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Autobiography of Bretislav Friedrich

Family background

I was born in Prague, now in the Czech Republic, on 29 May 1953, a single child of Bretislav and Sylva Friedrich. My paternal grandfather, Frantisek, came from a family that had lived for generations in a small village at the Bohemian side of the Austrian–Prussian border. He was able to graduate from high school one year ahead of time – because he was gifted and because he wanted to lessen the financial burden on his family. Upon his graduation in 1909 he was hired as a technical clerk by the Carborundum Company,[1] where he rose quickly through the ranks. His portfolio soon included one of Carborundum's main preoccupations, namely the development of abrasives and grinding tools – made not only from silicon carbide (carborundum), but also from aluminium oxide (artificial corundum), a pioneering feat which revolutionised the machining of glass worldwide while simultaneously eliminating the hazards of silicosis (grinder's asthma). He also founded a research library at Carborundum, a rarity at industrial companies of the time. Frantisek's relationship with the Czech communists or, since 1929, their Soviet sponsors[2] was not a congenial one: his spirit of a self-made man did not square well with the collectivist mentality of the communists. During a business trip to the Soviet Union in 1936, he was put under pressure by his hosts to reveal proprietary information about Carborundum's products and procedures. His brewing conflict with the communists came to a head in the Fall of 1945 when Carborundum was taken over by a communist leadership and my grandfather was forcefully retired – without (almost) any retirement benefits. My grandmother then took the only available job of a menial worker at a state farm and supported the family until the early 1960s when her husband was politically rehabilitated and his retirement benefits restored. My father, after his graduation from high school in 1940, found at first a refuge at Carborundum, but in 1943 was seized by the Nazis as a forced labourer[3] and deployed to the Wälzlager factory in Steyer, Austria. In April 1945 he fled Steyer and illegally returned to Bohemia. After the reopening of Czechoslovak universities he matriculated at Prague's Institute of Chemical Technology but, in the wake of the firing of his father, dropped out and took a job instead. My mother grew up after the death of her mother in the family of her mother's sister. During the Nazi occupation of the country she too was drafted as a slave labourer, folding parachutes for the Luftwaffe at the Prague airport. She got to know her father, Josef Fleischl, only after his return, in 1945, from a concentration camp. My grandfather Josef had a legal training and helped to push ahead with the rehabilitation of my grandfather Frantisek.

The greatest family influence has been my father's sister, Jitka, who had also crossed illegally the Czech–Austrian border, in 1949, but in the direction opposite to that followed by my father before her. Self-educated, self-reliant and self-less, she has been my confidant and an inspiring mentor throughout my life. It was also she who encouraged me to go to the Jan Neruda High School and later to study at the Faculty of Science of Charles University.

High school and college

To apply for admission at the Neruda High School (Gymnasium) was the first important decision that I made – I was about 14 years then. Located next to Kampa in Prague's Lesser Town (Mala Strana), it enjoyed the reputation of one of the best schools in town. I made the decision to go to the Neruda Gymnasium jointly with my childhood friend, Jiri Dedecek.[4] We were motivated in part by our shared desire to get to know the city and to inhabit it, so to speak. Over the next three years we would take a street car[5] to Mala Strana, which was enjoyable in its own right, as other classmates were joining the ride – and our boisterous conversations. These would range from literature to disputations about art to philosophising, usually performed in French, since big words are more easily uttered when they are foreign. We would not waste too much time inventing mischief but would rather incessantly do it. Although the school had some appealing teachers, it had equally or even more appealing students. Among them was our classmate, Adam Hoffmeister,[6] with whom Jiri and I formed a trio of inseparable friends. At that time, we would mainly act as a comic trio, entertaining our fellow classmates and ourselves by teasing our teachers and, of course, poking fun at everything else that moved.

Since I am vintage 1953, our first school year at the high school was due to start in September 1968. However, the beginning of the school year as well as everything else was overshadowed – and not just in Czechoslovakia – by the Soviet-led invasion of the country, on 21 August 1968. The Praguers, in a witty act of spontaneous resistance, had removed all street signs, so that it was impossible to navigate through the city unless you knew it. The occupying armies got stuck as a result, whereby David defeated Goliath, but only for a sweet, fleeting moment. The Soviet troops would stay in the country for the next 21 years, backing a regime that would make Czechoslovakia into a 'Biafra of the spirit'.[7] The implementation of the new, anti-reform regime[8] did not proceed everywhere at the same pace, however. Our high-school class maintained the spirit of resistance essentially until our graduation in 1971, collectively withholding much of its kindness from teachers who decided to conform to the new circumstances. The fact of the matter is that we showed little concern even for excellent, non-conformist teachers, hiding all too well our affection for them. I was especially fond of our resourceful class teacher, Vera Vetrovcova, who taught physics, Marie Kubyova, a chemistry teacher with a caustic humour and a research experience, and Helena Ciprova, an exquisitely cultured teacher of Russian language and literature, whose instruction we nearly boycotted during the first year or two. Our decadence was not unlike that featured in the cult works of the beatnik/hippie generations. It came to us pretty independently though, and may have reflected our experience of living in a repressed society.

Upon graduating from our political refuge and entering university, we suddenly found ourselves surrounded by fellow students who had adapted to the new political circumstances to the point that they were actually creating them. Apparently, it was quite common at the time that university students spoke the language of party apparatchiks, as if they had forgotten – or had never known – the humorous language characteristic of young people. This was also my (shocking) personal experience after I became an undergraduate student at the Faculty of Science of Charles University in Prague, in October 1971. A notable exception was, characteristically, my former high-school classmate, Zuzana Hostomska.[9]

Getting in was not entirely easy – one had to pass an admission exam which included a personal interview with the faculty. There was little political coercion at that stage, although applicants were screened for any politically hostile activity of their own or of their ancestors. I was able to pass unscathed. Apparently, the sins of my grandfather Frantisek had been forgotten – to his great relief. The political screening at Prague's Film Academy or at the Philosophical Faculty would have been much more severe, as would have been the requirements for political conformity upon admission. I was well aware of this when I was making up my mind about what to study. In the end, I concluded that science will be relatively free of politics and that I better suppress my humanistic inclinations. In hindsight, I'm very happy that I had embraced science instead of, say, film-making or history of art. Apparently I should be grateful for my choice in part to Czechoslovakia's communist rulers . . .

The brightest light at the Faculty of Science were the faculty members. Not all of them, but many if not most. Anyway, I was quite blind to any defects they may have had – political or other – for I was so humbled and impressed by what they knew and could talk about, often without notes, in their lectures. Jiri Stepanek's series of courses on 'mathematics for scientists' was a true cultural experience for me, conveyed in the spirit of the maxim that 'scientists/physicists must know at least as much math as mathematicians'. I still have his lecture notes and consult them occasionally. The presentation of the various equivalent forms of classical mechanics by Vaclav Frei introduced me to the pleasures of 'consistency' and 'deep understanding' which is possible in physical sciences. Jarmila Dlouha's pragmatic approach to quantum mechanics liberated me from thinking too hard about its venerable conceptual puzzles and helped me to concentrate on learning its nuts and bolts instead. Partly led by my inclinations that I had discovered while attending the mentioned courses, I picked physical chemistry – the 'chemistry of the future'[10] – as my specialisation. The physical chemistry department was headed by Jiri Dvorak, an excellent teacher and author of widely used textbooks on physical chemistry and electrochemistry. And a gentleman. And a communist. One of very few I have known where the phrase 'decent communist' did not sound like a contradiction in terms. I took two courses from him: thermodynamics, providing similar intellectual and aesthetic gratifications as Frei's course on analytical mechanics, and a course on chemical kinetics, a much messier but not less exciting subject. My fifth year in college was dedicated to producing a 'diploma' thesis. My advisor was the polymer physical chemist Karel Prochazka, a young assistant professor who was a member of Dvorak's department. Thanks to Jiri and Karel, I was bitten by the bug of polymer science and started working my way into the statistical mechanics of macromolecular solutions. My diploma thesis dealt with deviations from the ideal behaviour of dilute macromolecular solutions in mixed solvents, as revealed by light scattering, osmometry and viscosimetry. The virial coefficients characterising the deviations could be tuned by varying the solvent's mixing ratio [1].[11] Towards the end of my stint as a polymer physical chemist, Jiri Dvorak took me aside and told me that he has 'got a job for me'. I had heard about this 'job' earlier from him and from others: it was a PhD student position in the Department of Mass Spectrometry at the Heyrovsky Institute of the Czechoslovak Academy of Sciences. I was flabbergasted when it turned out that Dvorak intended to recommend me for this position.

Graduate student and staff scientist at the Heyrovsky Institute

The department of mass spectrometry was headed by Vladimir Hanus, one of the founders of the field of organic mass spectrometry and the first internationally known scientist I had met in the flesh. On Dvorak's suggestion, Hanus invited me to the department to meet Vladimir Cermak, who was looking for a graduate student. Cermak was another internationally renowned scientist, known mainly for his pioneering work on ion-molecule chemistry and for developing Penning ionisation electron spectroscopy, a technique whose foundations he had laid in Prague but which he perfected in the 1960s while visiting the Joint Institute for Laboratory Astrophysics (JILA) in Boulder, Colorado. Cermak was an elegant, cultured man, with a predilection for things French. To him science was as much a part of culture as art. Another eminent member of the department was Cermak's former pupil and coworker Zdenek Herman. Zdenek pioneered the study of ion–molecule reactions by the crossed-beam technique, which he had developed in the 1960s at Yale University and later transplanted to Prague. He is, in addition to being a scientist, an accomplished artist, whose drawings have characterised the life at the Heyrovsky Institute with an accuracy that often exceeded that of our best scientific measurements.[12] The department further comprised Hanus' brilliant coworker and former student Franta (Frank) Turecek, the mass spectrometrist Zdenek Dolejsek, and finally the electrical engineers Miroslav Pacak and Ladislav Hladek. The last were making instruments for our experiments that our colleagues in the 'West' could generally buy commercially. However, unlike the commercial products, Pacak and Hladek's instruments had a soul, in the sense that they were meticulously tailored to fit the needs of our experiments. And they served us very faithfully.

To be admitted to the Academy of Sciences was, however, not as straightforward as getting into college. As Hanus explained to me, the Academy of Sciences would not even look at applications from people who were not members of the 'Socialist Youth Union', a sort of kindercommunist party, looked after by the communist party for adults. So, I said to him that that will be a problem, because I don't get along very well with the members of the youth union at the university who would have to admit me as a new member first. Furthermore, the current members would be mad at me, because they had to waste their time attending meetings and such of the union during their five years of college whereas I had not to. However, in the end, I was able to persuade someone well-positioned at the union, who helped me to get in. Curiously, my admission happened at the very last meeting of our class' chapter of the union whose sole purpose was to dissolve itself because our class was graduating . . .

Upon passing an admission exam (similar to the finals at college), administered by the Heyrovsky Institute, I was able to join the mass spectrometry department and start working with Vladimir Cermak. However, his health was deteriorating rapidly (he had Parkinson's disease) and so after a while he suggested to me that I transfer to Zdenek Herman. Zdenek kindly accepted me and under his auspices I wrote my PhD thesis, in 1981, on the dynamics of the elementary chemical reaction between electronically excited boron ions and hydrogen molecules, studied by the crossed beam technique [4]. For a long time I was the only graduate student in the department. Perhaps it is worth mentioning that as a graduate student I was taking care of the 'international relations' of the Institute's youth union. These consisted of organising mutual visits and scientific symposia with our 'sister' organisation at the East German Academy of Sciences in Berlin. My counterpart there was Angela Merkel.

The Heyrovsky Institute (in Machova Street) where, after graduation, I became a staff scientist, was quite a special place, endowed with a *genius loci*. The Institute's building was an adapted former nunnery, whose chapel was converted into our science library. Either in the library or in the building's large central stairwell everybody was meeting everybody all the time. Among the people to meet were quite a few star scientists in residence at Machova: apart from Cermak, Hanus and Herman, there were Rudolf Zahradnik, Emerich Erdös, Zlatko Knor, as well as outstanding young people – Franta Turecek, Stepan Pick, Petr Carsky, Jirka Pancir and others. The atmosphere was cordial – there was a lot of laughter – and of solidarity. The identities of the communist party members, or, worse, agents of the secret police, were quite well known to everybody and I would say that for most of the time they served the rest of us as a laughing stock. Our academic freedom was mainly limited by modest funding and to a lesser extent by bureaucratic restrictions. Despite the presence – and warnings – of a permanently stationed special agent who kept reassuring us that the Institute was under a constant siege by Western spies who wanted to steal our results, we were able to publish our papers essentially in any international journal (as long as they passed the journals' peer review. . .).

In the Cermak–Herman laboratory, there was a constant stream of visitors from all over the world, who came to compare notes on their research and sometimes to stay for several weeks and take part in our measurements. I have a photo of the ceiling of the laboratory, embellished with the visitors' signatures – about 200 of them, including those of Nobel Laureates, past and future.[13] The main ceiling lamp hung from the 'centre of mass' of a Newton diagram circumscribed about it by Dudley Herschbach. Since my early days at the Heyrovsky Institute, Dudley was known to me as one of the main protagonists of the field of reaction dynamics. His lecture notes on the subject were made available to me by Zdenek, who had attended a series of lectures that Dudley delivered at Yale in the early 1960s, when the field was still in its, as Dudley called it, 'evangelical' stage. The lucidity, scope and beauty of Dudley's notes made an indelible impression on me and, as related below, would influence the future course of my life/career.

A singular source of enthusiasm and an incomparable intellectual delight during my time in Machova were my encounters with Rudolf Zahradnik, usually in the company of Zdenek Herman or other colleagues, over a cup of tea/coffee in Rudolf's crammed but meticulously organised office. Apart from his other great virtues, Rudolf has been in possession of a unique ability to listen to others and to respond in the most thoughtful and kindly of ways to what people had to say (even if sometimes they did not). I don't know how he did it, but things went often so far that the opportunity to present an idea to Rudolf was as gratifying as proving the idea in the laboratory.

As the reader has no doubt noticed, my memories of the time at the Heyrovsky Institute are mostly idyllic. I am well aware, however, that for others, including my mentors, these were difficult times, filled with often existential conflicts with the communist authorities. It was truly heroic of them to take these conflicts upon themselves and to create for us, their students and young colleagues, an environment that we can so fondly remember.

During my time at the Heyrovsky Institute, I also started my foreign travel as a scientist. My first trip in that capacity, in 1980 (I was only a graduate student then), took me to the East German Academy of Sciences for a stay at Lutz Zülicke's department of theoretical chemistry. The department was busy studying the dynamics of molecular collisions using both quasi-classical and quantum methods. In particular, the latter endeavour, led by Christian Zuhrt, was quite innovative. Another member of Zülicke's department, Utz Havemann, the son of Robert,[14] undertook in the 1970s cutting-edge trajectory simulations of the H_2^+ + He reactive collisions, studied concurrently in crossed-beam experiments by Zdenek in Prague. Angela Merkel, a fellow PhD student, was immersed in state-of-the-art calculations on the kinetics of unimolecular decay processes. It was a stimulating place, one that considerably brightened my earlier memories of East Berlin, which I had visited when I was 20 with the goal of improving my German.[15] The bustle of Zülicke's department was augmented by his collaboration with Evgeny Nikitin and his Moscow team, which included Evgeny's wife, Lena Dashevskaya. In the mid-1980s, I would be fortunate enough to collaborate with Nikitin myself, on the dynamics of single-charge transfer collisions from doubly charged ions [7,12].

In 1982, I was able to get out not only of the country, but also of the Soviet block and spend a year as a post-doc at the University of Utah, in Salt Lake City. It was a sort of miracle that the communist bureaucracy would let me go – in part a result of the support I received from the Heyrovsky Institute, but also of the fact that the members of the communist establishment held apparently no personal grudge against me. My stay at Utah was not an undiluted delight though. My host, Jean Futrell, was in deep trouble with both the funding agencies and the University, and so the conditions for my work were considerably less favourable than at the Heyrovsky Institute. However, with a combination of luck and determination, I was able to resurrect a crossed-beam apparatus and get some interesting results, e.g., on a vibrational Feshbach resonance in low-energy charge-transfer scattering of Ar^+ by N_2 that populates predominantly the first excited vibrational state of the N_2^+ product ion [8].

By far the most important event of my stay at Utah was, however, my trip, in the Fall of 1982, to the Northeast of the US, on invitation from Michael Henchman, a New Englander who moved to America from Old England in the 1950s. Michael knew me from his earlier visit to Zdenek Herman's laboratory in Prague, where we had worked together on the dynamics of the formation of CH_5^+, a 'non-classical ion', in collisions between methane ions and methane molecules [27]. When I once asked him, 'How come that after decades of living in the US, you don't have any American accent?' Michael retorted, 'Bretislav, this is probably because I'm not particularly gifted for foreign languages'. Years later, when I was settling in Boston, I would inquire with Michael about the sources that I should tap in order to improve my English. Whereupon he told me, 'There aren't that many. Shakespeare, Milton ... but, wait a minute, there's an excellent talk show on National Public Radio called Car Talk. They speak very good English!' – and he would give me a radio ...

Michael was a most gracious host, who organised for me a series of lectures at Brandeis (where he was a professor of chemistry), MIT, Yale, the Rockefeller University and Harvard. After my talk for Dudley Herschbach's research group, Dudley took me for lunch to the Faculty Club and, without much ado, invited me to join his research group as a post-doc, 'at a time that would suit me'. Needless to say, Dudley's invitation was a sea change for me. I would take it up in 1987.

My visit to Cambridge, Massachusetts, was fascinating to me in many respects, foremost because of my encounters with the Harvard and MIT faculty and students. The PhD students and post-docs aspired, with an almost tangible determination, to produce work that would keep up with that of their mentors. When they filled up a room, the air felt as if it were electrified. Sometimes one could even get a shock there ... Harvard enchanted me also by its aesthetic qualities – as a uniquely beautiful and highly cultivated place, embellished by (or embellishing) the city of Boston. The combination of all of the above made a huge impression on me, in fact quite ineffable.

Humboldt Fellow in Göttingen

From the East Coast I returned to Utah and from Utah to Zdenek's laboratory in Prague. The steady stream of visitors to the laboratory included guests from West-Germany. Peter Toennies, from the Max-Planck-Institut für Strömungsforschung in Göttingen, was among those who enjoyed stopping by and during one of his visits to Prague he invited me for a stay in Göttingen. On his recommendation, I applied for a Fellowship at the Alexander von Humboldt Foundation to support my stay in Germany, which was promptly granted. The Czechoslovak authorities gave me a particularly smooth ride this time around as well, because they considered my stay at Utah a success, and so on 3 March 1986, at the age of (almost) 33, I was able to take a train from Prague to Göttingen. It was a fateful journey. I would see Prague again only in 1992.

The Max-Planck-Institut für Strömungsforschung was yet another incarnation of a scientific paradise. As I had the opportunity to note elsewhere[16] , it was a Mecca of molecular beam work, with about 20 beam experiments running at the same time, some round the clock. One of the beam machines in Peter Toennies's department made it possible to detect hydrogen atoms formed in collisions of protons with atoms or molecules via charge transfer. If fast enough, the hydrogen atoms would show up after impinging on a conventional open photomultiplier tube, as discovered shortly before my arrival by Martin Noll and Peter. I was particularly intrigued by the possibility of obtaining vibrational and, for some molecular systems, even rotational resolution of the translational energy spectrum of the product hydrogen atoms, yielding similar information about the product molecular ions as photoionisation spectroscopy. However, the vibronic transition probabilities were found to deviate considerably from the corresponding Franck–Condon factors in favour of the enhancement of the quasi-resonant states [20,21]. Another type of a charge-transfer process that we tackled was charge transfer collisions between protons and Xe atoms. In this system, there is a quasi-resonance between the elastic and charge transfer channels, giving rise to characteristic oscillations in the differential scattering cross sections, which we could clearly resolve. In addition, the quasi-resonant nature of these collisions made it possible to calibrate the photomultiplier's detection efficiency for the hydrogen atoms produced in the charge transfer process [17,18].

The above work unfolded in the most congenial atmosphere of Peter's department, and was a result of a collaboration with Peter, Martin Noll, Gereon Niedner and Wolfram Maring. My time in Göttingen was further enhanced by pleasant interactions with other members of the Institute, in particular Victor Herrero, a Humboldt Fellow, and also of the Göttingen University, especially Jürgen Troe and Jörg Schröder. Long walks to the Heinberg Hill on the weekends provided a much-needed respite.

1986 was a year of happy reckoning for molecular collision dynamics: the chemistry Nobel Prize for that year was awarded to Dudley Herschbach, Yuan Lee and John Polanyi 'for their contributions concerning the dynamics of chemical elementary processes'. As my stay in Göttingen was coming to an end, I asked Peter Toennies to write to Dudley about my recent work and thereby to 'remind' him of his invitation. Dudley wrote promptly back and invited me to join his research group as a post-doc in the summer of 1987. When I arrived in Boston, in August, the idea was that I would stay for a year or two and then see what to do next. In the end, I stayed at Harvard for 16 years.

Harvard

Upon my arrival at the Harvard Chemistry Department, Dudley suggested to me to start working on vector correlations in molecular collisions. Up to that point, I was only familiar with one kind of vector correlation, namely the differential cross section, which is a correlation between reactant and product relative momenta. However, even for simple A + BC collisions, there are other vector correlations, such as the correlation between the reactant angular momentum and the reactant and product linear momenta, which held the promise of revealing even more about collisional mechanisms than differential cross sections. In fact, there is a subfield of collision dynamics – collision stereodynamics – which explores aspects of the alignment or orientation of the reactant angular momentum or molecular axis on the course of a collision. However, in order to be able to study collision stereodynamics experimentally, one needs to have the means available to align/orient the angular momentum or the axis of the reactant molecule. David Pullman, Dudley's PhD student, had set up an experiment to see whether molecular rotation could be aligned by a supersonic expansion. The idea was that a molecule seeded in a suitable carrier gas will suffer collisions that will make it rotate predominantly in a plane perpendicular to the beam axis (and thus to the mean velocity of the collisions with the carrier) along which the elastic scattering cross section exceeds that for molecules rotating in a plane parallel to the beam. David was able to show that this is indeed the case but, in a gratifying teamwork, he and I subsequently demonstrated that rotationally inelastic collisions (that change the magnitude of the rotational angular momentum – and hence lead to rotational cooling) can reverse the sense of the alignment attained, yielding more molecules rotating in a plane parallel to the beam axis. Thereby, we showed that in supersonic molecular beams the alignment of molecular rotation is there – whether desired or not – as a bonus, and that by varying the expansion conditions (and the carrier gas) we can not only tune its magnitude, but also change its sense [26,53].

My first two years in Boston were clouded by problems with my immigration status, which started once the Czechoslovak authorities refused to extend my permit to stay abroad and told me flatly that by overstaying I had committed a (harshly punishable) crime referred to as the 'abandonment of the republic'. In addition, I had my own misgivings about life in exile – it was hard for me to cope with the thought that I will perhaps never be able to return to my native country. However, once it became clear that I reached the point of no return, I had to deal with my immigration status. The legal possibilities were quite limited. In fact there was just one, namely to seek political asylum in the US. So, I did. However, I thought that my case was quite weak, especially since I did not want to lie and claim that I was treated badly in Czechoslovakia when it wasn't true. However, after a few interventions and many dollars spent on lawyers, I received a hearing at the Immigration and Naturalization Service. The kind lady who interviewed me there understood my case better than I did and granted me political asylum without much ado: my case was not 'empty', since had I returned to Czechoslovakia, I would have been harshly prosecuted for something that the US (and the Helsinki Conference) had not considered to be a crime at all but a right. This was in May 1989. The Cold War was still raging – and the Berlin Wall stood firm. Not for very much longer though. When it started crumbling, I got myself a TV set and watched in a trance how the impossible was coming true. I just couldn't miss a second of it! I was particularly impressed by the manner in which the communist leadership in Czechoslovakia was ousted: It was showed out by crowds of people chiming their keys – so as to sound the death knell to the totalitarian rule in the country. In June 1996, I was sworn in as a US citizen, in the historic Faneuil Hall in Boston. That sealed my US immigration status, but made me a foreigner in my native country.

Our frequent invocations of rotational cooling in our work on the alignment of molecular rotation in supersonic beams led Dudley and myself to reread a passage in the book by Townes and Schawlow[17] that discusses the typical values of the ratio of the molecular Stark energy to the rotational energy, and concluded that the former can easily prevail over the latter for polar molecules that are rotationally cold. That meant that an electric field could orient a molecular dipole (and hence the molecular axis) along the field's direction. Up to that point, the conventional wisdom was that molecular rotation would average out the dipole moment in first order (except for symmetric top molecules in their precessing states) and thereby preclude any sizable molecular axis orientation.[18] During the subsequent decade (and beyond), Dudley and I explored and exploited, both experimentally and theoretically, various aspects of our realisation, which we presented (as a duet) for the first time at a symposium in Göttingen celebrating Peter Toennies's sixtieth birthday, in 1990. There we learned that Hansjürgen Loesch, Dudley's former post-doc, reached a similar conclusion independently, and made use of the orientation achieved (which he called 'brute force orientation') in a collisional experiment. We took a different path, and explored the properties of the states created by the interaction of a molecular dipole with an electrostatic field spectroscopically [30]. Since the states arise due to a cosine potential and the axis of molecules in these states librates about the direction of the electric field, we named the states 'pendular'. Roger Miller soon thereafter carried out a particularly incisive experiment where the pendular hybridisation of rotational states resulted in a collapse of the infrared spectrum of a linear $(HCN)_3$ cluster into a single line, a result that we could readily explain quantitatively when Jan-Michael Rost joined in and applied his computational acumen [31]. During my visit in Göttingen in 1992, Horst-Günter Rubahn, N. Sathyamurthy and I complemented our previous efforts by undertaking a study of the collisional properties of pendular states [33]. We found that rates of rotational energy transfer are reduced due to increased spacing of the hybridised rotational levels in the electric field. With Alkwin Slenczka, who joined us at Harvard as a Feodor Lynen Fellow in 1993, we expanded our explorations of the directional properties of molecules in fields to include magnetic fields and combined electric and magnetic fields. Our combined-fields study of the ICl molecule (our *Escherichia coli*) enabled us to resolve an outstanding electronic structure problem, namely whether the ICl molecule changes the sign of its electric dipole moment when excited from the electronic ground state to the paramagnetic A state (it does not) [42]. Upon his return to Germany, Alkwin developed a new approach to polarisation spectroscopy based on pendular states.

Our work on the interactions of molecules with fields went to a higher gear once we included in our considerations a non-resonant (far-off-resonant) optical field. In this case, the pendular hybridisation of anisotropic molecules gives rise to strongly aligned low-field-seeking states [45,50]. The versatility and facility of the laser-induced dipole interaction have made it into an indispensable tool for manipulating both molecular rotation (alignment [59]) and translation (slowing [84], molecule optics [96], trapping [111]). For large classes of molecules (including linear molecules), the pendular hybrids occur as tunnelling doublets of opposite parity, whose splitting can be (almost) arbitrarily diminished by raising the intensity of the optical field [55]. In 1999, Dudley and I realised that for polar molecules, the opposite-parity members of the tunnelling doublets can be easily coupled by a superimposed electrostatic field, giving rise to mixed-parity states that are strongly oriented [81]. The combined-fields technique, as versatile as the anisotropic induced-dipole interaction itself, provides the means to strongly orient any polar molecule, regardless of whether it is linear or asymmetric [98,99,114,153]. Later, the technique would be used in experiments by the groups of Udo Buck [98,99], Michal Farnik [113], Henrik Stapelfeldt [153] and others to reveal the structure of clusters or to enhance imaging of molecules. We also showed that intersecting opposite-parity Zeeman levels of polar paramagnetic molecules can be efficiently coupled by an electrostatic field, lending the molecules strong orientation and other interesting properties controllable by the fields [85,87].

In 1997, on invitation from Victor Herrero and Javier Aoiz, I visited the Instituto de Estructura de la Materia (IEM) of the Spanish Concejo Superior de Investigaciones Cientificas in Madrid, as an Iberdrola visiting professor. Apart from an opportunity to tell my colleagues everything

I knew (and more) about field-dressed molecules [63], we jointly undertook a study of the steric effect in reactive collisions of H + DCl in an electric field. This revealed that the exchange channel is brought about by two collisional mechanisms, distinguished by the direction along which the H atom approaches the DCl molecule. Such insight can only be gained from the study of collisions of oriented reactant molecules [68].

Until my journey to Spain, my studies of molecules in fields were time-independent, relevant to experiments taking place in the *presence* of (external) electric, magnetic or optical fields. With Juan Ortigoso from IEM, we decided to look into the time dependence of the anisotropic polarisability interaction. The time dependence arises when the optical field is delivered by a non-resonant laser *pulse*. We found that for pulses shorter than the rotational period of the molecule (non-adiabatic pulses), the field creates rotational wave packets that recur in the absence of the optical field, after the laser pulse has waned. The alignment of the molecular axis pertaining to the rotational wave packets recurs then as well. Thereby, we showed that by making use of sufficiently short (and intense) laser pulses one can produce field-free alignment that undergoes periodic revivals – for as long as decoherence leaves the rotational wave packet alone [73,91]. It can also be orientation that is recurring, if the laser pulses are combined with a superimposed electrostatic field [90]. On the last topic (as well as others) I had the pleasure of working with a significantly precocious Harvard undergrad, Long Cai.

In 1994, when Alkwin Slenczka was about to leave for the University of Regensburg, I got a phone call from the other side of Oxford Street. It was John Doyle, a freshly anointed junior faculty member at Harvard Physics. John had been involved in pioneering work with Dan Kleppner and Tom Greytak at MIT on cryogenic cooling and magnetic trapping of atomic hydrogen and in studies of quantum reflection. Now, John sought to widen the scope of cooling and trapping of neutrals to include molecules. While at MIT, he had the idea that molecules (and atoms) could be cooled by elastic collisions with a cold He buffer gas, possibly to sub-Kelvin temperatures, since the elastic scattering cross sections in the collision energy regime in question could be presumed to be large enough for any species. For paramagnetic molecules (and atoms), this meant that it should be possible to load them into a magnetic trap. Because of my experience with molecules in fields, John proposed to me to join forces and work with him and his fledgling research group in the fledgling field of cold molecules. He did not have to ask me twice. What unfolded has been quite an adventure – in the spirit of Karel Capek's maxim that 'true adventure begins when imagination collides with reality'. With Dave Patterson, another significantly precocious Harvard undergrad, and Jinha Kim, John's first grad student, we were able to soon work out the basics [48], secure funding for the project and get going. Joined by the PhD students Jonathan Weinstein and Robert deCarvalho, we first demonstrated the technique by buffer-gas-cooling laser-ablated europium (Eu) atoms and loading about 10^{12} of them into a quadrupole magnetic trap, whose depth was about 3 K, due to the large magnetic moment of Eu; the atoms could be held in the trap for up to 10 s [60,86]. The large number of atoms trapped and the long trapping time attained were well in keeping with our optimistic expectations about the possibility of applying forced evaporative cooling to the magnetically confined ensemble and thereby reaching the ultracold (<1 millikelvin) regime of translational energies. Europium was chosen for our first experiment in part because it is not amenable to laser cooling, the 'work horse' of cold-atom physics that proved its worth with alkali atoms and some other species, but was deemed unsuitable for the rest, including molecules. Soon after our first successful experiment we ventured to trap a molecule. Our choice fell on calcium monohydride (CaH), a molecule that could be readily made by laser-ablating a pellet of calcium dihydride and easily detected via laser-induced fluorescence. Our effort soon resulted in the trapping of the first molecule – or rather about 10^8 of them – at a temperature of about 400 millikelvin [72]. Like before with Eu, the experiment demonstrated that one could do a rather revealing spectroscopy on the trapped ensemble, leading, e.g. to insights into the coupling of the molecule's electronic states [74]. However, our original expectation that one could evaporatively cool the trapped molecules into the ultracold regime has not materialised so far (with the exception of metastable helium [124] and the OH radical), due to high rates of dipolar relaxation – a process that flips a magnetically trappable state (low-field seeker) of an atom or molecule to an untrappable one (high-field seeker).

An appealing variant of the technique came about after the arrival in 1999 of another Feodor Lynen Fellow from Germany, Wieland Schöllkopf: instead of producing molecules from 'suitable' precursors by laser ablation inside the cryogenic cell, we 'brought in' the molecules by a molecular beam through a hole in the cryogenic cell's wall. This required striking a fine balance between the ineluctable beam of helium rushing out of the cell and the molecular beam that we wished to coax into getting in. With the application of John's cryo-wizardry, it was possible to make the scheme work [94,103]; perhaps more importantly, it spawned later work by John Doyle *et al*. on what became the cold and intense 'buffer-gas beam'.[19]

In 1995, Zdenek Herman and Rudolf Zahradnik came up with the idea of transplanting me back to Prague, and enticed the dean of the Faculty of Mathematics and Physics of Charles University to get in touch with me about it. The dean invited me to submit a habilitation thesis and indicated that the faculty would subsequently appoint me as an associate professor. In response, I wrote a habilitation (on molecular interactions with and in fields), defended it before the Scientific Board of the faculty and was indeed named associate professor in 1996. However, as I had made known from the outset, I was unwilling to move to Prague unless the faculty went the whole way and accorded me a full professorship. However, there was a bureaucratic hurdle in the way, namely a protectionist measure that stipulated that in order to become a full professor at Charles University, one had to teach there for five years prior to the appointment. Somebody at the faculty had the idea of circumventing the measure by sending me a PhD student who would do his work with me at Harvard but defend his thesis at Charles University in Prague. I liked this idea, but then, when push came to shove, I was told that I cannot be on the student's PhD committee, because I was not a resident member of the Prague faculty. This Catch 22 pretty much upended my return to Prague.[20]

Fortunately, something good started looming at Harvard: on Dudley's suggestion and under John Doyle's co-sponsorship, I was to be promoted to the position of Senior Research Fellow. The last such position was granted over a decade earlier, and so nobody in the university administration really knew what it entailed and which appointment procedure to invoke. In the end, the dean decided to make use of Harvard's tenure procedure, which included an *ad hoc* committee. After a great effort on the part of my sponsors and their colleagues, my appointment went through in 1997.[21] Shortly afterwards, Jim Anderson, the chairman of the chemistry department, asked me to teach a course on experimental physical chemistry, so I also became a Lecturer. I loved the teaching assignment and developed a course (Chem 165) that became quite popular. One class even issued T-shirts commemorating their 'pchem' experience. A few years later, I was offered the possibility to teach a Freshman Seminar. Proposed by Harvard's benefactor Edwin Land, Freshman Seminars were supposed 'to provide small-group instructions to freshmen in the College, and thus to encourage close and early contact between undergraduates and members of the faculty'.[22] My seminar, titled 'The Unfolding Story of Light,' was a hybrid between physics and the history of physics. It started with Empedocles' fire of the eye and ended with the quantum teleportation of photons. What added colour to the course was the seminar's venue – we could meet in the spectacular Senior Common Room of Winthrop House, where I had become a member, thanks to the invitation of Winthrop's Master Paul D. Hanson.

Berlin

In the Fall of 2002, Gerard Meijer, well known for implementing Stark deceleration and electrostatic trapping of polar molecules as well as for his work in molecular spectroscopy, came to Harvard to give two colloquia – one at Physics and one at Chemistry. I participated in hosting his visit, for I knew not only his work, but also him personally, from meetings of the growing cold-molecule community. At the very end of his visit, on the way to the airport, Gerard told me that the Fritz Haber Institute of the Max Planck Society in Berlin was in the process of establishing a new department of molecular physics with him as a director, and asked me whether I would consider joining in as a senior scientist. This possibility appeared like a *deus ex machina*, but it did not take long for me to say yes. The growing emphasis on 'chemical biology' at American universities, often at the expense of chemical physics (with Harvard Chemistry leading the way), combined with the lean years of the Bush II era led to a funding crisis, which was adversely affecting our ability to do research. I was impressed by the levelheadedness of the Max Planck Society, demonstrated by its willingness to vouchsafe for the field of molecular physics at a time when it was viewed as unfashionable in many quarters in the US and elsewhere. Based on what I had seen in Göttingen, I was well aware that the possibilities of a Max Planck Institute were essentially unlimited. Moreover, Helmut Schwarz expressed an interest to 'connect' me to the Technische Universität Berlin (which would materialise, in 2006, in the form of a Honorarprofessur), and so my decision to move proved to be a no-brainer. I cheerfully emigrated for the second time – arriving in the tree-lined academic district of Berlin-Dahlem in September 2003.[23] I may add that I missed Europe while in America (about as much as I now miss America while in Europe) and so my reunion with the old continent was an altogether happy one. I was also happy about the prospect of seeing my relatives and old friends and colleagues more often, including those who lived in Berlin . . .

The first problem that I tackled upon my arrival in Berlin was the dynamics of the Stark deceleration process. A Fourier analysis of both the spatial and temporal dependence of the applied electric field revealed that the field inside the decelerator consisted of a multitude of pairwise counter-propagating waves with well-defined phase velocities [104]. The equations of motion implied that a given wave would give a ride to those molecules whose velocity came close to the phase velocity of the wave. To top the joys of modelling the problem, it turned out that the equations of motion had an analytic solution. There is also an isomorphic problem, namely that of a biased pendulum (pendulum under a constant torque), that can be used to visualise the Stark decelerator dynamics. A detailed account of the dynamics, worked out with Koos Gubbels and Gerard Meijer, demonstrated that the analytic 'wave model' encompasses all the longitudinal physics encountered in a Stark decelerator, including marginal phenomena that occur due to the 'interference' of the various waves [110]. During the summer of 2005, I was joined by another precocious Harvard undergrad, Monika Schleier-Smith, with whom I explored various subtleties of the longitudinal dynamics in a cylindrical Stark decelerator, whose advantageous properties we established by trajectory simulations. Monika and I also proposed a trap for high-field-seeking molecules which was later implemented by Melanie Schnell and coworkers [112].

A special issue of the *European Physical Journal D* [105] and a monograph dedicated to cold molecules [121] that I had the pleasure to co-edit with John Doyle, Roman Krems, Francois Masnou-Seeuws and Bill Stwalley may have contributed to the disciplinary identity of the thriving cold-molecule research, and several popularising accounts may have helped to spread the word about cold molecules across disciplinary boundaries [119,124,134,140].

My current research revolves around interactions of molecules with electric, magnetic and optical fields, and with their combinations. The following specific research topics are currently being pursued: (1) manipulation of molecules by means of external fields; (2) molecular collisions in fields; (3) spectroscopy and imaging of molecules in fields; (4) cold/slow molecules; and (5) quantum computing with molecules. Although chiefly theoretical (with a predilection for an analytic approach), the research is closely linked to ongoing experiments.

With Mikhail Lemeshko, a graduate student in a league of his own, who arrived from Rostov-on-Don in 2007, I returned to vector correlations in molecular collisions. We first refined a model of collisions based on Fraunhofer scattering of matter waves, and showed that the model maintains its analyticity for rotationally inelastic collisions of atoms with diatomics even if these take place in electric, magnetic and optical fields [115,118,120]. This initial venture, which mapped out possible field effects for thermal and hyperthermal collisions, was followed by a frontal attack on vector correlations in rotationally inelastic collisions, which led to an analytic model of vector correlations that proved to be surprisingly accurate and helpful in assessing collisional mechanisms. The striking agreement between the model and exact polarisation moments showed that the stereodynamics of rotationally inelastic atom–molecule collisions at thermal energies are governed, for many systems, by diffraction of matter waves from a two-dimensional repulsive core of the atom–molecule potential. Furthermore, the model polarisation moments characterising the stereodynamics were found to coalesce into a single, distinctive pattern, which can serve as a 'fingerprint' to identify diffraction-driven stereodynamics in future work [130]. Mikhail's PhD thesis was featured in 2012 among the best four in a worldwide competition run by the Division of Atomic, Molecular, and Optical Physics (DAMOP) of the American Physical Society (among the four finalists, Mikhail was the only theorist). Mikhail was also involved in work on accurately probing weakly bound molecules by non-resonant laser pulses [122], as well as other topics, some of which he initiated [148,151]. I am particularly fond of a piece of work that – by making use of Harald Friedrich's quantisation function[24] – answered the question of whether a given weakly bound vibrational state can support rotational states and how many [123].

Analytic solutions are the gems of physics: beautiful and rare, furnishing an unrivaled insight into a problem's nature. It is a part of the culture of physics to seek them. Supersymmetric quantum mechanics (SUSY QM) provides a method to 'discover' analytic solutions, as first realised by Lev Gendenshtein in 1983 when examining Edward Witten's toy model of SUSY quantum field theory. It was an unrivaled pleasure for me to work with Sabre Kais and Mikhail on the SUSY of the molecular Stark effect. Although the problem lacks the requisite shape invariance to be analytically solvable in all generality, it can be solved in a closed form for the subset of 'stretched' states and a particular ratio of the permanent and induced dipole interaction parameters [135]. Apart from being beautiful and rare, this analytic solution is also practical, for it allows to reverse-engineer the eigenproblem and find the values of parameters required for creating quantum states with preordained characteristics [138].

Recently, Sabre Kais invited me to work with him and with Dudley Herschbach on the topic of quantum computing. This work has revolved around David DeMille's proposal to make use of an array of trapped polar molecules as a scalable quantum computation platform. The qubits envisioned in the proposal are pendular states, created by an inhomogeneous electrostatic field that precludes quenching of the molecular dipoles and simultaneously ensures addressability of different qubit sites. Our work furnished analytic expressions for key characteristics of the platform, such as entanglement among the pendular qubits and characteristic frequency shifts needed for optical control of quantum gates operating on such qubits [136,149]. These results may also find application in the quantum simulation of condensed-matter systems, another field where ultracold polar molecules are playing an increasingly prominent role.[25] Last but not least, we also presented a systematic approach to implementing basic quantum logic gates and showed that, for the pendular qubit states, NOT, Hadamard and CNOT gates can be realised with high fidelity [158].

At the Institute for Optics and Atomic Physics of the Technische Universität Berlin, I teach an elective course entitled 'From the new world of cold molecules'. In the course, I provide an introduction to molecular physics and share in some of the excitement that has been generated by the field of cold molecules. Cold molecules are particularly hot in quantum simulation, quantum computation, metronomy and few-body physics, including ultracold chemistry.

History of science

My dormant humanistic inclinations were awaken by my visits to the Harvard Physics Library, or rather occasional peeks into a small adjacent depository room that held 'old' journals and books. Like many other good things that happened to me, my visits to the depository were triggered by Dudley Herschbach. Dudley has had a deep affection and admiration for Otto Stern and liked to tell stories about him, some of which he had heard from Stern himself – whether about the Stern–Gerlach experiment or other feats of the heroic age of quantum physics. A paradox became apparent in one of my conversations with Dudley about the Stern–Gerlach experiment, namely how come that the 1922 experiment, which, as most textbooks would tell you, was about electron spin, could have been carried out three years before the discovery of spin, which was made only in 1925. The depository provided an answer: it was not spin, but orbital angular momentum that Stern and Gerlach had set to examine, but *Nature* had camouflaged as spin angular momentum, due to the anomalous gyromagnetic ratio of the electron and the electron's half-integer spin quantum number. Our 'Lucky Star of Otto Stern' paper appeared in a special anniversary issue of the *Daedalus*, which was titled 'Science in Culture' [66]. There could not have been a better venue for my history of science debut. I co-authored some more papers on Otto Stern, one in the genre of 'experimental history of science' with Dudley [100] and one on Stern's life's work, written jointly with Peter Toennies and Horst Schmidt-Böcking [150]. In 2008, I was asked by the directors of the Fritz Haber Institute to write a book on the institute's history for the centennial year of its founding in 1911. Jointly with Dieter Hoffmann from the Max Planck Institute for the History of Science, who had been invited as my fellow senior coauthor, and with two post-doctoral fellows, Jeremiah James and Thomas Steinhauser, we mapped out the illustrious history of the institute. During the Weimar era, the institute played a key role in the transition from classical physical chemistry, preoccupied with thermochemistry, to modern, quantum-mechanics-based chemical physics, focused on structure and later dynamics. So there was a lot to write about – and to put into the historical context of two world wars and two atrocious dictatorships that shaped both the scientific and institutional aspects of the institute's history. To do justice to this history proved to be a Herculean task, especially in view of the 2011 deadline set 100 years earlier, but the four of us managed to deliver the book on time [144,145]. There was no need to postpone the institute's centennial celebrations to the next year or worse ... My latest venture in the history of science, with Massimiliano Badino, examines the pioneering exploits of Otto Sackur[26] in the quantum mechanics of gases – a first, bold step towards understanding quantum degenerate gases, embodied by the twenty-first century's ensembles of ultracold atoms and molecules [159].

Family

It was in Berlin where I was fortunate enough to meet my wife, Christine, nee Storch. She is a musician and music teacher, with her own perspective on waves and beauty. We have three children, Juliane (b. 2007), Christian (b. 2008) and Jitka (b. 2010), whom we are raising bilingually, between English and German, and hope to include Czech, too, as a third language. We live in Dahlem, a stone's throw from the Fritz Haber Institute. My daughter Jana (b. 1982), from my marriage with Helena Friedrich (1955–2002), is a graphic-design artist, living in Prague.

Acknowledgements

The assignment from the editors of this special issue to write my (scientific) autobiography caught up with me during my stay at the Kavli Institute for Theoretical Physics, University of California, Santa Barbara, where I was attending, accompanied by my family, a magnificent programme on 'Fundamental science with ultracold polar molecules' from January to March 2013 (funded in part by NSF PHY11-25915). In telling my story, I tried to heed Evelyn Waugh's apt advice:[27] 'Don't give your opinions about Art and the Purpose of Life. They are of little interest and, anyway, you can't express them. Don't analyze yourself. Give the relevant facts and let your readers make their own judgments. Stick to your story. It is not the most important subject in history but it is one about which you are uniquely qualified to speak'.

Notes

1. Carborundum is located in Benatky nad Jizerou, a small town about 40 km north-east of Prague.
2. The Czechoslovak Communist Party was taken over in 1929 by a Stalinist faction that prided itself on implementing 'bolshevisation' of the Party, which meant elimination of democratic practices and turning the Party into Stalin's fifth column.
3. In 1939, Bohemia and Moravia, the Czech parts of Czechoslovakia, came under Nazi occupation, while Slovakia seceded and became an antisemitic fascist puppet state. The Nazi leadership drafted several 'solutions to the Czech question': According to one, a half of the Czechs had to be assimilated and another half 'eliminated' by combination of killing and deportation to the 'East'. The Czech intelligentsia, regarded as an obstacle to the assimilation plans, was slated for the harshest treatment. Czech universities were closed during the 1939–1945 occupation. The Nazi governor of the 'Protectorate Bohemia and Moravia', Reinhard Heydrich, was the same Heydrich who drafted the 'final solution of the Jewish question'. He was assassinated by members of the Czechoslovak resistance in 1942, five months after the 'Wannsee conference' (see, e.g. M. Teich, *Bohemia in History* (Cambridge University Press, Cambridge,1998).
4. Jiri Dedecek is a poet, singer-song writer and essayist. Since 2008 he has been the chairman of the Czech PEN Club.
5. The street car line in question had number 17. For a possible deeper significance of this number, see D. Herschbach, J. Irreproducible Results **37**, 27 (1992).
6. Adam Hoffmeister is an artist and gallerist in Prague. Since 2013, he is the chairman of the Manes Union of Fine Arts. His father, Adolf, was a man of many parts, with a creative scope that ranged from painting to writing to art and literary criticism. Adam's parents' house was a meeting point of Czechoslovakia's artists and literati, both before and after 1968.

7. This phrase was coined by Louis Aragon, in the wake of the massacres and famine in Biafra, the break-away region of Nigeria that existed as a state from 1967 until 1970.
8. The reforms that were being suppressed by the Soviets in 1968 Czechoslovakia bore a striking resemblance to those promoted by Mikhail Gorbachov nearly 20 years hence under the banner of Perestroyka and Glasnost.
9. Zuzana Hostomska, nee Chytilova, graduated from the Institute of Organic Chemistry and Biochemistry of the Czechoslovak Academy of Sciences. Upon emigrating to the US, she worked at Agouron Pharmaceuticals as a biochemist focused on structure-based drug design. Later she served as a Vice President for Research at Pfizer, La Jolla, California.
10. Wilhelm Ostwald: 'Physical Chemistry is not a branch of chemistry but the chemistry of the future'. [cited in Martin Quack, Bunsen-Magazin **14**, 225 (2012)].
11. I will refer to some of the papers that I authored or co-authored with numbers (in square brackets) that they have been assigned in my list of publications available either in this special issue or at http://www.fhi-berlin.mpg.de/mp/friedrich/BF_PubList.pdf.
12. Some of Zdenek Herman's drawings related to the Heyrovsky Institute can be found in B. Friedrich, M. Henchman, and D. Herschbach, J. Phys. Chem. **99**, 15317 (1995).
13. Following upon the relocation in 1988 of the Heyrovsky Institute to a modern building, visitors have been invited to sign the wall of Zdenek's new laboratory there. After his visit in 2000, Graham Cooks made the following observation: 'When you signed that wall in Prague you meant something in the field of ion–molecule reactions' (Michael Volny, private communication) (cf. J. Pol and M. Volny, editors, *The History of Czechoslovak Mass Spectrometry* (Czech Society for Mass Spectrometry, Prague, 2012), in Czech.
14. Robert Havemann (1910–1982) was a physical chemist and political activist, a prominent member of the anti-Nazi resistance during the Third Reich period and one of the best known dissidents during the era of the German Democratic Republic.
15. I was spending my mornings by memorising a (small) German–Czech dictionary, making use of the alphabetic order of the words as a mnemonic to help me remember them [cf. J. Foer *Moonwalking with Einstein* (The Penguin Press, New York, 2011), p. 97].
16. B. Friedrich, J. Phys. Chem. A **115**, 6739 (2011).
17. C.H. Townes and A.L. Schawlow, *Microwave Spectroscopy* (Dover, New York, 1975), pp. 248-250.
18. Compare with, e.g. P.R. Brooks, Science **193**, 11 (1977).
19. See, e.g. D. Patterson, J. Rasmussen, and J.M. Doyle, New J. Phys. **11**, 055018 (2009).
20. However, ever since, or rather already since 1993, I have tried to write each year at least one paper for a Czech journal (typically in Czech) or to contribute a paper to an international journal honoring a Czech scientist. My academic ties to my native country have been further strengthened in 2011 when I was elected an honorary member of the Learned Society of the Czech Republic (Societas Scientiarum Bohemica).
21. There were only four Senior Research Fellows at Harvard in 1997.
22. See http://www.freshmanseminars.college.harvard.edu/icb/icb.do.
23. Dahlem was the royal demesne of the Hohenzollerns, which Wilhelm II granted in 1911 the Kaiser Wilhelm Society, apart from his 'name and protection'. The Kaiser Wilhelm Society, the forerunner of today's Max Planck Society, is the third institutional innovation that came out of Prussia during the 'long' nineteenth century, after a research university (now Humboldt University, founded in 1810) and a national laboratory for metronomy (now the Physikalisch-Technische Bundesanstalt, founded in 1887). Unlike the former, the latter two institutions have been emulated worldwide.
24. H. Friedrich and J. Trost, Phys. Rep. **397**, 359 (2004); **451**, 234 (2007).
25. See http://www.kitp.ucsb.edu/activities/dbdetails?acro = coldmoles13.
26. Otto Sackur (1880–1914) was Ottor Stern's *Doktorvater* and Fritz Haber's collaborator.
27. Evelyn Waugh, The Tablet (5 May 1951).