Zdenek Herman Festschrift

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Photograph by Dr. Friedrich

Zdenek Herman, Bohemian at Large

Few countries have an ambassador quite as beloved. For a host of colleagues throughout the world, Zdenek Herman has been the introduction not only to the mechanism of ionic reactions but to the culture of Bohemia and Prague—its Golden Capital. On the ceiling of the old laboratory in Máchova Street one could read the signatures of over 250 of the world's best known atomic, molecular, and chemical physicists. They came—many on repeated visits—to pay court to Vladimír Čermák and Zdenek Herman, inspired by the quality of their science and charmed by the warmth of their friendship.

The following dialogue traces the difficulty of trying to do science in Czechoslovakia during the latter half of this century. It traces too the remarkable science which nevertheless emerged. It traces Zdenek's singular scientific contributions, and it places them in the context of the remarkable scientific achievements of this small country.

We hope that a recognizable silhouette of our honoree emerges from these pages. His courtesy is legendary. Only later do we appreciate from his revealing drawings how carefully and shrewdly we have been scrutinized. Zdenek's qualities as an artist—draughtsman, painter, sculptor—compare with his scientific qualities. Less appreciated is that his technical artistic skills have played an essential role in his science. The crossed-beam machines worked so well, in part, because they were so small—too small for stray fields to distort the measurements—and it took a sculptor's hands to achieve that.

Zdenek continues to be our guide ... introducing us to new vistas of science, of the world, of friendship, and of our own humanity. Long may this continue!

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A Dialogue with Zdenek Herman

BF/MH/DH: *This Festschrift honors your achievements as a splendid exemplar of the Czech tradition. At the same time, by inviting contributions from the many Czech scientists active in your field today, we celebrate the ongoing vigor of this tradition, some of it re-rooted in North America and Western Europe because of the politics of the past 50 years.*

What are the roots of this creative tradition? Is it nurture or nature or a bit of both? One approach to this question is to ask you about your own education.

ZH: I was born in the little town of Libušín, just outside Prague. I went to the gymnasium (high school) at the nearby steel-and-mining town of Kladno (which incidentally you see to your right as you fly out of Prague). There I had excellent teachers in biology, chemistry, and physics. When I graduated, I entered the Charles University in Prague, intending to study biochemistry. Once at university, my interests focused on physical chemistry. I was influenced by Michal Heyrovský and Zdenek Dolejšek, two lifelong friends who were fellow students and who, since then, have been colleagues at the Heyrovský Institute of Physical Chemistry at the Czech Academy of Sciences. As undergraduates, we attended exciting advanced courses from Professor Brdička (Figure 1) and one of the last courses given by Professor Heyrovský (Figure 2), the founder of polarography and a Nobel Laureate. To learn any quantum mechanics in those days, one had to go to the Physics Department. Then as now in Europe, a science degree requires a thesis (*Diplomarbeit*), and so the three of us, as third-year undergraduates, started research with Professor Běhounek (1898−1973) in radiochemistry. It was an entirely new discipline, and we were his first diploma students.

Professor Běhounek had always been something of a legend to me. Marie Curie had recommended him, as a young man, to the Italian explorer A. Nobile to serve as physicist/dosimetrist on an expedition to the North Pole on an airship. The airship crashed ... and some members of the expedition did not survive. Subsequently, in addition to his science, Professor Běhounek wrote adventure stories about polar exploration for boys and girls. As a young boy, I had read these stories with enthusiasm—they fired my interest in science and technology and taught me much—and then, ten years later, I found myself as his student! He was an old-fashioned type of professor. He brought me a photocopy of a French radiochemistry paper, told me to "try to do something in that direction", and more or less expected me to appear after some time with a completed thesis. Providentially several individuals came to may aid—Čestmír Jech (Figure 3), one of his former students, with a Geiger counter, and three analytical chemists, Drs. Doležal, Simon, and Zýka, with chemicals and technical advice. With a dipolma thesis on the quantitative determination of small amounts of Ag, Mg, and Bi using precipitation with labeled compounds, I graduated in 1957. Having survived this, I judge it to have been the best schooling for a beginner—to be left largely on one's own.

BF/MH/DH: *But you did not continue as a radiochemist. What let you to a doctoral thesis with Vladimír Čermák, a chemical physicist whose science and personality were later to be so admired throughout the world?*

ZH: Mass spectrometry is linked to radiochemistry, so in 1957 and at the age of 23, I joined the mass spectrometry group of Vladimír Čermák (Figure 4) and Vladimír Hanuš (Figure 5). This group was part of the Institute of Physical Chemistry, under the directorship of Professor R. Brdička, which itself was part of the new Czechoslovak Academy of Sciences, which had been formed in 1952.

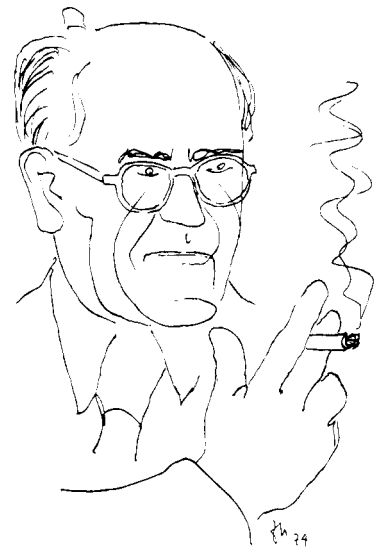

Figure 1. Rudolf Brdička.

They were exciting times. We were very short of space and, for a while, twenty people did their experiments together in one enormous room. Like many of the pioneers who laid the foundations of modern Czech science, Čermák and Hanuš had been born in the twenties, and their education had been postponed by the Second World War. They had been trained by the dominant Czech physical chemists—*J.* Heyrovský and his pupil, R. Brdička—in the dominant research area—polarography—and only subsequently did they branch out into other areas.

BF/MH/DH: *But what of your doctoral thesis?*

ZH: We had one home-built mass spectrometer, dating from 1954, and for many years it was the only mass spectrometer in Czechoslovakia. Initially we analyzed everything with it—$^{15}N/^{14}N$ for biological samples, rare gas mixtures, organic mass spectrometry—and it was only in 1960, when Vlád'a Hanuš obtained a commercial Russian mass spectrometer for the analytical program, that we could adapt the old instrument for collision studies.

We had started these studies even before 1960. The impetus had been the systematic studies of ion−molecule reactions in hydrocarbons, begun in 1952 by V. Talrose, D. P. Stevenson, Frank Field, and Joe Franklin, and others. It was a simple matter to raise the gas pressure in the ion source of the mass spectrometer and to look for ion−molecule chemistry. Using simple inorganic gases, such as N_2, O_2, CO, CO_2, we found that the corresponding product ions, N_3^+, O_3^+, C_2O^+, CO_3^+, were formed from reactant ions in *excited electronic states*, according to $(N_2^+)^* + N_2 \rightarrow N_3^+ + N$. (For reactant ions in their ground states, these reactions are endoergic.) In those days, this was considered important proof that electronic energy could be used to overcome energy barriers.

BF/MH/DH: *Tell us about your discovery of what came to be known as the Čermák/Herman technique.*

ZH: It was serendipitous. We obtained some odd experimental results. Once we understood their origin, we realized that we had identified a technique able to answer previously unanswerable questions.

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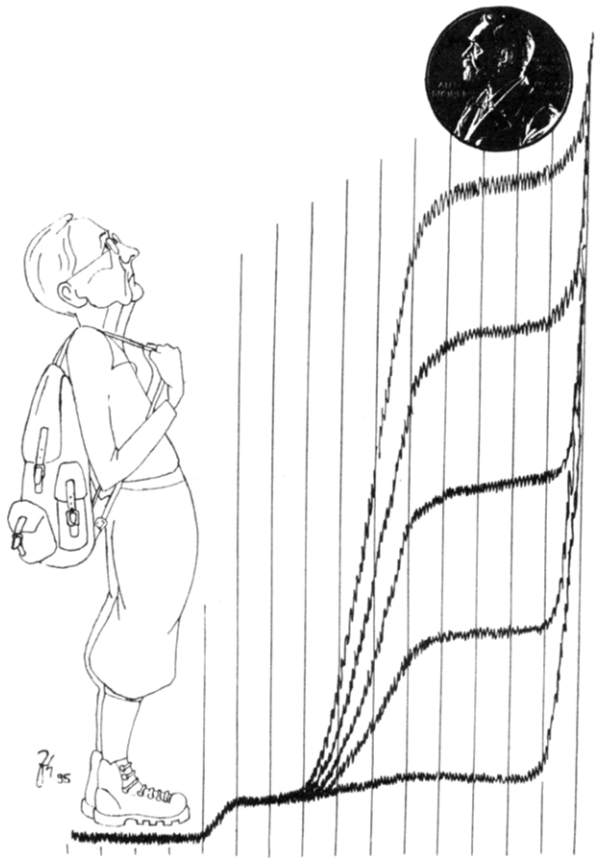

Figure 2. Jaroslav Heyrovský.

Figure 3. Čestmír Jech.

At high source pressures, we were measuring appearance potentials of reactant and product ions. We still observed ions when the energy of the electrons in the ionization chamber was less than the energy needed to ionize the molecules present. This seemingly impossible situation had a simple explanation. The electrons were being accelerated away from the ionization chamber into a trap. Ions were being formed in the region of the trap and were being accelerated back across the ionization chamber. In the ionization chamber they were undergoing charge-transfer reactions, forming thermal product ions. These thermal ions were extracted from the ionization chamber by the repeller field and were being recorded in the mass spectrum. By adjusting a few potentials in the mass spectrometer ion source, we were able to achieve some of the capability of a transverse-tandem mass spectrometer. It was a "poor man's" perpendicular beam machine.

We used the technique, in the pre-beam era, to test the quasi-equilibrium theory of mass spectra. Ions with different recombination energies were reacted by charge transfer with a target molecule, and the corresponding mass spectrum was measured. The dependence of the mass spectrum on the recombination energy—was the so-called breakdown curve—could also be predicted by the quasi-equilibrium theory. Another powerful application was to proton-transfer reactions. The technique showed in 1960 that a chemical reaction could proceed via a direct mechanism at high energies and via a complex mechanism at low energies. It was the first demonstration that the mechanism of a reaction could vary with translational energy.

BF/MH/DH: *Your thesis research dealt with ionic reactions involving metal vapors.*

ZH: I investigated dozens of ion−molecule reactions involving metal vapors (Li, Na, K, Cs, and Hg) and gases. In addition I investigated chemi-ionization reactions (Penning ionization and associative ionization) of excited neutrals with metal vapors, e.g., $Ar^* + Hg \rightarrow ArHg^+ + e^-$. Our old machine was pumped with mercury/glass diffusion pumps; it was tin soldered and patched with black wax; but the source was differentially pumped, and it had a multiplier detector. I remember a visit of Vernon Dibeler from the National Bureau of Standards in the mid-sixties. He looked at our machine and remarked rather incredulously, "So this is where you do your experiments?"

BF/MH/DH: *Your major contributions have been to chemical dynamics. That started during your postdoctoral work with Richard Wolfgang at Yale.*

ZH: I finished my thesis in 1963, and at that time Dick Wolfgang visited Prague and offered me a postdoctoral position. Czechoslovakia was enjoying a political thaw in 1963—it was the era of Khrushchev—and several members of our Institute already had obtained leaves to work abroad. After a twelve-month hassle, I too got a one-year leave-of-absence and flew to the United States in 1964. It was my first trip to the West, and coming to America and to Yale was a great new experience. The flavor of the Ivy League University and of the Sterling Chemistry Laboratory, the exciting seminars both in chemistry and physics, the immense kindness and helpfulness of Dick, the friendly international community of graduate students and post-docs—all was new, and I absorbed the atmosphere feverishly. Steve Berry was still at Yale; Lars Onsager, Harry Wasserman, and others were helpful and kind; and Colin MacKay made frequent visits.

By that time, Dick Wolfgang and Sherry Rowland had already achieved a substantial understanding of hot-atom chemistry, using macroscopic, nuclear-recoil techniques. Dick was considering a new approach to hot-atom chemistry using beam techniques. This I was to have started. Considerable excitement was beginning to be generated from the neutral−neutral reactive scattering experiments at thermal energy. The logical first step was to develop a reactive scattering experiment for ion−molecule reactions, and so I began to build EVA for that purpose. EVA—Dick derived the acronym from "electron-volt apparatus" to complement the Yale Bevatron, which was housed next door—was small and inexpensive, could work at laboratory energies down to 0.5 eV, and was delivering angular distributions within the year. We realized that we also had to measure the velocities of the products. At the end of the year, I was forced to return to Prague; but within a year, I was able to return—with an energy analyzer in my pocket, made in Prague by our master machinist Mr. Protiva (Figure 6).

Figure 4. Vladimír Čermák.

Figure 5. Vladimír Hanuš.

With a phase-sensitive detector installed on the machine, we (ZH, Dick Wolfgang, assisted by Tim Rose and Jim Kerstetter) achieved, within a few months, the necessary energy and angular resolution to construct contour diagrams for $N_2^+ + D_2 \rightarrow N_2D^+ + D$. (As a weird coincidence, many groups at that time chose that reaction as the first one to study—an aspect of which Lew Branscomb has termed "intellectual phase-locking"—but, as a four-atom system where the vibrational energy content of the N_2^+ was ill defined, it was not the best choice!) Further stimulus was provided by Dudley Herschbach, who visited Yale to give a special lecture series on scattering (called "Harvard Lectures"), and by Jim Cross, who arrived to join the Yale faculty.

BF/MH/DH: *What new results did EVA produce?*

ZH: EVA was firstly a technical achievement, providing the first means to study the chemical dynamics of ionic reactions at low energies. Unexpectedly, the first scattering patterns found for ionic reactions resembled those found previously for neutral reactions. We distinguish between neutral reactions, occurring *with* controlling energy barriers on a *repulsive* surface, and ionic reactions, occurring *without* controlling barriers on an *attractive* surface ... yet similar patterns of reactive scattering were observed for both. For the ionic case, the much touted, long-range, attractive r^{-4} potential simply did not make a qualitative difference.

Specific results of interest included the following. The simpler three-atom system $Ar^+ + D_2 \rightarrow ArD^+ + D$, occurring on an attractive surface, proceeded by a direct mechanism, even at the lowest energies. Of more interest to organic chemists, the *condensation* reaction $CH_3^+ + CH_4 \rightarrow C_2H_5^+ + H_2$ (involving the making of C−C bonds) proceeded by a *direct* mechanism, even at the *lowest* energies. John Weiner subsequently showed that hydrogen scrambling occurred during this direct reaction—an unsurprising finding (in retrospect!) since the rotation of the collision complex is slower than the C−H vibrations which drive the scrambling. The "simplest" chemical reaction between $D^+ + H_2$ was far from simple; its non-adiabatic character prompted the development, by Sally Chapman, Richard Preston, and John Tully, of a trajectory surface hopping model.

During that period I was dividing my time between Dick Wolfgang's group and Prague. My third visit to the United States, in the spring of 1968, was to the University of Colorado at Boulder, where Dick's group had moved. It was the time of the so-called Prague Spring; my family was able to accompany me for a whole year; and it was one of the happiest periods of my life. The friendly atmosphere of the Boulder Chemistry Department, combined with the Joint Institute for Laboratory Astrophysics (with Dick Zare in residence and Dudley Herschbach and Christoph Ottinger as visitors) and the National Bureau of Standards laboratory (Eldon Ferguson and his group), produced an unforgettably stimulating research environment. Our group included Pete Hierl, John Tully, Al Lee, and John Krenos. Because of different personal schedules, we were able to study the dynamics of complex formation around the clock. Since that time, I have cherished Boulder as a second home. To visit friends there is always a special privilege.

In August 1968 the Russians overran Czechoslovakia, crushing the "Prague Spring". For a while it seemed that we might not be able to return. Eventually we did, prompted by strong family ties back home and by long conservations with Čermák, who visited Boulder in 1969. During that period, several people offered me facutly positions in the United States—Sherry Rowland at Irvine, John Magee at Notre Dame, Stan Gill at Boulder, Howard Taylor at Southern California, and George Springer at the University of New Mexico. Their support and the constant help, stimulation, and friendship of Dick Wolfgang and colleagues at Boulder during that difficult period I will never forget.

BF/MH/DH: *Back in Prague, in spite of the repressive Communist regime, you somehow managed to re-create another EVA to continue the research program.*

ZH: The political consequences were serious for the Institute of Physical Chemistry in Prague. Many colleagues emigrated (quite a few of these are contributors to this issue). Professor

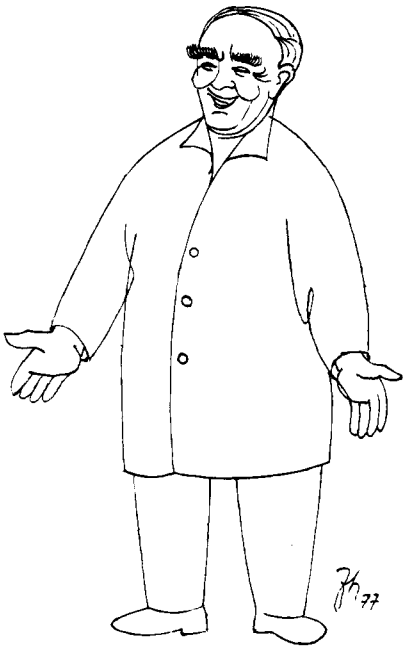

Figure 6. Josef Protiva.

Brdička, who had provided the Institute with political protection, died, and in 1972, it was merged with the Institute of Polarography. After the purges of 1972 Čermák lost his position as department head and I was forbidden to travel abroad for three years. I began to lose contact with the United States, and the loss was compounded by the trajedy of Dick Wolfgang's death in 1971.

Working with a new graduate student, Vojtěch Pacák, I designed and built the Prague crossed-beam machine EVA II, which produced its first data in 1971. As the political climate deteriorated, we endeavored to maintain a microclimate of scientific productivity and, to this end, were much aided by several visitors under various exchange schemes—Keith Birkinshaw and Peter Hierl for longer visits and Jean Futrell, Mike Henchman, and Andy Yencha for shorter ones.

Some new research topics were started, including (i) the mechanism of charge transfer—previously unexplored and a technical challenge for a crossed-beam experiment because of the need to detect thermal product ions—for both simple diatomic systems ($Ar^+ + NO$) and for complex polyatomic ones ($Kr^+ + CH_4$); (ii) the so-called (20/11) process (exemplified by $Ar^{2+} + He \rightarrow Ar^+ + He^+$) where the products are subject to a Coulomb field; (iii) collision-induced dissociation; (iv) the adiabatic, endoergic process $H_2^+ + He \rightarrow HeH^+ + H$, which was modeled in collaboration with Lutz Zülicke and his group; (v) the influence of electronic energy on differential cross sections and branching ratios, as in $B^+(^3P) + H_2 \rightarrow BH^+(^2\Sigma, ^2\Pi) + H$; and (vi) "simple" proton-transfer reactions, such as $CH_4^+ + CH_4 \rightarrow CH_5^+ + CH_3$, which are neither simple nor even proton-transfer reactions.

After 1975 the situation improved marginally. I was allowed to make about one foreign visit a year. Even though I never requested financial support from the Czechoslovak government, only one request out of eight was ever granted. But the adverse situation strangely had its positive side. We practiced "small budget research" because we had no choice. We had one or two simple machines for which we built the mechanical and electrical components ourselves (thanks to the "golden hands" of Mr. Protiva and his colleagues). We generally had a stock of spare parts, we did our own repairs, and we could rebuild components if necessary.

BF/MH/DH: *You have played a unique role in revealing the chemical dynamics of ionic reactions. The keystone of this achievement was your collaboration with Richard Wolfgang, who obviously contributed his sense of the useful problems and systems to be studied, his uncanny intuition in interpreting experimental results, and, last but not least, the resources to build EVA and staff its operation. As Dick was first to acknowledge, your contribution was truly unique—the design and construction of EVA and the coaxing from her of the all-important data.*

This raises an important issue of general interest. To what influences can your development as a physical chemist be attributed? To what extent did the extraordinary intellectual climate of Czechoslovakia play a role? How did physical chemistry develop in Czechoslovakia?

ZH: The first lectures in physical chemistry were given in Czechoslovakia by Professor F. Wald (1861−1930), who had been appointed at the turn of the century to the Technical University at Prague on the recommendation of the "father of physical chemistry" Wilhelm Ostwald of Leipzig. Like his illustrious sponsor Ostwald, Wald taught mainly thermodynamics and was skeptical about the existence of atoms. Systematic research in physical chemistry was initiated by Jaroslav Heyrovský (1890−1967), who took his Ph.D. with F. G. Donnan in Liverpool, habilitated at the Charles University in Prague, and became a Professor there in the mid-twenties. His early work on the dependence of the surface tension of mercury droplets in solution on the potential applied led him into electrochemistry and to the discovery and development of polarography. Between the wars his institute became internationally famous, drawing many students and visitors from abroad. His colleague and friend Václav Dolejšek (1895−1945) was a well-known spectroscopist who had worked with M. Siegbahn and F. Paschen: from his research laboratory in physics, much of modern Czech physics derives. Heyrovský's and Dolejšek's laboratories were adjacent: in Czechoslovkia from the beginning, physical chemistry developed in close contact with physics.

Within the young Czechoslovak Republic, which had been established in 1918, there was a remarkable respect for education and science. Its first President, T. G. Masaryk, was a philosopher and a university professor; when famous scientists, like Marie Curie or Paul Langevin, visited Prague, they would meet with him.

This was terminated when the Nazis overran Czechoslovakia in 1939. One of the first acts of the Nazis was to close the universities. Professor Dolejšek perished in a concentration camp. After the end of the war, Professor Heyrovský and his pupil Rudolf Brdička (1906−1970) re-established physical chemistry and electrochemistry at the Charles University. What they managed to achieve—good teaching, systematic research, lively seminars with many visitors from abroad—is still remembered with nostalgia by those fortuante enough to witness it.

BF/MH/DH: *You work in the area of chemical dynamics. How did this evolve from a tradition of electrochemistry?*

ZH: Even though the school of electrochemistry in Prague was internationally recognized, both Heyrovský and Brdička encouraged their students to develop new interests in science.

Thus in the early fifties, it was in the Institute of Physical Chemistry that Čermák and Hanuš built a mass spectrometer (aided by Čestmír Jech, who later returned to his research on radiochemistry); Jaroslav Koutecký began his work in quantum chemistry; as did Ivo Kessler and B. Matyska in polymerization catalysis; while J. Koryta, P. Zuman, J. Kůta, A. Vlček, and others developed the program in electrochemistry. At the Technical University, many active research programs were

Figure 7. Rudolf Zahradník.

started—in chemical engineering thermodynamics, combining excellent courses in physical chemistry, by G. Standard, an American émigré, aided by E. Hála, E. Erdös, and B. Reiser (all later fired for political reasons, with the last two subsequently joining our institute); in polymer chemistry by Otto Wichterle, leading to the development of the soft contact lens (purged in 1958, he founded his own Institute of Macromolecular Chemistry at the Academy of Sciences); in natural product chemistry by F. Šorm; in infrared spectroscopy by Joe Plíva (Jiří Jonás was one of his first students), who subsequently joined our institute. Later at our institute, research in catalysis was started by Kamil Klier, Zlatko Knor, and Vladimír Ponec. Joe Paldus and Jiří Čížek (former students of Koutecký) joined the theoretical group, while Rudolf Zahradník (Figure 7) arrived in 1960 to develop organic quantum chemistry, and Joe Michl was one of his first students.

In the early sixties, the Institute of Physical Chemistry, which had been moved to more spacious premises in a town house in Máchova Street, was a remarkable place. Shielded from the somersaults of the Communist regime by the authority of its director Rudolf Brdička, it was an oasis of creative research in congenial surroundings. Although the budgets were low, foreign travel almost impossible, and foreign visitors few, the scientific enterprise was productive and morale was high. The 100-odd members of the Institute enjoyed a fine social atmosphere of parties, trips, and the annual highlight—the soccer match against the Institute of Polarography, with Professor Heyrovský starting the kickoff (he never forgot his top hat for the occasion) and Professor Brdička as a highly active halfback.

As the sixties came to an end, the situation changed drastically. Heyrovský and Brdička died, and with them the protection Brdička provided for the Institute. And the Russian takeover drove many members of the Institute permanently abroad.

BF/MH/DH: *Thanks to those of you who stayed, the Institute survived and continued to be productive under appallingly difficult circumstances. How did you accomplish that?*

ZH: In 1980 I was granted permission to accept a Joint Institute for Laboratory Astrophysics Fellowship. As I was leaving for Boulder, Vladimír Čermák died. He had been widely respected but at no place more than Boulder. My collaboration there with the groups of Carl Lineberger and Steve Leone was wonderful; my family was able to visit for the summer; and reunited with old friends, we had an especially happy time.

Back in Prague we continued to be thwarted by political restrictions. I was allowed only one graduate student at a time (but was fortunate indeed that these graduate students were Břet'a Friedrich and Honza Vančura!). Because the regime was paranoid about "the exposure of the young generation to unwanted influences", I was not allowed to teach at the university: finally, thanks to my good friends, Jiří Dvořák and Jiří Vacík, professors of physical chemistry at Charles University, I was able to teach there an advanced course on reaction dynamics, which kept me in touch with students. I was debarred from habilitating, and only after twelve years, was I allowed to apply for and be awarded the degree of Doctor of Science.

During that time, I developed the laboratory program at home in Prague and collaborative projects with several groups abroad, facilitated by short, productive visits—with Inosuke Koyano at the Institute for Molecular Science on the reactions of state-selected reactants, with John Beynon's group at Swansea to develop our study of doubly charged ions, with Marie Durup-Ferguson on the reactive dynamics of negative ions. I maintained contact with Peter Toennies, Christoph Ottinger, and Jürgen Troe in Göttingen. I participated in the Russian Phobos Space Mission and served on an ongoing International Atomic Energy Agency committee in Vienna with Mitio Inokuti.

BF/MH/DH: *And then in 1989, finally and unexpectedly, the wall came tumbling down. To be free again after 50 years seemed a miracle, the answer to all your problems. What has been the reality?*

ZH: The scientific life of the country had to be reconstructed. Hitherto deprived of administrative responsibilities, I was suddenly overloaded with them—Deputy Director of the Institute, Chairman of the Collegium of Chemistry (awarding doctoral degrees), Deputy Chairman of the first agency to award grants in the country, and eight others. This left little time for the laboratory (now ironically filled with students).

By 1992, the initial atmosphere of enthusiasm and cooperation within the country had started to abate. Slovakia split off, and the Czechoslovak Academy of Sciences became the Academy of Sciences of the Czech Republic. I was able to return to the laboratory, satisified that some of my administrative efforts had been fruitful and grateful for the privilege of having been able to work with the then President of the Academy, Professor O. Wichterle.

In 1992 I was awarded an Alexander-von-Humboldt Research Prize to spend a year in Göttingen with Peter Toennies and Christoph Ottinger. By spreading this award over several years, I continue to work there while maintaining my research program in Prague, now supported by several modest research grants. Collaborations abroad include Göttingen, Chicago (with Steve Berry), Innsbruck (with Tillmann Märk and Werner Lindinger and where I am an Honorary Professor), and, through the European Union Networks, with Orsay, Munich, and Chemnitz. I have finally been allowed to habilitate, and with the recent invitation to become Professor of Physical Chemistry at the Institute of Chemical Technology in Prague, my dreams of combining research with graduate and undergraduate teaching are about to be fulfilled. Who, in the coming years, could wish for more?

Břetislav Friedrich
Harvard University

Michael Henchman
Brandeis University

Dudley Herschbach
Harvard University

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Scientific Collaborators

Graduate Students		Diploma Students	Stipendists
V. Pacák (1977)	L. Brabec (1993)	J. Pátek (1972)	O. Votava (1990)
D. Šťulík (1980)	M. Fárník (1995)	M. Sadílek (1988)	J. Flieder (1990)
B. Friedrich (1981)	J. Polách	M. Fárník (1990)	
J. Vančura (1988)	J. Žabka	P. Žďánská (1995)	
M. Sadílek (1992)	L. Mrázek	L. Mrázek (1995)	

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