such as the temperatures of the helium droplets.

Molecular Spectroscopy in Superfluid Helium Droplets

Andrej Vilesov (now a professor of chemistry at the University of Southern California) arrived from Saint Petersburg in November 1990 with a postdoc fellowship from the Alexander von Humboldt Foundation to do spectroscopy in Ottinger's research group within our department. Andrej attended the seminars on He droplet research and around 1992, stimulated by the success of the Scoles group in observing sharp infrared absorption lines of SF₆ in helium droplets (41), started to work part-time on a new attempt in our department to perform infrared spectroscopy in helium droplets. At that time, our colleagues Udo Buck and Rudolf Dören had kindly lent us both a line-tunable CO₂ laser and a continuously tunable lead-salt diode laser.

In early August 1993, a young postdoc Ralf Fröchtenicht and Andrej were finally ready for their first experiment. Prior to his departure a few days later for summer holidays in Russia, Andrej came to my office on a Friday afternoon to ask me which of the two lasers to use first. Recalling advice received from Norman Ramsey in 1957, I told him that, of course, he should use the CO₂ laser since he could then rely on Scoles's results. I also emphasized the well-known disadvantages of diode lasers that are attributed to the uncertainties in the actual frequencies within their narrow tuning ranges and their very low powers.

A few days later, Andrej informed me that he had, in fact, not heeded my advice and that with the diode laser he had found a series of narrow lines in a spectral region different from that reported by the Scoles group. Today we know that his successful gamble opened up an entirely new field of matrix spectroscopy! These, and subsequent experiments, with the narrow line-width diode lasers revealed the resolved P-, Q-, and R-branches indicating that the molecules rotate freely inside the helium clusters (42). This remarkable phenomenon of free rotations is totally unexpected for an ordinary liquid. Some years later, in 1998, our group could provide evidence that free rotations were a microscopic manifestation of superfluidity, which we called molecular superfluidity (43). Of course, Andrej was very fortunate that his particular diode happened to operate in just the right spectral region and, of course, that the rotational lines with a line width of only 10^{-2} cm^{-1} were sufficiently narrow to make full use of the high spectral brilliance of the diode laser. We were, of course, also lucky to have had a diode laser available. I'm sure no agency would have funded such an experiment for which there was absolutely no a priori evidence nor theoretical prediction that the lines would be nearly as sharp as those of a free molecule.

Today there are over 25 groups worldwide who have switched from using seeded beams to helium droplets as matrices for high-resolution spectroscopy (44, 44a). The temperatures are generally much lower, either 0.37 K (⁴He droplets) or 0.15 K (mixed ⁴He/³He droplets) and apply to all degrees of freedom including the vibrations of large molecules. Most importantly, it is possible to create new



Figure 5 Photo taken on the occasion of my 60th birthday showing (*back row*) Bruce Doak, Christoph Ottinger, Ekkehard Hulpke, Dudley Herschbach, Ned Greene, Hans Pauly, Hans Gerhard Bennewitz, Christoph Wöll, and Yuan Lee; (*front row*) Wolfgang Paul, Susanne Toennies, J.P.T., Annette Toennies, Monika Toennies, and Heinz Georg Wagner.

van der Waals complexes with unique compositions or structures. Also, the new phenomenon of molecular superfluidity provides new insight into the microscopic foundations of superfluidity, and recently the new method made it possible to find superfluidity in another system, para-hydrogen (45).

Manipulating Matter Waves with Nanostructures

Prior to these experiments, a new student, Wieland Schöllkopf, had been assigned a project to develop a tunable laser for He droplet spectroscopy. But while I was lecturing in the United States in the fall of 1993, he decided on his own to do something else. By chance he had heard about an idea of three to four years earlier to use nanostructured transmission gratings to diffract atoms. My students, at the time, didn't think much of my idea, quite rightly pointing out that the diffraction of atoms from single crystal surfaces was quite common in our group and had a long history going back to 1929 when Stern and collaborators determined the de Broglie wavelengths of H_2 and He by diffracting them from LiF single crystal surfaces. I could only allay their critical stance when I proposed that in this way we could detect the helium dimer, the existence of which was still controversial. All the parts were available including a gold transmission grating with 500 nm period, which had been purchased from a German company. Thus, during my prolonged absence, Schöllkopf had sufficient time to try out the experiment. One can well imagine my surprise when, on my return, Schöllkopf confessed that he had decided to abandon the laser project. Even worse, Schöllkopf had to admit that he hadn't seen any diffraction and that, in fact, he couldn't even get the atoms to pass through the grating. A few weeks later, the grating had been cleaned and we saw our first diffraction pattern and, within a month or two, the diffraction of helium dimers, which provided the first unequivocal evidence for their existence (46). On hindsight, without Schöllkopf's initiative I'm not sure if we would have ever got around to starting this type of research. Thanks also to Hank Smith and his capable Ph.D. student, Tim Savas at MIT, who have provided us with excellent 100-nm period gratings, these experiments have since flourished. In close collaboration with the theoretician Gerhard Hegerfeldt and his students, Thorsten Köhler, Martin Stoll, and Christian Walter at the local university, we were later able to determine the binding energy of the dimer (32) and carry out the first measurements of atom/molecule-surface van der Waals forces for the rare gases and several molecules (47). With Rüdiger Brühl, we were able to commission a three-grating Mach-Zehnder interferometer. We call it a universal matter wave interferometer because it is the first one not restricted to alkali atoms. Also, we have succeeded in focusing an atom beam to a 1 micron spot with a transmission Fresnel zone plate (48). With a greatly improved detector, recently developed, we are now in the position to construct a helium atom microscope. At the present time, this line of research is continuing with two excellent Russian students, Anton Kalinin and Oleg Kornilov. Their work has very recently led to the unexpected discovery of magic numbers in ^4He clusters with up to about 80 atoms (R. Brühl,

A. Kalinin, O. Kornilov, J.P. Toennies, R. Guardiola, & J. Navarro, unpublished manuscript).

By now, the reader will certainly have noted that all these stories involve helium. In fact, we have, over the years, become increasingly enraptured with helium and its unique properties. This has prompted us to write, with Bruce Doak's assistance, the following ode to helium on the occasion of being named an Honorary Fellow of the International Molecular Beam Symposium series in 1999.

Marvelous Mysteriously Magic Helium

This little atom helium
is cause of great delirium.

When one predicts what it will do,
one often finds, one has no clue.

At cryogenic temperature,
where other atoms cease to stir,
helium alone does not stagnate,
but forms a superfluid state.

If in this enigmatic juice
a spinning molecule's let loose,
instead of slowing naturally,
it keeps on twirling merrily.

In supersonic expansion,
it has a mammoth cross section,
and it cools down to just a smidge:
a streaming supersonic fridge.

The dimer is so weakly bound,
that it has only just been found.
Diffraction gratings were the key—
wave-particle duality!

Though superfluid helium
was studied *ad absurdum*,
by Landau, London, Yang, and Lee,
their theory functions miserably.

All this is but a partial file,
of helium's capricious wile,
but serves as ample overview,
and so I now propose to you:

Let's toast our noble helium,
a marvelous continuum,
to sow its pandemonium,
into the next millennium.

The above stories illustrate some of the many similar adventures involving a large number of the more than 200 Diplom- and Ph.D. students who have passed through our group. Each illustrates the spontaneity and vitality coming from the close involvement of young students and postdocs in the research process. Being relatively unburdened by too much experience in their research activities, there was plenty of leeway for serendipity to come into play. These stories also illustrate the great flexibility and freedom that we enjoyed and was made possible by adequate funding without the usual delays and risks incurred by grant applications and peer review. The lesson I learned was that good science cannot be planned and, moreover, good science can blossom best in a humus of generous support coupled with freedom of ideas and doing.

Soon after I joined the Max Planck Society I heard a lecture by Fred Dainton (then in Cambridge, later professor of physical chemistry at Oxford) where he cited Alfred North Whitehead from his 1929 essays entitled *The Aims of Education* (50): "The primary reason for the university's existence is not to be found either in the mere knowledge conveyed to the students, or in the mere opportunities for research afforded to the members of the Faculty The justification of a university is that it preserves the connection between knowledge and a zest for life by uniting the young and the old in the imaginative consideration of learning Youth is imaginative, and if the imagination is strengthened by discipline this energy of imagination can in great measure be preserved through life. The tragedy of the world is that those who are imaginative have but slight experience, and those who are experienced have feeble imagination. Fools act on imagination without knowledge, pedants act on knowledge without imagination. The task of the university is to weld together

imagination and experience.” Our experience, as I have attempted to relate above, bears out the wisdom of these words.

BENEVOLENT CONSTRUCTIVE PEER REVIEW

Of course, we did in fact have the benefit of something similar to peer review, albeit of a more benevolent, constructive nature, in the form of a constant influx of guest scientists of all ages from all over the world. Those projects, which attracted numerous guests, who in many cases brought the necessary theoretical expertise to analyze the new experimental results, tended to flourish; whereas other experiments, which did not attract active outside participants gradually withered away. In this manner, the guests helped to provide guidance to the research direction and, of course, through their involvement, to the success of the various projects on which they worked. A few stories involving guests will serve as examples to illustrate their important influence and great impact.

Intermolecular Potentials

About 1974, K.T. Tang inquired at several places about possibilities of a sabbatical visit and, as he confided to me later, I happened to be the first to respond. In 1975, he came to Göttingen for the first time for a one-year sabbatical. Several years earlier, I had published a simple paper in which we had pointed out that a modified Buckingham van der Waals potential model given by $V(R) = A \exp(-bR) - \frac{C_6}{R^6} - \frac{C_8}{R^8} - \frac{C_{10}}{R^{10}}$ could, with recently calculated values of the corresponding 5 parameters, explain the potentials for the He, Ne, Ar, and Kr dimers that had been rather precisely determined by Yuan Lee’s experiments of 1970 and multiproperty analysis (51). Shortly after Tang’s arrival, Giacinto Scoles visited and gave a seminar in which he described an improvement on my potential model. Tang immediately realized that all these and earlier models did not take proper account of the asymptotically divergent nature of the dispersion expansion. Using a reliable approximation for the best estimate for the dispersion, we were able to come up with a new model potential. It was very gratifying to see how this correction led to an improvement in the agreement with experiments. As more experimental and theoretical potential parameters and ab initio calculated potentials became available, we continually improved our potential models. Inspired by a 1981 article by Koide et al., we eventually found a more elegant expression for a damping function for the dispersion series. In March 1983, we submitted a long paper, describing how, with this model, it was possible to predict the interaction potentials for not only the rare gases, but also $H_2(^3\Sigma)$, $NaK(^3\Sigma)$, and $LiHg$ complexes. We were very proud of our paper, but the referees were disdainful. One referee wrote that “simple models were no longer needed in view of recent advances in ab initio theory, etc.” Our second submission was also rejected by two referees. One wrote, “This paper is so poorly written, that he is certain Peter Toennies has not even

read it.” That was quite a shock for me since I had written it! Finally, in the third round, after a long persuasive letter to the editor one referee conceded, “the test of the pudding is in the taste. Since it worked in so many cases, to his taste it should be published,” and our article was finally accepted in December 1983 (52). Before that time and since, I have not had to fight so hard to get a paper published. However, it was worth the effort since with 540 citations, it is my (and Tang’s) most cited article.

With Tang and his Pacific Lutheran colleague, Chang Li Yiu, we later had much fun in developing the surface integral method into a useful tool for understanding, in simple physical terms, the role of exchange in atom-atom interactions (53, 54). Out of this work we were able to propose a simple perturbation theory potential model for the He-dimer (55) that agrees perfectly with Jim Anderson’s recent no-approximations brute-force Monte Carlo calculations (56).

Surface Interactions

In 1973, I attended an Enrico Fermi Summer School in the romantic little village of Varenna on Lake Como in Northern Italy. I had hoped to learn about surface phonons, but then I was a bit discouraged by a lecture on the subject presented by a young Italian who confronted the audience with more than 100 formulas in the course of his first lecture. Seven years later, when Bruce Doak had measured the first surface phonon dispersion curve out to the zone boundary, we realized the urgent need for theoretical guidance. The two world experts at the time were Fritz de Wette (Austin, Texas) and that same young Italian, Giorgio Benedek (now professor of physics at the University of Milan-Bicocca). With some trepidation we decided to invite Giorgio Benedek to Göttingen for a visit. Much to our delight, we quickly discovered that Giorgio could, in fact, also explain the theoretical concepts with only a few equations and in such a way that we could understand them. Since then, we have collaborated on over 46 publications, mostly in the area of surface physics. Through him we developed contacts to Vittorio Celli (University of Virginia), Dick Manson (Clemson University), Virginio (BiBi) Bortolani (Modena, Italy), and Giorgio Santoro (Modena, Italy), all of whom have greatly contributed to our understanding of surface phonons and energy transfer in gas-surface collisions.

Giorgio Benedek, being a particularly talented all-around theoretician, also provided the theory that enabled Andrej Vilesov and myself to interpret some spectral features of glyoxal molecules in He droplets as evidence for their superfluidity (57). Most recently, Giorgio has provided the theory for a new phenomenon, called the Geyser effect, in the expansions of solid ^4He into vacuum (R. Grisenti, J.P. Toennies, G. Benedek & F. Dalfovo, unpublished manuscript). Surprisingly solid ^4He , although it has a well-defined crystal structure, flows through the orifice at the same rate as liquid helium, but exhibits regular oscillations with a period in the range

of minutes. This new effect may provide insight into supersolid behavior, another bizarre quantum phenomenon about which there are only theoretical speculations at present.

Other Guests

These visits and those of many other guests were funded from a variety of sources; the most prestigious funding was provided by the Alexander von Humboldt Senior Award (Prize) program that financed year-long visits of Jim Anderson (USA), Michael Baer (Israel), Paul Bagus (USA), Giorgio Benedek (Italy), Vittorio Celli (USA), John Fenn (USA), Franco Gianturco (Italy), Robert Gomer (USA), Zdenek Hermann (Czech Republic), Eldon Knuth (USA), Don Kouri (USA), Dick Manson (USA), David Micha (USA), Roger Miller (USA), Earle Muschlitz (USA), Russel Pack (USA), Dick Porter (USA), Don Secrest (USA), Kwong-Tin Tang (USA), Bob Watts (USA), and Birgitta Whaley (USA). The Alexander von Humboldt Society also gave postdoctoral fellowships to Serge Alimpiew (Russia), Jaqueline Appell (France), Walter Beck (Australia), Pascal Brault (France), John Close (Australia), Peter Day (USA), Alan Dickinson (Great Britain), Bill Dimpfl (USA), Michal Farnik (Czech Republic), Jost Frenken (The Netherlands), Bratislav Friederich (Czech Republic), Roger Grice (Great Britain), Victor Herrero (Spain), Jane Hinch (Great Britain), Bodil Holst (Denmark), Andrzej Kowalski (Poland), Isao Kusunoki (Japan), Angela Lahee (Great Britain), Shu-Tao Li (China), Jeong-Long Liu (USA), Julian Lower (Australia), Salvador Miret-Artes (Spain), Paolo Ruggerone (Italy), Naraya Sathyamurthy (India), Hubert Van den Berg (Great Britain), and Andrej Vilesov (Russia). Additional funds were also generously provided by the Max Planck Society that enabled us to carry out long-term collaborations with a number of the above and many other scientists.

OUR DESCENDANTS AND HEIRS

In summary, playing with molecular beams has been, and still is, full of surprises. At age 73, I am having a lot of fun and enjoying several new adventures with three talented students and several colleagues all over the world. Working with students and young people has, in the past and especially now, provided lots of stimulus not only for our research, as emphasized above, but also for helping to keep ourselves young, especially in spirit.

As I have matured, I have noticed that one of the rewards of a long career in science is the perspective gained from having experienced and taken part in the evolution and development in a field of human endeavor, even if it has only been in a rather narrow area. For example, it is gratifying to be able to recall how little we knew about intermolecular interactions 50 years ago and compare that with what is known today. Based on this vantage point, it is indeed tempting to make predictions for the future. But, as illustrated by my stories, this is

indeed a very dangerous undertaking. Rather, the future will be determined by our many students who have chosen to continue in research either in industry or at universities and have matured further as scientists after leaving our laboratory. We are happy that, as a result of our changes in interests and my official retirement in 1998, we could pass on much of our equipment to friends and former students. Now we find great pleasure and pride in seeing how many of the research areas in which we have been involved have developed further in the hands of many capable young scientists, some of whom have passed through our laboratory.

EPILOGUE

In view of my experiences described above, I cannot resist joining in with the few colleagues who have voiced criticism of the commonly encountered overly bureaucratic system of reviewing research grants. Present funding programs in many countries, and now in recent years in the European Community, appear to be too funneled into areas highlighted by consensus views of what should be funded. Thus a proposal centered around one of the highly popularized instruments—as the laser was in earlier days or, in recent years, an STM or an AFM—especially in a high-profile research area, such as nanotechnology or molecular nanobiology, has a much greater chance of being funded than equally good proposals in largely unknown areas. Certainly it is true that new instruments open new perspectives, but funding agencies also must take on the responsibility to germinate novel developments in unexplored areas. I even wish to go so far as to propose that especially new and original proposals cannot and should not be subjected to peer review because in such cases how can one be certain that the reviewers are truly peers? Rather, reviews should only be based on the applicant's past performance (59). Even this is dangerous since many great discoveries are only recognized much later (60). The only solution, in my opinion, is to reserve substantial funding for off-beat projects for gifted outsiders and to provide them with an environment where there is plenty of room for fruitful serendipity (61). Perhaps the truly ideal solution was provided by the Max-Planck-Institute system in the earlier times described here. Unfortunately, the Max Planck concept has not found many followers outside Germany. In my opinion, our world society certainly would benefit greatly by providing more room for scientists to find and follow their own paths without undue consensus pressures.

ACKNOWLEDGMENTS

I am grateful to K. Fischer (University of Kaiserslautern), W. Marx, and H. Schier (Max-Planck-Institut für Festkörperforschung) for discussions and for providing references on science funding policy issues. Also, I wish to thank H.G. Bennewitz, R. Düren, E. Hulpke, E. Knuth, and Ch. Ottinger for comments, and Ch. Schneider-Rogier for meticulous care in preparing the manuscript.

The Annual Review of Physical Chemistry is online at
<http://physchem.annualreviews.org>

LITERATURE CITED

1. Toennies JP, Wagner HG. 1955. *Z. Elektrochem.* 59:7–15
2. Greene EF, Toennies JP. 1959. *Chemische Reaktionen in Stosswellen*. Darmstadt, Ger.: Steinkopf. 202 pp.
3. Greene EF, Toennies JP. 1964. *Chemical Reactions in Shock Waves*. London: Arnold. 368 pp.
4. Toennies JP, Greene EF. 1955. *J. Chem. Phys.* 23:1366
- 4a. Toennies JP, Greene EF. 1957. *J. Chem. Phys.* 26:655–62
5. Amdur I, Pearlman H. 1940. *J. Chem. Phys.* 8:7–12
6. Estermann I. 1959. In *Research in Molecular Beams*, ed. I Estermann, pp. i–vii. New York: Academic
7. Kantrowitz A, Grey J. 1951. *Rev. Sci. Instrum.* 22:328–32
8. Kistiakowsky GB, Slichter WP. 1951. *Rev. Sci. Instrum.* 22:333–39
9. Deleted in proof
10. Taylor EH, Datz S. 1955. *J. Chem. Phys.* 23:1711–18
11. Bennewitz HG, Paul W, Schlier Ch. 1955. *Z. Phys.* 141:6–15
12. Ramsey N. 1955. *Phys. Rev.* 98:1853–54
13. Bennewitz HG, Kramer KH, Paul W, Toennies JP. 1964. *Z. Phys.* 177:84–110
14. Jawtusch W. 1952. *Z. Phys.* 133:541–57
15. Becker EW, Bier K, Henkes W. 1956. *Z. Phys.* 146:333–38
16. Pauly H. 1960. *Z. Naturforsch. Teil A* 15:277–78
17. Paul W, Steinwedel H. 1953. *Z. Naturforsch. Teil A* 8:448–50
18. Toennies JP. 1962. *Discuss. Faraday Soc.* 33:96–98
- 18a. Toennies JP. 1965. *Z. Phys.* 182:257–77
19. Erlewein W, von Seggern M, Toennies JP. 1968. *Z. Phys.* 211:35–50
20. von Seggern M, Toennies JP. 1969. *Z. Phys.* 218:341–51
21. Grice R, Mosch JE, Safron SA, Toennies JP. 1970. *J. Chem. Phys.* 53:3376–78
22. Born M, Bormann E. 1920. *Phys. Z.* 21:578–82
23. Born M. 1975. *Mein Leben*, pp. 269–70. Munich, Ger.: Nymphenburger. 400 pp. Engl. transl. 1978. *My Life: Recollections of a Nobel Laureate*. New York: Scribner
24. Herschbach D. 1973. *Discuss. Faraday Soc.* 55:233–51
25. Pauly H, Toennies JP. 1965. *Advances in Atomic and Molecular Physics*, ed. DR Bates, I Estermann, 1:195–344. New York: Academic
26. Probst FM, Piper TC. 1967. *J. Vac. Sci. Technol.* 9:53–56
27. Hulpke E. 1975. *Surf. Sci.* 52:615–40
28. Glenewinkel-Meyer Th, Kowalski A, Müller B, Ottinger Ch, Rabenda D, et al. 1992. In *Electronic and Atomic Collisions*, ed. WR MacGillivray, IE McCarthy, MC Standage, pp. 459–64. Bristol: Hilger
29. Bossel U, Hurlblut FC, Sherman FS. 1969. In *Symposium of Rarefied Gas Dynamics, 6th*, ed. L Trilling, HY Wachman, 2:945–64. New York: Academic
30. Toennies JP, Winkelmann K. 1977. *J. Chem. Phys.* 66:3965–79
31. Luo F, Kim G, McBane GC, Giese CF, Gentry WR. 1993. *J. Chem. Phys.* 98:9687–90
32. Grisenti RE, Schöllkopf W, Toennies JP, Hegerfeldt GC, Köhler T, Stoll M. 2000. *Phys. Rev. Lett.* 85:2284–87
33. Wang J, Shamamian VA, Thomas BR, Wilkinson JM, Riley J, et al. 1988. *Phys. Rev. Lett.* 60:696–99

-
34. Faubel M, Kohl KH, Toennies JP. 1980. *J. Chem. Phys.* 73:2506–7
35. Brusdeylins G, Doak RB, Toennies JP. 1980. *Phys. Rev. Lett.* 44:1417–20
- 35a. Brusdeylins G, Doak RB, Toennies JP. 1981. *Phys. Rev. Lett.* 46:437–39
36. Hofmann F, Toennies JP. 1966. *Chem. Rev.* 96:1307–26
37. Graham AP, Hofmann F, Toennies JP, Chen LY, Ying SC. 1997. *Phys. Rev. B* 56:10567–78
38. Becker EW. 1986. *Z. Phys. D* 3:101–7
39. Lewerenz M, Schilling B, Toennies JP. 1993. *Chem. Phys. Lett.* 206:381–87
40. Scheidemann A, Toennies JP, Northby JA. 1990. *Phys. Rev. Lett.* 64:1899–1902
41. Goyal S, Schutt DL, Scoles G. 1992. *Phys. Rev. Lett.* 69:933–36
42. Fröchtenicht R, Toennies JP, Vilesov A. 1994. *Chem. Phys. Lett.* 229:1–7
43. Grebenev S, Toennies JP, Vilesov AF. 1998. *Science* 279:2083–86
44. Toennies JP, Vilesov AF. 1998. *Annu. Rev. Phys. Chem.* 49:1–41
- 44a. Toennies JP, Vilesov AF, Whaley KB. 2001. *Phys. Today* 54:31–37
45. Grebenev S, Sartakov B, Toennies JP, Vilesov AF. 2000. *Science* 289:1532–35
46. Schöllkopf W, Toennies JP. 1994. *Science* 266:1345–48
47. Grisenti RE, Schöllkopf W, Toennies JP, Hegerfeldt GC, Köhler T. 1999. *Phys. Rev. Lett.* 83:1755–58
48. Doak RB, Grisenti RE, Rehbein S, Schmahl G, Toennies JP, Wöll Ch. 1999. *Phys. Rev. Lett.* 83:4229–32
49. Deleted in proof
50. Whitehead AN. 1929. Universities and their function. In *The Aims of Education and Other Essays*, 7:136–52. London: Williams & Northgate
51. Toennies JP. 1973. *Chem. Phys. Lett.* 20:238–41
52. Tang KT, Toennies JP. 1984. *J. Chem. Phys.* 80:3726–41
53. Tang KT, Toennies JP, Yiu CL. 1998. *Int. Rev. Phys. Chem.* 17:363–406
54. Tang KT, Toennies JP, Yiu CL. 2001. *J. Chin. Chem. Soc.* 48:365–69
55. Tang KT, Toennies JP, Yiu CL. 1995. *Phys. Rev. Lett.* 74:1546–49
56. Anderson JB. 2001. *J. Chem. Phys.* 115:4546–48
57. Hartmann M, Mielke F, Toennies JP, Vilesov AF, Benedek G. 1996. *Phys. Rev. Lett.* 76:4560–63
58. Deleted in proof
59. Abrams PA. 1991. *Soc. Stud. Sci.* 21:111–32
60. Garfield E. 1989. *Curr. Contents* 23:3–9; 38:3–8; 49:3–8
61. Campanario JM. 1996. *Scientometrics* 37:2–24