# **MEMOIRS**

# זכרונות

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#### Preface

This year, we are celebrating the 60<sup>th</sup> anniversary of the establishment of the State of Israel. This celebration also recognizes Israel's unique achievements in scientific discoveries, which were the direct result of the research activity of world-class motivated researchers who came from completely diverse origins.

In this regard, the Schulich Faculty of Chemistry at the Technion–Israel Institute of Technology can be seen as the microcosm of the State of Israel.

Since its inception, the Department of Chemistry, through its members, was motivated to create and nurture a lively, vibrant and successful research atmosphere.

To honor those pioneers who shaped our future, I took it upon myself to edit this book and to bring together, in a single volume, these very touching biographies, including in some cases, a short description of their academic careers. I didn't change a word of what you are going to read. Some biographies are still missing and I do hope that a second edition will appear in a not too distant future.

Finally, I would like to express my sincere and deepest gratitude to all our Professors Emeriti who agreed to write their biographies as well as to Shimona Ginsburg, Ruth Ben-Ishai and Wanda Folman, Dorit Ringart, Orna Kohavi, Ari Folman, Alon Hoffman, Yafa Kosirowski, Aviva Lubetzky who wrote about the late David Ginsburg, Dov Ben-Ishai and Mordecai Folman respectively and Sabine Cioara for her constant and precious help.

> Ilan Marek Schulich Faculty of Chemistry Technion-Israel Institute of Technology July 2008

Since the publication of the first edition in July 2008, new chapters are constantly added by new Emeritus Professors.

#### The History of Chemistry at the Technion - Israel Institute of Technology

Abstracted by Kochava Reznik from Carl Alpert,

"Technion – The Story of Israel's Institute of Technology", American Technion Society, 1982

#### C.A. p. 115

The course of instruction was set at 38 hours per week, with an additional 6 hours optional. The full curriculum for the first year comprised the following courses:

Mathematics, Physics, **Chemistry**, Mineralogy, Technical Mechanics, Descriptive Geometry, Drawing and Modeling, Building Construction, Building Materials, Geodesy, Building Plans, Architecture, Highways and Railways, Hydraulic and Sanitary Installation, Job Estimating and Management, Bookkeeping and Fundamentals of Business, Languages (Hebrew and English), Hygiene and First Aid, Physical Exercise.

The first chemist who joined the Technion staff was Prof. A. Ilioff, who came from the Polytechnicum of Kharkov. He was both chemist and geologist. Prof. Ilioff was over sixty when he came. He knew no Hebrew, but was engaged with the understanding that he would work hard at acquiring a knowledge of the language. In the meantime he had all his lectures translated into Hebrew, which he then laboriously transcribed into cyrillic letters and he read the material phonetically to the class. He never believed that his students understood a word of what he was saying, and so he would go over the material thereafter, illegally in Russian. Since most of the students were from Eastern Europe, they managed to understand. One day it was explained to Ilioff that a new student was from German speaking Danzig, and he did not know Russian. Ilioff reassured them. "Don't worry, he will!" And he did!

Prof. Ilioff produced a hand-thrown bottle bomb which was known as the Ilioff cocktail. It was mass production in the war of independence, and the Syrian tank halted at the perimeter of Kibbutz Degania is said to have been a victim of the device.

Later came a demand for an instrument that could throw the Ilioff Cocktail and other small bombs a considerable distance.....

In still another field, Shimshon Ben-Tur (Turetzky), of the Chemistry Department, created mixtures which when ignited in a small "leben" cup, produced a smoke screen.

Prof. Ilioff retired in 1940, but continued his academic activities in the Technion to his last days. He died at the age of 82 in 1947.

Prior to the opening of the Technion, Chemistry was mentioned as a subdepartment to the Engineering Department.



**Yitzhak Kalugai**, whose interest with the Technion started while he was still serving as an industrial chemist with the Starr Chemical Laboratories in the U.S.A., joined the Technion staff sometime around 1939-40. He began teaching chemistry in the evening classes and served as assistant to Prof. Illioff. He retired in 1956 with the rank of professor.

C.A. p. 176A laboratory for industrial chemistry was opened in 1936.C.A. p. 190

In the cloudy months of 1938 and 1939 no one could have predicted what was to come. Even against mounting, ominous signs, perhaps in unconscious preparation for vital tests ahead, the Technion continued to grow.

The new hydraulics laboratory building, constructed with the help of a Government grant, was completed and put into use. The Institute's academic program was organized in tree departments: Civil Engineering, Architecture and Industrial Engineering, with the latter providing speci alization in electro-mechanical and chemical engineering. The student exercises as well as research were carried out in 11 laboratories: Building Materials Testing Hydraulics, Electrical, Heat Engines, Metallography, Metrology, Industrial Chemistry, General and Inorganic Chemistry, Physics, Physical Chemistry an Soil Mechanics.

A new academic constitution was formally approved on January 24, 1939, and the first Council of Professors was set up (later to be known as the Senate) composed of Prof. Ilioff, etc....

#### C.A. p. 204

As early as June, 1941, in response to inquiry from the Chairman of the Jewish Agency Executive, **David Ben Gurion**, Technion could list specific work being done in the laboratories for the war effort. The report singled out:.....

Chemistry Laboratories: producing active carbon from local materials, to be used in gas masks; material for pharmaceuticals; improving the quality of the flour made from domestic wheat; study of Huleh peat for fertilizer or fuel; studying methods of substituting sodium sulphate for sulphuric acid, which was unavailable. .....

#### C.A. p. 206

The Chemical Laboratories, too, were deeply involved. Of the long and varied activities, special mention was made of the following: development of a substitute for sulphuric acid for accumulators; preparation of industrial and cosmetic emulsions; research on aniline dyes and dyeing; development of tanning materials from Palestine herbs and trees; solidification of benzine; routine test work on food, metals, agricultural supplies; investigation on the corrosion of iron tubes in water supply lines and the methods for prevention; electrolytic production in a pilot plant of persulphates for both flour and textile industries. Other investigations included methods for

electrolytic production of cooper tubes and wire; methods for making aromatic compounds by treating carbide, without which no welding could have been done in Palestine.

#### C.A. p.215

Important academic changes were instituted with the opening of the 1945-46 school year. The Department of Industrial Engineering was divided into two independent units' Mechanical-Electrical, and Chemistry. Together with Civil Engineering and Architecture, the Technion now had four departments.

#### C.A. p. 232

On January 26, 1948, Dr. Kaplansky paid tribute to the American friends who had provided \$400,000 thus far and announced that the money was being used for the construction of two chemistry laboratories, one for analytical and microchemistry, and the other for physical chemistry. These were the first of the laboratories in memory of Brigadier Kisch.

#### C.A. p.241

In the meantime in 1950 two Americans were added to the staff, the first of a large number to follow. The were Prof. Menahem Merlub-Sobel, in Chemical Engineering, and Prof. Judah L. Shereshefsky, who served as Visiting Professor in Physical Chemistry.

#### C.A. p.265 & 266

In 1953, at the opening of a new era the Technion had five Faculties and Departments: Civil Engineering, Electrical Engineering and Chemical Engineering, the latter previously known as Industrial Chemistry. The new academic development of the Institute was in breath as well as in depth. Until 1951 the basic sciences had been taught only as service courses to the engineering disciplines, and it was difficult to obtain good faculty members under such circumstances. In that year it was decided to set up a separate Faculty of Science comprising four divisions: Mathematics, Chemistry, Physics and Mechanics. Degrees were to be offered in these subjects, research would be fostered, and the sciences would be taught, as ends in themselves, not merely to meet the basic requirements of the other departments. In explaining the innovation at the graduation exercises in January, 1952, Gen. Dori emphasized that the sciences are "the foundation stones of technology"

and declared that they must "take the first place in the whole system of instruction and research".

C.A. p. 268

To prevent academic inbreeding, additional academic personnel were brought in from abroad. In 1952 the Dori-Goldstein administration arranged to bring to the Technion from the U.S. Prof. Nathan Rosen in Physics; Dr. William Resnick and DR Jacob M. Geist, Chemical Engineering; Dr. Otto Schnepp, Physical Chemistry; and Dr. Arrigo Finzi, originally from Italy, Mathematics.

In 1954 **Prof. David Ginsburg** was appointed as head of the Department of Chemistry to be established.

#### C.A. p. 275

Better progress was made in other directions. By the end of 1954 some 50 students were already registered for graduate studies, and the Graduate School was in effect reality.

The first ceremony held by the Technion to grant D.Sc. degrees, took place on 15/11/55 in Cinema Armon. Out of four graduates, two were of Chemistry - **Michael Cais** (Organic Chemistry); and **Mordechai Folman** (Physical Chemistry). Both acquired later the status of Professor, and both acted at different periods as Chairman of the Department of Chemistry. Because of the Sinai war in 1956, only two years later, in November 1957, a second ceremony took place and two more chemists were granted the D.Sc. degree. **Magda Ariel**, who later was also granted the rank of a Professor and also acted as Chairman of the Department of Chemistry, and **Naphtali Steiger (Shafrir)** who later joined the Nuclear Science Unit in the Technion. On both occasions a number of M.Sc. degrees in Chemistry were also granted.

#### C.A. p. 309

In the fall of 1962, Prof. David Ginsburg, in addition to his being Chairman of the Chemistry Department, was appointed as Dean of Students.

BIOGRAPHIES

## **Dov Ben-Ishai** Born Israel, 1923-1993



**Ph-D thesis**: Hebrew University of Jerusalem and Weizmann Institute of Science, 1951

**Post-doc position**: University of Illinois, 1953-54

#### **Technion Position**:

Senior Lecturer, 1957-61 Associate Professor, 1961-Professor,

#### Sabbatical leaves:

Massachussetts Institute of Technology, 1964-65 University of Illinois, 1971 Massachussetts Institute of Technology, 1972

#### Major departmental responsibilities:

Dean, faculty of Chemistry, 1962-63

**Field of Research** *Chemistry of nitrogen heterocycles, amino acids, enamides and acylimines* 

#### **DOV BEN-ISHAI**

1923-1993 (by Ruth Ben-Ishai) March 2008

Dov passed away on November 2<sup>nd</sup> 1993, 3 weeks before his 70<sup>th</sup> birthday.

I, Ruth Ben-Ishai, his spouse of 45 years am writing this memoir. I did not know Dov until his 20<sup>th</sup> birthday, and the information I have about his roots, childhood and adolescence, is from a booklet his father wrote after the tragic death of his younger brother Akiva, and also from talks with his sister Alisa. Dov's picture albums served as a wonderful source of his days as a youngster in the Haganah and later in Jerusalem and at the University.

Dov's scientific research is being reviewed by Dr. Janina Altman, who had concluded her Ph.D. thesis under his guidance and subsequently was a frequent Research Associate in his laboratory. I could not have found anyone better suited for this difficult endeavor, in view of their mutual scientific interaction and collegiality. I thank Janina with all my heart.

To understand a person it is important to know his roots and the environment in which he grew up. Dov is a descendent of the Morderchai Katchalsky family from Bialistock. His father, David, was the youngest of 5 sons and two daughters. The atmosphere in their home was that of religious Jews with consciousness of Zionism. Dov's father 's dream was to join the Pioneers in Palestine and he was the first of his family who made Aliya in the year 1921 at the age of 21. He and the other Chalutzim of the 3<sup>rd</sup> Alija were naïve youngsters who literarily came to build and be rebuilt in Eretz Israel. At first they worked on the land in what were called Plugot Avoda, and then in paving roads and as watchmen "armed" with an iron rod. David who had grown up in Bialistock and was of light complexion, was unaccustomed to the heat, the long hours of physical work, the poverty, mosquitoes and malaria, but – despite the hardship – he stayed with the Pluga until they dispersed.

On Friday, 3<sup>rd</sup> of November 1922, David married Dov's mother Jocheved. Her father, Reb Jakob Jehoshua Carmin, was a *talmid chacham* and enthusiastic Zionist. He taught in what was called a modern Cheder in which Hebrew was taught in Hebrew. It is hard to believe but when he heard that a Hebrew High School was going to be opened in Jaffa –Tel Aviv he sent his 15 year old son Joseph all alone to this school, and Joseph was a graduate of the first class of the Herzlia gymnasium. During the first world war Joseph served as an officer. in the Turkish army and afterwards he studied and obtained a Ph.D. degree in Biology in the USA.

In 1921 Reb Carmin sent three of his daughters, with Jocheved among them, to Palestine. The Arab uprising started while they were on their way and the British High Commander Herbert Samuel stopped all immigration to Palestine, so the three young women were stranded in Alexandria for three months. After arriving in Palestine, Jocheved first worked in the graveyards of Rehovoth and, after moving to Tel Aviv, in a factory for candles.

After their marriage David and Jocheved settled in a hut (*tzrif*) in the Neve Shaanan quarter of Tel Aviv, and were later joined there by the rest of the Carmins. At that time they decided to change their family name to a Hebrew name and chose Ben-Ishai. Their family life started in poverty and hardship, David, who had been an accountant in Bialistock, had to find a different occupation, so they opened a small shop in which they toiled from morning till night. In spite of their hardship they felt that their dream of life in Eretz Israel was being fulfilled. They worked in their shop for about 20 years, managing to make a living and providing education for their children. At the age of 52 Dov's father became a conscientious civil servant in the



governmental department of agricultural statistics and Jocheved became a volunteer in Yael - an organization of women helping in hospitals.

Their eldest son Dov was born in 1923 and the twins Alisa and Akiva followed two years later. From the picture album one can see that during his first years Dov was a thin, small boy; he was very active and called Dovele by everyone; this nickname stuck to him until we moved to Haifa (*Enclosure 1*).

Enclosure 1

Dov went to elementary school in the "School for Boys in Achad Haam Street", in Tel-Aviv. Years later he still spoke with affection of the 8 years he spent in this school, especially of the quality of the teachers and their devotion.

Dov excelled in Mathematics; therefore, his father thought that he should become a merchant and enrolled him in the Geula Gymnasium of Commerce.. The twins went to the Herzliah Gymnasium.

Dov did not show much interest in Commerce and was more active in sports than in studying. Without the knowledge of his parents and with the cooperation of his brother he became a member of the Maccabi Tel-Aviv Youth Football Team, and sneaked out every Saturday to join the team. Apparently he was very good, because years later he was still remembered by professional players.

While in High School Dov had already joined the Haganah, and volunteered each summer to help at some settlement. After matriculation it was a custom to spend a year of National Service (*Schnat Sherut*), and he chose to become a Ghaffir, a member of the Jewish Settlement Police Force. He was stationed in Kfar Hachoresh and as a Ghaffir was given a uniform and a rifle to protect the nearby settlements.



I am in possession of the letters that Dov wrote to his parents every week, in which he described the life of a Ghaffir and their interaction with the Jewish settlements and the British commanders. He worked in the fields and, when on night shift, fed the cattle and cows. One of the letters was written on Pay-Day: it tells about the British Sergeant who gave him his salary of 71/2 lirot (Palestine Pounds). From this  $2\frac{1}{2}$  *lirot* were deducted for food and 200 mils for laundry. He thought himself rich being left with 5 *lirot*. Throughout his life money did not play a dominant role in his decisions and he was satisfied with whatever he had. A picture of Dov as a Ghaffir is shown (*Enclosure 2*).

In 1943, after being released from the Police Force, a major decision had to be made as to his future. Whenever the family had to make an important decision Dov's father consulted his eldest brother, Jehuda Katchalsky, in Jerusalem. Jehuda was the father of Aharon and Efraim Katzir who were already graduates of the Hebrew University. Efraim convinced Dov to enroll at the Hebrew University and study Chemistry as his major subject and Physics as his minor. I have the impression that at that particular time, with the Second World War in progress, Dov would have preferred to join the Palmach or the Jewish Brigade.

The faculty of Chemistry of the University was in Jerusalem on Mt. Scopus and it so happened that Dov and I were in the same class of about 20 students. The studies in Chemistry lasted 5 years and led directly to a MSc. degree. The conditions of our studies at the University were in some respects ideal.

During the first and second year, our lab work consisted of qualitative and quantitative inorganic analysis, taught by Prof. Bobtelsky; each student was given his own laboratory bench and could perform the analyses at any time when not attending lectures, the lab bench was like a second home. During the first year we had one lecture course in Physics taught by Prof. Shmuel Sambursky, who was an excellent lecturer. Organic Chemistry was taught by Moshe Weizmann, the brother of Chaim Weizmann, a pleasant man who tried to help students even during exams. Of the prominent teachers, I wish to mention Prof. Ladislaus Farkas who taught Physical Chemistry; he was a poor lecturer but an excellent scientist. Unfortunately, he was killed in an airplane accident. Biological Chemistry was taught by Prof Fodor, who was among the few lecturers who had come to the University as a Full Professor. However, he had several fixed ideas. Thus, he dismissed the possibility that enzymes were proteins, and we had to study material that – even as students – we knew to be outdated and better forgotten after the exam.

Among the younger scientists, our class adored Aharon Katzir; he was one of the best lecturers we had and knew how to stimulate the students, talking with them not only about chemistry but also about philosophy, art and biology. Under his influence the book of the physicist, Schroedinger, on "What is Life" was read and discussed by our class.

Dov worked on his Master's thesis in Prof. Frankel's laboratory, more or less under the guidance of Efraim Katzir. It was no pleasure working in Frankel's lab, because he was a strange, secretive person who feared competition by his own students and kept everything locked up. When there was a scientific problem to be discussed, he asked Dov to come with him to get the mail, because he could not find any other time to talk with him. Luckily, he invariably left the lab at 3 o'clock and then research could get started.

Our class was together in most Chemistry laboratory courses. Those who took Physics or Microbiology as minor subjects were taught these subjects separately. On the whole, we students interacted well and helped one another in our studies, as well as socially. Among us were two middle-aged members, one was Igek Slomnitzki from Beth Alfa who, after being an expert in growth of vegetables for years, decided to get a formal education. The other was David Rothstein, who had been the founder of the citrus factory in Ashdot Yaakov. Both of them especially Slomnitzki, contributed a lot to the atmosphere that prevailed. For example, the students at the Hebrew University belonged to several political parties. When the time came for election of the students' representatives, the 40 or 50 year old Slomnitzki – with several others – decided to create a new party which they called "The Young Student". If I remember correctly the "Young Student" party got the majority of votes. I do not remember exactly who were elected as representatives, but Igek and Dov were among them.

Among the students of our class, Israel Gottlieb and Miko Wechsler were both killed during the unrest of 1948 and I shall write of them below. There were also two students from Egypt, one, Jaqueline Cataui, a very good looking aristocratic young woman, who did not mix with the rest of the crowd and who completely disappeared after our third year, and Raffi Pappo, an excellent organic chemist who worked with David Ginsburg at the Weizmann Institute after the War of Independence. When David moved to Haifa, Raffi emigrated to the USA and became a senior chemist at Searle in Chicago. He now lives in Palo Alto. Of our class Shulamith Lev Goldhaber and Michael Glaubman became full professors at distinguished universities in the USA. Zvi Sommer, another student of our class, also emigrated to the USA. During his study Zvi roomed with Dov in Schhunat Hamekasher (a picture of Dov and Zvi in the physical chemistry lab is enclosed). Zvi was among the founders of the chemical factory "Agan" in Mottza. In the beginning they were unsuccessful, so Zvi sold his shares and emigrated to the USA. He worked as a government employed Chemist in the vicinity of Washington and I imagine that he must have regretted leaving Agan which is now Machteshim-Agan, a multi-million dollar factory. Also in our class were Nechemia Sternberg, Jehuda Mazur and Mela Pecht, all of whom were for many years scientists at the Weizmann Institute. Another member, Ruth Sakalik, was a leading Chemist at Nes Ziona (enclosure 3 shows a class picture with Prof. Moshe Weizmann, enclosure 4 shows Dov and Zvi in the physical chemistry lab and enclosure 5 shows Dov studying).

Enclosure 3



#### Enclosure 4



The Haganah took up much of Dov's time, as it was hard to resist their claim that "the Yishuv needs you". Already during his first years at the University he disappeared

twice to take part in a "Mem Kaf" (squad commander) course, to become an instructor in topography.

Enclosure 5



In about 1945-46, what was to become Hemed (Hel Madah – Scientific Corps) began operating. In Efraim's laboratory, Dov and the other students prepared chemicals, which the Haganah required. These activities had to be done in secrecy after Prof. Frankel

left the lab. Dov used to tell of a dialogue between Efraim and Frankel, in which the latter asked "if you are told by the Haganah to kill somebody, would you kill him?", to which it was said that Efraim replied, "Of course, Professor Frankel". Since then Frankel never asked what was being done in his lab.

I remember two episodes connected to Chemed at this time: In one, Dov and Efraim were busy preparing some chemicals and finished late in the evening. On this particular day Etzel killed some British soldiers in Jerusalem and the British imposed a curfew. Dov and Efraim did not know about the curfew and walked home late in the evening. On their way they were arrested and transferred to the Kishley - the Jerusalem jail, which was located in the old city. It was one of those freezing nights in Jerusalem, and the two heroes kept walking back and forth all night to keep warm.

During Dov's work for Chemed he had to prepare a chemical known as 2-4-D. One of the intermediates in the synthesis had a terrible smell, which stuck to him. When Dov came to meet me at the Brazil coffee shop, the waitress would announce, "Oi, the smell comes." Even Prof. Weizmann asked him at that time to get rid of the smell.

These are amusing anecdotes, but the situation during out 4<sup>th</sup> and 5<sup>th</sup> year of study was grave, especially after the United Nations' approval of the partition plan on November 29, 1947. Unrest started in various parts of Jerusalem, and Dov with many other students were often called up by the Haganah. The first major disaster occurred on January 15, 1948, when thirty five of their fellow students were murdered on their

way to bring supplies to Kfar Ezion. The 35 set out on foot but were detected and after a fierce battle with hundreds of Arabs were brutally killed. Waiting for a sign at the Haganah meeting place, Dov already knew that they did not reach Kfar Ezion and feared for the worst – he knew all the students and their commander Danny Mass, and never forgot this day.

The next disaster occurred when a convoy to Hadassah Hospital and the Hebrew University was attacked. It was becoming dangerous to travel to the University and on April 13, 1948, a convoy to Mount Scopus was organized. The convoy was attacked by the Arabs and the buses were set on fire. We saw the attack and fire from the city but were unable to help. About eighty persons were killed and among them Mico Wechsler of our class. He was a gifted student, liked by everyone and a good friend - he can be seen in the front row of *Enclosure 3*.

After the tragedy of the Mount Scopus convoy all teaching stopped and Dov was send to become a "Mem Mem" (commander of a platoon) in the Haganah. The course was the first of its kind and took place in Juara, conducted by Chaim Laskov a tough commander, who later became chief of staff of the Israeli army. It was considered a privilege to be a Mem Mem in the Haganah.

When Dov returned to Jerusalem the city was already under siege. After the declaration of Independence on May 15, 1948, shelling of the city by the Arab Legion began, and it was dangerous to walk the streets of Jerusalem. It was also hard to get food and sometimes the only meals we got were "Grissei Pninim" (goats), provided that we managed to get to the Schneller army base for meals.

At that time Dov and I were in our 5<sup>th</sup> year of Chemistry and we had started to study together for the last major exam in Organic Chemistry, using the text book written by the Swiss chemist Paul Karrer (Nobel Prize 1937). Thus Organic Chemistry and the shelling of Jerusalem became our matchmaker.

After the Declaration of Independence, the Students Company in the Infantry had been created and it took part in several battles. One that was often mentioned by Dov was the unsuccessful attempt to break into the Jewish quarter in the Old City by making a hole in the thick city walls with the aid of a large explosive cone (shaped charge). Scientists in Jerusalem had calculated the size of such a cone. Dov was posted with his students' unit opposite the Jaffe gate of the Old City, but – for reasons unknown to them – their attack was postponed, and by the time the order came they had been discovered. The commander of the Jerusalem sector, Colonel David Shaltiel

has been blamed for failure of this operation and the surrender of the Jewish quarter of the Old City.

#### Weizmann Institute

In 1946, when we were in our  $3^{rd}$  year of study, we students were told that the cornerstone for the Weizmann Institute would be laid in Rehovoth and that distinguished scientists would be participating at the event and lecturing. Our whole class traveled to this ceremony (without having been invited) to hear the talks of Professors Herman Mark of the Brooklyn Polytechnic Institute, Kurt Stern, the geneticist, and Louis and Mary Fieser of Harvard – among others. The Fiesers had just published a textbook in Organic Chemistry, which was more advanced and modern then the Karrer textbook that we were studying. Several of the distinguished scientists promised to return and become permanent members of the future Weizmann Institute, but – as only very few came – the Institute could offer a considerable number of opportunities for scientific research to Israelis.

During the second truce, in about September 1948, Dov was transferred by the army to Chemed in Rehovoth to participate in the military research that was being conducted at the Weizmann Institute. This continued until the Armistice agreement in 1949, when Dov became a civilian. He started research towards a Ph.D degree in the laboratory of Efraim Katzir, who was engaged in the synthesis of poly-amino acids. The title of his thesis was "Keto Derivatives of Oxzolidine and 1,3-Oxazine and the Mechanism of their Formation". At that time the degree of Ph.D. was conferred by the Hebrew University and not by the Weizmann Institute, where the research was conducted.

A list of the more established scientists who were at that time at the Weizmann Institute, as well as younger ones, is shown. Among them are David Ginsburg, Frank Herbstein, Eli Loewenthal and Dov, all of whom later joined the Chemistry Department at the Technion (Enclosure 7).

One could not be a student at the Institute without being influenced by Prof. Ernst David Bergmann, the scientific director of the Institute. He had a phenomenal memory and knew what everyone was trying to achieve in his or her research. He was respected and liked by the Ph.D. students and the students revolted when he was dismissed by Chaim Weizmann. In 1949 Dov and I got married and we settled in Rehovoth. The years there were very satisfying, scientifically as well as socially.

Among our friends were Dov and Riva El-Ad, Israel and Shuna Miller, Pnina and Dave Elson and the Michaelis.

After completing. Dov's Ph.D thesis, he and Arieh Berger developed a classical technique for the synthesis of peptides (*J. Org. Chem* **1952**, *17*, 1564-1569) while Efraim was on a sabbatical abroad. This was considered an important scientific achievement.

Soon thereafter Dov and I had our first postdoctoral experience at the University of Illinois in Urbana. The university had an excellent department of Organic Chemistry, headed by Roger Adams, and with chemists such as Carter, Cori and Nelson Leonard. Dov worked with Carter, who specialized in the chemistry of natural products. Years later, Dov returned to Urbana to do some common research with Nelson Leonard. Urbana was scientifically stimulating but otherwise a typically boring Midwestern town. There we met Bilha and Chaim Mannheim, both of whom are now Professors Emeriti of the Technion and remained our close friends.

Upon return to Rehovoth our daughter Anat was born.

We stayed two more years in Rehovoth until David Ginsburg convinced Dov that he should move to the Technion, where the Department of Chemistry was expanding. At that time Dov also taught Organic Chemistry at the newly established University of Tel-Aviv, and continued to do so after moving to Haifa,.

The decision to join the Department of Chemistry at the Technion was reached in 1957 after much hesitation, mainly by me. The physical conditions at the Technion were inferior to those at the Weizmann Institute and the Organic Chemistry laboratory was situated in a wooden hut, in which Dov and others worked until the big move to the new building of the Chemistry Department in Neve Shaanan in 1963. This occurred a year after Dov served as head of Department.

I assume that the other Professors Emeriti will review these years and those following it in detail. As Dr. Altman is reviewing much of Dov's scientific research, I want to limit myself briefly to Dov's sabbaticals and extracurricular activities (as far as I can remember so many years after his death). First, however, the devotion to his students and the importance he attributed to his lectures should be mentioned. (He received twice the trophy for best lecturer of the year). Even now former students sometimes approach me to tell how much they enjoyed his lectures. He also took his role as a member of the editorial board of Heterocyclic Chemistry very seriously.

Dov's first sabbatical in 1965 and second in 1972 were spent at MIT in Boston, where Dov formed a lasting scientific relation with John Sheehan, an expert in Penicillin Chemistry. Our 'Bible' at home was the 1094 page book "The Chemistry of Penicillin" edited by Hans T. Clarke, John R. Jonson and Sir Robert Robinson. Dov was interested in the chemistry of penicillin as a result of his studies on amidoalkylation reactions, as outlined by Dr. Janina Altman. Much of the work in Sheehan's laboratory was directed towards synthesizing penicillins and synthetic derivatives.

Two other sabbaticals were spent in Princeton University in the Edward C. Taylor laboratory. Among the many letters of condolences that I received, I enclose one of Ted's because he mentions their joint efforts to defend a patent by Abic just a month before Dov's death (Enclosed 8). Prof. Taylor also showed unusual interest in Dov's last papers, particularly in those of restricted peptide analogues, because of their potential as new cancer drugs.

Peptide chemistry brought Dov to Murray Goodman's Department in the University of California San Diego; much of the time there was spent in planning and organizing the annual international meetings on Amino Acid and Peptides.

Dov considered the pharmaceutical and agricultural chemical industry in Israel to be very important' and he served as adviser to Abic, Plantes, Machteshim and Teva. As adviser he helped synthesizing new products and improve production. In his last years he had a new agreement with Chil (Chemicals of Israel) and DuPont Agricultural Products.

Prof. Apeloig, as Dean, created a fund for summer scholarships in Dov's name from funds received from DuPont. A few of the many chemicals synthesized by Dov's group and tested by DuPont showed potential as herbicides. Unfortunately this line of research could not be pursued further after his death. Of Dov's many colleagues, a special friendship was formed with Dr. Ichio Inoue of Osaka, when both spent some time in Nelson Leonard's department in Urbana, Ill. Dr. Inoue worked for Tanabe Seiyaku chemical company in Japan. On the occasion of the UPAC meeting in Tokyo, Dr. Inoue invited Dov to give a few lectures at his company. We stayed in Dr. Inoue's home, eating and sleeping like Japanese, a treat not common in Japan (Enclosure 6). This is just one example of friends and colleagues, all of whom I want to thank for their help and for the exciting days spent in the pursuit of science.

#### Enclosure 6



Princeton University Department of Chemistry Princeton, New Jonet 08544-1000

November 15, 1993

Mrs. Ruth Ben-Ishai Rechov Freud 40 Haifa, Israel

Dear Ruth,

I was devastated to learn of Dov's death. He was such a wonderful person, and I'll always remember him with fondness, with respect, and with a special glow in my heart. The work Dov and I were doing together on the Abic patent case was such fun, and it was a lovely opportunity for us to get together again. My heart goes out to you.

Sincerely.

Edward C. Taylor

June-August ---Phone: (802) 457-3163 | FAX: (802) 457-2205 September-May---Phone: (609) 258-3914 | FAX: (609) 258-1368 Totermet: sumsp@chemyus.princetur.edu

## Scientific activities of Professor Dov Ben-Ishai

The following review covers Professor Dov Ben-Ishai's scientific activities only partially.

The chemistry of  $\alpha$ -amino acids, the study of amidoalkylation reactions and the acylation of nitrogen containing heterocycles: imidazoles, benzimidazoles and adenine, were among his main scientific interests.

He had many master and doctoral students as well post-doctoral fellows. Because of his commitment to his co-workers, many wanted him as their thesis supervisor in the field of organic chemistry. Work with him provided the opportunity to learn from his clear and rational way of thinking, which served as a guide in all subsequent research.

# I. Peptide Synthesis – Activation of Amino Acids, Protection and Removal of *N*-Carbobenzyloxy Protecting Group

Peptide synthesis requires one amino acid function to be activated whereas the other has to be protected and subsequently removed. The method of removal protecting groups are of major importance. Dov Ben-Ishai elaborated a general procedure of removal of the *N*-carbobenzyloxy protecting group with HBr in acetic acid, which replaced the use of phosphonium iodide used until then, and soon became the standard method, permitting the preparation of a variety of peptides and polypeptides without side reactions. Benzyl esters of peptide derivatives are removed as well by this method.<sup>1</sup>

#### **II. Amidoalkylation**

1) Amidoalkylation of aromatic compounds with glyoxalic-amide or carbamate adducts.

<sup>&</sup>lt;sup>1</sup> (a) Ben-Ishai, D., Berger, A. J. Org. Chem., 1952, 17, 1564-1569 (b) Ben-Ishai, D., Berger A. J. Org. Chem., 1954, 197, 62-66 (c) Ben-Ishai, D., Katchalski, E, J. Am. Chem. Soc., 1954, 74, 3688-3689 (d) Ben-Ishai, D., J. Am. Chem. Soc., 1979, 79, 3688-3689.

Professor Dov Ben-Ishai elaborated the general synthetic pathway to a variety of unnatural  $\alpha$ -amino acids *via* the amidoalkylation procedure. This general method was based on the introduction of the carboxylic acid and the  $\alpha$ -amino functions into the desired frame in a single step by using as reagent  $\alpha$ -hydroxyhippuric acid (1) or  $\alpha$ -methyloxy-*N*-methoxycarbonyl (or *N*-benzyloxycarbonyl) glycine ester (2). These reagents give rise in acidic media to an "electrophilic glycine equivalent" for Friedel-Crafts type amidoalkylation of benzene, toluene and xylene. 1 is the adduct of glyoxylic acid with one equivalent of benzamide, 2 is the adduct of glyoxylic acid with one equivalent of benzyl carbamate which is subsequently methylated with methanol under acidic conditions. In the presence of an excess of amide or carbamate, bis adducts 3 or 4 are formed, which may act as well as amidoalkylation agents.<sup>2</sup>



Amidoalkylation of aromatics with the adducts 1, 2, 3 or 4 is an electrophilic type substitution which proceeds through *N*-acyliminium cation 5. The reactions with 1 are carried out in concentrated sulfuric acid, whereas with 2 an aprotic medium is required (BF<sub>3</sub> catalysis in absolute ether or dioxane) yielding respectively the corresponding products 6 and 7. By this method a variety of substituted phenylglycines are obtained (after removal of protecting groups, and transformation of the substituent on the aromatic ring). They find applications in the synthesis of semisynthetic penicillins and cephalosporines as parts of their side chains.<sup>3</sup>

<sup>&</sup>lt;sup>2</sup> Zoller, U.; Ben-Ishai, D. *Tetrahedron*, **1975**, *31*, 863.

<sup>&</sup>lt;sup>3</sup> (a) Ben-Ishai, D., Sataty, I., Berler, Z. J. Chem. Soc. Chem. Commm., **1975**, 349 (b) Ben-Ishai, D., Sataty, I., Berler, Z. J. Chem. Soc. Chem. Commm., **1975**, 349 (c) Ben-Ishai, D.,



R = CI, Br, OH, CHO, COOH, CH<sub>2</sub>CI, CH<sub>2</sub>R

2) Amidoalkylation of olefins

a) The reaction of **1** with olefins in sulphuric acid follows a lactonisation to two isomeric lactons **8** and **9** which are separated by chromatography, whereas 2, in aprotic media (dioxane,  $BF_3$  catalysis), yields two isomeric oxazines **10** and **11**.<sup>4</sup>

b) Dienes yield mixtures of cyclic products consisting of lactons and piperidines. Their transformation leads to linear amino acids.<sup>5</sup>



3) Amidoalkylation of active methylene compounds

Active methylenes such as 1,3-dicarbonyl compounds,  $\beta$ -keto acid amides or  $\beta$ -keto esters, react with glyoxylic acid-amide adducts **1** leading to the corresponding

Sataty, I., Bernstein, Z. *Tetrahedron*, **1976**, *32*, 1571 (d) Scheffer-Dee-Noor, S.; Ben-Ishai, D. *Tetrahedron*, **1994**, *23*, 7000-7018.

<sup>&</sup>lt;sup>4</sup> Ben-Ishai, D., Moshberg, R., Altman, J., *Tetrahedron*, **1977**, *33*, 1533.

<sup>&</sup>lt;sup>5</sup> Ben-Ishai, D., Hirsh, S., *Tetrahedron*, **1988**, *44*, 5441-5449.

butenolides **12** or  $\alpha$ -acylaminobutyrolactone **13**. They may be converted to  $\gamma$ -hydroxy acids or to  $\alpha$ ,  $\gamma$ -diamino acids.<sup>6</sup>

4) Amidoalkylation of aromatics with glyoxylic acid-γ-lacam adducts: pyroglutamic acid amide and ester

Pyroglutamic acid (pyroGlu) – amino acid having  $\gamma$ -lactam moiety – appears as the end group in biologically active peptides (gonatrophin-releasing hormone), depsipeptides (didemnins) and naturaly occuring pseudopeptides. Retro-inverso analogues and pseudopeptides containing  $\gamma$ -lactams are known to adopt stable conformations in the solid state or in solutions to imitate or block at the receptor level biological activity of natural peptides. For this reason we were interested in the synthesis of phenylglycine (Pheg) pseudopeptides substituted on nitrogen by  $\gamma$  lactam moiety. (S)-PyroGlu methyl ester (14) yielded, in presence of an excess of methyl methyloxyglyoxalate hemiacetal, the hydroxy adduct (15). According to the 1H NMR 15 was a 1:1 mixture of two isomers. In the course of amidoalkylation in H<sub>2</sub>SO<sub>4</sub>, the chiral center near the hydroxy group was lost upon formation of the N-acyliminium cation 16, (see scheme). Two rotamers 16E and 16Z are possible, each with different steric requirement in the transition state. Probably the planarity requirement of the Nacyliminium cation, with less steric repulsion, is better fulfilled by the form 16E. In the reaction mixture two diastereoisomers were detected. The diastereoisomers of 17 were not separated but their ratio was determined by GC-MS and found to be 97:3.<sup>7</sup>



<sup>&</sup>lt;sup>6</sup> Ben-Ishai, D., Berler, Z. Altman, J., J. Chem. Soc. Chem. Commm., 1975, 905.

<sup>&</sup>lt;sup>7</sup> Roth, E.; Altman, J.; Ben-Ishai, D. *Tetrahedron*, **1995**, *51*, 801-810.

#### 5) Intramolecular amidoalkylation

Binding between two neighbouring amino acids in peptides leads to conformationally restricted modified peptides which can imitate or block at the receptor level biological function of natural pepdides, and are stable to proteolysis. For this reason there has been a growing trend to use the modified peptides as a new type of drugs.

The intramolecular amidoalkylation was extended to synthesize the cyclic dipeptide analogues of Phe-Gly **19** and Homophe-Gly **22** in which the side chain of phenyalanine or homophenylalanine was connected to the  $\alpha$ -carbon of glycine. Glycine residue was incorporated as well into longer peptides **19 e - h**. The ultimate goal was to prepare analogues of biologically active peptides, e.g. substance P (and related peptides) substituting the normal Phe-Gly moiety for cyclic analogues **21** and **24**.

In the case of the peptide **19** this process induced, upon substitution, a transition state of seven-membered cyclic *N*-acyliminium aromatic ion **20**. The site of approach of the aromatic ring appears to be sensitive to steric factors. The *S* configuration of the *N*-carbomethoxy group and the conformational behaviour of the *N*-acyliminium ion determines the chirality of the new stereocenter leading to the formation of a single isomer **21** in 67% chemical yield. Acording to its H-1 H-4 NOE correlation, it was the cis 1*S*, 4*S* isomer with preference for the equatorial orientation of two substituents in the seven-membered ring product.



The amidoalkylation of the of the higher analogue dipeptide 22 required eight membered cyclic *N*-acyliminiun 23 ion which is sensitive as well to steric factors, leading to the formation of a single isomer 24, as proved by the 1,4 H NOE correlation. Its chemical yield was 52%.



In summary: in intramolecular amidoalkylation, when *N*-acyliminium cation undergoes cyclisation either with a proximal aromatic ring or with a double bond, stereoselectivity is observed. In intermolecular amidoalkylation of aromatics with adducts derived from pyroglutamic acid derivatives, in which the *N*-acyliminium ion has an acyl function incorporated into the ring, stereospecificity was detected as well. The configuration of the substituent on the five membered ring appears as an inducer of high stereoselectivity in the formation of the new center.<sup>8</sup>

#### 6) Inter-versus intramolecular amidoalkylation of aromatics

Molecules containing a system of an aromatic or olefinic moiety together with a side chain  $Y = (CH_2)n$ , n > 1, bearing suitable remote amidoalkylating function, have shown a preferable tendency to undergo an internal intramolecular cyclisation to seven or eight membered rings even in the presence of excess of benzene or toluene in the medium. When n = 1, as in the case of CH<sub>2</sub> or C(Me)<sub>2</sub>, in the presence of benzene, the intermolecular pattern is observed. However some electronic factors can influence the direction of the cyclisation. Formation of five membered rings has been observed when Y = N.<sup>9</sup>

#### III Acylation Reactions in Imidazole, Benzimidazole and Adenine Series

<sup>&</sup>lt;sup>8</sup> (a) Rabi-Barakay, A.; Ben-Ishai, D. *Tetrahedron*, **1994**, *50*, 10771-10782 (b) Roth, E.; Altman, J.; Ben-Ishai, D. *Tetrahedron*, **1995**, *51*, 801-810.

<sup>&</sup>lt;sup>9</sup> (a) Ben-Ishai, D., Peled, N., Sataty, I. *Tetrahedron Letters*, **1980**, *21*, 569-572 (b) Ben-Ishai,
D., Sataty, I., Peled, N. Goldshare, R., *Tetrahedron*, **1987**, *43*, 439-450 (c) Ben-Ishai, D.,
McMurray, R., *Tetrahedron*, **1993**, 40, 6399.

Acylation of imidazole, benzimidazole and adenine under Schotten-Baumann reaction condition (aqueous potassium hydroxide and benzoyl chloride as acylating agent) has been investigated by Bamberger and Gerngross. They have reported ring-cleavage of the imidazole ring upon acylation.

#### 1) Acylation of imidazole

In the present investigation, acylation reaction of imidazole 25 was extended to carbobenzyloxy chloride as acylating reagent in the presence of aqueous sodium bicarbonate as a base and ethyl acetate as a co-solvent. Under these milder basic conditions, and playing with the number of equivalents of acylating reagent and base, the monoacylated cyclic product 26 could be isolated, which is more stable than the monobenzoylated compound analogue, diacylated cyclic compound 27 and open chain products 28 and 29.<sup>10</sup>



#### 2) Acylation of benzimidazole

The reactions of carbobenzyloxy chloride with benzimidazole leads to the analogous reaction pattern as in the imidazole series. Benzimidazole with benzyl group on nitrogen **28** yielded the open chain product **29** with the formyl group exclusively on the more basic nitrogen.<sup>11</sup>



<sup>&</sup>lt;sup>10</sup> Babad, E., Ben-Ishai, D. J. Heterocyclic Chem., **1969**, *6*, 235-237.

<sup>&</sup>lt;sup>11</sup> Ben-Ishai, D., Babad, E., Bernstein, Z. Israel J. Chem., **1968**, *6*, 851-867.

#### 3) Acylation of adenine

Adenine, having five nitrogens in its molecule, with different basic and nucleophilic properties, is expected to yield different products under different reaction conditions. a) With benzoic acid anhydride, under anhydrous condition, the nitrogen of the 6-amino group yields  $N^6$ -benzoylated product **30** as a single monoacylated product. b) Under Schotten-Baumann reaction conditions the reaction is pH dependent. Using potassium acetate as a base, pH 7-8, and ethyl acetate as a co-solvent, benzoyl chloride or carbobenzyloxy chloride reacted with the most nucleophilic nitrogen at position 7 yielding N-7 benzoyl adenine **31** as a single reaction product.



c) At higher pH, in the presence of aqueous sodium hydroxide, when ionisation at position N-9 takes place, a mixture of two isomers is obtained, with the acyl group at N-7 **31** and the other at N-9 **32**.

d) Pure **32** can be obtained when dry sodium salt of adenine (prepared from sodium ethoxide in ethanol) is treated with benzoyl chloride in dry tetrahydrofuran.

e) Carbobenzoxylation of 9-benzyladenine **33** in the presence of sodium bicarbonate as a base undergoes imidazole ring fission to the pyrimidine derivative **34** with *N*-6 formyl group attached to the more basic nitrogen.<sup>12</sup>



<sup>&</sup>lt;sup>12</sup> Altman, J., Ben-Ishai, D. J. Heterocyclic Chem., **1969**, 7, 670-692.

# 4) Selectively protected 1,2,4-triaminobutanes via Schotten-Baumann ring cleavage of histamine

Selectively protected 1,2,4-triaminobutanes are required as ligands in *bis* platinum complexes and in the field of bifunctional chelating reagents which are needed for radiolabeling and imaging. Bamberger ring cleavage of imidazole ring, studied by Dov Ben-Ishai, inspired a series of works in the Weizmann Institute towards synthesis of bifunctional chelating reagents, whereas in the Institute for Inorganic Chemistry, Ludwig-Maximilians University, Munich, it was applied to prepare ligands for *bis*-platinum complexes. The choice of histamine as a starting compound offered the possibility to employ acylating reagents with different sensitivities to pH. Thus the fluoroacetyl group, which is stable in acidic conditions, was used to protect the amino function in the side chain of histamine **35**, whereas di-*ter*-butyl dicarbonate, in the presence of potassium acetate as a base, could be applied to cleave the imidazole ring to **36a,b**.



The hydrogenation with Ra-Ni reduced in one step the imine double bond and removed the *N*-formyl groups yielding the required racemic  $N^{1}$ , $N^{2}$ -di-*tert*-butoxycarbonyl- $N^{4}$ -fluoroacetyltriaminobutane **37**. Its treament with base, removed the trufluoroacetyl, making possible further transformations towards bifunctional ligand for *bis*-platinum complex **38**. Attempt was made to adapt this procedure to optically pure compounds by using (1R, 3R, 4S)-menthyl chloroformate as chiral acylating reagent. Two chiral acyl groups near the double bond in the open chain ene-diamine **39** were expected to induce the formation of the new chiral center during the hydrogenation. Conditions of homogeneous hydrogenation were also investigated using a series of rhodium catalysts with chiral ligands. The best results were obtained

with (+)-DIOP- rhodium complex, however the ratio of R:S isomers did not exceed 74:26 (e.e. 48% of R).<sup>13</sup>



#### 5. Chiral 1,2,4-triaminobutanes and chiral vicinal diamines

Chiral 1,2,4-triaminobutanes have been obtained by stereospecific synthesis starting from commercially available (S)-pyroglutamic acid methyl ester (6-oxo-L-propine) **41**.



The ester was reduced to (S)-5-(hydroxymethyl)-pyrrolidone **42**, which was converted to azide **43**. The azide was reduced to (S)-5-(aminomethyl)-2-pyrrolidone **44**. The amide underwent acylation with Boc<sub>2</sub>O, in the presence of dimethylaminopyridine to the di-Boc-amide derivative **45**, which was cleaved to the open chain acid **46**. Hofmann rearrangement of the acid afforded isocyanate **47** which could converted either to the partialy protected  $N^1, N^2$ -di-Boc- $N^4$ -1,2,4-triaminobutane **48** or to  $N^1, N^2$ di-Boc- $N^4$ carbobenzoxy- triaminobutane **49**. Starting from (R)-pyroglutamic acid

<sup>&</sup>lt;sup>13</sup> (a) Schuhmann, E., Altman, J., Karaghiosoff, K., Beck, W. *Inorg. Chem.* **1995**, *34*, 2316-2322 (b) Altman, J., Ben-Ishai, D., Berkovich, E., *Tetrahedron*. **1993**, *49*, 2515-2593

ester the other opticaly pure isomer was obtained.<sup>14</sup> Studies in Munich have shown that the binding properties of *bis*-platinum complexes to DNA, prepared from chiral or racemic ligands, showed the same pattern, and that there was no advantage in using tediously prepared chiral ligands. Thus the acylation ring cleavage reactions in the series of imidazoles, studied by Dov Ben-Ishai and his co-workers in the early sixties, acquired new life thirty years later, as they provided a short route to vicinal diamines and 1,2,4-triamines.

<sup>&</sup>lt;sup>14</sup> Altman, J., Ben-Ishai, D., Beck, W., *Tetrahedron: Asymmetry*, **1994**, *4*, 887-894.

# Michael Cais

Born Roumania, 1924



**Ph-D thesis**: Technion-Israel Institute of Technology, 1955

#### **Post-doc position**:

Wayne State University, Detroit, Michigan 1955-57 Ethyl Corp., Detroit, Michigan 1957-58

#### **Technion Position**:

Lecturer, 1958-61 Senior Lecturer, 1961-64 Associate Professor, 1964-68 Professor, 1968

#### Sabbatical leaves:

California Institute of Technology, 1965-66 Syva Research Institute, Palo Alto, California, 1970-71

#### Major departmental responsibilities:

Dean, Faculty of Chemistry, 1972-76

#### Major Technion/International responsibilities:

President, Israel Chemical Society, 1967-71 Chairman of the 11<sup>th</sup> International Conference on Coordination Chemistry, 1968 Chairman, National Committee for Chemistry Curriculum, Ministry of Education 1973

#### **Field of Research**

Synthesis of  $\pi$ -bonded organometallic compounds; Homogeneous catalysis; Metallocenyl-carbonium ions; Organometallic radicals; Metalloimmunoassays, Medical diagnostics, Separation technology

# Celebration of inorganic lives: interview with Michael Cais

E. Amitai Halevi

Department of Chemistry, Technion – Israel Institute of Technology, Technion City, Haifa 32000, Israel

Professor Michael Cais was born in Roumania (October 10, 1924). He received the B.Sc. (in textile chemistry) from the University of Leeds, Leeds, UK, in 1950. Following graduation, he did 1 year post-graduate research on the synthesis and UV spectroscopy of aromatic compounds with Dr. A. Burawoy at the Institute of Technology, Manchester. In 1951, he returned to Israel and began doctoral studies with Profs. Ernest Bergman and David Ginsburg in the Department of Chemistry, Technion—Israel Institute of Technology, Haifa. After receiving the D.Sc. in organic chemistry in September 1955, he was offered a post-doctoral fellowship to do research in natural products chemistry with Prof. Carl Djerassi at Wayne State University, Detroit, MI. He remained at Wayne for 2 years, until the autumn of 1957 when Carl Djerassi went to Syntex, Mexico City, for sabbatical leave. At that time, Michael Cais was offered a position as research chemist with Ethyl Corporation Research Laboratories in Detroit, MI. At Ethyl, he was given the project of developing the organic chemistry of cyclopentadienylman- ganese tricarbonyl. In September 1958, Michael Cais was appointed as lecturer in the Chemistry Department at the Technion and he returned to Israel. He was promoted to Senior Lecturer in 1961, Associate Professor in 1964 and Full Professor in 1968. He was elected Chairman of the Department from 1972 to 1976 and President of the Israel Chemical Society from 1967 to 1970. He received the ACS-PRF Faculty Award in 1965; was a Member, Editorial Board, Inorganica Chimica Acta, 1968–1988; Member, Editorial Board, Israel Journal of Chemistry, 1967–1976; Member, Editorial Board, Metalbased Drugs, since 1993. He retired as Professor Emeritus in October 1993. He has held Visiting Appointments at the Institute of Pharmacology, Bonn University, Bonn, Germany (June-August, 1962); California Institute of Technology, Pasadena, California, USA (1965–1966); Synvar Research Institute, Palo Alto, California, USA
(1970–1971); Institute of Physical Chemistry, Basel University, Basel, Switzerland (Kahlbaum Lecturer, June–July, 1973); Institute of Inorganic Chemistry, Technical University, Munich, Germany (June-August, 1978); Laboratoire de Chimie des Organométalliques, Université de Rennes, Rennes, France (March-May, 1979); Memorial Sloan-Kettering Cancer Center, New York, NY, USA (June-August, 1979); Houston Medical Center, Houston, TX, USA (June–August, 1992); University of New South Wales, Sidney, Australia (September 1992–January 1993); Institute of Physical Chemistry, Okazaki, Japan (February-April, 1993). He was one of the pioneers to begin work in the field of organometallic chemistry in Israel in 1958, and was successful in his efforts to establish international links for Israel in this area. The latter achievement is exemplified by the choice of Israel in 1968 as the venue for the XIth International Conference on Coordination Chemistry (XIth ICCC) and his election as Chairman of that Conference. In his initial research programs at the Technion, he continued working on the chemistry of natural products, synthesis of pharmaceutically active compounds and on development of the chemistry of ferrocene and cyclopentadienylmanganese tricarbonyl. He suggested the trivial name cymantrene for the latter compound, by analogy with the estab- lished trivial name ferrocene for bis-(cyclopentadienyl)iron. The name cymantrene has been accepted in the chemical literature. Some of his work with cymantrene resulted in the first published approach to the synthesis of pentalene and benzopentalene through stabilized organometallic precursors. The work with ferrocene led to the elucidation of the properties and structure of alpha-ferrocenylcarbonium ions and the synthesis of metal complexes of the sesquifulvalene and calicene systems. Research in the area of organometal carbonyl complexes led to the discovery of selective hydrogenation of conjugated dienes with arenechromium tricarbonyls as homogeneous catalysts and to mechanistic studies of this catalytic process. Expansion of his research interests to the area of bio-coordination chemistry, led Cais to the development of the new concept of metalloimmunoassays, whereby metal complexes are used as markers for antigens and/or antibodies, in place of radioisotope markers, for use in diagnostic immunoassays. Work in this area also resulted in the development of a free-radical immunoassay for the detection of cannabinoid metabolites in biological fluids. The work with biological substrates also led to the discovery and development of novel concepts and devices in separation technology, with potential applications to medical diagnostics, such as non-centrifugation solid-liquid separations and liquid-liquid (solvent extraction) separations. In 1982 Michael Cais received the I-R 100 Award for these developments. Further work in this area also resulted in the development of the concepts of dynamic column liquid chromatography (DCLC), dynamic column solid phase extraction (DC-SPE), and dynamic column affinity chromatography (DC-AC). For the development of DCLC, Michael Cais was awarded, the I-R 100 Award in 1983, for the second year running. The scientific results and his inventions have been re-ported in about 150 papers, reviews and worldwide-allowed patents.

#### **Relevant Publications**

M. Cais (Ed.), Progress in Coordination Chemistry, Elsevier, Amsterdam, 1969.

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M. Cais, et al., Dynamic column liquid chromatography (DCLC): a novel concept, J. Chromatogr. Sci. 22 (1984) 548–553.

P. Ashkenazi, Ch. Yarnitzky, M. Cais, Determination of synthetic food colours by means of a novel sample preparation system, Anal. Chim. Acta 248 (1991) 289–299.

E.A.H.—Michael, let us establish some guidelines for this interview. I would like us to focus primarily on the human story, on "Michael the Man", on how you ended up where you are. Obviously, we shall also cover the highlights of your work. So, let's go first into your early history. You were born in Roumania and, 26 years later, you graduated from a British university. What happened in-between and how did you come to choose chemistry?

I was born of Jewish parents in Vaslui, a small town in the province of Moldova in the northeastern part of Roumania. My father was the owner of a shop on the main street of the town, where he held the agency for distributing the daily newspapers published in Bucharest, the capital of the country. He also held the agency for selling and servicing the Philips-made radios in Vaslui and surrounding villages. In addition, the shop sold school books, school supplies and general literature books and magazines. Our apartment was situated in back of the shop, and both my parents worked in the shop from early morning to very late in the evening. Even though they had no education themselves beyond primary school, they had a very high regard for learning and aimed to do everything they possibly could to enable my younger sister and me to obtain a university education. This was not a simple thing for Jews to do in Roumania. Antisemitism was widespread in the whole country. Jews were not allowed to own land or to serve in any public or government offices. Only a few were admitted to universities because of numerus clausus regulations. My parents hoped that I would be able to finish the required 8 years of secondary school, pass the obligatory matriculation exam, by which time they would have saved enough money to send me to study abroad, preferably in Paris.

### E.A.H.—What happened in your case?

I completed four years of primary school and passed the admittance exam to secondary school in 1935. By the time I completed 5 years of secondary school in the summer of 1940, the situation of Jews in Roumania had become extremely precarious. The Second World War was in full swing, the King of Roumania had been forced to abdicate by a fascist government. The Iron Guard, a Nazi Party, was openly persecuting, kidnapping and beating Jews and was pressing the government to enter the war on the side of Germany. A law was passed that excluded Jews from attending any state school, primary, secondary or university. They were only allowed to attend private Jewish schools in those cities where such schools existed. There was no Jewish secondary school in my town, but my parents were unwilling to give up their plans for me. They found out that there was a Jewish secondary school in the capital. My mother's

brother—a well-established lawyer who had studied in Paris and was probably my parents' role model for me—and two of her sisters lived in Bucharest. My father traveled to Bucharest, a 7-hour train journey, and—after a family council—it was decided that I should come to live in Bucharest with one of my aunts and be enrolled as a pupil in the sixth form of the Jewish secondary school "Cultura". So, I left home—barely 16 years old—and started school in September 1940. My parents were thankful, not only because I was given the chance to continue my schooling; they were glad I had left Vaslui, because some of my non-Jewish former classmates, who had joined the Youth Cadres of the Iron Guard, came around to the shop asking for my whereabouts. They wanted to catch me and beat me up, as they were doing to other Jewish children in the town. A number of my Jewish classmates had been arrested and sent to labor camps in Transnistria, then part of the Ukraine. When my father refused to tell them where I was, they kidnapped him and beat him severely. He was eventually released after payment of a ransom and was ill for a few weeks until he recovered from the beating. I was not aware of any of this.

### *E.A.H.*—Under these circumstances, how long did you continue your stay in Bucharest?

At about that time, the Roumanian Government, led by the dictator Antonescu, decided to declare war on Great Britain and France, on the side of Germany. Following this declaration of war, the Ambassador of Great Britain in Bucharest had to leave Roumania. Before he left, well aware of the persecution of the Jews in Roumania, the ambassador made a humanitarian gesture: he granted to the Jewish Agency in Bucharest 300 "immigration certificates" (official entry visas to the British-mandated Palestine) for children aged 14-16, allowing them to emigrate from Roumania to Palestine. My parents heard about this opportunity from my lawyer uncle, who was active in the affairs of the Jewish Community in Bucharest. My uncle told them that he might be able to obtain one of these "certificates" for me but this would involve payment of a very substantial sum of money to cover expenses for the trip, maintenance for a period of 2 years at some institution in Palestine and bribes to be paid to the Roumanian authorities to obtain an exit permit. After a great deal of soul searching, and in view of the continuous worsening of the Jewish situation in Roumania, my parents decided to make the enormous sacrifice of being separated from their only son, in the belief and hope that going to Palestine would mean saving my life. It is hard for me to describe my own feelings at the prospect of being separated from my family, my home and my friends. All I want to say is: I knew that, as an obedient son, I was going to do what I would be told to do.

### E.A.H.—Did you get such a "certificate"?

Yes, I was lucky to get a certificate. At the end of March 1941, a group of 300 children, I among them, embarked on the ship "Carol I" in the Roumanian port of Constantza for travel to Istanbul, Turkey, the first stage on our way to Palestine. After an overnight passage, we arrived in Istanbul, where we disembarked and were transferred to the Asian part of Turkey. There we boarded a train on which we journeyed for 4 days and nights through Syria and Lebanon, as far as Beirut. From there we traveled for 3 hours by bus until we reached the village of Atlith on the outskirts of the city of Haifa, in—for us—Eretz Israel (the Land of Israel). To our surprise and dismay, our buses stopped at the entrance gates of a concentration camp, surrounded by barbed wire, where immigrants of all ages without "certificates" to enter British-mandated Palestine were imprisoned, some of them for years.

### *E.A.H.—You left Roumania to escape being sent to a concentration camp and your first stop in Palestine was at a concentration camp! How did you feel?*

It was traumatic for all of us. We protested that we had "certificates" given to us by the British Ambassador and therefore were "legal" immigrants. We were told that this was only a formality, a bureaucratic process. We would have to remain in the camp only a short time, until we passed the security check, after which we would be assigned to our final destinations. I do not want to go into any details about the very difficult conditions in the camp. Fortunately, after about 3 weeks in the camp, the authorities began to release the children of our group and send them to different locations in Palestine. I, together with about 10 other children, was allocated by the "Youth Aliyah" Organization (a Jewish Agency Department dealing with the immigration of children into Palestine) to go to an Agricultural School in the Ben Shemen Youth Village. There we were to be given boarding and lodging, attend school 4 hours a day and work in the fields 4 hours a day. The aim of the school was to prepare agricultural workers, whoafter 2 years in Ben Shemen—would join a kibbutz or a farming village. In parenthesis, I should point out for our readers that this aim was in line with one of the main aspirations of Zionism. After so many centuries in which Jews lived under antisemitic discriminatory laws which did not allow them to own and work the land, Zionist tenets encouraged Jews to take up working the land upon return to their old, historic country.

I would like to mention first that upon arrival in Palestine I did not have any knowledge of either Hebrew or English. My native tongue was Roumanian; I also had a good knowledge of French, which I had studied for 5 years in my secondary education and also some knowledge of German from 2 years' study in secondary school. By the way, knowing French turned out to be lucky for me, because my roommate in Ben Shemen who was from Turkey—also spoke French; thus we were able to communicate freely. However, this did not help me at school, where all the studies were conducted in Hebrew. The 4 hours of my daily school time were devoted solely to the study of speaking, reading and writing Hebrew. I understood and accepted that I had to do this but, of course, it was quite different from taking math, science, etc., the subjects I was used to have in school!

#### E.A.H.—What about the 4 hours of work in your daily schedule?

With your permission I would like to answer this at some length, because it covers one of the most critical aspects in my subsequent development. The work requirement proved to be the most difficult part for me. Not because I objected to work as such; it was because of the kind of work I had to do. As I told you, Ben Shemen was structured to train agricultural workers. My first assignment was to the vegetable garden. After working at this assignment for several weeks, I came to the conclusion that working the land was the most boring thing in the world for me, that I had no interest in becoming a farmer and that I was not ready to give up my dreams of continuing my studies. I did not hide my feelings from my instructors and teachers, and I requested formally to be transferred to one of the technical secondary schools, which I knew were under the auspices of the "Youth Aliah" Organization. My request fell on deaf ears, so one day I decided to rebel. I refused to go out to work at my assigned place in the vegetable garden. As punishment, I was transferred to the housekeeping section and was put to do all kinds of menial housework. I had to clean rooms, sweep and wash floors, wash dishes, darn socks, iron the garments of other pupils, and so on. I did this kind of work 4 hours a day, every day except Saturday, for several months. When the administration saw that they could not "break" me, I was called to an interview with the principal of the school. I reiterated my request to be transferred to a technical school because I did not want to become a farmer and wanted to continue my studies up to matriculation. They said that before considering this request, I must take a psycho-technical test to determine whether I had the required abilities. I was sent to Tel Aviv to take the 4-hour test and, happily, I passed with high marks. Upon my return to Ben Shemen with the test results, I was told that there was no free place at any of the technical schools, but since I had passed the test and did not wish to become an agricultural worker, I was being transferred to work and be trained in the locksmith workshop of the school!

#### E.A.H.—So, you became a qualified locksmith!

Are you kidding? Frankly speaking, doing the house keeping work had begun to wear me down and even though I had no idea what exactly being a locksmith entailed, I thought I had no choice but to give it a try. In the first assignment, I was given a piece of an iron bar and told that I had to learn how to shape it into a hammerhead. I worked in the locksmith workshop for about 3 months. It was better than doing the housekeeping or the vegetable garden work. At the same time I realized that learning to become a locksmith was not taking me any nearer to achieving my dream of finishing high school and qualifying for acceptance at a university. I spent many hours thinking about this and slowly arrived at the conclusion that there was no other choice than to get away from Ben Shemen in order to look for other ways to pursue my plans.

E.A.H.—You were 17 years old, you had no money and no relatives to take you in or support you, and yet you considered leaving Ben Shemen, where at least you were assured of boarding and lodging for up to 2 years. Weren't you afraid to contemplate such a step?

You are quite right to ask this question and I assure you that this was a very hard decision to take. I discussed my plan with several of the friends who had been sent together with me from Atlith to Ben Shemen, and tried to persuade them to leave Ben Shemen with me. Some of them had relatives in Tel Aviv and several had other sources of financial support. I knew that, for their own reasons, they too were not happy in Ben Shemen. Thus, I was not surprised that they were willing to consider the idea of running away from Ben Shemen and discuss it. After lengthy discussions, we formulated our plan and arrived at a decision to act. One rainy morning in October 1941, five of us took our few belongings and—without telling anyone—boarded a bus to Tel Aviv. Upon arrival at the bus station in Tel Aviv we separated. Two of my friends went to their relatives. The remaining three of us walked to an address in Tel Aviv where we knew that, on the third floor, lived the Deputy Director of the "Youth Aliah" Organization that was supposed to be taking care of us. We rang the bell. When the Deputy Director, a lady in her fifties, opened the door, saw us and heard who we were, she refused to talk to

us. We decided that we would not move until she met with us and helped solve our problem. We placed our belongings on the staircase, in front of the door of the apartment, and settled in for a sit-down strike. In retrospect, I believe this was probably the first time such a strike took place in Tel Aviv! We remained there for 3 days and 3 nights, sleeping on the floor, in front of the Deputy Director's door, quietly and without any violent demonstration whatsoever. During all this time the Deputy Director came and went about her business, pretending that we did not exist. Finally, on the morning of the fourth day, she called us into her apartment and told us that since Ben Shemen was not willing to take us back, the "Youth Aliah" Organization had decided "to wash its hands" of us, "the rebels". In other words, it refused to take any further responsibility for us. When we claimed that our parents had paid for 2 years of school, with board and lodging, she said that the only thing the organization was prepared to do would be to give each of us the sum of three pounds per month for 3 months, in order to enable us to make our own arrangements. With this payment the "Youth Aliah" would terminate all their obligations towards us. We were flabbergasted! We were being thrown out on the street! What were we to do? We decided that we had no choice but to accept this offer, even though we had no idea whether it was possible to subsist on three pounds a month and did not know what would happen to us at the end of three months. We were asked to sign a document declaring that we renounce all further claims on the "Youth Aliah" Organization. We were then each given three pounds in cash, the first monthly payment of the three offered to us, and ordered to collect our belongings and leave the building.

### E.A.H.—This must have been catastrophic for you. You found yourself thrown out on the street, without a sponsor and with your dreams of continuing your studies shattered. What did you do next?

You are quite right. At first this was catastrophic for me because, naively, I had not prepared myself for such an out- come. As you said, I found myself on the streets and it was clear to me and my friends that the first thing we must do was to find a place to stay. We started walking the streets, looking for notices of furnished rooms to rent. Eventually, after several hours of searching, we did find an apartment owned by a widow who was taking in boarders. A room with three beds was available and the rent was oneand-a-half pounds per month per person, including one meal a day, lunch or dinner. We did not hesitate long. It was winter, the rainy sea- son, and evening was approaching. We agreed to take the room and settle in, even though this meant paying half of our total monthly allowance. The important thing was that we had found a place to stay and would not have to sleep on the street! I did not sleep much that night, thinking over my situation and trying to plan what to do next. It became clear to me that the first thing to do was to find some kind of work to earn a living because it would not be possible to subsist for even 3 months on the allowance we had received. The following morning I went to the Youth Employment Office of the Histadruth Trade Union and asked for work, any kind of paying work. It turned out that the only employment available for young people was as temporary menial workers paid by the hour. I had to report at the Employment Office every evening to find out if I would have work on the following day and where. I did all kinds of work for the next few weeks and was glad that I was at least getting some additional income, however small, for my subsistence. One evening, the clerk at the Employment Office, who knew some of my history and was sympathetic to my tribulations, told me that he had found a full-time job for me. "Delfiner", a wellknown textile manufacturing company in Tel Aviv-owned by the Yohananoff familywas looking for an apprentice to be trained to work in the dye-mixing room of the cotton and silk-dyeing section of the plant. The starting salary would be 4 pounds a month, for 8 h a day, 6 days a week, with additional pay for overtime. He asked: "Would you be interested?". I jumped to embrace him and remember telling him, with tears in my eyes, that this was the answer to all my hopes and prayers. The next day I presented myself at the plant and, after going through some bureaucratic formalities, I began work at the new place.

### *E.A.H.*—Would it be a safe guess that this new employment was also your first close encounter with chemistry and had a bearing on your choice of chemistry as a career?

This would be a very good guess. In your first question in this interview you asked how I came to choose chemistry. We have now reached the point where I can try to answer that part of your question. Let me explain first what was involved in my work in the dyemixing room. I was apprenticed to Tobias, the Dye Master, whose duty was to formulate and prepare the required mixture of dyes and other appropriate chemicals for each batch of cloth sent for dyeing in the plant. The dye-mixing room stored hundreds of tins and bottles containing every kind of synthetic dye as well as other chemical compounds used in the dyeing process. Tobias was a deaf-mute but he had been trained to speak somehow and could lip-read. He would write the formulation on a special form, with the name and quantity of each dye and chemical to be used in the dyeing process of a particular batch of cloth. He would give me the form and I was supposed to weigh out, or measure, the exact quantities of the dyes and other chemicals, solids or liquids, for that particular formulation. This work, though it may seem pedestrian, entailed a very high degree of responsibility. Any mistake in selecting the right dye prescribed in the formulation, or in weighing out the exact quantity required in the mixture, could cause irreversible damage, by spoiling the whole batch of cloth being dyed.

### E.A.H.—Did this responsibility worry you?

Thinking back, I believe that I was awed by the responsibility and worried that if I made a mistake and did not do my work well I would be fired. I realized that I had to be as careful as possible, check and recheck every measurement and selection of the proper chemical. I worked hard and endeavored to learn and memorize the name and location of each dye and chemical and to learn as much as I could about its properties.

### E.A.H.—Did you realize that in fact you were doing some form of chemistry?

Not at the beginning. This realization came to me by an evolutionary process. The Dye Master was a very demanding kind of guy but at the same time he was willing to spend time teaching me and supervising my work. I later found out that he was a chemistry graduate from a German university. In addition, Dr. Chazan, the Chief Chemist of the plant, to whom the Dye Master reported, took an interest in me when he saw that I was hardworking and willing to learn how to do my job well. Sometimes he would stop and talk to me and explain some of the processes of cloth dyeing. He once told me that through his chemical work he had invented a new type of synthetic soap. He said that his invention was a secret and—in order to keep it secret—he had to prepare the soap by himself for use in the dyeing processes in the plant. I realized later that he was talking about having invented a synthetic detergent and, in fact, I know that he eventually set up his own company to manufacture and sell this synthetic soap, which is still being marketed in Israel. Anyway, I came to admire and worship him because of his kind interest in me and it may very well be he who is largely responsible for my becoming interested in chemistry.

E.A.H.—You had now settled down to a job which you liked and your financial situation was more secure. When did you start thinking again about continuing your studies?

I never stopped thinking and dreaming about it. My problem was that I could not hope to attend a regular secondary school because I had to work for a living. I had to find an alternative. I learned that every year the British Council in Palestine held examinations in Jerusalem for the London University Matriculation Certificate. Anyone was permitted to sit for this examination as an external student without any prerequisites. The important thing for me was that this matriculation certificate granted the right to be admitted to a British university. It became obvious to me that sitting for this examination was the only

way open for me to try and reach my goals. My major problem was that these examinations were held in English. Whereas I had made reasonably good progress during the past year in learning Hebrew for every day use, my English was very poor in fact practically non-existent. Therefore, before anything else, I had to learn English. I was given the name of a highly recommended, but expensive, private teacher, a lady who was a graduate of London University in English phonetics. I made an appointment and explained that my aim was to be able to sit for the London Matriculation Certificate exam as early as possible. The teacher agreed to accept me as her pupil for a fee of half a pound per 50-min lesson. I arranged to come to her twice a week. You must understand that this meant a very heavy burden on my budget. My wages at this time had risen to about 6–7 pounds a month including overtime. After paying one-and-a-half pounds for rent (which included one meal a day) and 4 pounds for the English lessons, I had very little left for other needs. This did not really bother me. I was determined to make do with whatever was necessary to achieve my goal.

### *E.A.H.*—What about the other subjects of the examination. How did you prepare for them?

The London University Matriculation required examination in five subjects: two obligatory, English and Mathematics, and three electives, including a foreign language. My electives were Chemistry (naturally), Physics and French. From perusal of the exam syllabus, I learned that most of the subject matter for math, chemistry and physics was known to me from my years of study in the secondary school in Roumania. It mainly remained for me to review all this material in English. I also found out that I should have no problem with the French syllabus, for the same reason. I acquired the required text books and studied every day after work, for 6–8 hours, either on my own or together with two of my friends, who were also preparing themselves for the same exam.

### E.A.H.—When did you sit for the exam?

I continued with my studies for the whole of 1942 and part of 1943. I registered to sit for the exams scheduled for June 1943. I took leave from work, went to Jerusalem, rented a room for a week at a youth hostel, took the exams—one every day for 5 days and returned to my job in Tel Aviv. About 1 month later the British Council in Jerusalem notified me by mail that I had passed all the exams and that the London University Matriculation Certificate was being mailed to me directly from London. It is hard to express my jubilation. The first thing I did was to send my parents in Roumania a 25-word Red Cross message (the only way one could communicate with hostile countries during the war) informing them of my success. I knew that it would take about 3 months for them to receive my message and another 3 months to receive their reply. Eventually their response arrived, expressing their joy and happiness at my achievement.

### E.A.H.—What did you do next, now that you had realized the first part of your dream?

My feeling was very much that of "mission accomplished" and I had to decide what to do next. I felt that there was not much more room for advancement at the textile plant and began to look out for other possibilities. I heard that the Jewish Agency, in its attempts to promote and support the war effort, was looking for volunteer recruits to join a Jewish unit in the British Army for 18-month service in the neighboring countries of Lebanon and Syria. I decided to volunteer and, in August 1943, I found myself in a military camp in the Bekaa Valley, Lebanon, a soldier in 479 General Transport Company, Royal Army Service Corps of the British Army. I will not dwell on details about my time in the army except to say that I was a clerk in charge of the Company office and the Company stores. Our Company did not do any fighting and was mainly engaged in the transportation of troops, supplies and equipment through Lebanon and Syria. Every 2 or 3 months I would be given a week's leave, which I spent visiting friends in Tel Aviv. At the end of 1944 my company was disbanded, its soldiers were returned to Palestine and discharged from the army.

### E.A.H.—I understand that at this time you wanted to begin your studies at a university.

Having managed to save most of my army pay, I thought I had enough money to start with, and planned to enroll at a university to begin my further studies and thus fulfill the second part of my original dream. Obviously, by this stage, I had already decided I wanted to become a chemist! At that time there were only two institutions of higher learning in Palestine: The Hebrew University in Jerusalem and The Technion-Institute of Technology in Haifa. Admittance to either of these institutions required having attained "Bagruth", the matriculation certificate awarded after a national examination at the end of secondary school studies in Palestine. Thinking that I possessed a matriculation certificate, I applied to be admitted to studies for the 4-year first degree in chemistry at both institutions, but to my dismay I was informed that neither of these institutions recognized the London University Matriculation Certificate as equivalent to "Bagruth" and, therefore, I was denied admittance by both institutions. The Technion was willing to accept me with the status of a "non-registered student", which meant that, by paying the full university tuition fees, I would be allowed to attend lectures, participate in lab work and sit for examinations, but I would not be given any official credit to- wards a degree. I thought that this was better than nothing; at least I would be

attending regular courses at a university and thus advance my education. Also, I hoped that if I did well in exams, the university authorities might perhaps eventually reconsider their decision and agree to change my status to that of a regular student.

E.A.H.—When did you start at the Technion under these conditions and how did it work out?

The first thing I had to consider was my financial situation. My meager savings, after paying tuition fees, would not last for more than a few months so I had to find some kind of long-time work that would enable me to earn enough for the university fees and for my upkeep. Let me say that by this time I was reasonably fluent in English and this helped me to find a job as night clerk at the Savoy Hotel, a major hotel *cum* restaurant in Haifa that catered mostly to British government personnel. The working hours were from eight in the evening to six the following morning. This suited me perfectly because it allowed me to attend classes during the day. Furthermore, the conditions of employment included a salary and meals from the hotel kitchen. The duties were relatively light and usually, after eleven o'clock in the evening, I had lots of free time when I could read and study at my desk. I was very happy. This was the first time since leaving Roumania that I was again really going to school, attending classes, doing laboratory work in chemistry and physics, mixing with fellow students and making new friends. I worked very hard at my studies and enjoyed them. I was not even bothered by the fact that I was not a "registered student". My employers at the hotel were satisfied with my work and I did not have to worry about keeping the job. In the meanwhile I also began thinking about the possibility of going to Britain after the war to continue my studies. The war was coming to an end and I thought that my service in the British Army would increase my chances of being admitted to a university in the UK as an army veteran. In addition, to prepare myself even better, I enrolled in a correspondence course of London University for the Intermediate London University Examination, which was equivalent to the first year of study at that university. I felt that things were coming along well for me. I was in an up-mood and I could even allow myself to think of my personal life and do something about it.

### E.A.H.—What do you mean by "do something" about your personal life?

In our conversation until now, I have attempted to give a factual account of the major events in the course of my life. I tried to avoid being emotional and dwelling too much on my personal sentiments and feelings. Even though I had friends and acquaintances, most of the time I felt alone and lonely. I missed my family very much and longed for a

normal family life. Even though I was still under 21, without any tangible means and/or prospects, I began having daring thoughts that perhaps I should consider founding a family of my own. During my army service, and after my release, I had been corresponding with Judy, a girl I had met and dated in Tel Aviv during my leaves from the army. Judy was studying for a Teacher's Diploma at the Lewinsky Teachers College in Tel Aviv. Her parents and brother lived in Hadera, a small town about 30 miles north of Tel Aviv. The more I got to know her, the more I liked and respected her as a person as well as admired her mental qualities. I felt that I could talk to her on any subject or problem and she would respond in an intelligent and sympathetic way. Without realizing it, I was falling in love with her. No wonder, then, that in my daring thoughts of getting married I began thinking about Judy as my future companion in life. Eventually I got up enough courage to ask her if she would marry me and accompany me to England, where we would both continue our studies. I told her that I had nothing to offer but the belief that this was the right thing to do and the will and promise to work hard for our future together. She confessed that she, too, had fallen in love with me and, though she was not yet nineteen, she would be happy to marry me. However, we had to obtain her parents' consent. I had visited with her parents several times in the past but had never given any hint that I planned to marry their daughter. Judy and I went to see them together and told them that we wanted to get married and go to England to continue our studies. They were greatly surprised and told us that they would agree to our marriage but wanted us to remain in Palestine. I explained to them that I had no chance of being admitted to a university in Palestine and that my only chance of continuing my studies was by going to England. Eventually they came around, and we were married in December of 1945. In parenthesis, I want to add that after the end of the war in the summer of 1945, I was very happy to learn that my parents and sister had survived the war, and were safe and sound in Bucharest. We were able to communicate by regular mail and I had informed them of my impending marriage and received their blessing.

## *E.A.H.*—When did you go to England, where did you settle and how did you manage there?

After our marriage we rented a furnished room in Haifa. I continued my work as night clerk and my studies as "non-registered" student at the Technion. Because of my having previously worked and gained experience in the cloth dyeing plant, I decided to continue my studies in the field of textile chemistry. In 1947, I applied to the University of Leeds to be admitted for studies towards a B.Sc.degree in the textile department. My having served in the British Army and my possession of the London University Matriculation Certificate were sufficient grounds for admittance. Judy, who held a

Teacher's Diploma was also admitted to study at the University of Leeds in the Department of Biblical Studies. We arrived in Leeds in September 1947. We had enough money to pay tuition fees for the first year and to subsist for a few months. Luckily, Judy found a position as teacher in a Hebrew school that paid enough for us to live on very frugally. I worked summers in all kinds of job, including one as a weaver in a textile plant in the nearby City of Bradford. One summer, Judy and I worked as instructors in a summer camp in Bognor Regis for Jewish children from London. Thus we were able to earn at least enough to pay our tuition fees at the university.

#### E.A.H.—Tell us something about your chemistry studies in the textile department.

We had to take all of the usual basic courses in chemistry, such as general, physical, inorganic and organic chemistry. In addition, there were courses on the chemistry and structure of wool, silk, cotton and synthetic fibers. The Head of Department was Prof. J.B. Speakman, a chemist. He was world-famous for his contributions to the elucidation of the structure and chemistry of wool and silk fibers, and was a fantastic teacher. However, the textile department curriculum for a B.Sc. degree included a large number of compulsory courses in textile manufacturing, such as cloth design, structure of the loom, structure of spinning machines, etc. From the beginning, I did very well in the chemistry courses. My previous studies at the Technion gave me a head start and I managed to be among the top three students in the exams in the chemistry subjects. I did not do as well in the textile manufacturing courses. At the end of 3 years' studies, one had to sit for comprehensive examinations in all the subject matter covered over the 3 years. The happy end was that, in the summer of 1950, I passed all the examinations and graduated with the B.Sc. degree of the University of Leeds. To complete my happiness, another very good thing happened at about the same time. My parents and sister, who had survived the war and had been living in Bucharest under the Communist regime, received permission to leave Roumania and immigrated into Israel in the summer of 1950.

#### E.A.H.—What did you plan to do after your graduation?

As you remember, I told you of my earlier desire to become a chemist. Throughout the 3 years of my studies in Leeds, I carried with me the nagging feeling of wasting time because the major part of my studies consisted of non-chemistry subjects. I was not interested in following a career in textile manufacturing and I wanted to find a way to deepen my knowledge of chemistry. Let me deviate for a moment at this point. One of our problems of living in Leeds was the weather! We had difficulties not only because of the heavy winters, but because of the bad weather through all the seasons; gray sky,

fog and rain a great deal of the time. I even developed a knee problem, probably some form of rheumatism. Now, it so happened that I had a friend, one of the Ben Shemen "rebels", who was studying chemistry at the University of Manchester. Whilst in Leeds, about once a year, in the summer, we went to spend a weekend with my friend in Manchester. Every time we went there, the weather was absolutely beau- tiful. So, no wonder I thought it would be nice to transfer to Manchester and continue with my chemistry studies there. Little did we know that our previous visits to Manchester coincided with the 1 or 2 days of that year in which Manchester had nice weather!

### E.A.H.—Did you then enroll in the University of Manchester for a post-graduate degree?

No! You see, my B.Sc. was in textiles and Manchester University did not have a textile department, where I felt I had a better chance of being accepted for post-graduate studies. Fortunately, such a department existed at the Manchester Institute of Technology, which at that time was a separate institute, independent of the university. (A few years later this Institute became the College of Technology of the University of Manchester.) I went for an interview with several members of the teaching staff at the Institute of Technology and was very glad that Dr. A. Burawoy, a Senior Lecturer in the Chemistry Department, agreed to accept me as a student for post-graduate studies beginning in the fall of 1950.

# E.A.H.—What type of research did you do with Dr. Burawoy? At that time I was a graduate student at University College, London, and recollect that he was involved in a bitter dispute among chemists in Britain.

Dr. Burawoy, a refugee scientist from Germany, was an organic chemist and UV spectroscopist. He became involved in the controversy of resonance versus tautomerism in benzene and its derivatives. By studying and analyzing the UV spectra of suitable benzene derivatives he was trying to prove the existence of equilibria between tautomeric species as opposed to resonance between canonical forms. My research topic was the synthesis of *ortho*-nitrosophenols and their oxime derivatives, followed by the measurement of the UV spectra of these compounds in various solvents. I worked very long hours at the bench, learning and practicing organic synthesis. In addition, I attended advanced courses in organic and physical chemistry, thus filling in the gaps from my undergraduate chemistry studies. Also, I learned to operate the Hilger Uvispek Photoelectric Spectrophotometer and to read the photographic plates in order to analyze the wavelength bands obtained with this instrument. Let me say that this was considered an advanced spectrophotometer at the time. How far from the automatic

spectrophotometers of today! I must tell you that after reading and deciphering hundreds of photographic plates over a period of about a year, my vision deteriorated to the extent that I had to have prescription glasses for shortsightedness. Since then I have had to wear glasses for driving and other activities that require distant vision. On the other hand, maybe because of my having become shortsighted at a relatively early age, I have never needed reading glasses, even nowadays as I approach 80!

### *E.A.H.*—*How long did you stay with Dr. Burawoy and was anything from your research with him published?*

Let me first answer the second part of your question. My name was included among the co-authors in two papers published by Burawoy in the November 1955 issue of the British Journal of the Chemical Society. To return to the first part of your question, we remained in Manchester for only 1 year because something completely unplanned for happened. In the summer of 1951 we received a message from the Israeli Embassy in London, where we were registered as Israeli citizens after the declaration of the establishment of the State of Israel in 1948, informing us that the Principal of the Reali School in Haifa, one of the most prestigious high schools in Israel, was touring Britain to recruit teachers for his school. The Principal, Mr. Joseph Bentwich, who would be visiting Manchester, had requested to meet with Judy and me. We agreed to the meeting and were curious to hear what he had to say. The Principal invited me to come and teach chemistry in the upper forms of his school and offered Judy a position as English teacher in the lower grades of the school. In addition to the regular teacher's salary, he offered to provide us with rent-free housing for as long as we would be teaching in his school. He would also cover part of the travel costs to Israel. Considering the state of our finances at the time, this was a very tempting offer. Nevertheless, I told the Principal that it was difficult for me to consider his offer because it meant that I would have to give up my plans to continue my studies towards the doctorate. He said that, in his opinion, it should be possible for me to continue the doctoral studies in the Chemistry Department of the Technion in Haifa and suggested that I try to get approval from the relevant authorities to make my research in Manchester part of my eventual doctoral thesis at the Technion. He promised to give me his full support to carry out such a program if I was prepared to teach chemistry in his school at the same time. This was an offer I could not refuse. I understood the risks involved. I also realized that I would be taking upon myself very heavy responsibilities. I had no teaching experience and could not know whether I would be a good teacher. It also meant that I would have to put in very long working days to find time both for full-time teaching and for doing research in the laboratory. However, the latter issue was the least of my concerns; I was used to working long hours. After much deliberation we decided to accept the offer and returned

to Israel in August of 1951.

*E.A.H.—One of the risks involved in this plan was that you could not know for certain whether the Technion would admit you for doctoral studies. How did this work out?* 

I should mention that before I decided to accept the Principal's offer, I had discussed the whole thing with Dr. Buroway. He was very understanding, and told me that he would have no objection to the eventual inclusion of the research I had carried out under his supervision, in the doctoral thesis that I would eventually submit at the Technion. He even offered to read and edit the draft I would write on the Manchester work as part of the thesis, if necessary. This was very encouraging because I thought it would increase my chances with the Technion, and it helped me decide to take the risk that you rightly mentioned. It turned out that, luckily for me, at that time-the end of 1951 and beginning of 1952-Prof. E.D. Bergman, of the Hebrew University of Jerusalem, a world-class scientist and the doyen of organic chemists in Israel, was also a permanent Visiting Professor at the Technion. He had been asked to help develop organic chemistry research at the Technion and was interested in attracting research students in this field. He was willing to accept me as his student and agreed to consider my work in Manchester as part of my eventual doctorate thesis. One of Prof. Bergman's interests was in the area of poly- cyclic aromatic compounds and their carcinogenic properties. He proposed that I work on the synthesis and spectroscopic properties of triphenylene and its derivatives. Relatively few modes of entry into the triphenylene series had been worked out at that time and he thought this would be a suitable topic for a doctoral thesis. Obviously, I accepted his suggestion without hesitation! Now that I was a chemistry doctoral student at the Technion, you can imagine my happiness that the major risk involved in my decision to return to Israel had been eliminated. Nor could I help remembering that, 6 years earlier, the Technion had refused to admit me as a "registered" student for a B.Sc. degree in Chemistry!

### *E.A.H.*—*How did you combine your teaching duties and your research? What was your daily schedule?*

The housing that was provided to us under the conditions of my employment was located on the campus of the Reali High School, which was situated on top of Mount Carmel, Haifa. This was convenient for my teaching duties, which were Sunday to Friday, between 8 a.m. and 2 p.m.; I did not have to waste time on travel to the school. However, this was less convenient for reaching the lab at the Technion,

located at the time in mid-town Haifa. We did not have a car, a luxury at the time, and I had to travel by bus, a 50-minute ride each way. I would arrive at the lab about 3 p.m. and worked there until about 9 p.m. Upon my return home I had to prepare my lectures for the next day's teaching schedule. Fortunately, there were many school holidays as well as the 3-month summer vacations when I could put in about 12 hours a day in the lab. This enabled me to come along reasonably well in my research. Prof. Bergman, on hisweekly visits at the Technion, reviewed my research results and advised me on how to continue. This state of affairs continued for about 2 years until the summer of 1954, when a new development occurred.

E.A.H.—You are of course referring to the arrival of Prof. David Ginsburg at the Technion. I say this because at about that time David Ginsburg invited me to join the Department of Chemistry at the Technion.

Yes! As you know, Dr. David Ginsburg, an American scientist who had joined the Organic Chemistry Department at the Weizmann Institute of Science in Rehovoth, was a rising star among the young organic chemists in Israel. He had achieved world-wide recognition in the chemical community for his work on the total synthesis of morphine. Ernst Bergman, who had been asked by the Technion to help strengthen its Chemistry Department, was empowered by the Technion authorities to offer David Ginsburg the appointment of Professor of Chemistry and Head of the Department of Chemistry at the Technion. Ginsburg accepted this offer and he took up his appointment in June 1954.

### E.A.H.—How did this development affect you?

With Ginsburg's appointment, Bergman ceased being a Visiting Professor at the Technion. Ginsburg, with Bergman's approval, suggested that I stop the work on triphenylene. They thought I had enough research material to include a chapter on this topic in my thesis. At the same time, Ginsburg thought that, in order to have enough material overall for my thesis, I should begin work on a new topic, under his supervision. Ginsburg was interested at that time in the total synthesis of the alkaloid colchicine, a tricyclic compound with two seven-membered rings, by methods developed by him in his synthesis of morphine. He proposed that I work on the synthesis of suitable intermediates in the tropone series, aimed at developing feasible paths towards an attempted total synthesis of colchicine. I enthusiastically agreed to start on this project because it meant I would be gaining experience in new methods of synthesis and would be introduced to the field of natural products chemistry, which was probably the

most fashionable topic in organic chemistry at that time. Let me also mention here that when Ginsburg agreed to accept the appointment at the Technion, he also demanded and received—substantial funds both for hiring new staff and for the purchase of teaching and research equipment. The facilities and working conditions in the laboratory were significantly modernized and enlarged. This had a highly beneficial effect on my research work. I was able to make very satisfactory progress, gaining experience in modern research methods while getting results in my new research project. Not least helpful was the fact that I could talk with my research supervisor on a daily basis, rather than—as before—once a week during Bergman's visits to the Technion.

### *E.A.H.*—*How much longer did you have to continue in order to complete your work for the doctorate and when did you get your degree?*

By the summer of 1955, one year after Ginsburg's arrival at the Technion, he told me that I had accumulated sufficient research results and could start working on the manuscript for my thesis. It took me about 3 months and several drafts, which I had to type myself since I could not afford a professional typist. In September 1955, I submitted the thesis to the Doctoral Committee appointed for my examination. In order to connect between the three different topics of the research I had carried out, I entitled my thesis "Synthetic and Spectroscopic Studies in the Aromatic Series". At that time, as you know, the regulations at the Technion were that a doctoral student had to "defend his thesis" in an examination before a Doctoral Committee. The examination took place in the main lecture hall in an open public session that anyone could attend. The candidate was given 50 minutes to present his work, after which each of the three committees members asked the candidate questions pertaining to the research as well as general chemistry questions. At the end of the examination, the committee members would retire for about 15 minutes, after which they would return to the lecture hall and announce their decision as to whether the candidate had passed or failed the doctoral exam. The members of my committee were Profs. Ginsburg

(Chairman), Bergman and Kalugai. I was able to answer all the questions asked by the examiners, except one. Let me tell you about this question. In my thesis, in reviewing the literature in the introduction to the chapter on the triphenylene work, I mentioned that some X-ray measurements on the crystal structure of triphenylene "agreed fairly well with Coulson's theoretical calculations made by the molecular orbital theory". Prof. Kalugai asked me to explain "what was the molecular orbital theory". I was stunned by the question! I knew practically nothing about molecular orbital theory. To my very great embarrassment, I had no choice but to say so in answer to the examiner's question. You can imagine my trepidation while waiting for the return of the committee members from their deliberations. The committee returned after about 10 minutes, which

to me seemed like 10 hours. The Chairman called me to the podium, congratulated me and announced publicly that I had passed the examination for the Doctor of Science degree. This was the first Doctorate in Organic Chemistry awarded by the Department of Chemistry at the Technion. I remember that I had tears of happiness in my eyes. However, I can tell you that when I speak of that moment even to day, nearly 50 years later, I can recollect the embarrassing and, for me, traumatic experience of that examination. It does not even help that, after the examination, Ginsburg told me he knew that Kalugai did not have the faintest idea of what molecular orbital theory was about and was merely asking for information!

### *E.A.H.*—After finishing the doctorate you went to USA for post-doctoral research with Carl Djerassi. Why this particular choice?

When Ginsburg told me that I could start writing the thesis, he also asked me if I had any plans for what I was going to do after finishing the doctorate. I told him that I would very much like to continue in academia and was interested in continuing research on the chemistry of natural products. He said that there might be an opening in his department, but one of the necessary conditions for such a position was post-doctoral experience at a university abroad. He suggested that I apply for a post-doctoral fellowship, gave me the names of several scientists known for their work on natural products chemistry and suggested that I write to them. I applied for a post-doctoral fellowship to all of them, giving Ginsburg's name as reference and received several offers in return, among them one from Carl Djerassi, at that time a professor at Wayne State University in Detroit, MI. After consultation with Ginsburg, and upon his recommendation, I decided to accept Djerassi's offer, even though, financially, the stipend that he offered was the lowest one!

#### E.A.H.—How long did you stay with Djerassi and what did you work on with him?

Accompanied by my wife and 3 year old daughter, I ar- rived in Detroit towards the end of September 1955 and con- tinued in Djerassi's group until September 1957. My major research project was the determination of the structure and absolute configuration of cafestol, a pentacyclic diterpene constituent of coffee. The work consisted partly in extracting cafestol from coffee oil, supplied by General Foods Company, from whom Djerassi had a research grant. I extracted gallons of coffee oil by soxhlet extraction. During these extraction operations the whole lab, and in particular my clothing and hands, were imbued with the aroma of coffee! Djerassi's group at Wayne consisted of about 20 people, including doctoral students and post-doctoral fellows from all over the world. Djerassi would come in the lab twice a day, stopping to talk and discuss the work with every one in the lab. There was very good interaction among all the people in the group. Everybody was familiar with everyone else's work. We had weekly group seminars where we discussed the research going on in the lab as well as current topics in the chemical literature. Pioneering work was going on in the lab in the areas of mass spectrometry and optical rotatory dispersion as tools for the structural elucidation of natural products. In September 1957, Djerassi decided to take a year's leave of absence to go to the pharmaceutical company Syntex Mexico, for whom he was a consultant on the development of the "pill". He took several of the people in his group to work with him at Syntex and he asked me to join him as well. I told him that I had planned to remain with him in Detroit for another year because my wife, who was working on her Master's Degree at the University of Detroit, needed another year to complete her studies. I explained to him that even though this was a very tempting offer, I would not leave my family alone in Detroit to go to Mexico and that, regretfully, I could not accept his offer.

#### E.A.H.—You went to work for Ethyl Corporation instead. Why did you choose Ethyl?

I already knew in the summer, the time when scouts from industry visit universities to interview graduating students for potential employment, that Djerassi would be away from Detroit. I took advantage of the occasion and presented myself to be interviewed by representatives of several companies in the Detroit area, one of which was Ethyl Corporation. In all of these interviews I explained, up front, that I was from Israel and was looking for a temporary 1 year research position in organic chemistry. Ethyl Corporation was the only company that made me an offer of employment under these conditions. As you know, Ethyl at the time was-and probably still is-the biggest producer of tetraethyl lead (TEL), the anti-knock additive to car fuel. TEL was considered one of the main sources of pollution of the atmosphere, and Ethyl was very interested in finding new anti-knock agents to replace TEL or reduce its use. It was known that ferrocene, a new organometallic compound, which had been first reported by Pauson and Kealy, as well as another group, in 1951, had combustion-improving properties. Similarly, cyclopentadienylmanganese tricarbonyl, discovered by Wilkinson and Piper in 1955, was found to be a combustion-improver. By 1957, Ethyl Corporation had developed pilot-plant capabilities for the manufacture of cyclopentadienylmanganese tricarbonyl (a crystalline yellow-colored solid) and its derivative. methylcyclopentadienylmanganese tricarbonyl (a yellow-colored liquid). Ethyl wanted to investigate the organic chemistry of these two compounds, which were thought to be chemically similar to ferrocene. They wanted to test as many manganese-containing derivatives as possible for anti-knock properties as a potential substitute for TEL. I was

asked whether I would be prepared to work on such a project. Even though the area of organometallic and coordination chemistry was a complete departure from the fields of chemistry in which I had worked before, the project seemed fascinating to me. The discovery of ferrocene had opened up an entirely new field in the area of organometallic and coordination chemistry. Moreover, I was offered a salary that was nearly three times as much as I was making from my post-doctoral fellowship! I was very happy to accept the Ethyl offer.

E.A.H.—In retrospect, it seems that Ethyl Corporation was instrumental in causing your entrance into coordination chemistry. Did your work at Ethyl progress well enough to have had this effect on you?

In retrospect, I think that agreeing to work for Ethyl was probably one of the most auspicious decisions I took on shaping the direction of my scientific career. Soon after I began work on the cyclopentadienylmanganese tricarbonyl project, I realized, from perusing the chemical literature, that I had before me a golden opportunity that rarely befalls a young chemist in the early stages of his research career. There were very few reported data on the chemistry of this new compound. This was probably due to the fact that preparing cyclopentadienylmanganese tricarbonyl in quantities sufficient for the investigation of its chemical properties was not an easy task. Here I was, having unlimited quantities of a rare starting material and the whole gamut of known chemical reactions of aromatic compounds before me. In addition, by this time, ferrocene chemistry had become abundant in the literature. All I had to do was to develop the right conditions to carry out with cyclopentadienylmanganese tri-carbonyl reactions similar to those reported for *ferrocene*. I had me a ball! I was running four or five reactions simultaneously. After work-up in the isolation and purification of the reaction products, I had several new compounds every week in sufficient quantities to send for chemical analysis as well as for anti-knock testing. I can say without equivocation that this was my most productive period ever in doing chemical work at the bench!

#### E.A.H.—What was the publication policy at Ethyl?

Like any other industrial company, Ethyl would allow publication only after it had taken the necessary steps to protect its intellectual property by submitting the appropriate patent applications. Here was an additional bonus for me in my experience at Ethyl. The company had an in-house patent department with several patent attorneys who would meet regularly with the research scientists to discuss their work and to determine whether there was any new patentable material. Due to my productivity in the lab, I had very frequent meetings with one of the patent attorneys to discuss my results and to go over draft patent applications with him. Because most of my compounds were new, and thus patentable, and because of Ethyl's interest in manganese compounds as potential anti-knock additives, Ethyl applied for patents on all of the compounds I prepared during my stay with Ethyl. This intensive interaction with the patent attorney enabled me to learn a great deal about the use of suitable patent language and the rules involved in preparing patent applications. Eventually 15 patents, with my name as co-inventor, together with Dr. John Kozikowsky, who was working in parallel with me on the chemistry of methylcyclopentadienylmanganese tricarbonyl, were allowed to Ethyl. This was not bad for 1 year's work! Especially so, since I was also remunerated with the proverbial One US Dollar for each patent allowed!

### *E.A.H.*—*Scientifically, this was an eventful year for you. Did anything else happen during this year that had a bearing on your future?*

Several additional events that occurred during this year required me to make decisions about my future. David Gins- burg, with whom I had kept in touch, wrote to offer me an appointment as Lecturer in the Department of Chemistry at the Technion. I also had an offer to come and work with Frantz Sondheimer, who at that time was Head of the Organic Chemistry Department at the prestigious Weizmann Institute of Science. Additionally, Dr. Gus Ligget, the Research Director at Ethyl Corporation, had asked me to continue working at Ethyl with a promotion in grade and a raise in salary. The latter offer, though very enticing (a salary of four to five times higher than any offer from Israel), I rejected outright. Our daughter Daphna was approaching school age and, considering my past history, my wife and I were deter- mined to live and bring up our children in Israel, where Jews would never be persecuted and discriminated against merely for being Jewish. I had to decide between Haifa and Rehovoth. On one of Carl Djerassi's frequent visits to Detroit from Mexico, to meet with his students who were continuing their work at Wayne, I met with him to ask his advice. He knew both Sondheimer and Ginsburg well. Carl told me that if I go to Sondheimer, I would have to work only on topics of his choosing. On the other hand, Ginsburg would give me independence and encourage me to work in areas of my own interest. Obviously, I chose to accept Ginsburg's offer and we returned to Israel in September 1958. I never had reason to regret this decision!

*E.A.H.*—What happened to your expectation of research independence when you joined the Technion? Could you choose your own area of research?

First, let me mention that, while still in the States, I had received Ginsburg's permission to apply to the ACS-Petroleum Research Fund for a research grant. My application was

approved, so I was able to bring my own research funds with me. This in itself established my research independence. My problem was to decide on the direction of my research program. As I mentioned before, natural products chemistry was the fashionable area of research. In addition, organic chemistry was dominant in Israeli chemical research. No one in Israel was doing organometallic chemistry research at that time. Indeed, for quite a few years I remained a "lone wolf" in Israel carrying out research in this area. I must mention here that before I left Detroit, Ethyl Corporation agreed that if I wanted to continue investigating the chemistry had of cyclopentadienylmanganese tricarbonyl and its derivatives in Israel, they would supply any quantities of starting materials I required. Ginsburg encouraged me to continue working in this field. He too was aware that organometallic chemistry was a rapidly expanding field of research and was glad to have someone in his department working in this area. Nevertheless, I was not yet ready to stop working on some topic connected with natural products chemistry. So, I entered into a collaborative research project with Ginsburg on the isolation and characterization of the constituents of Catha Edulis, a plant that was presumed to have narcotic properties. My first doctoral student at the Technion, Asher Mandelbaum, the top student in his class, agreed to take this topic for his thesis. As you know, Asher subsequently joined the staff of our department and became our expert on mass spectrometry. He retired as Professor Emeritus last year.

### E.A.H.—How many people did you have working with youto start with?

I had a good start. In addition to Asher, I had two research assistants supported from the PRF research grant funds. I also had my own work-bench in the lab and managed to do quite a bit of chemistry myself.

### E.A.H.—You said you were "a lone wolf" in organometallic chemistry in Israel. What contacts did you make with scientists abroad in this field?

My first direct contacts were made in September 1959 when I attended the XVIIth IUPAC International Congress in Munich, Germany. I presented a paper on the UV Spectraof Organometallic Compounds on work carried out jointly with Richard Lundquist while still at Ethyl Corporation. In fact Ethyl paid for my travel expenses to Munich. At this congress I had the opportunity to meet and talk with many of the leading people working in the field of organometallic chemistry. In particular, I became friendly with E.O. Fischer and Peter Pauson, a friendship that continued for many years. Subsequent to this Congress, I began to attend the International Conferences on Organometallic and Coordination Chemistry regularly and got to know, and make friends with, many colleagues in these fields, world-wide. I was invited to participate in

the meetings of the International Organizing Committee of these Conferences, as the representative of Israel. All of these contacts helped me eventually to bring the XIth ICCC to Israel in 1968.

E.A.H.—We'll come back to the XIth ICCC later. Let us talk some more about your research. I notice that one of your early Visiting Appointments was at the Institute of Pharma- cology in Bonn, Germany. Why there?

In 1961 I initiated a joint research project with Prof. William Taub, of the Weizmann Institute of Science, on the synthesis of a new class of delta-lactones. Taub, a leading scientist in Israel on Pharmaceutical Chemistry, also held an appointment as Visiting Professor at the Technion, where he taught a graduate course every year on the chemistry of pharmaceutical compounds. We also collaborated on this project with Prof. Domenjoz, Head of the Institute of Pharmacology at the University of Bonn, who was testing the pharmacological properties of our compounds. At about the same time, I had been asked to serve as secretary and coordinator of the Pharmaceutical Committee of the Israel National Re- search Council. One of my tasks in this extracurricular activity was to investigate and prepare a report on the feasibility of setting up pharmacological testing facilities in Israel to serve the country's pharmaceutical industry. Visiting with Domenjoz was very helpful in the preparation of my report.

What's more, during my 3-month stay in Bonn, I even had he opportunity to do some work in the lab and managed to get enough results to publish a paper with Domenjoz. Let me tell you an amusing story in connection with my service on the National Research Council. In the summer of 1962, I was invited to present a paper at an International Symposium on "The Theory and Structure of Complex Compounds" in Wroclaw, Poland. At that time Poland was part of the Soviet bloc and, as an Israeli citizen, I was worried about traveling to Poland. I happened to talk about this at one of the National Research Council meetings. The Chairman of the Council told me that I should not worry, he had a solution for me. As a member of the Council, which was a Government institution, I was entitled to travel on a diplomatic passport and he would recommend that I receive one. So, for 1 year, I enjoyed the perks and benefits of traveling on a diplomatic passport!

### *E.A.H.*—*Let's return to organometallic chemistry. What were the next milestones in your progress?*

Perhaps I should first mention some personal, non- chemical milestones. In 1961, I was given tenure and promoted to the rank of Senior Lecturer, followed 3 years later by promotion to Associate Professor and then, in 1968, to Full Professor. Also, a muane

but important event: in the summer of 1964, the Chemistry Department moved from the old, mid-town Technion campus, to its own, very spacious building on the new campus, that was being built at that time, on the eastern slopes of Mount Carmel. As posing building on the new campus. This achievement was due mostly to Ginsburg's standing on the Technion Faculty and to his talent for soliciting funds from donors, members of Technion Friends Societies throughout the world. I was given an office and two laboratories with room for about 10–12 research students. It did not take too long to fill up this space with graduate students who chose to work with me for their master's and/or doctoral degree, as well as with post-doctoral fellows, whom I could support with funds received from two major 5 year grants that I had obtained from the US Air Force and the US Department of Agriculture for research in organometallic chemistry. Another important event for me was my forthcoming sabbatical at the California Institute of Technology.

### E.A.H.—How did you come to choose CalTech for your sabbatical?

As you know, the employment conditions at the Technion entitle the academic staff to take a paid sabbatical year after each 6 year period of teaching and research. I was due for a sabbatical for the 1965–1966 academic year. In 1964, for the inauguration of our new building, Ginsburg organized an international symposium, attended by many distinguished chemists from all over the world. One of these distinguished visitors was Jack Roberts, at that time Head of Chemistry at CalTech. I discussed with him the possibility of my coming to CalTech on sabbatical leave. He told me he would be glad to arrange such an appointment for me but said that he did not have the funds to support my stay there. I explained to him that I did not require any funds, because I had fortunately just received a PRF Faculty Award from the American Chemical Society, and could use it for any research purpose I desired. I assured Jack that this award, together with my sabbatical leave stipend, were enough to support my stay at CalTech. Upon his return to Pasadena, Jack Roberts sent me confirmation of a Visiting Appointment at CalTech for the 1965–1966 academic year. Accompanied by my wife and two daughters, I arrived in Los Angeles in September 1965. We rented an apartment in Hollywood, one block from a school for the children and half way between UCLA, where Judy had enrolled to attend classes in linguistics, and Pasadena, where CalTech was located.

#### E.A.H.—Tell us about your sabbatical at CalTech and why was it important to you?

First, I wish to mention that I was given a very warm welcome at CalTech by the Chemistry Department staff in general and by Jack Roberts in particular. I was given a nice office furnished with a large desk, which—I was told—had been Pauling's desk! The major reason for choosing CalTech was because I had decided to use my sabbatical to "go back to school". I thought that CalTech, one of the most famous schools in the world, would be ideal for my purpose. I felt that after 6 years of hectic activity to establish my position at the Technion, I needed a pause in which I would devote time to filling in some gaps in my own chemical education. The "trauma" of having failed to answer the molecular orbital question in my doctoral exam 10 years earlier was still on my mind and I had to do something about it. After settling in at CalTech, I requested permission from various faculty members to attend their regular courses for both undergraduate and graduate students. For example, I took such courses as theory and practice of NMR, quantum chemistry, group theory, chemical reaction mechanism and some others. I learned how to use the computer (at that time data were entered by means of punched cards) for carrying out simulations of NMR spectra and doing Huckel molecular orbital calculations. In addition, I attended departmental as well as group seminars, and lectures by visiting scientists. Altogether, I had a very busy schedule and enjoyed myself immensely. I even collaborated with John Richards and one of his Ph.D. students, in a study of NMR spectra of alpha-ferrocenylcarbonium ions. I was also busy maintaining a regular correspondence with my group at the Technion. I had arranged for one of my post-doctorate fellows, Dr. Dennis Dell, to be in charge of my group and to send me monthly progress reports from each one of the 10 co-workers in my group. One of the highlights of my sabbatical was an invitation to lecture on my work on alpha-ferrocenylcarbonium ions at one of the famous Winstein Seminars at UCLA. These seminars began at 7 p.m. with a 1-hour lecture by the invited speaker, which was followed by questions from the audience and discussion that continued until around midnight. I assure you that this was an unforgettable experience! To be "grilled" on your work for 4 hours by some of the best minds in the chemical world and to end up feeling good about yourself is something that is not easily forgotten! The last thing I did at the end of my sabbatical was to make a stop in Sweden on my way back to Israel. I had erolled to attend a 3-week Lowdin workshop on theoretical chemistry in Abisko. I must make it clear that I had neither the intention nor the inclination to become a theoretical chemist. I did not have the necessary qualifications for this. My purpose was simply to add to the education acquired in my studies at CalTech. I am sure I did gain from attending this workshop and I can say that, all in all, Abisko was a very interesting intellectual experience. Perhaps I should add that I also enjoyed having had the opportunity to experience the phenomena of "midnight sun" and the "twenty-four hour day of continued daylight"!

E.A.H.—I know that after your return home from sabbatical you became involved in a

#### number of extra-curricular, chemistry-linked, activities. Will you tell us about them?

The heaviest responsibility awaiting me upon my return to Haifa was to start work on the preparation and organization of the forthcoming International Conference on Coordination Chemistry (XIth ICCC) due to take place in Israel on September 8–18, 1968. I felt that this forthcoming conference was going to be of paramount importance to the development of coordination chemistry research in Israel. I worked very hard on this project for nearly 2 years and, with your permission, I would like to go into it at some length. The first priority was finding the necessary financial support for the conference. I contacted all of the five universities in Israel, a number of Israel Government Ministries, The Israel Academy of Sciences, The National Council for Research and Development and the Israel Atomic Energy Commission. I claimed that the ICCC would be the most important international scientific conference to take place in Israel to date. My arguments must have been convincing because I received firm financial commitments and pledges from all the above organizations. These commitments, together with the expected income from participation fees, assured the conference budget. The Israel Chemical Society and IUPAC, the International Union of Pure and Applied Chemistry, were co-sponsors of the conference. The organizing committee of XIth ICCC included David Ginsburg as Honorary President, me as Chairman of the Conference, Frank Herbstein as Secretary, and one representative from each of the five Israeli universities. The conference was to take place at two venues: 8-12 September in Haifa and 15-18 September in Jerusalem. Izhak Marcus, of the Hebrew University of Jerusalem, as co-Chairman was responsible for the Jerusalem part of the conference, which was held jointly with the Fifth International Conference on Solvent Extraction Chemistry (Vth ICSEC). The previous ICCCs had established a successful organizational framework for these meetings and many traditions had evolved. Nevertheless, I thought that perhaps time was ripe to experiment with some changes in the structure of the conference program. We decided to emphasize a few selected topics by organizing plenary symposia with contributions from a panel of five speakers and to allow ample discussion time for audience participation. Each symposium would consist of one main lecture (40 minutes) followed by four shorter lectures (20 minutes each). After a coffee break (20 minutes) the plenary would reconvene for the discussion. The five previous speakers would then become a discussion panel, which would answer questions and comments from the floor. Previously appointed discussion leaders would lead the discussion. We selected three topics for three plenary symposia in Haifa and two topics for two plenary symposia in Jerusalem. We also had three 1-hour plenary lectures, two in Haifa and one in Jerusalem. The plenary lectures and symposia were held in the morning sessions. In addition, about 150 invited papers (20 minutes each) were presented in afternoon parallel sessions, four in Haifa and two in Jerusalem.

#### E.A.H.—How did you select the topics for the conference and for the symposia?

The choice of topics for the symposia and plenary lectures was partly governed by the results of an opinion poll con- ducted among coordination chemists throughout the world. The first circular of the XIth ICCC was distributed during April and May, 1967, to more than 2500 chemists. We requested recipients, irrespective of whether or not they planned to attend the conference, to make four selections in order of preference from a list of eight topics that were being considered by us for our conference topics. By December 1967 we had received about 600 replies from 35 countries, in which 95% of the respondents indicated the intention to participate in the XIth ICCC. This was a gratifying result in itself, considering the turmoil which had just occurred in our geographical part of the world.

### E.A.H.—Are you referring to the June 1967 "Six Day War"?

Yes! You remember the tension and fear that were prevalent in our country at that time. Every one thought that war against Israel was imminent. Let me give you an illustration of the atmosphere by recalling a personal example. At that time my research grant from the US Department of Agriculture was in progress, and one of their senior scientists, Dr. E.N. Frankel, had joined my group as Visiting Scientist in September 1966 for a 1 year stay, accompanied by his wife and five children. At the end of May 1967, the situation had grown so tense that Dr. Frankel decided to return to the USA immediately. He and his family sailed by ship from Haifa to New York. By the time he landed in New York, 2 weeks later, the "Six Day War" was over! Upon landing, Dr. Frankel sent me a cable that he wanted to return to Haifa to complete his 1 year stay. He came back, this time without his family and continued his work in my lab for the remaining three months of his originally intended stay. I believe that the above example gives an illustration of what might have occurred in the minds of the potential participants between the receipt of our conference first circular in May 1967 and their subsequent confirmation of intention to participate in the conference.

### *E.A.H.*—*What sort of response did you get from the scientists you wanted to invite for the plenary and symposia main lectures?*

The response was tremendous. Not only from the scientists invited for the main lectures, but also from most of the leading coordination chemists from all over the world, who were willing to participate in the conference whether invited to present a paper or not. I would like to mention some names. I apologize in advance to all those whose names

deserve mention but are omitted because of my poor memory. Among the main plenary lecturers we had Manfred Eigen, Gottingen (Germany) (subsequently a Nobel Laureate), Arthur Adamson, Los Angeles, USA, Albert Eschenmoser, Zurich, Switzerland, Jack Halpern, Chicago, USA, Owen Mills, Manchester, UK, Henry Freiser, Tucson, USA, Leonard Katzin, Argonne, USA. Additional names of participants in symposia and session lecturers: E.O. Fischer, Munich (subsequently a Nobel Laureate), J.C. Bailar Jr., Urbana, Stan Kirschner, Detroit, Larry Dahl, Madison, Ron Nyholm, London, Joe Chatt, Birmingham, Gordon Stone, Bristol, Paul Heimbach, Mulheim, Leslie Orgel, San Diego, Jannik Bjerum, Copenhagen, Ugo Croatto, Padova, Fausto Calderazzo, Pisa, Umberto Belluco, Padova, Luigi Sacconi, Florence, Jian. Reedijk, Leyen, Kazuo Yamasaki, Nagoya, and many others. We received a total of 252 abstracts for the Haifa sessions and 61 abstracts for the Jerusalem part. The Proceedings of the Conference, which included all of the abstracts submitted, were published by Elsevier Publishing Company in a hard cover, 850-page, book titled "Progress in Coordination Chemistry". This book was distributed to all the conference participants and could alsobe purchased from Elsevier.

### *E.A.H.*—*Having participated in the Conference I can testify that XIth ICCC was an exceptionally successful meeting. In retrospect, to what do you attribute this success?*

Thank you for your compliment and for your question. I would like to address my comments, in particular, to those readers who might become involved in the organization of future conferences; they may find this information to be of some use to them. First and foremost, I attribute the success of the conference to the participation of a very high number of leading coordination chemists and to what I believe was the high scientific level of the papers presented by the participants. I would like to take this opportunity to express again my deepest appreciation and grateful thanks to all the participating scientists for their contribution to make this conference a success. An additional factor which, I believe, contributed to the success of the conference was the very rich social program that we had organized for all the participants, not only for "accompanying persons", as is customary at such meetings. This was made possible by the generous financial sponsorship I had managed to obtain prior to the conference. To start with, the registration fee for active participants was only US \$20.00 per registrant and US \$8.00 per accompanying person. (Compare this to current registration fees of

US \$500 and more.) All the registrants were entitled to receive free tickets for nearly all the events, including transportation on the shuttle service between hotels and the conference location, the social functions and all but two of the nine excursions: (1) an archeological visit and beach party followed by a gala performance in the excavated Roman Amphitheater at Caesarea on the shore of the Mediterranean Sea (US

\$8.00/person), and (2) the conference banquet (US \$7.00/person). The following events were offered: on Sunday evening, 8 September, the opening ceremony and reception by the President of the Technion-Israel Institute of Technology; on Monday, an evening reception by the Mayor of Haifa, followed by a Folklore Show; on Tuesday morning a fashion show for the ladies program; on Tuesday evening, all the foreign participants (over four hundred of them), in groups of 10–12, were invited to spend the evening and have dinner in an Israeli home (this was the first time such an event occurred at an international conference); on Wednesday afternoon and evening, the Caesarea outing; on Thursday morning the ladies program provided a visit to a Women's Army Base. The Conference Banquet took place on Thurs- day evening. On Sunday, 12 September, the Jerusalem part of the conference was opened with a lecture on "Masada" by Prof. Yigael Yadin, the famous archeologist who had been responsible for the excavations at Masada. Following the lecture, there was a reception by the President of the Hebrew University of Jerusalem. On Tuesday the participants were invited to a reception by the Mayor of Jerusalem at the Israel National Museum, which included a tour of the museum. The 3-day interval between the Haifa and the Jerusalem sessions (Friday, 13 September to Sunday, 12 September) was left free, in order to enable participants in the conference to join excursions and sightseeing tours offered by the official travel agency appointed by the conference.

#### E.A.H.—What happened after the conference?

On the day after the conference ended, I took my family to Eilat and spent a whole week on the beach. Believe me, I needed a rest! Upon our return to Haifa, I still had some work to do winding up the affairs of the conference, such as preparing an audited financial report for the sponsors. I was pleased to be able to report that we had stayed within the budget and even had a small surplus. Most important for me was being able to resume full contact with my research group. There was only one more extra curricular activity to which I had to devote some of my time: In September 1967, I had been elected President of the Israel Chemical Society for a 3 year term. This was not a fulltime position and I had no problem coping, especially compared with the work involved in organizing the conference.

### E.A.H.—In 1970–1971, you went for your sabbatical to the Synvar Research Institute, in Palo Alto, CA. Why did you make this particular choice?

My research in organometallic and coordination chemistry was progressing quite satisfactorily. However, for some time I had begun to recognize that an important area in chemical research was evolving in the direction of bio-organic and bio-inorganic

chemistry. More and more I was driven to the idea that, considering my background in natural products chemistry and experience in organic synthesis, I should find an entry into research in this developing field. I learned of Synvar from two of our colleagues in the Chemistry Department who had gone there on sabbatical as Visiting Research Fellows. The Synvar Research Institute had been set up as a joint venture between Syntex, the pharmaceutical company, and Varian, the instrument makers, both located in Palo Alto. The declared aims of Synvar were, among others, to use the principles and knowledge of chemistry, physics and biology in the development of novel analytical methods with potential industrial applications. I also learned that one of the areas of research at Synvar was the development of non-radioactive methods for immunoassays and thought that this might be an interesting vehicle for my entry into the field of bioorganic and/or bio-inorganic chemistry. The Research Director of the Institute was Dr. Ted Ullman, whom I did not know personally at the time. However, one of the most influential members of the Board of Directors at Synvar was Carl Djerassi, who--in the meantime- had moved to Stanford University as Professor of Chemistry and was still strongly connected to Syntex. I wrote to Carl to enquire about the possibility of spending my forthcoming sabbatical at Synvar. He replied that he would be glad to support and recommend my appointment as a Visiting Research Fellow at Synvar. Not surprisingly, with Carl's recommendation, my application to Synvar for such an appointment was approved, and in August 1970 I arrived in Palo Alto accompanied by my family.

### E.A.H.—What work did you do at Synvar?

One of the major areas of research at Synvar was the use of stable nitroxide radicals as spin-labels for the qualitative and/or quantitative detection by electron spin resonance (ESR) of various substrates in biological fluids. The idea was to replace the radioactive isotopes used in radioimmunoassay (RIA) by stable nitroxide radicals and thus remove the hazards of working with radioactive materials. Instead of using gamma- and/or beta-counters to measure the radioactivity in the analytical process, one would use an ESR instrument to detect and measure the spin-label signal. This was very much in the interest of Varian, one of the major makers of ESR instruments.

E.A.H.—Let me interrupt you at this point. Some readers of this interview, including myself, may not be too familiar with the area of immunoassay. Would you give us a few definitions and a brief description of the principles involved in the development of an immunoassay? I assume that this would also answer my previous question about the kind of work you did at Synvar.

Antigens (Ag) are substances which, upon introduction into the body of an animal species, trigger the formation of antibodies (Ab). Antibodies are glycoproteins formed in pertinent cells of the organism on intrusion of antigenic substances. Antisera are sera that contain antibodies. Haptens are small molecules (mol. wt. <5000) that exhibit antigenic properties only when covalently bonded to a macromolecular carrier. The specific recognition and binding of an antigen by an antibody with sufficient binding affinity forms the basis of the immunological reaction equilibrium: Ab + Ag = AbAg If the antigen Ag is replaced by a labeled antigen Ag-L, it is possible that a similar equilibrium will be established: Ab + Ag-L = AbAg-L provided that the label L does not cause immunological changes in the antigen, i.e. that the antibody Ab exhibits equal-or very nearly equal-recognition of both the un- labeled antigen (Ag) and the labeledantigen (Ag-L). In this case, the binding constant (or affinity constant), K is the same, or very nearly the same, for the complexes AbAg and AbAg-L. If mixtures of variable amounts of antigen (Ag) and a constant amount of the labeled antigen (Ag-L) are allowed to compete for a limited and constant concentration of antibody-binding sites, the reaction mixture-upon equilibration-will consist of a "free" antigen phase (unbound Ag and Ag-L) and a "bound" phase of antibody-antigen complexes (AbAgand AbAg-L). After separation of the two phases, the amount of label present in either the "free" or the "bound" phase is determined by the appropriate analytical method. Preparation of a calibration curve, plotted for standardized amounts of labeled antigen and unlabeled antigen, provides the means of determining the quantity of analyzed substance in unknown samples. The work in this area at Synvar comprised the following steps in the development of a spin-label immunoassay: (1) synthesis of suitable stable nitroxide radicals to be used as spin-labels; (2) attachment of the spinlabels, by the methods of organic synthesis, to hapten molecules which would sub-

sequently be analyzed for their presence in biological fluids; (3) attachment of the hapten molecule by a covalent bond to a high-molecular-weight compound found in the serum of animal biological fluids, for example bovine serum albumin (BSA), to obtain the antigen: (4) immunization of animals such as rabbits or sheep by injection of a solution of the antigen, to produce and then isolate antibodies produced in the serum of the animals. These antibodies were then expected to specifically recognize the presence of the analyte molecules in human biological fluids, such as urine or blood serum; (5) utilization of the reagents produced in (1)–(4) to develop the immunoassay protocol, as described above. In the protocol using the ESR system, an aqueous solution containing appropriate known amounts of specific antibodies and spin-labeled hapten was titrated with increasing amounts of the unlabeled hapten. After an incubation period, a precipitation gapent was added to the reaction solution to induce precipitation of the high-

molecular-weight antibody. The precipitate, which contained the "bound" fraction, was separated from the liquid by centrifugation. The supernatant liquid, containing the "free" fraction was monitored with the ESR instrument to determine the amount of competitively displaced spin-labeled hapten. The absence of "free" (non-antibodybound) labeled analyte ESR signal in the supernatant liquid indicated that no unlabeled analyte had been present in the urine or serum sample to compete with the spinlabeledanalyte in binding to the antibody. In other words, the result was a negative test. An ESR signal in the su- pernatant liquid showed the presence of "free" spin-labeled analyte. This indicated that competing unlabeled analyte had been present in the original urine or serum sample, and was therefore a positive test. A semi-quantitative result could be obtained by running appropriate calibration curves. Without going into further detail, I worked on all the above steps, except the immunization of animals. The lat- ter step was carried out on a special animal farm owned by Synvar, somewhere in California. My project was to develop a spin-label immunoassay for the detection of barbiturate metabolites in human biological fluids. I must say that I really enjoyed working again full time at the bench. The work progressed quite well. I could follow and apply general procedures which had already been established in previous work at Synvar. By that time, the researchers at Synvar had advanced very far in the development of a spin-label immunoassay for morphine metabolites in both urine and blood serum. In fact they were already testing for morphine in urine samples of heroin addicts, collected from drug rehabilitation centers. By mid-1971 Synvar had a well-tested working spin-label immunoassay for the screening of urine samples from suspected heroin users. The test was able to detect the presence or absence of morphine metabolites. The success of this test came to the attention of the Pentagon authorities, who at that time had a serious problem with identifying heroin addicts among the returning veterans from Vietnam before their return to the States. The Synvar urine spin-label immunoassay appeared ideal for large screening operations, so Synvar was offered a contract to send a team to Vietnam to set up screening operations among the American troops. The Synvar team took an ESR instrument with them and was able to carry out the screening of many thousands of soldiers over a period of several months. The success of this operation was a turning point in the future development of Synvar. Subsequently, Synvar was incorporated as a company and changed its name to Syva Corporation, because the name Synvar had already been appropriated by a *synthetic var*nish company. Within a relatively short time, Syva became successful in the manufacture and sale of spin-label immunoassay kits for the detection of a large number of metabolites, including the barbiturates on which I had been working during my stay at Synvar.

E.A.H.—How did the work you carried out during your stay at Synvar meet your expectations of entering a new field of research?

I remained at Synvar until August 1971. While doing work on the bench on the immunoassay project, I also spent many hours reading as much as possible on the subject of immunology, a completely new area for me. During all this time I was trying to think of a way to connect this new knowledge I was acquiring to my expertise in organometallic and coordination chemistry. One day an inspired thought flashed through my mind: If one could use spin-labels to develop non-radioactive immunoassays, why not use non-radioactive metal atoms as labels in non-radioactive immunoassays? The metal atoms, in the form of their organometallic or coordination derivatives, could be attached to analytes by suit- able chemical reactions, just as the stable nitroxide radicals were being attached for use in the spin-label immunoassays. Instead of utilizing ESR for detection of the label, one could employ one of the many analytical methods al- ready available for the detection of metal atoms. I became very excited by this idea. I went to discuss it with Ted Ullman and to ask If Synvar might be interested in collaborating with me in this area of research. Ted thought that it was an interesting idea but did not believe that it was compatible with the expertise at Synvar and its research programs. However, he said, Synvar would be interested in entering into a collaboration with me, after my return to Israel, in the development of additional spinlabel immunoassays, such as, for example, the detection of marijuana and hashish metabolites in the urine of users of these drugs. I gladly agreed to this proposition, because it provided me with a mechanism to continue working in this new field and to prepare the ground in my laboratory for work on my new idea. We agreed that Synvar would cover half the cost of a Varian ESR instrument for my laboratory in return for my submitting regular progress reports to Synvar on the spin-label research. We also agreed that I was free to work on the idea of metal-labels without any obligation towards Synvar.

#### E.A.H.—What happened after your return from your sabbatical?

We returned to Haifa in September 1971 not knowing beforehand that I would be faced by a surprising development. Some of my colleagues suggested to me that I should submit my candidacy in the forthcoming November elections for Chairman of the Department of Chemistry for the term beginning 1 January 1972. I was very reluctant to do so because I knew that the position was very demanding. I did not like the idea of a full-time job doing administrative work. Furthermore, I was afraid that this would interfere with my research plans, in particular with my intention to set up the new immunoassay project. I want to remind you, Amitai, that I turned to you and asked that you should be the candidate to stand for election as Department Chairman. For your own good reasons, you refused to do so and tried to convince me that I should be a
candidate. David Ginsburg, who had been Chairman for many years, put a lot of pressure on me to accept the candidacy. I agreed finally and was elected Department Chairman, a position I held for the years 1972–1976. I believe I did a reasonably good job but—in retrospect—I think that this was the most difficult period of my life in academia. Remember that during that time we also went through the traumatic experience of the 1973 Yom Kipur war!

E.A.H.—I know that in spite of taking up this new position you did proceed with your plans to initiate the immunoassay program in your lab. How did you go about it and what happened to your other research projects?

The organometallic chemistry research was coming along very well. We were getting good results in the work on the catalytic properties of arenechromium tricabonyl complexes in the stereospecific and regioselective hydrogenation of conjugated dienes. Need, I remind you that you were my consultant on reaction kinetics and that the two of us published a joint paper on one of the hydrogenation topics? The work on alphaferrocenylcarboniun ions was very productive. A milestone in this work occurred when one of my doctorate students, Sanda Dani, managed to obtain a stable crystalline salt of such a carbonium ion, and we were able to determine its crystal structure in collaboration with Frank Herbstein, the crystallography expert in our department. About half of the people in my group were working on these and related topics. The other half agreed to help me set up the immunoassay project and begin research in this area. My aim was to become self-sufficient in all the various facets of the project. I was glad to associate Dr. Harriett Gershon, the immunology expert in the Technion's School of Medicine, as consultant to the project. With her help we set up an animal room on the roof of our building, with 24 rabbit cages, for immunization in the production of antibodies. All the students and technicians working on the project were required to undergo training in the immunization of the rabbits, collection of the blood and isolation of the anti-sera. In January of 1972, as agreed with Synvar, we received the ESR instrument from Varian. Everyone in my group was trained to use the ESR machine and to carry out calibration curves with the nitroxide spin labels. Alfred Modiano, one of the more experienced technicians in my group, learned to synthesize the various nitroxide radicals needed for our work. By agreement with the Haifa branch of the Department of Drug Control in the Ministry of Health, I received a permit to obtain various kinds of abusive drugs from pharmaceutical companies and store them in a special safe in the laboratory. They included barbiturates, methadone, amphetamines and kilogram quantities of hashish, the last from the police stocks of confiscated drugs for use in our research; I had to submit monthly reports to the Ministry of Health on the amounts of drugs used in experiments during that month. We set up facilities for the extraction and

isolation of the cannabinoid constituents of hashish. I already had experience with suchextractions from my work on *Catha Edulis*. We also collaborated on the Mechoulam of the Hebrew University of Jerusalem, who was the first to synthesize tetrahydrocannabinol (THC), the active constituent of hashish and marijuana. He sent us samples of various THC derivatives for comparison with the products that were isolated and synthesized in our laboratory. We were now ready to proceed with the development of a spin-label immunoassay for the detection of hashish and marijuana metabolites in the urines of users of these drugs.

# *E.A.H.*—*How successful were you in developing this assay, and how long did it take you to complete this project?*

Remember that we started the immunology work from scratch. The chemical work was no problem. The most important aspect was the production of specific antibodies for the recognition and binding of cannabinoid derivatives. We knew from reports in the literature that a number of laboratories in Europe, USA and Canada were trying to develop such antibodies. Some of these reports stated that they may have developed antibodies, but they were of rather low specificity and not suitable for the development of an immunoassay. This was rather discouraging, but on careful perusal of these reports I noticed two interesting points: first, all these reports emanated from biological laboratories; second, the experimental descriptions in these reports convinced me that the researchers involved were not chemists with experience in the isolation of pure compounds from natural products. I came to the conclusion that the low specificity of their antibodies was due to their having used impure mixtures of antigens in the immunization process. Consequently, I instructed my coworkers to pay very careful attention to the various purification and characterization steps in the preparation of the cannabinoid antigens used for immunization of our rabbits. My assumption must have been correct, because we obtained antibodies of very high specificity. In fact, following publication of our data, Dr. S.P.M. Lems, of the Immunochemistry Department, Central Laboratory of The Netherlands, Red Cross Blood Transfusion Service, in- formed us that he was carrying out comparative studies of cannabinoid anti-sera from various laboratories in Europe and requested samples of our anti-sera for inclusion in his tests. Subsequently, Dr. Lems reported to us that our cannabinoid anti-sera were found to be the most highly specific of all the anti-sera tested. To cut this long story short, once we had good antibodies, we felt we had in our hands the means of developing a working spin-label immunoassay for the detection of hashish and marijuana metabolites. It took us about 2 years to complete all the various aspects of the production of a satisfactory free radical immunoassay, and to be confident enough to publish our results jointly with Mechoulam and Gershon in FEBS Letters in July 1975. Naturally, during all this time

we fulfilled our obligation to Synvar, who were kept fully informed of our progress through quarterly reports. Eventually, Syva marketed their own spin-label immunoassay kit for the detection of hashish and marijuana metabolites in urine.

E.A.H.—When did you start working on your concept of using metal-labels in immunoassays?

We decided from the start that in parallel with the work on the spin-label work we must also begin working on the metal-labeling project. In order to develop a strategy to prove the feasibility of the concept, we had to decide on an approach to two major problems in the task before us: (1) choice of the analytical method to be used in the assay for metal detection and (2) the development of suitable chemical reactions for the attachment of coordination metal complexes to various types of organic compounds known to be analytes in existing immunoassays. The analytical methods under consideration were emission, absorption and fluorescence spectrometry, electrochemical methods, neutron activation, etc. The method of choice for us was atomic absorption spectrometry (AAS). Our department possessed two flameless-operation AAS instruments: a Perkin-Elmer 403 with HGA-70 graphite furnace and deuterium background corrector as well as an Instrumentation 351 with 455 flameless unit and programmable calculation data system kit. These instruments were available for use by my research group. In addition, we had the benefit of expert advice by Prof. Magda Ariel, from the Analytical Chemistry section in our Department. I insisted that everyone working on the metal-label project become proficient in the use of the AAS instruments. As model analyte compounds in the chemical work, we chose the steroid hormones estrone, estradiol and estrastriol, barbiturates, such as phenobarbitone and seconal, and cannabinoid derivatives. We prepared our own antibodies to these compounds. The extensive chemistry of transition metal-organic complexes that had been developed in the previous two decades provided a rich and versatile source for the synthesis of "metal-labeled" haptens and antigens. We were able to produce metal-labeled analytes, which we designated metallohaptens and/or metalloantigens, by attaching a variety of metal complexes to the abovementioned model analyte molecules. To give several examples, we attached Fe atoms by means of ferrocene derivatives; Mn atoms through cymantrene derivatives; Pt and Rh through cycloctadiene-acetylacetonato complexes; Pt and Pd through atoms functionalized ortho-catecholato complexes; Cr and Co atoms through functionalized tris-acetylacetonate complexes. We thus had in our hands the components required to begin testing the feasibility of the metal-label immunoassay concept. After much hard work and many problems, we were able to show that the concept was feasible, and in October 1977 we sent a communication, entitled "Metalloimmunoassay", to the prestigious journal *Nature*. This article summarized over 5 years of work by many co-

workers, graduate students and post-doctorate fellows. All of them, nine in all, were named as co-authors of this communication. To our delight, after only 3 weeks we received notification that the article had been accepted for publication without any comments by referees requesting revisions and/or corrections! The article was published in the 8 December 1977 issue of Nature. The publication of Metalloimmunoassay (MIA) in Nature was timely for us. Bio-inorganic chemistry was becoming a very fashionable research area, in particular due to Prof. B. Rosenberg's discovery of the anti-tumor properties of platinum complexes. I thought that our experience in developing methods for binding metal complexes to haptens and antigens might find applications in this interesting field as well. In October 1978, I visited Prof. Rosenberg at Michigan State University in East Lansing to discuss with him some of my thoughts on this subject. For example, one of the ideas I suggested was the possibility that binding a *cis*-platinum complex to an estrogen steroid molecule might provide a "magic bullet" that would be selectively recognized by the estrogen receptors on the tumor cells in breast or ovarian cancers. Rosenberg was receptive to these ideas and encouraged me to go ahead with research in this direction. Our own facilities to do work in this field were quite limited and we lacked the necessary expertise. Consequently I entered into collaboration with several groups. In fact, this was the reason for my Visiting Appointment at the Memorial Sloane-Kettering Cancer Center in New York for 3 months in the summer of 1979. I asked Moshe Shimoni, a talented and industrious doctorate student, to come with me and, in collaboration with several of the scientists at Sloane-Kettering, we synthesized mercury derivatives of estradiol and tested their selective recognition by estrogen receptors in mammary-tumor-bearing rats. We did not get conclusive results because of difficulties in the determination of mercury by atomic absorption. However, in 1982 we repeated some of this work in collaboration with a group in Israel led by Dr. J. Shani, using radioactive 203-Hg in preparing the mercuriestradiol derivatives. The results showed conclusively that the selective accumulation of mercury in the mammary-tumor tissue of the tested rats was greater by a factor of more than 10 compared to other tissues. In fact, the paper reporting these results was awarded the first prize in the competition for the Genia Czerniak Award in nuclear medicine and radiopharmacology, held in Israel in 1982.

*E.A.H.*—Let's go back to the development of MIA. You mentioned that you encountered problems in this development. What were these problems and how did you overcome them?

As I said, we encountered many problems. Some of the major ones were inherent in the protocol of an immunoassay. If you remember, the end point of such a protocol involves determining the relative proportions of the "free" and "bound" labeled reagent, after

the physical separation of these two fractions. A variety of methods to separate "bound" from "free" fractions had been described in the literature. However, the large majority of the practical methods available required the addition of a precipitating or adsorbing reagent after the primary incubation step, to be followed by incubation, the MIA system, the required addition of a precipitating reagent constitutes a highly critical and error-prone step, since the reagent must be ultra-pure insofar as metal ion content is concerned. The analytical method in MIA, as in other immunoassay systems, must determine the labeling-metal atom content in concentrations of g/ml or lower. Consequently, the introduction of any additional reagent into the protein-binding reaction system carries with it the danger of contamination with the same metal atom or ion being used for the labeling of the antigen. In a flash of inspiration, it occurred to me that a potentially useful solution to this problem was to investigate the possibility of using solvent extraction methods to separate the "free" from the "bound" fractions, instead of using precipitating or adsorbing reagents. Metal-free organic solvents of analytical reagent or purum grade were readily available and did not require additional purification. I asked Moshe Shimoni, who had previous experience in immunoassays, to start investigating this idea as part of his doctoral work. After a number of initial experiments, it transpired that the use of an extraction solvent for the required separation eliminated the need for centrifugation. Instead of performing this step, one could mix the aqueous and organic phases with a short (20-30 s) vortex operation. Then, after the spontaneous phase separation, aliquots could be taken with a pipette from the solvent and/or aqueous phase and transferred to another tube or vial to be taken to the analytical instrument. We soon found out that the method, as just described, involved too many error-prone manipulatory steps. It was clear that we had to find a way to permit the whole assay, including the solvent extraction and complete segregation of phases, to be carried out in the same tube. After testing a number of approaches, we finally designed a device which consisted of two parts: (1) a polypropylene test tube, the assay tube, in which mixing of the aque- ous immunoassay mixture and the extracting organic solvent would take place; (2) a piston rod that we called a mixer-separator piston, also made of polypropylene, with a channel traversing its vertical axis and shaped at its upper end in the form of a collecting container. Slightly above its lower end, the piston was fitted with a sealing element, such as a rubber or Teflon O-ring. When the mixer-separator is pushed down into the assay tube, the O-ring seals the tube hermetically. The upper liquid phase is aspirated through the channel and is transferred into the collecting container. At a predetermined distance from the upper end of the mixer-separator piston the collecting container is wider than the assay tube. In this way, on pushing the piston down into the assay tube as far as it would go (until stopped by the contact between the collecting container and the rim of the assay tube), it is possible to achieve quantitative

collection of a predetermined volume of liquid as well as the required physical separation between the two liquid phases.

#### E.A.H.—How did this new device perform in practice?

We had to use an established immunoassay system to test the efficacy of the device and to carry out comparative studies in order to better analyze the results obtained in the MIA work. We had become convinced that the solvent extraction method need not be limited to the MIA system, and felt that this approach could also have interesting potential for use with immunoassays in which markers other than metal atoms or ions were used for labeling the haptens. We decided to choose RIA, the most sensitive assay available, for our comparative studies. This necessitated the transformation of one of my two labs into a radioactive work area and we had to acquire beta- and gamma-counters for the RIA tests. We bought commercial RIA kits, for testing estriol, digoxin and thyroxine. The reagents were labeled with either tritium for beta-counting or (125)iodine for gammacounting. We carried out parallel comparative experiments: one experiment, following exactly the instructions in the commercial kit protocol and a second experiment with the same reagents, but following our solvent extraction method with the new device. The results were highly satisfactory and we were able to demonstrate conclusively the feasibility and generality of the solvent extraction separation method in immunoassay protocols. However, though successful, we were aware that use of this method in immunoassays was limited to relatively low-molecular-weight haptens for which suitable extraction solvents could be found. On the other hand, it occurred to me that the concept of the design and operation of our device could be applied to the development of a novel solvent extraction apparatus to replace the ubiquitous separatory funnel. Interestingly, the separatory funnel in its presently familiar shape was first described by the famous Swedish chemist Berzelius in his textbook published in 1841. It is surprising that in spite of the many well-known drawbacks and problems encountered-especially by students-in the operation of separatory funnels, no significant conceptual changes in the design of the separatory funnel have occurred over such a long period of time. With the help of technicians in the glass blowing workshop of the Chemistry Department, we constructed prototypes of a new glass instrument for laboratory liquid-liquid extraction, based on the principles of operation in the immunoassay solvent extraction device. One could say that we invented the "separatory cylinder" to replace the "separatory funnel"! I coined the name Mixxor (from the words "mixer" and "extractor") for this new instrument. I am pleased to say that the Mixxor is currently available commercially for small and medium volumes (2, 5, 10, 20 and 50 ml) in solvent extraction operations and is in use in many laboratories worldwide. You may be interested to know that the web search engines Google and

Overture come up with more than one hundred web references in response to a search request for the item "mixxor".

E.A.H.—I believe that you expanded the Mixxor liquid–liquid separation method to a new device for solid–liquid separations and to a novel concept in liquid chromatography. Would you tell us something about these developments?

For those unfamiliar with immunoassays, let me explain that the most widely used procedures are based on adsorption (charcoal, silicates), fractional precipitation (ammonium sul- fate, ethanol, dioxin, polyethylene glycol), double antibody and solid phase reagents, all of which end up with a system of suspended particles in a liquid medium. The selection of any particular procedure is determined by consideration of many interrelated factors, such as solubility of the compound, characteristics of the antiserum, fraction to be counted, degree of non-specific binding, and type of radioisotope. However, one feature that is common to all of these techniques is the need for a centrifugation step to effect aggregation of the suspended solid particles, followed by a decantation (or suction) step to separate the solid and liquid phases physically. Our success with the non-centrifugation, non-decantation device in the solvent extraction procedure opened the way to the development of a modified device which, subsequent to mass transfer between a fluid and particles suspended in it, would achieve a solid-liquid separation by allowing for mass transport through a selective barrier. Clearly, for such a separator to be useful in immunoassays, it should-among other things-eliminate both the centrifugation and decantation manipulations. Moreover, the separator device should be suitable for use with any of the aforementioned separating reagents, without prejudicing the freedom of selection of any particular reagent, as may be dictated by a consideration of the various interrelated factors enumerated above.

The piston of the mixer-separator used in the solvent extraction procedure was modified as follows. The lower end of the piston had a cavity, in which a membrane of choice was fitted and held tightly in place by a ring disc. A sealing element (O-ring), at a predetermined distance from the membrane cavity, allowed the piston to fit snugly into the test tube and slide along its inner walls. A circular groove was designed on the piston segment, between the membrane cavity and the O-ring. The opening on top of the collection chamber could be closed with a tight fitting cap. The whole device was made of polypropylene. On operation, the mixer-separator piston is pushed down into the test tube. This causes the liquid phase to be aspirated through the orifice bore of the ring disc, then through the axial channel, and finally to accumulate in the collection container. The membrane in the cavity at the bottom of the piston acts as a selective barrier, preventing the solid particles from being transported together with the liquid phase into

the upper container. The groove between the membrane cavity and the O-ring plays a crucial role in the performance of this device. During the downward movement of the piston, the groove serves to entrap a predetermined amount of air (an "air pocket") which—on compression—acts as a cushion, relieving part of the pressure resulting from the resistance of the membrane to liquid flow. Upon decompression any liquid that may have crept up into the space between the inner wall of the test tube and the outer walls of the terminal end of the piston is forces back into the tube. The manual operation of the steps involved in the use of this device in an immunoassay protocol is very simple and can be easily carried out by a technician. We have also designed and built a simple automatic instrument that can process up to 40 assay tubes simultaneously in less than 3 minutes. As with the previously described solvent extraction device, we used the solidliquid separator to carry out comparative RIA experiments with commercial kits and obtained excellent results. The methodology and design of the separator practically eliminate potential contact with the radioactive reaction mixture, thus ensuring maximum safety from radiation hazards. Our results show that the use of this separator provides a feasible, practical alternative to centrifugation and decantation in the immunoassay protocol. Furthermore, beside immunoassays, this device has many other potential applications in fields where selective filtration is required. Indeed, a slightly modified version of this device is currently being produced and marketed under the generic name "syringeless filter" by a well-known filtration products company that acquired the patent rights for our invention.

#### E.A.H.—What about applications to chromatography?

The principle of a hollow piston moving up and down in- side an outer tube under hermetically closed conditions, as applied in the liquid–liquid extraction and solid–liquid separation devices, was so simple that it induced thoughts in the direction of other potential applications. For an experimental chemist like me, it was inevitable that such thoughts would lead towards the field of liquid chromatography, which was being used routinely in our laboratory. As you know, in conventional liquid chromatography of all types, the mobile phase percolates through a stationary phase as a result of a pressure drop induced by various means, e.g. gravity, pumping systems, gas pressure cylinders and capillary forces. Except for the last, all these driving forces are external to the stationary column. In the case where capillary forces alone are responsible for movement of the mobile phase, the flow is not uniform along the chromatographic bed and eventually stops. We applied our methodology to the development of a new concept, which I called dynamic column liquid chromatography (DCLC). In DCLC, flow of the mobile phase is achieved by forcing a densely packed chromatographic column through the eluent in a system that is completely closed, except for the column outlet. Under such conditions, an intrinsic pressure develops in the system that causes the mobile liquid phase to flow through the moving chromato- graphic bed in a direction opposite to the movement of the packed column. This is in direct contrast to the principle of a conventional syringe pump, in which the liquid is forced out in the same direction as that of the plunger movement. An important, unique feature incorporated in the DCLC system is that the flow rate of the mobile phase is fully con- trolled by the movement of the column. No flow occurs when the column is at rest. Another important aspect featured in the DCLC concept is that, as a consequence of the intrinsic pressure that develops in the closed system, external pumping systems can be replaced by a simple mechanical device that holds the dynamic column and governs its movement. Withoutgoing into further detail, I can tell you that we designed special glass chromatography columns which were pushed through syringe reservoirs by means of a simple mechanical instrument and were able to prove the feasibility of the DCLC concept. We also applied the dynamic column methodology to the development of a solid phase extraction device (DC-SPE). We published our results in the appropriate chromatography journals.

#### E.A.H.—What is the I-R100 Award mentioned in you bio- graphical sketch?

The Chicago-based magazine, Industrial Research and Development, runs a competition every year in which a panel of distinguished scientists and industrialists select the "100 Most significant New Technical Inventions of the Year". The I-R100 Award, in the form of an engraved silver plaque, is presented at a ceremonial black-tie banquet held at the Museum of Science and Industry in Chicago, where the selected inventions are exhibited over a period of 1 month in one of the halls of the Museum. In 1982 we received an award "for the development of solid–liquid and liquid–liquid separating devices" and again in 1983, "for the development of dynamic liquid chromatography column". It was very flattering for us to find ourselves on the list of awards together with recipients from prestigious companies and institutions such as, Argonne National Laboratory, Bell Laboratories, Dow Chemical Co., E.I. Du Pont, General Electric,

Los Alamos National Laboratory, Oak Ridge National Laboratory, Standard Oil Company of California, Westinghouse Electric Corp., to mention just a few.

#### E.A.H.—Is there anything more that you'd like to tell us?

In August 1982, I gave a lecture at the Gordon Research Conferences entitled "Serendipity in a Bio-Organometallic Chemistry Research Project". The Oxford Concise Dictionary defines serendipity: "the faculty of making happy and unexpected

discoveries by accident". I think serendipity has made my life interesting. There's never been a dull moment, and, if I may quote my favorite Frank Sinatra song, "I did it my way"!

#### Acknowledgements

First, I want to thank Barry Lever for initiating this interview and you, Amitai, for conducting the interview and for your patience in listening to my story. Second, I wish to thank all my students, co-workers and collaborators without whose diligence, devotion and research contributions there would have been no story to tell. Last, but certainly not least, I wish to express my deep gratitude and heartfelt thanks to Judy, my wife, companion and friend of nearly 60 years, for her unswerving help, support and understanding through good times and bad; and to thank our two precious daughters, Daphna and Ruthi, for their steadfast love and respect for their parents.

## *Mordechai Folman Born Poland, 1923-2004*



**Ph-D thesis**: University of Cambridge, England 1958

#### **Post-doc position**:

Wayne State University, Detroit, Michigan 1955-57 Ethyl Corp., Detroit, Michigan 1957-58

#### **Technion Position**:

Lecturer, 1954-58 Senior Lecturer, 1958-63 Associate Professor, 1963-67 Professor, 1967

#### Sabbatical leaves:

NBS Washington, 1966-67 University of Bristol, 1967

## Major departmental responsibilities:

Dean, Faculty of Chemistry, 1976-79

## **Field of Research**

Physical Chemistry Physics and chemistry of surfaces, spectroscopy of absorbed molecules, field emission microscopy, surface electron microscopy

## מרדכי פולמן1923-2004

## <u>חלק א. ביוגרפיה אישית</u>

## נכתבה בידי אשתו ד"ר ונדה פולמן, בעזרתם של בנותיהם דורית רינגרט וד"ר אורנה כוכבי ובנם ארי פולמן)

מרדכי פולמן נולד ב- 14.4.1923 בלודז' שבפולין. בן זקונים להוריו אריה ואיטה לאה, ואח לאדם ולאווה.

אריה אביו, היה רואה חשבון בחברת טכסטיל מובילה, בנו של הרב ישעיהו פולמן, שהיה מוציא לאור של ספרי קודש, מפיוטרקוב. אימו עסקה בעיקר בטיפוח משפחתה, כמו גם בפעילות ציבורית. אביו של מרדכי נפטר לפני המלחמה בהיותו בן 13. מרדכי, שהיה תלמיד מצטיין במקצועות הריאליים, המשיך את לימודיו בגימנסיה העברית על שם בראודה וכדי לממן לימודים יוקרתיים אלה, היה נותן שיעורים פרטיים. במקביל היה פעיל בתנועת "הנער הציוני".

ב- 1940, אחרי פרוץ מלחמת העולם השניה ופלישת הגרמנים לפולין, גורשה משפחת פולמן- מרדכי עם אימו ואחותו לגטו לודז'. אחיו אדם שהה אז בורשה ועם סיומה של המלחמה נפגשו. עד 1942 התקיימו לימודים בבתי הספר התיכוניים בגטו ומרדכי סיים שם את לימודיו עם תעודת בגרות מלאה בפולנית וכן תעודת בגרות עברית. גם בגטו היה פעיל בתנועת הנער מלאה בפולנית וכן תעודת בגרות עברית. גם בגטו היה פעיל הנועת הנער הציוני והיה מבין מייסדי ההכשרה הראשונה. ההכשרה התקיימה במסגרת של חיי קומונה שיתופיים במטרה להכין את הנער לעליה לפלסטינה, שם הכיר את ונדה בת כיתתו. החיים בהכשרה נמשכו כשנה ועם התפרקותה, ונדה ומרדכי המשיכו את חייהם המשותפים ומאז לא נפרדו. בגטו מרדכי הועסק בעבודות שונות: פקידות והוראה בבתי הספר היסודי. ב-1943 נפטרה אחותו אווה משחפת והיא בת 23. ב-17 לאוגוסט, עשרה ימים לפני חיסול הגטו ולפני הגירוש לאושוויץ, ונדה ומרדכי נישאו וחתונתם היתה האחרונה בגטו לודז'. באושוויץ ניספו אימותיהם ואחרי זמן קצר אחותה של ונדה.

בהגיעם לאושוויץ הופרדו ונדה ומרדכי אך בעזרת אסיר פולני והמחתרת הפולנית, הצליחו להחליף ביניהם מכתבים והבטיחו לעצמם להיפגש שוב בלודז' בתום המלחמה.

מרדכי הועבר מאושוויץ לבראונשוויג (Braunschweig), שם עבד בעבודת כפיה במכניקה עדינה. לקראת סיומה של המלחמה נעשתה עיסקה לשיחרור אסירים יהודיים ממחנות ריכוז, ביניהם המחנה בו הוחזק מרדכי. היתה זו יוזמתו של ברנדוט, דיפלומט שוודי, שפעל לשיחרורם של היתה זו יוזמתו של ברנדוט, דיפלומט שוודי, שפעל לשיחרורם של האסירים לשבדיה בדרך ימית. מרדכי עלה על סיפונה של אניה ובדרכו לשחרור, עת פרצו המשוחררים בשירת "התקווה" לנוכח הגרמנים שהצדיעו לשחרור, עת פרצו המשוחררים בשירת "התקווה" לנוכח הגרמנים שהצדיעו להם, התקבלה הודעה על ביטול העיסקה והשבויים הוחזרו למחנות. מרדכי הועבר למחנה וובלין, (Wobbelin) ושוחרר ב- 2.5.45 . אז החליט לקבוע תאריך זה כיום הולדתו ולמעשה רק בעת פטירתו נודע לילדיו תאריך הולדתו הטבעי.

מרדכי חזר ללודז', שם פגש מחדש את אשתו ותוך זמן קצר החל את לימודיו במכון הטכנולוגי בלודז' (Polytechnika Lodz), את עבודת המסטר עשה בהנחיית פרופ' אליציה דורביאלסקה (Alicja Dorabialska), שהיתה בעבר האסיסטנטית של מריה קירי. בשנים אלו היה מרדכי פעיל ב-Z.A.S.S., אירגון אקדמי ציוני סוציאליסטי.

לאחר סיום לימודיו ב- 1950, עלה הזוג ארצה. בתחילה עבד מרדכי במעבדת חיל הים בנמל חיפה בניקוי מיכלים ממשקעים ימיים, אך מאד רצה להמשיך את לימודיו. הוא התקבל ע"י פרופ' אורי גרבצקי לפקולטה לכימיה בטכניון ב-1951 וסיים את עבודת הדוקטורט ב- 1955 אשתו ונדה סיימה את לימודי הרפואה באוניברסיטה העברית בירושלים. מרדכי נפטר ב- 19.10.2004 בחיפה, שם מתגוררת ונדה, רופאת עיניים בגימלאות.

לזוג שלשה ילדים:

ד"ר ארנה כוכבי, רופאת עיניים הנשואה להיסטוריון פרופ' אריה כוכבי ולהם שלשה ילדים: טליה אורי ודורון. דורית רינגרט ציירת הנשואה ליגאל רינגרט, מהנדס מכונות, ולהם שלשה ילדים: גיל מאיר ויצחק. ארי פולמן במאי ותסריטאי הנשוי לענת אסולין, מפיקת קולנוע וטלוויזיה, ולהם שלשה ילדים: לי-ים מורי וליבי.

## <u>חלק ב. ביוגרפיה מדעית</u>

נכתבה בידי פרופסור אלון הופמן, פרופסור יפה קוזירובסקי וד"ר אביבה לובצקי)

כאמור, מרדכי פולמן עשה את עבודת המסטר בהנחיית פרופ' אליציה דוראביאלסקה בלודז בשנת 1950 , וקבל את התואר בהנדסה כימית (Chem.Eng. M.Sc.).לאחר סיום הלימודים עלו הפולמנים ארצה. לאחר עבודה במעבדות חיל הים בחיפה התקבל לתואר שלישי בטכניון בהנחייתו של פרופסור אורי גרבצקי. ב-1955 קבל מרדכי פולמן את התואר . של פרופסור אורי גרבצקי. ב-1955 קבל מרדכי פולמן את התואר . של הטכניון וכך נהיה הדוקטור הראשון של הפקולטה לכימיה. את הפוסט-של הטכניון וכך נהיה הדוקטור הראשון של הפקולטה לכימיה. את הפוסט-דוק עשה פולמן בקמבריג' ועבד במעבדתו של 1. געבודת מחקר זו זכתה אותו בתואר שלישי נוסף בשנת 1958. עם חזרתו לארץ התקבל פולמן כמרצה בכיר בפקולטה לכימיה בטכניון. בפקולטה עסק בהקמת מעבדות מחקר בכימית השטח, נושא אשר ההתעניינות בו הלכה וגברה באותה תקופה בעולם ובארץ עקב חשיבותו הן בתחום המחקר הטהור והן כנדבך חשוב בתעשיה.

מחקריו הראשונים של פולמן עסקו בתכונות התרמודינמיות של שטחים וכן בתכונות הדיאלקטריות של מסופחים בתחום תדירויות של R.F. ו- .M.W. הוא הקים קבוצה, שכללה מספר סטודנטים לתואר שני ושלישי, שעסקה בתכונות של מסופחים על משטחים יוניים, חצאי מוליכים, פחמן לצורותיו הגבישיות השונות ופולרן, ונחקרו במשך תקופה ארוכה בשיטות של I.R. ו F.I.R. בתחום זה בעיות של תזוזות ספקטרליות , בליעות מושרות, בליעות הנובעות מאתרי ספיחה שונים נחקרו הן ניסויית והן תיאורטית. חישובים של פוטנציאלי ספיחה עבור מערכות שונות של מספח/ מסופח חושבו והושוו לממצאים ניסויים. כבר בשלבי ההקמה הראשונים של קבוצת המחקר הצטרפה אליה, לאחר סיום התואר השלישי שלה , דר' יפה קוזירובסקי ועבדה איתו בשיתוף פעולה בנושא זה במשך קרוב ל-40 שנה. במקביל עסק פרופ. פולמן בחקר תגובות קטליטיות על גבישים יחידים .(Field Electron Emission) בשיטות של פליטה אלקטרוני F.E.E. הציוד החדשני והמחקרים הרבים שבוצעו במעבדות השונות נתמכו בין השאר תודות למענקים שנתקבלו בזכותו של פרופ. פולמן ממשרד הבטחון הישראלי, מחיל האוויר האמריקאי, ממכון התקנים האמריקאי ומקרנות בין לאומיות חשובות נוספות.

מרדכי פולמן קיבל את הפרופסורה בשנת 1967 והיה דקן הפקולטה לכימיה במהלך השנים 1976-1980 . בסוף שנות השבעים היה שותף בהקמת המכון למצב מוצק ושימש כמנהלו בין השנים 1982-1985. במכון למצב מוצק הקים פרופ' פולמן בשנת 1979, את מעבדת השטח. מעבדה זו הייתה הראשונה בארץ המצוידת במכשור שאיפשר אנליזות שטח על בסיס ספקטרוסקופיה אלקטרונית ויונית. מאז ועד היום משרתת מעבדה זו מחקרים רבים בפקולטות השונות בטכניון, באוניברסיטאות אחרות בארץ, ובתעשיה. על בסיס השירות אשר ניתן במעבדה זו בוצעו וקודמו מחקרים רבים והרבה תעשיות נעזרו במעבדה בפיתוח מוצריהם. כיום קיימות בארץ מספר מעבדות הנותנות שירות דומה, אך למעבדת השטח במכון למצב מוצק בטכניון זכות ראשונים בהקמה והפעלה של ציוד מודרני לאנליזת פני שטח. מעבדה זו ממשיכה להתפתח מאז ימיה הראשונים וכעת מצוי בה ציוד משוכלל ומתקדם. המעבדה ממשיכה לשרת חוקרים צעירים רבים באקדמיה ובתעשיה.

בכל מהלך עבודתו המחקרית המגוונת הנחה פרופ' פולמן סטודנטים רבים לתארים מגיסטר ודוקטור ( מעל ל-30 ) . הוא עקב, היה מעורב והתעניין מאוד בכל שלבי עבודות המחקר שנעשו על ידי תלמידיו. בוגרי התארים הללו נקלטו במהלך השנים בתעשיה ובאקדמיה.

מלבד פעילותו המחקרית הענפה היה פרופ' פולמן מספר שנים ראש המעבדה לכימיה פיסיקלית בפקולטה לכימיה. במסגרת זו יזם הכללתם של ניסויים חדשניים שונים המיועדים לסטודנטים לתואר ראשון ובכך הוא קידם את היכולת המחקרית של כלל המחלקה. ניתן לציין כי פרופ' פולמן ראה את ההוראה כחלק בלתי נפרד וחשוב של עבודתו המדעית בטכניון. הוא לימד קורסים רבים ביניהם כימיה כללית, כימיה פיסיקלית ברמות שונות, וקורסים קשובי השטח המתקדמים המיועדים לסטודנטים לתארים גבוהים. הוא היה קשוב לבעיות וקשיי הסטודנטים וניסה לעזור כמיטב יכולתו בפתרון הבעיות שהתגלו.

במהלך שנות עבודתו האקדמית בטכניון בילה פרופ' פולמן מספר שנות שבתון במעבדות בארה"ב ובאירופה. הוא הצטיין בקשריו הטובים עם האנשים אשר איתם עבד במהלך חייו, בארץ ובחו"ל. שמר על קשריו עם אוניברסיטת קמבריג' והיה חבר באגודת הבוגרים שלה:

.Member of the Trinity Hall Association, Cambridge

עם השנים פיתח פרופ' פולמן קשרים מיוחדים עם מדענים פולנים. קשרים אלו הביאו לקיומם של מספר כנסים דו- לאומיים בין ישראל לפולין בנושאי מדעי השטח וקטליזה הטרוגנית. כנסים אלו תרמו להידוק הקשרים בין שתי המדינות. פרופ' פולמן יצא לגמלאות בשנת 1993, ועד פטירתו ב-2004, היה פרופ' אמריטוס בפקולטה לכימיה.

## בנימה אישית - דר' אביבה ג. לובצקי

בשלהי שנת 1967 התקבלתי כסטודנטית לתואר שלישי ע"י פרופ' מ. פולמן בפקולטה לכימיה, המחלקה לכימיה פיסיקלית בטכניון. הצטרפתי בכך לקבוצתו של פרופ' פולמן שעסקה בחקר מולקולות קטנות מסופחות על משטחים מוצקים שונים בעזרת ספקטרוסקופיה ב-.I.R, שכבר התפרסמה בגין עבודות חדשניות בתחום זה.עבודתי אכן כללה חקר ספיחה של מולקולות על משטחים יוניים אבל הורחבה לחקר ספיחה פיסיקלית וכימית על משטחים של חצאי מוליכים. באותה עת גברה התעניינותו של פרופ' פולמן בחקר שכבות דקות של פחמן ואכן עבדנו ופתחנו שיטות שונות לקבלת פילמים של פחמן בעלי שטח פנים גבוה. בשיטות דומות הצליחו לקבלת פילמים של פחמן בעלי שטח פנים גבוה. בשיטות דומות הצליחו התעניינותו זו של פרופ' פולמן בשטחי פחמן לא פסקה ובמשך השנים אף הגליחו במעבדתו, בהנחיתו והנחית פרופ' י. קוזירובסקי לקבל ספקטרא הצליחו במעבדתו, בהנחיתו והנחית פרופ' י. קוזירובסקי לקבל ספקטרא

הקשר ביני לבין פרופ' פולמן לא הסתיים עם סיום התואר ופרט להפסקות שונות עקב יציאתי לפוסט דוקטורט ולשנות שבתון אכן המשכנו לעבוד בשיתוף פעולה בתחומי האינפרא אדום הקרוב והרחוק.

בשנת 2003-2004 ביליתי את שנת השבתון שלי בטכניון. ביוזמתו של פרופ' פולמן ובשיתוף פעולה עם פרופ' אלון הופמן, שהמחקר נעשה במעבדתו, משטחי יהלום וסיבי פחמן( C- nanotubes ) נחקרו.

בשנה זו התמודד פרופ' פולמן עם מחלתו הקשה אבל לא ויתר על קשר יום-יומי עם המעבדה ועקב אחר עבודתנו המשותפת.

אין לי ספק כי אהבתי למקצוע ולמחקר יהיו קשורים לעד עם אהדתי והכבוד הרב שרכשתי לפרופ' פולמן שנמשכה על פני קרוב ל-40 שנה.

## Haggay Gilboa

Born Israel, 1936



**Ph-D thesis**: Technion-Israel Institute of Technology, 1955

#### **Technion Position**:

Lecturer, 1960-72 Senior Lecturer, 1972-83 Associate Professor, 1984-2003

## Major departmental responsibilities:

Head of Physical Chemistry Laboratory, 1988-1992

## **Field of Research**

Magnetic resonance as applied to structural and dynamical problems in chemistry and biology

## Haggai Gilboa

#### (June 2008)

I was born in Jerusalem in 1936. Shorty after by birth my parents moved to Tel Aviv, where we lived in a small single room apartment. The toilets were shared between four families and there were no running hot water. The usual conversation between my parents was how to get some more work. My mother was a seamstress and mended torn shirts for the neighbors, she did not earn much doing this work. My father, who studied agriculture in the MIKVE ISRAEL Agricultural School, had to move to the city because both he and my mother were infected by Malaria during their work as farmers. For several years he worked in the Gardening Department of the Tel Aviv municipality. Since he did not belong to the MAPAI party, he was fired every other day. As a Gardner my father got some private work tending gardens of "wealthy" people. Very often after school he took me to help him with his work. I hated it, instead of playing with my friends I had to clean YABLIT in gardens. In order to eradicate the YABLIT you must to find every trace or root of it, so it would not grow again. It was a hard and boring work and as a result till today, I dislike any sort of gardening! In spite of those poor circumstances I felt quite well, and did not think about it or compared our conditions to those of other neighbors.

As a child my playing grounds were the streets or the sandy roads of Tel Aviv. We never played at home, and group plays were very common. The children in those times were very independent. I remember myself walking alone to the nursery schools at the ages of four and five, where the distance to the destiny was more than a kilometer.

I remember when Italian planes bombed Tel Aviv in 1940. A bomb fell close to our place, and a deaf child, with whom I used to play, was killed. We had no electricity in our apartment. I read and did my home work using a small oil lamp.

As a teenager I joined the marine branch of the HSHOMER HTZAIR youth movement. We sailed every Saturday and on vacations along the coast from ROSH HANIKRA to ASHKELON. At high school I was interested in Chemistry, and decided to open my own laboratory at home. At a second hand bookshop I bought a book on Analytical Chemistry written by Kirzon (in Hebrew), for first year students at the University. In the beginning of that book I found a least of the reagents needed. I bought most of them, but could not understand why no one agreed to sell me KCN! I went from Pharmacy to Pharmacy in Tel Aviv and always got the same negative answer. Only later I realized that KCN and "Cyan Kali" are the names of the same poisonous compound.

As a soldier I served in the NAHAL. After my release from the army I joined kibbutz BEIT NIR. On leaving the kibbutz I started my academic studies. I got my B.Sc. and M.Sc. degrees from the Technion under the supervision of Prof. A. Loewenstein. The "Six Days War" began in the period while I was studying for my D.Sc. degree. During the war I was badly wounded, that happened on the northern front. Professors A. Loewenstein, D. Ginsburg and Dr J. Altman helped me to finish my studies and get my DSc degree. After a convalescence period I joined the Faculty of the Chemistry Department at the Technion.

In all my academic research I used the NMR technique applying it to chemical and biological systems. My M.Sc. and D.Sc. Theses were mostly on the magnetic non equivalence of organic molecules, like carboxylic acids in aqueous solutions, we also used the properties of magnetic non equivalence for kinetic studies and calculated energy barriers to ring inversion of di- and tri-cyclic compounds like decalins, hexalines and propellanes. From the results we were able to estimate the ring inversion path.

I also studied relaxation times and line shapes to get the appropriate correlation times of molecular movements in Liquids and solids. This work done in collaboration with my colleagues A. Loewenstein, B. Silver, A. Ron. S. Shlik , and the students R. Bar-Adon and I. Perlman.

Later I studied the properties of Sodium and Lithium in biological systems, in particular of hallophilic bacteria. This work was carried out with my students M. Goldberg, R. Melamud and A. Sakhnini. With Prof. Y. Avi-Dor we tried to

#### **David Ginsburg**

Born USA, 1920-1988



**Ph-D thesis**: New York University, 1947

#### **Past Positions:**

Weizmann Institute of Science, 1948-54

#### **Technion Position**:

Professor, 1954-1981 Research Professor, 1981-1988

#### Sabbatical leaves:

Harvard University, 1952-53 and 1960-61 Brandeis University, 1960 and 1968 University of Zurich, 1961 University of Saskatchewan, 1964 New York University, 1964, 1965 Boston University, 1967 Mac Gill University, 1968, 1969 University of Oxford, 1970-71

#### Major departmental responsibilities:

Dean, Faculty of Chemistry, 1955-59 Dean, Faculty of Chemistry, 1963-67

#### Major Technion/International responsibilities:

Chairman, Israel Research Council, 1961-62 President, Israel Chemical Society, 1955-57 and 1966-67 Member bureau IUPAC, 1963-69 Technion, Vice-President for Research, 1959-64

### **Field of Research**

Alkaloids, Stereochemistry

#### **David Ginsburg the Man**

By Simona Ginsburg (July 2008)

David Ginsburg was, of course, a scientist, but to his family and close friends he was, foremost, a man who lived his life with great energy; he worked, played, relaxed and experienced the world with immense intention and intentionality, whether doing chemistry, listening to music, reading a menu, savoring a new landscape, or buying a gift. His stamina radiated towards his close ones, to whom he was totally devoted, and from whom he derived joy and pride.

Born in New York (September 5, 1920), he was barely 5 years old when his mother died. Though growing up motherless, the young child was the center of attention of several adoring adults. He was raised by his aunt Haya Ginsburg-Friedman, by his father Simon (1890-1944), and by his uncle Jekutiel. Simon Ginsburg was born in the small village Lipniki in the Ukraine, a year after his elder brother Jekutiel (1889-1958) and 4 years before his younger brother Pesach (1894-1947). In 1897 the whole family moved to Zhitomir and in 1909 Simon moved to Odessa, where he held the post of personal secretary to Sholem Yakov Abramovich, better known by his pseudonym Mendele Mocher Sfarim. The three brothers, their sister Haya and their mother Beile reached the United States in 1912-13. Simon, by then a Hebrew poet and writer, with his wife Taibel (née Abramovich) launched a Hebrew school in New York. David's first language was Hebrew, in Ashkenazy accent, with English mastered soon after, once he began attending public-school. The young David thus grew up bilingual, his English and Hebrew serving him well throughout his entire life, making him feel at home in Israel where he eventually settled, and virtually anywhere else he lived or visited. Uncle Jekutiel, who was a mathematician (he headed the Mathematics Department of Yeshiva College) had a knack for explaining complicated problems in simple but precise terms; he himself was childless, but he adored young people, and acting as second father to David may have influenced the latter in whetting his interest in science.



David as a child with adoring adults Clockwise from top left: Pesach, Simon, Jekutiel, Beile, David, Haya

Simon and David (then 13) came to Palestine in 1933. They lived in Tel-Aviv, and Simon was an active member of the Hebrew Writers Association ("Agudath Hasofrim") and quickly became part of the milieu of local artists and writers, including Bialik, who was a family friend. The Bar-Mitzvah boy was highly impressed with the national poet, started composing poems himself and received written critiques and criticisms from Haim-Nachman until his death in 1934. Judging from several remnants of those attempts at poetry, this was fortunately a short-lived career; but the early training in writing, coupled perhaps with natural talent inherited from Simon, did leave a lasting mark on David's later work. He was swift with his pen, and unlike many scientists, the act of writing was completely painless to him. He enjoyed reaching the stage of writing up scientific results, and took pleasure in adorning his papers with non-scientific quotations and other literary embellishments.<sup>1</sup>

Shortly after settling in Israel, Simon Ginsburg re-married, and the relations between David and his father's wife were strained. David could not wait to finish high-school and leave home. He was graduated from Gymnasia Herzliya before turning 17 and went to study chemistry at the Hebrew University in Jerusalem, on Mount Scopus. (30 years later, just after the Six-Day-War, an analytical-chemistry copybook of his as a first-year student was salvaged from the undergraduate chemistry lab, located in a building that was cut off from the rest of Israel during the 1948 war.)



A teenager in the lab

In Jerusalem, David lived in a rented room, and his main meal every week was Friday night dinner at the home of his aunt Hannah Tepfer-Ginzburg (Pesach Ginzburg's

<sup>&</sup>lt;sup>1</sup> A letter of his to a chemical journal in 1977 is entitled "A plea for a renaissance of humanistic style in scientific papers" (*Nouveau Journal de Chimie*, Vol. 1, p. 3).

widow).<sup>2</sup> The ground-floor neighbors of this aunt were the Ber Family, who had one daughter, around David's age, and three younger sons. The private English lessons that David gave these sons contributed to his upkeep. The children's mother, Rachel, was scandalized to realize how under-nourished David was, and pretty soon he became a member of the family, not a mere tutor. More importantly, it was during those days that the romance between him and the elder daughter, Hemdah (1919-1990), began, and it was to last for half a century.



Hemdah

In 1938, Simon Ginsburg moved back to New York, to raise money for the Hebrew Writers Association, and became the executive director of The Palestine Hebrew Culture Fund ("Keren Hatarbut"). David joined him and studied for his BSc in chemistry at City College, New York. In the summer of 1939, Hemdah completed her Teachers' training studies in Jerusalem and joined him. They were married in 1940, quite young. World War II kept them in the United States for quite a while and David earned his MSc from Columbia in 1941 and his PhD from New York University in

<sup>&</sup>lt;sup>2</sup> Pesach and Simon used different spellings for their surname, both in English and in Hebrew. David Ginsburg himself was extremely particular about keeping to his father's spelling, and demanded that everyone around him take note – those who did not, would not fail to remember it on future occasions...

1947. During those years their two daughters were born (Jonina, 1944 and Simona, 1947).<sup>3</sup> While David was studying, and Hemdah working in part-time jobs, the young couple was also supported by the extended family. On special occasions, such as birthdays, David and Hemdah used to exchange gifts of books, thus accumulating many classics and art albums which were to comprise the basis of their impressive library in later days.



A young couple in New York

Amidst the last battles of the Israel War of Independence (1948), they and their daughters (aged 4 and 1) boarded the Portuguese ship "The Jack Star", and set out for Israel. The boat was carrying several passengers but also some ammunition for the young state. The family settled in Rehovot, David joining the Weizmann Institute. Life in Rehovot on the early Weizmann Campus Shikun (housing), in which many of

<sup>&</sup>lt;sup>3</sup> Jonina's and Simona's nicknames are Nini and Moncy, respectively. Jonina was named after David's mother Taibel (Yiddish for "little dove", i.e. Yonina in Hebrew) and Simona after David's father.

the institute staff lived, was a bit like a Kibbutz community. Some families, e.g. those of Israel Dostrovsky and the Katchalsky brothers had their private houses, but most of us lived in modest 2.5-room apartments. The modern electrical washing machine that the Ginsburgs brought from the States served many a family, and even the large imported refrigerator was a focus of wonder and admiration. This was the era of Austerity, notorious for food rationing; whenever someone got hold of a delicacy, such as a dozen eggs or even a whole chicken brought over by a relative living in a Moshav, it was shared by all neighbors. Wives of foreign visitors who came to the Institute were shocked to learn how difficult it was to make ends meet; one of the most notable early memories that my sister and I cherish is the single cube of Hershey chocolate that David used to hand us after dinner, from the food packages that those thoughtful wives sent us on returning home. Despite material shortages, an optimistic spirit prevailed in Israel and at the Weizmann Institute, and since nearly everyone was in the same boat, there was no sense of real deprivation; social life flourished in the Shikun, with small parties in tiny apartments, with joint journeys to the beach in a crowded truck on Saturdays, and with a considerable amount of inevitable gossip.



David and family just after settling in Israel



David, family, and pipe, at the Weizmann Institute Shikun

On the recommendation of Ernst D. Bergman, in 1954 David was offered and accepted a position at the Technion and he became Professor of Chemistry and head of the chemistry department there. Compared to the Shikun in the small Moshava Rehovot, our first apartment in the city of Haifa, on Mount Carmel, was huge, and marked a significant increase in the standard of living: it had 3 bedrooms and the living-room was supplemented by a roof-sized balcony. The extra bedroom was used for guests, rather than serving as a study for David. Throughout his life, whenever working at home, he preferred to do so in the living-room, without physically removing himself from the rest of the family; concentrating on his work, noise and daily on-goings did not distract him, and he liked to pause once in a while to see what the others were doing. The large apartment and balcony were just right for the new professor, who took upon himself to build a new department, and who considered it one of his pleasant duties to entertain the newly hired staff, in an attempt to set up comradeship. Hemdah was an ideal companion for planning and carrying out these gatherings. With her gentle airs and social skills, she complemented David's bouncing robust style, and some of his less likable traits that went along with these (i.e. his short temper and tendency to express cynical remarks). On Wednesday evenings, research students and new colleagues would assemble in the living-room; the furniture was too scant, and overflowing guests arranged themselves on the floor carpet or in the balcony; after chatting, the whole company would listen to music from a collection of classical records, and sample one of Hemdah's cakes. The music was highly biased towards Mozart, David's favorite composer. (In his last ailing days, one of the only things that brought him relief and some pleasure was the sound of the Quintet for Clarinet and Strings, K. 581; and he used to follow the music by accompanying it with conductor-like handwavings.)

Even in the late 1950-s and early 60-s, an unusual cosmopolitan streak colored our house, due to foreign post-doctorate fellows and co-workers who visited David's lab. The Danish Niels Clauson-Kaas and his light-blond-headed wife and three blue-eyed sons, the Gandhi-like thin Bringhi from India, and the overly polite Kametani from Japan stirred up considerable attention on the streets of the Carmel, and our neighbors flocked out to get a good glimpse whenever they paid us visits. Extending out hospitality was a natural gift shared by David and Hemdah, and Saturdays were crammed with gatherings, dinner-parties and trips, in those years and up to the early1980-s. Many colleagues from the department were also friends - Amitai Halevi, Michael Cais, Mordechai Rubin, to name but three; social ties were lively (including some rivalries), and from hints gathered as a child and adolescent I suspect that many soul-searching conversations took place among these chemists and among their wives. Typical outings with foreign guests included the beach – with swimming and watermelon during the summer – and picnics in the woods of the Carmel hills, where we picked in winter dozens of deep-pink cyclaments, fire-red anemones and wild daffodils (when it was still politically-correct to do so); other favorites were trips to archeological sites such as Beit-Shearim and Caesarea and Acre. Perhaps the most exciting outings were the folklore-oriented stopovers in the Carmel Druze villages; in one of these, Osefia, David had a student who invited him - with his guests - to weddings and other feasts, where David was the guest of honor, a role he accepted with relish.

The early 1960-s were highlighted by the construction of the new Chemistry Building on the Neve-Shaanan Campus of the Technion. Every single Saturday morning, after a big breakfast prepared by David – the (only) meal he could cook – the kitchen was left in havoc, and we went to the building-site to explore the grounds and trace developments. Each stage of construction, from the earliest designs on paper to the final layers of paint applied to the walls, from the most mundane item to the most exhilarating (to us children - the gushing fountain) was closely scrutinized by David, to the very last detail. When the building was finally finished, it had state-of-the-art student laboratories, lecture-halls and research facilities, and David reveled in showing it off. The signatures of prominent chemists on the "Hall of Fame" in the main lecture-theater were a point of continuous commotion, which David treasured. In many of his academic and administrative roles, both at the Technion and in the national academic establishment, David appreciated that "Im ein kemach ein Torah" (without physical provisions [literally: flour] there is no teaching); from the moment he moved to Haifa, he realized that hiring a critical mass of young staff and assembling appropriate facilities in a modern building were crucial for creating a good department.<sup>4</sup> But in his wisdom, and as a man of vision, David knew that more was needed to ensure high standards; once the building was in place, he initiated a visiting lectureship to guarantee that the students would also be exposed to top quality researchers from other countries, mostly from Europe and the USA. Visiting lecturers came for periods of one to three months, delivered courses (in English), and interacted with the faculty.

Close and lasting ties developed between our family and many of the families of David's foreign colleagues; with the Dreidings we traveled – a party of 8 – on two separate tours through Europe; frequent visits were exchanged between various members of our family and those of Andre Dreiding, Edgar Heilbronner, Jack Dunitz and Albert Eschenmoser (all from Switzerland), Saul G. Cohen and Orrie Friedman (from the USA); in later years, warm associations were also formed in Germany (Rolf Gleiter, Rolf Huisgen and their families), France (Lionel Salem, Jean-Marie Lehn) and England. At times, it was hard to match the styles of some of these figures: for instance, both Edgar and Ruth Heilbronner were superb gourmet cooks, and a dinner

<sup>&</sup>lt;sup>4</sup> As Michael Cais put it: "Let me also mention here that when Ginsburg agreed to accept the appointment at the Technion, he also demanded – and received – substantial funds both for hiring new staff and for the purchase of teaching and research equipment." Halevi, E.A. (2004). Celebration of inorganic lives: interview with Michael Cais. *Coordination Chemistry Reviews*, Vol. 248, pp. 903–924.

in their house was unsurpassable; but since they enjoyed Hemdah's Gefilte Fish (and would not dream of preparing it themselves), whenever David went to lecture in Switzerland, he would carry on the flight a tray of the fish, to be immediately delivered to Ruth. He would also carry with him several jars of peanut-butter, since the Dreiding children were introduced to it during their stay in Israel, became addicted, and for some reason it was not to be found in Switzerland. David was especially pleasing to the children of these families, since he always made a point of bringing them presents, and remembered what they really liked; thus, an air of excited expectancy accompanied his arrival, as I heard years later from these children when they became adults.

David's friendship was appreciated by his colleagues and many of them consulted him in personal matters and decisions, to which he devoted much reflection. He was an avid letter-writer, and kept relationships alive by frequent correspondence. His friends reciprocated, went to great lengths in entertaining him on his visits, and wrote him long letters themselves, sometimes adding various treats and surprises. Jack Dunitz, for example, composed in his honor a wonderful piece written in Biblical fashion, relating the story of the construction of the chemistry building in Haifa and the signatures in the lecture-hall ("The Book of David"),<sup>5</sup> Edgar Heilbronner used to end his letters with humorous riddles concerning David's own scientific work, which required considerable talent to decipher and brought a sense of joyful triumph once solved; and Albert Eschenmoser teasingly offered to synthesize "Israelane" and "Helvetane" – a Star of David shaped molecule and a cross-like molecule – as a tribute to David's main research topic in later years, propellanes (and to the national flags of Israel and Switzerland).

<sup>&</sup>lt;sup>5</sup> See Appendix.



With Friends; The 4 middle figures are (left to right):David, Heilbronner, Eschenmoser, Dunitz (barely visible)



Albert Eschenmoser drawing Israelane and Helvetane

David was particularly thoughtful about his students and younger collaborators, and invested much energy in helping them write CVs, finding post-doctorate fellowships and advancing their careers, be it in attaining university appointments or jobs in the chemical industry. They appreciated his advice and kept in touch years later and he was always delighted to hear about their progress. Furthermore, it seems that some

grateful students of his, who did *not* keep in touch, also exist out there, as evident from the following excerpt, by chance stumbled upon on the Internet:

"I owe all my education to Israel, in particular to the late professor David Ginsburg. [...] I was born in Cairo, Egypt, and my father was one of the 32 Jews who were randomly put in prison (1957). Luckily, we had Italian citizenship so we succeeded in being deported to Italy, but they confiscated all our belongings. After a while in Padova, the Sokhnut paid our travel to Israel. I was 15 and had not finished highschool. I wanted very much to study (jealous of my brother who was sent to France to do so). [...] I was introduced to Professor David Ginsburg of the chemistry department of the Technion. I did not know a word in Hebrew. This man, with a great heart, accepted me to the Technion and I lived in the Meonot (dormitories). I got a BSc and MSc and he then sent me to Yale University to do a PhD and to Harvard for a post-doctorate! I will always be grateful to him! I am now a full professor in Bioorganic Chemistry at I.N.R.S Institut Armand-Frappier, Canada, for the last 20 years."<sup>6</sup>

To undergraduate students, David seemed formidable. He was a very good teacher, they acknowledged, but his academic demands were high and strict, and he expected them to take their studies seriously. Those who did not were subject to sarcasm if they dared answer his questions groundlessly, rather than admit to ignorance. But any student, once becoming a collaborator, got to know another David, supportive and even soft-hearted when it came to personal matters. True, he could not suffer fools, and let that be known openly, especially if they spoke at length. David had a superb sense of humor, and his letters are scattered with witty remarks sometimes reflecting long-going jesting matches with their recipients. His persuasive powers were a legend, and there are many stories about his one-man-shows at the Technion Senate, in which decisions were overturned after one of his speeches.

<sup>&</sup>lt;sup>6</sup> Comment added by Lolita Ora Cohen-Zamir (6.6.2007) on signing the petition "Solidarity With Israeli Academics Facing New Academic Boycott Threats and Actions from UK Professors Union".



Lecturing on propellanes

The assessment of David's work should best be put in the hands of organic chemists. Judging by the mere volume of his writings – several authored and edited books and about 200 papers – he certainly did not have to worry about the maxim "Publish or Perish". On the basis of his complete synthesis of morphine he was awarded the Weizmann Prize (1954); his later contributions to the synthesis and structure determination of complex organic molecules and to propellane chemistry earned him the Rothschild Prize (1965), the Israel Prize (1972) and the Hofmann Prize (1983). Always hard-working, David had, in addition, an interest in general academic affairs and liked to exert his influence on these; for many years he was head of the chemistry department at the Technion, where he also served as Vice President for Research (1959-60), Acting President (1960-61), the first Dean of Students (1962-64) and Vice-President for Development (1965-66). To us, his family members, it seemed that he never looked upon his work as a career, but rather as an occupation combining labor and play. His sense of playfulness in chemistry was most evident when encountering the unexpected, or when a new application came to his mind:

"Since the syntheses of morphine by both the Gates group and the Ginsburg group include steps which were at least partly unexpected, the rigid logician might be doubtful as to whether the stamp of final approval should be placed on the closure of the morphine story. However, the organic chemist who is aware of the trials and tribulations involved in the degradation and synthesis of complex natural products, is usually more charitable than the rigid logician and accepts morphine into the ranks of substances whose structure is unequivocally established. [...] The major importance of work done on the synthesis of a complex molecule does not lie merely in the successful achievement of the synthesis. The fun involved in the work should not be underestimated; nor should the satisfaction in successful completion of a long and difficult road. Surely the major importance lies in the development and application of synthetic methods and in the cross-fertilization of different fields of research in organic chemistry."<sup>7</sup>



At the Technion, conferring an honorary degree on Ben Gurion

Many looked up to David, but he himself had some idols, and people he admired were forgiven for their small sins. One of them was David Ben-Gurion, the first Prime Minister of Israel, whom David admired for his foresight and leadership, the latter sometimes verging on tyranny. David was fond of relating how Ben-Gurion decided to establish the Israel Academy of Sciences and Humanities: the Prime Minister assembles about 20 of Israel's most distinguished scientists and scholars as an

<sup>&</sup>lt;sup>7</sup> Ginsburg, D. (1956). The problem of morphine synthesis. United Nations Office on Drug and Crime Bulletin in Narcotics, Vol. 1, pp. 15-17.
advisory committee to decide whether at all and how the Academy should be formed; as it turned out, each one of them, without exception, spoke against launching the institution at that time; the collective wisdom was that in such a small country with relatively few academics, it was premature to set up an Academy; upon which Ben-Gurion summed up: "Now that we have consulted all of you, it is decided that an Academy of Sciences and Humanities shall at once be created!".<sup>8</sup> Another of David's heroes was the wizard of Organic Synthesis, R.B. Woodward (for whose nomination he wrote several letters to the Nobel Prize Committee). With twinkling eyes, David would describe in detail Woodward's three-hour long lectures, admiring the air of drama the lecturer produced as he moved from one stage of synthesis to the next, beautifully drawing formulae with colorful pieces of chalk, never using slides. After one of these talks in Haifa, a dinner-party was arranged in our house; in my teens at that time, I used to ask various chemists who visited us to write down for me something about their work, thus forming a collection of memorabilia. While I was asleep, Woodward drew on a small blackboard kept in my bedroom the structure of Vitamin B12, adding a few personal words. David loved to tell later that when he asked Woodward, what would I do - his magnum opus B12 was on the blackboard and not on paper - the reply was: "Well, buy her a new blackboard!"



Touring in Jerusalem, Left to right: Sarel, Woodward, David

<sup>&</sup>lt;sup>8</sup> In 1976, David became a member of the Academy.

David had a big appetite for traveling, and felt totally at home in many cities that he returned to for short or long stays, among them New York, Boston, Zürich, Heidelberg, London and Paris. When it was still rare to do so, he visited Russia and other countries in Eastern Europe as a guest of the Academies of Sciences of the USSR, Poland, Hungary and Czechoslovakia, and went on several lecture-tours in the Far East, in Japan and Taiwan. In such places, David saw himself as a bit of an envoy, and decided to act on behalf of Israel. Thus, in Moscow, he asked his hosts to take him to the Jewish synagogue, but they refused, saying that tourists were not allowed in; as a retaliation, at the end of his lecture he surprised the audience and announced that he would not answer any questions; he let them know that his request to visit the synagogue was declined because he was a mere tourist, and hence he would now take leave and go to the Pushkin Museum! Even after the Iron Curtain had lifted, the era of globalization was still far away, so that different countries kept their quintessential marks, and David had the talent to absorb their unique characters and convey them to others. He tried to share with his family some of the more physical aspects of the places he visited by carrying home from his trips French cheese, black cherries, dried mushrooms and even Coca-Cola concentrate (!) when such goods were unavailable in Israel.

As a husband, father and grandfather, David was extremely protective. He was generous to us in material things and lavish in his attentions; he followed our daily lives and milestones, our achievements and failures, worries, sorrows and joys, with deliberation and utmost consideration. Family matters came first, with Hemdah's Friday night dinners almost sacred; no invitations to go out were accepted then, while guests would frequently join the table at home. To secure the necessary resources for purchasing apartments for his offspring, David taught summer-courses abroad, and acted as a consultant in pharmaceutical companies; his constant dependability was an asset and source of confidence that accompanies us years after his death. When my sister was in Montréal, her husband Coby studying for his PhD, or when I was studying for my DPhil in Oxford, he found ways to visit us (and check on us) and see for himself how we were doing. It was no accident that his lecture tours followed the footsteps of members of the family. Some of these visits were Sabbaticals, others were shorter, but they always included family holidays, with walks, trips, and excursions to museums, art-galleries, concerts, bookshops, restaurants and shopping centers. Once Nini's children, Sharon and Danny, were old enough to travel, David and Hemdah (who were already grandparents in their mid-40s) would occasionally take them on their journeys, recapitulating the past. A good illustration of David's commitment and wisdom as a father is the family ceremony he invented when we, his daughters, were still very young: once in a while at night, unable not fall asleep, we would call out to David and Hemdah, saying: "I have some worries"; they would both come and sit on the edge of the bed, patiently enquiring what the problem was; the issue was taken utterly seriously, no matter how trivial it may have been; David would always come up with a solution or idea that made the "worry" go away.

Being both rational and emotional, I believe that David was somewhat prone to autosuggestion. After decades of consuming 40 daily cigarettes (plus pipe tobacco), he quit smoking in one go. Over the head of his 4-year-old granddaughter he swore that his next smoke would be a cigar, on her wedding. Unfortunately, he did not live to light that cigar, but his strong will made him keep the promise. With his domineering nature, David found it highly frustrating to face situations and events that were out of his control. On such occasions, he would chant to himself "Man proposes, God disposes", a mantra that seemed to help. During large scale crises though, such as the Yom-Kippur War, his wise advice to everyone was to maintain daily life as much as possible, concentrating on work, or on whatever one was best at doing; during personal calamities he practiced what he preached, as evident from the way he kept on writing when he could no longer go to the lab. About his severe heartattacks he was "fatalistic" (his own word), and refused to undergo heroic measures i.e. a heart implantation suggested by several surgeons to improve his "quality of life" (their words). He used to say; "I know my limits, and I know Science's limits". David, who loved art and music, did not much appreciate philosophy, so it was never clear how meaningful this last sentence of his, about the limits of science, was. Therefore, I was recently delighted to discover in one of his papers, written nearly 30 years ago, a note that would not discredit any professional philosopher of science:

#### "General Comment

Finally, we must make a general comment in criticism of our thesis regarding the efficacy of secondary orbital interactions in controlling the steric course of the various chemical reactions discussed above. As is usual in science we support our arguments

by adding more and more constructions to buttress the battlements. We do not prove our thesis in the mathematical sense; we provide an interpretation which is apparently supported by more and more data but we must recognize that the interpretation is nevertheless not necessarily correct." <sup>9</sup>

Thus, twenty years after his death, David the Man – and the scientist – does not cease to amaze.



David Ginsburg as we like to remember him

<sup>&</sup>lt;sup>9</sup> Gleiter, R. and Ginsburg, D. (1979). Propellanes. XLVI. Steric and electronic effects as observed in reactions of propellanes. *Pure & Applied Chemistry*, Vol. 51, pp. 1301-1315.

#### In Memoriam

#### DAVID GINSBURG, 1920-1988

David Ginsburg died on March 9, 1988, shortly before his scheduled retirement as Research Professor of Chemistry at the Technion — Israel Institute of Technology in Haifa. Neither impending retirement nor a protracted heart ailment dimmed his drive for achievement and until the end he was actively planning work on biographies of two of the outstanding organic chemists of our time who were colleagues and personal friends.

The native Israeli is called a "sabra" after the fruit of the cactus plant which presents a prickly exterior but is soft and tender once one penetrates the thorns. Although David Ginsburg was born in New York City, the son of a Hebrew poet and educator, he qualified unreservedly as a "sabra". Single-minded and tough where principles were concerned, he was also very much aware of human relations and could be a warm and supportive friend. His father's wanderings in search of a livelihood brought the family to Palestine in 1933 where David completed high school and his first year of university training before returning to the U.S. in 1938. Schooling continued at City College and Columbia University and culminated in the Ph.D. degree from New York University in 1947. Meanwhile David had been joined by Hemdah Ber, his lifelong helpmate, who came to New York in 1940 where the two were married and their daughters, Jonina and Simona, were born.

David Ginsburg's independent scientific career began in 1948 when he joined the staff of the fledgling Weizmann Institute of Science after returning unorthodoxly from the U.S. when the gates of Israel were opened to immigration. Six years later his first major achievement, the total synthesis of morphine, was reported (together with the late Dov Elad, his first graduate student; D. Elad and D. Ginsburg, J. Am. Chem. Soc., 76, 312 (1954), J. Chem. Soc., 3052 (1954)), closely following Marshall Gates' first synthesis. David Ginsburg had made his first impact on the chemical scene. A key ring-forming step in the morphine synthesis involved the Michael reaction, which was the subject of a number of research papers from the Weizmann Institute and subsequently was treated to a 376 page review including over 1000 references (E.D. Bergmann, D. Ginsburg, and

R. Pappo, Organic Reactions, R. Adams, Ed., Wiley, New York, 1959, Chapter 3). Other work at the Weizmann Institute included studies of thermochromic fulvenes and ethylenes (with E.D. Bergmann), various aspects of alicyclic chemistry, and explorations of the reactivity of *t*-butylhypochlorite with a variety of substrates. The latter compound, some of whose chemistry had formed the basis for his doctoral thesis, remained a love of his youth to which he periodically returned.

The reputation he established at the Weizma Institute and the international contacts with chemists such as Prelog, Robinson, Todd, and Woodward, resuked in a call to the Technion in 1954 as Professor and Department Head. The job of converting the small, service department into a modern research and teaching chemistry department of international standard was no small challenge. It involved establishing a proper library, stores of chemicals and equipment, and recruiting a staff of young people who would mature into world-class chemists. The many scientists who have visited Haifa in the intervening years can attest to the success of his efforts as an administrator, building designer, evaluator of the potential of young chemists, etc. In addition, David played an important role in the development of the Technion, bringing his administrative talent and vision to functions such as Dean of Students, Vice-President for Development, Vice-President for Research, and Acting President (1961-2). Had he chosen, he could undoubtedly have become President of the Technion and spent the later years of his life as a university administrator. Instead, David Ginsburg chose to live out his life as a teacher and researcher.

One aspect of his approach to the development of chemistry at the Tochnion deserves special mention. As Head and the only full professor in the fledgling department, he could have introduced a system in which the newly hired "youngsters" would have formed the base of a pyramid with David Ginsburg at the top. This undoubedly would have resulted in an impressive publication list, Instead, he chose to give each new staff member, no matter how lowly his rank, full independence in the choice and prosecution of his research. Budget, space,

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and graduate students were available to all on the basis of merit. Throughout his career, be encouraged the development of younger chemists at the Technion and classifiers in Israel, not only with advice and moral support, but also in the most tangible way, financial support. The younger generation responded by seeking out his advice and accepting his high standards. In the intervening years, all of the universities in Israel have adopted the system, originally only found in Haifa, of giving young talent full opportunity to develop.

One of the many papers which appeared in Tetrahedron Supplement 8 honoring Sir Robert Robinson on the occasion of his 80th birthday was estided "Propeltanes" - 1. Tricyclic Compounds Conjoined in a Carb-on-Carbon Single Bond" (J. Aliman, E. Bahad, Y. Inchaki and D. Ginsburg, Tetrahedron Suppl. 8, Part 1, 279 (1966)). As pointed out in that paper, such compounds had been prepared by a number of earlier workers but David Ginsburg was the first to recognize the special features present in propellanes, such as their potential in stereochemical investigations and their offering of unique types of bonding. The major thrust of his research effort in subsequent years was concerned with synthesis and chemistry of propellanes. Paper number 100 (a cherished ambition of his later years as noted in a postscript to Sequel II of Propellanes) opens this special issue of the Israel Journal of Chemistry. This number would have been much larger were it not for the paucity of students going into chemistry in Israel during the past ten years; from the late seventies on, the Ginsburg group rarely numbered more than two co-workers. Two long time associates, Dr. J. Altman (1960s) and Dr. P. Ashkenazi (since 1974) were responsible for a large share of the published work. David Ginsburg's monograph Propellanes (Verlag Chemie) appeared in 1975 and was followed by two privately printed Sequels (Technion, 1981 and 1985). The increasing awareness of the usefulness of propellanes for studying many aspects of chemistry is evidenced by the fact that the number of references in these three volumes shows a regular increase with time

The paper "Propellanes 1" proposed the simple nomenclature which has become generally accepted, and reported the original approach used for synthesis of many [4.4.n]propellanez. This involved a known double Diels-Alder addition of buadiene to acetylene dicarboxylic ester to give compound 1, from which 2 (anhydrides, imides, amines, ethers, factores, etc.) could be prepared by simple transformations. Propellatetraenes of type 3 were particularly useful substrates for a variety of studies including the first clarification of the deailed photochemistry of cir-9,10-dihydronaphthalene. An in-

teresting footnote to this work was revealed in discussions that took place when V. Prelog was Visiting Professor in Haifa in 1967. It turned out that the Prelog group had also prepared 3 and studied certain aspects of its photochemistry, but the work had not been published. Over the years a considerable number of propellanes were prepared in Haifa. A propellane which resisted synthetic approaches based on photochemical decarbonylation (in collaboration with this author) or on heterocyclic ring contraction was [2.2.2]propellane (4). This compound and [1.1.1]propellane have attracted a great deal of interest over the years. Additional efforts during his last years were devoted to synthesis of a propella-catenane (Ternahedron, in press). In a paper written in his inimitable fashion (Tetrahedron Lett., 507 (1973)), David Ginsburg described the merits of, and approaches to, "Paddlanes" (5), a name suggested by J. Bloomfield, whose suggestion of the name "propelleranes" he had squashed many years earlier.



By far the greatest effort, herwever, was devoted to the study of a variety of organic reactions in which the special stereochemical features inherent in the propellane structure could provide new insights. Propellanes can incorporate whatever functionality the ingensity of the chemist is able to introduce. They are thus available for the investigation of all kinds of chemical reactions. A particular feature, as illustrated in diagram 6, of propellares in which at least one ring is different from others, is the presence of two faces which allow reactions to proceed from the face ryn or anti to the third ring. This aspect of propellane chemistry occupied a large part of the work in the Ginsburg group.

Collaboration with colleagues was a strong aspect of David Ginsburg's approach to research. During his 34 years at the Technion, more than half of his Haifa colleagues appeared as co-authors on one or more publications. Collaboration with chemists all over the world was a common feature of the propellane work. Particularly worthy of mention in this connection are the following: W. von Philipsborn (Zurich) in numerous NMR studies isvolving a variety of nuclei; E. Vogel (Cologne) in studies involving a variety of addition reactions to methano[10]annulones and their valence tautomeric forms ([4.4.1]propetlatesraenes, cf. this issue); J. Bloomfield (Monsanto) in nomenclature, as mentioned above, and in connection with structural work in the photochemical studies; R. Gleiter (Heidelberg) on the theoretical anpects of secondary orbital effects in orientation of addition reactions to propellanes (cf. 6); H. Wamhoff (Bonn) in synthesis of macrocyclic propellanes and R. Mac Farlane (College Station, Texas) in the special many spectrometric techniques which established the structures of these macrocycles (one of which graced the cover of Angewandte Chemie some years ago); W.J. Richter (Basel) in a number of mass spectrometric investigations (together with A. Mandelbaum from the Technion); G. Wilke (Malheim/Ruhr) in the study of a number of organometallic complexes of propellanes; R. Benn (Mulbein/Rulu) with special NMR measurements; and G. Maier (Glessen) in some of the studies of stereochemistry of additions to propellanes. David Ginsburg had the talents not only to interest others in his work but also to impel them into action.

In addition to his book Propellanes -- Structure and Reactions and its two sequels mentioned above, David Ginsburg wrote a number of review articles for Accounts of Chemical Research and for Tesrahedron on various aspects of propellane chemistry. In the pre-propellane years he authored reviews on opium alkaloids, on the Michael Reaction (as mentioned above), and on organic hypohalites. Together with his daughter Simona, he wrote two volumes (in Hebrew) on the subject of symmetry in chemistry for use in courses at the Open University in Israel and a book, Concerning Amines (Pergamon, 1967). He also edited a number of volumes, including Non-Benzenoid Aromatic Compounds (Interscience, 1959), Alicyclic Compounds (M.T.P., 1975), and Solid State Photochemistry (a memorial for G.M.J. Schmidt, Verlag Chemie, 1976).

In addition to his research activities, David played an important role in the development of chemistry in Israel. He was a frequent adviser to government and academic institutions, chairman of the board of directors of Israel Mining Industries for ten years, a member of the select Israel Academy of Sciences, president (twice) of the Israel Chemical Society, and representative to the Bureau of IUPAC for seven years — among a host of other functions. In particular, he was a standard bearer for

continuation of the tradition of excellence which E.D. Bergmann had brought to Palestine in the 1930s. His contributions were recognized by the three major awards Israel offers its scientists, the Weizmann Prize of the City of Tel Aviv (1954), The Rothschild Prize for Chemistry (1965), and the Israel Prize for Exact Sciences (1972). His contributions to chemistry and to the renewal of scientific relations between Israel and the Federal Republic of Germany were recognized by the award of the Hofmann Medal of the Gesellischaft Deutscher Chemiker (1983). His talent as a lecturer and a discussion partner and his breadth of vision brought him appointments as a visiting professor at many universities, including the Swiss Federal Institute of Technology (ETH), Harvard, Oxford, Cambridge, Stanford, Heidelberg, and Munich, to name but a few. These visits and the active visiting professor program which he promoted in Haifa made him an international figure with professional and personal contacts throughout the world.

Chemistry was clearly a serious vocation for David Ginsburg; in addition, it was fun. His list of publications includes over 200 published papers, books, reviews, etc. plus a section entitled "Alchemy" with papers (authored by "G. Dinsburg") such as "Organic Chemistry and the Problem of Life", "Reflections of an Organic Chemist During a Theoretical Chemistry Seminar, The GDAP Approach to Electron Correlation", and others, published in the Journal of Irreproducible Results, Nachrichten, and Nouveou Journal de Chimie. He brought his sense of humor to his lectures and was invited back to many institutions all over the world as evidenced by lectures entitled "Propellanes", "More Propellanes", and "Still More Propellanes". He felt strongly that chemistry should not be a dry subject devoid of humanium and, in typical fashion, went to the trouble of writing a letter on the subject to the editor of Nouveau Journal de Chimie. His books and review articles are replete with anecdotes, talmudical comments, and a general sense of life which makes them a delight to read, over and above their scientific content.

The summary above provides only a brief overview of David Ginsburg's many activities. To all who knew him, David was a vibrant personality who beought life and enthusiasm to all his dealings. He was with us and we could not ignore him. As we were so aware of him when he was present, so much is his absence felt now. May his memory be blessed

> Mordecai B. Rubin Haifa

### **Tribute to David Ginsburg**

by E. Amitai Halevi

Delivered at the Symposium in Memory of Professor David Ginsburg, Jerusalem, September 20, 1989.

I trust that you will excuse me for reading from a prepared script. David Ginsburg played a major role in my life; his influence was profound and long lasting. Therefore, since I would like to keep my tribute to his memory within reasonable bounds as regards both length and content, I feel safer with a written text in my hand, even if it means repeating a good deal of what has already been said.

Many of you were among those of David's friends and colleagues who gathered in the Chemistry Building of the Technion before accompanying him to his last resting place in Rehovoth. After I said on that sad occasion that David and I had been friends for fifty years, I was charged with exaggeration; actually, it was an underestimate. We were born - within two years of each other - to members of the very exclusive group of Zionist families in New York in which the children were brought up in Hebrew as their mother tongue. We grew up in different parts of the city, so, though our families were acquainted, my first genuine contact with David took place some 55 years ago - prophetically, on board ship from New York to Haifa.

For the next twenty years we went our separate ways, maintaining occasional contact with one another while each followed his own path into Chemistry. In 1954, David was called to the Technion and took upon himself the task of setting up a modern Department of Chemistry. I joined him at his invitation the following year, and from that day on we were in nearly daily contact. Each of us was, of course, concerned with developing his own research program, but he also enlisted me - along with his other junior partners - in a common endeavor: trying to build a balanced, viable, Chemistry Department. Our friendship, and that between our families, matured over the years, as we did, and his death left me with a profound sense of personal loss.

Chemistry existed at the Technion before the coming of David Ginsburg, but nobody will deny that the Department of Chemistry in its present form is predominantly his creation. He arrived from the Weizmann Institute, a vigorous young scientist acclaimed for his total synthesis of morphine, and completely rebuilt the Department, demanding - and being granted - budgetary support on a scale hitherto unheard of at the Technion. He invited promising young chemists in Israel and abroad to join the faculty, promoted and encouraged the better chemists already present at the Technion, and cleared the path for each of them to his or her maximum scientific achievement. David often referred to himself as the Department's *George Washington*. There is no better indication of his being its *founding father* than the fact that his "children" sometimes felt the need to rebel against him in order to assert their independence. Independence, incidentally, was something that he never denied anyone, anyone - that is - who was capable of making use of it.

No one who knew David - and most of those here present did - would characterize him as "soft"; no *empire-builder* ever is! But tough-mindedness and firmness of purpose were only one aspect of David's complex character. His intellectual honesty was impeccable, and - not too well hidden under his gruff manner - there were loyalty, warmth, and compassion.

David had a passion for excellence and an almost infallible eye for talent. When recruiting faculty for his new Department, and - with their assistance - inviting younger members to join its ranks in the following years, he imposed his own high standards, and tried - with at least partial success - to make them those of the Department. As a result, since - for better or for worse - all organizations are prone to self-perpetuation, there is hope that the Department of Chemistry at the Technion will not only maintain the quality that it achieved under David's guidance, but will continue to improve in the years to come.

The members of the chemical community are perhaps less aware of how instrumental David Ginsburg was in the process of converting the Technion within a

few years from a species of "Polytechnikum" to a genuine Institute of Technology, in which the engineering curriculum is based on a thorough grounding in the natural sciences. He sat on all of the influential committees, was the Technion's first Dean of Students - a position that he created and took very seriously, and was Vice President for Research and Vice President for Development. He served for a year as Acting President of the Technion and was offered its Presidency, but turned it down because acceptance would have meant the end of his scientific career. From his personal point of view, and that of Chemical Science, he undoubtedly made the correct decision, but I cannot help thinking wistfully what a wonderful President he would have made, and how much the Technion would have benefited from his continued leadership.

David Ginsburg's impact on Chemistry in Israel is too well known to everyone present for me to have to spell it out in detail. He was granted all of the major awards available to an Israeli scientist, including the Israel Prize, and was a member of the Israel Academy of Sciences and Humanities - which is honoring his memory as co-sponsor of this Symposium. Not only was he twice elected President of the Israel Chemical Society, but was the first Israeli who had the audacity to present himself as a candidate for membership in the Bureau of IUPAC; his election paved the way for other prominent Israeli chemists to join this prestigious body: the late E.D. Bergmann soon thereafter and - more recently - the Chairman of the present session, Joshua Jortner.

Nor did David confine himself to the academic scene. Being a very practical person, he was intimately involved with Israeli chemical industry and active in the Defense establishment.

I will not attempt to summarize the scientific achievements that earned David Ginsburg such widespread national and international acclaim, but merely list his more important contributions: The total synthesis of morphine; his exhaustive review of the Michael condensation - which played an important role in that brilliant synthetic effort; the review on organic hypohalites - which harks back to the research that he had done in New York for his Doctoral thesis; his studies of nonbenzenoid aromatics in collaboration with the late Professor Ernst David Bergmann, culminating in his editorship of the classic book on the subject; and - finally - his hundred articles and two books on propellanes, which mirror his ever-increasing fascination with molecular symmetry. I also have to mention his "tongue in cheek" publications in the Journal of Irreproducible Results and elsewhere - products of his ironic sense of humor, as well as his more serious pleas for humanism in chemistry.

This, however, is hardly the occasion for an evaluation of David's work, and there are many in the audience who are better qualified than I am. Let me refer you to Mordecai Rubin's excellent summary of David's research accomplishments in the current issue of the Israel Journal of Chemistry, which is dedicated to his memory. On behalf of David's friends and colleagues, I would like to thank the staff of the Weizmann Science Press for getting the Memorial Issue into print in time for this Symposium.

In the context of David Ginsburg's standing in the international chemical community, let me return to the predominant characteristics that I have already noted: his regard for scientific excellence and his unerring eye for quality. David had unstinting respect for the outstanding chemists of his generation; the admiration was mutual, and many of the most prominent became his close personal friends. More important for the long run was his ability - by no means restricted to organic chemistry - to recognize exceptional talent in young scientists. Many of the present leaders in their respective fields of research - in Europe and the United States as well as here in Israel - were encouraged by David's early recognition of their high promise and had their way smoothed for them as a result of his enthusiastic support.

The Organizing Committee of the Symposium that is now reaching its conclusion has tried to reflect some of this in putting together its scientific program. We did not limit its scope to synthetic organic chemistry, David's own field of research, because his interests were much wider: The theme "Stereochemistry and Mechanism" does not quite cover their full range, but comes closer. We drew our lecturers from the worldwide pool of David's personal friends; each is an eminent scientist in his own right. A few represent his near-contemporaries; the others exemplify those members of the younger generation whose promise - that he recognized so early - has since been admirably fulfilled. I have no doubt that David would have enjoyed this Symposium immensely, and regarded it to be a fitting tribute to his fruitful career.

He will be long remembered by all who knew him.

## Appendix

### Wall of Frame

#### From the Reminiscences of David Ginsburg

(This essay was composed by David Ginsburg in late 1987 or early 1988).

### The ''Wall of Fame''

After we built our chemistry building on Mount Carmel, the building sank some millimeters in a few places. While equilibrium was established throughout, a large and ugly crack formed above the blackboards in our main auditorium. Consultation with the man responsible for the esthetics of our interior decoration brought forth a suggestion, that as in *auditorium maximum* of chemistry in Jerusalem, we cover the area with a large, readable periodic table. This did not satisfy me, it was too commonplace. It led, however, to the idea of a human periodic table carrying the names of the leaders of chemistry through the ages.

The area was divided into separate inter fitting name plates made of Formica which would bear in permanent fashion the appropriate names in alphabetical order. The size of each plate was 19 cm x 39.5 cm so that each name was obvious (if legible) also in the last row. Not being the collector type and not wishing to spend the effort in collecting more ancient signatures, which presumably may be traced, I decided to use Roman capitals for these. But for people alive in 1964, when the building was opened, their magnified signatures would be employed and when a person died his signature would remain rather than turn into a Roman cap.

A flattering letter was sent to about 100 living people asking each to send us a signature, about twice the usual size with a black pen, so that no detail was lost during the magnification process. Only few reminders were necessary and we had very few happy hopes expressed for the future of the department occupying this building; two to be exact, but I shall not name names. Most sent several exemplars of their signature lest one be insufficient to trace the wall, which in time was irreverently called the "Wall of Fame".

Those new Nobel Laureates related to chemistry or physiology whose names were not yet on the Wall, were asked to supply signatures. Some now require many reminders so that we are, as everything else in life, imperfect. But such imperfections are not unique. Although all Nobel Laureates were automatically listed by signature in the usual alphabetical order, those who were Nazis (checking some by Israeli Intelligence), were omitted. This ought to be humanly understood by all. There are no Israelis with Hebrew signatures on the Wall. It is too small a country for this kind of honor. If and when an Israeli receives the Nobel Prize in chemistry or a subject related closely enough to it, we shall have our first Hebrew signature. May I say cynically that his colleagues will be so disappointed at the prize award that their place in the wall will be of little moment. This will not happen during my responsibility for the contents of the wall since I have handed this over to our present chairman two years before my retirement.

There is still room on the wall. Eventually this may be expanded to the side walls. There is a model on the back wall at the auditorium arranged exactly as the original but making all signatures legible and stating where the person is from and the year of his Nobel Prize, if he has one.

The wall is esthetic in appearance, it is educational for the students, and we have had much fun from it. It is certainly satisfying, that during many years it was not necessary to ask for signatures of new Nobel Laureates because their signatures were already there. One just had to add to the model on the back wall the fact and date of the Nobel award. But, of course, there have been imperfections. Not all of the "wallflowers" names were anticipated. I hope standards remain high as to candidates and do not become lower. But I shall invoke no right to postpone my retirement on this account (I have already stated elsewhere herein my firm belief in the retirement, on time, of the old fogies), I shall only hope for the best as I shall for any other aspect of the "house that David built" and chiefly the young people working in it.

Finally, a book entitled "Reflections of a Physicist" by Anatole Abraham, (Clarendon Press. Oxford, 1986) was pointed out to me in our library. Its last (unnumbered) page (opposite page 158) is headed "the Writing on the Wall".

#### LETTER FROM PROFESSOR GINSBURG

#### May 1964

It may interest you to learn that in the main lecture hall of our new Chemistry Building we are planning to cover the wall behind the lecturer with facsimile signatures of great chemists who have made important contributions to various fields of chemistry. I am sending out this letter to about one hundred chemists the world over asking for a facsimile signature. We then intend to add about 2-3 additional names annually to this chemical hall of fame.

We should be honored if you would be kind enough to send us a facsimile of your signature, written on ordinary paper with a thick pen and about twice the size of your usual signature. Our students will then be in the fortunate position of learning, also by this means, the names of the leaders of chemistry in the world today.

# REPLY TO PROFESSOR GINSBURG

### May 1964

Dear Professor Ginsburg,

Your very kind proposal to have my name on a wall as a great chemist demonstrated that either you do not know me or that I do not know what a chemist is. My ignorance of chemistry makes me a laughing stock among my young collaborators and if the gravity of your functions did not put you above suspicion, I would have suspected that someone was pulling my leg.

If you define chemistry as the study of properties of matter in bulk I might perhaps qualify as a chemist, but is not that a little farfetched. However if in the light of what I have said you still wish to keep me on your wall, here is my signature more than life-size.

A. Abraham

I am pleased that a physicist of the caliber of Abraham is proud to join the chemists since "what's in a name?" It is often arbitrary. He happens, owing to alphabetic order, to be the first on the wall. Lise Meitner who, of course, should have had her signature on the Wall replied that she was a physicist and anyway she converted from Judaism and hence I will not want her signature(!),

necessitated as gentle a reply as possible, chiefly to her second point, and though I had her signature on her letter I followed *her* wishes and did not use it. Notes:

A copy of correspondence concerning the bet between R.B. Woodward and 0. Jeger on which institution has the larger number of names on the wall, Harvard or the ETH, is available from Prof. Rubin.

The Writing on the Wall

In time Jack Dunitz, a Jewish Scot, therefore a Biblical scholar, and concurrently a beautiful writer, wrote the "Book of David" in connection with the Wall:

### The Book of David

By Jack Dunitz<sup>10</sup>

AND it came to pass that David became king and ruled over the land in Israel. And there was peace in the land.

AND David caused a great palace to be built on Mount Carmel, and he furnished it with costly things from distant lands. And the people called it the house of David.

AND David said: let the wise men of all lands write down their names, each in turn, and let their names be carved in tablets of stone and set forth on the walls of my house, that the people may look upon them and wonder.

SO messengers went out into all the lands of the earth and they spoke to the wise men, even the priests, scribes and prophets, that they hearken unto the words of David and write down their names, each one in his turn.

AND the wise men hearkened unto the words of David and they wrote down their names. And their names were set forth on the walls of David's palace, even as the king had commanded. And the people came and looked on them and wondered. They came from far and near to look.

BUT the messengers returned unto David, saying: there are among thy servants some that do not hearken unto thy words, for they do not write down their names so that they may be set forth on the walls of thy house. And David asked: who are they that do not hearken unto my words?

<sup>&</sup>lt;sup>10</sup> The original handwritten piece is not dated, but it was probably written in 1964. [S.G.]

AND the messengers said unto David: even Moser the priest, Edgar the prophet, and Jacob the seer, he that looks at precious stones, and they live near the city of Zuri.<sup>11</sup>

AND David was angry against his servants, they that did not hearken unto his words, and he commanded that no man give them fish or wine to satisfy their hunger and thirst. But David also wept and tore his garments, for he loved his servants and delighted in them, even they that did not hearken unto his words.

AND David sent messengers to the city of Zuri and they spoke unto Moser, Edgar and Jacob, saying: reflect ye a little and write down your names, even as David hath commanded, lest the king smite ye down and destroy Zuri with fire and sword. But Moser, Edgar and Jacob laughed and made mock of David. And the messengers returned unto David and told him that which had happened.

NOW Prelog the high priest spoke unto Jacob the seer, saying; render unto David that which is David's. Hath he not commanded thee to write down thy name along with the names of Moser and Edgar that they may be cut out in tablets of stone and set forth on the walls of his house? Thereby doth not David pick ye out from all his servants that the people may look upon your names and honour ye?

AND Prelog the high priest spoke further, saying; hearken unto the words of David, lest he punish thee and smite thee down, lest he come with his men and destroy our city, even Zuri, with fire and sword. For Prelog the high priest stood in awe of David and trembled before him.

AND Jacob the seer hearkened to the words of Prelog the high priest and saw that they were good. And he spoke with Moser the priest and Edgar the prophet. And they did write down their names and send them to David the king. And David's heart was gladdened when he saw that his servants had hearkened unto him and obeyed his commandment.

AND Hemdah the wife of David saw that he was glad and she spoke unto him, saying: go forth unto thy servants, they that did hearken unto thy words. Go forth and let them look upon thy countenance, they that did obey thy commandment.

<sup>&</sup>lt;sup>11</sup> Moser the priest = Albert Eschenmoser, master of synthesis; Edgar the prophet = Edgar Heilbronner, predictor of chemical structure; Jacob the seer, 'he that looks at precious stones' = Jack Dunitz, the crystallographer; Zuri = Zürich. When Dunitz, Eschenmoser and Heilbronner were asked to sign their names for the "Hall of Fame" in the chemistry building, they modestly declined; Vladimir Prelog, who was their senior in Zürich ('the high priest', see below), willingly agreed. [S.G]

SO David went forth and travelled over the ocean and across the high mountains till he came to the city of Zuri.

AND he brought gifts of fish and of wine.<sup>12</sup> And David and his servants ate and drank and they were merry until reading of the morning prayer. Then Moser the priest, Edgar the prophet, and Jacob the seer, said unto David: the hearts of thy servants are full of gladness that thou hast come to our city, even to Zuri. And they did write down their names again, to do pleasure for David.

AND David grew merry and drank much of the wine, that which he had brought, and that of the vineyards of Zuri. And he was drunk and could not stand or sit so that he fell down and banged his head. So they called the name of the place Rosh-Likon, which is near Zuri.<sup>13</sup>

AND David returned to Israel to Mount Carmel and he caused the names of his servants to be cut in tablets of stone and set forth on the walls of his palace. And the people came and looked on them and wondered.

AND David ruled over the land. And there was peace in the land.

Some explanations:

Verse 8 and 15: We used to bring my wife's home made gefullte fish to Zurich since it was very much appreciated.

Verse 16: At the time, the Heilbronners and the Dunitzes were living in a suburb of Zurich called Ruschlikon. *Rosh li kan*, in Hebrew, means "I have a head here".

An annotated Hebrew version of Dunitz' "Book of David" has been prepared by Professor Halevi.

 $<sup>^{12}</sup>$  The fish was Hemdah's Gefilte, and the "wine" was most probably Remy Martin, David's favorite cognac, which he used to buy for his Swiss hosts at the duty-free shop, before boarding the plane. [S.G]

<sup>&</sup>lt;sup>13</sup> The Dunitz family lived at the time in Rüschlikon, a small town on the lake of Zürich. On one of his trips to Switzerland, David indeed stumbled and hit his head, but how sober he was is a matter of poetic license. [S.G]

# Amitai Halevi Born USA, 1922



**Ph-D thesis:** University College London, 1952

**Past Positions:** Hebrew University Jerusalem (Instructor 1952-54, Lecturer 1954-55)

### **Technion Positions:**

Senior Lecturer 1955-60 Associate Professor 1960-64 Professor 1964-1990 Professor Emeritus 1990-

### Sabbatical leaves:

Cornell University. 1959-60. Swiss Federal Institute of Technology, Zürich: Summer 1964; June 1986. Brookhaven National Laboratory. Summer 1966. Oregon State University: Fall-Winter 1966-67. University of Oregon: Spring 1967. Argonne National Laboratory. Summer 1967; Summer 1986. Purdue University: Summer 1969. Feinberg Graduate School, Weizmann Institute of Science, Fall-Winter 1972-73. University College London: Spring-Summer 1972. University of California, Irvine: Fall-Winter 1975-76, Fall 1990. Ben-Gurion University, Beer-Sheva: Spring 1976. M.P.I für Strahlenchemic, Mülheim/Ruhr: Aug. 1980; Mar. 1981; May 1986. University of Munich. Sept.-Oct., 1980. University of Frankfurt: Nov.-Dec., 1980. University of Heidelberg. Jan.-Feb., 1981. University of Graz: March 1986. University of Paris (Orsay). April 1986.

Clarkson University: Fall 1986. California Institute of Technology: Jan.-Feb, 1987. M.P.I. für Med. Forschung: Heidelberg, Summer 1988. Simon Frazer University, Jan.-Feb. 1991 University of Wisconsin, March-April 1991

### Major departmental responsibilities:

Chairman, Department of Chemistry 1983-85

### Major Technion/international responsibilities:

Dean of the Graduate School 1969-71.
Member, Academic Development Committee 1972-74.
President, Disciplinary Tribunal for Academic Staff 1974-75
Member, Review Committee on Operations and Functions of the Vice President for Finance and Administration 1979.
Member, Committee for Senior Faculty Appointments 1979-81.
Member, Committee for Appointment of Research Professors 1982-83.
Senate Representative, Board of Governors 1973-5, 1981-84.
Member, Senate Steering Committee 1982-5, 1988-90.
Senate Representative, Committee to Consider the Election or Reelection of the President 1989-90.
Member, Disciplinary Tribunal for Academic Staff 1995–2000

### **Field of Research**

*Physical and Theoretical Organic Chemistry* Experimental and computational studies of isotope effects on molecular properties,

equilibria and kinetics.

The dependence of reaction mechanism on state-, orbital- and spin-symmetry: qualitative theory and computation

### Amitai Halevi

### (April 2008) Isotopes, Symmetry and Music

### 1. Roots

Obeying the Red King's instructions<sup>14</sup>, I will begin at the beginning, go on till I come to the end, and then stop. Accordingly: I was born on May 22, 1922 in Brooklyn, N. Y. to Rose *née* Taran and Mordecai Halevi. Both of my parents were born in the Ukraine, then part of imperial Russia. My mother immigrated to the United States as a teen-ager in 1908 with her father, mother, two brothers and four sisters. They settled in Rochester, N.Y., where her father, Moshe Taran, who had been ordained to the rabbinate but disliked its practice, chose to engage in business instead – with moderate success. After qualifying as an accountant, my mother moved to New York City with her older sister Rae.

My father's route to the U.S. was less direct. In 1911, soon after being drafted into the Czar's army, he deserted, embarked for Palestine and enrolled in the Teacher's Seminary in Jerusalem. When the First World War broke out in 1914 he was interned by the Turkish authorities as an enemy alien, was redeemed by a suitable payment of baksheesh, and made his way to Egypt. There he enlisted in the Jewish Legion of the British army and soon found himself at the Dardanelles, a Corporal in the Zion Mule Corps under Captain Joseph Trumpeldor. After the ill-fated Gallipoli campaign, the Corps was shipped back to Alexandria and disbanded, whereupon my father eventually made his way to the United States. He settled in New York City, where he met and married my mother and began making a living as a Hebrew teacher. However, when the United States entered the war, he joined Yitzhak Ben Zvi and David Ben-Gurion in organizing the American Jewish Legion for Palestine, enlisted once more and trained with the Legion in Canada, but illness intervened and WW1 was over before he could get to the front<sup>15</sup>.

<sup>&</sup>lt;sup>14</sup> Lewis Carroll, "Through the Looking Glass", Chapter 3.

<sup>&</sup>lt;sup>15</sup> My father's autobiography would evidently have been much more interesting than mine can possibly be.

### 2. Childhood

Following my maternal grandfather's death at the early age of 49, the family was reunited in Brooklyn, settling in the Crown Heights neighborhood, now a Lubavicher stronghold but then populated largely by secular and moderately observant middleclass Jews. The three oldest daughters were married. At the time of my birth I had two older cousins – one from each of my aunts; two more cousins, a brother and another two cousins, completed a tightly knit, nearly self-sufficient clan.

Both my father and my Uncle Mitia, Aunt Rae's husband, taught Hebrew at the Brooklyn Jewish Center, a large complex comprising a Conservative synagogue; an afternoon Hebrew School – somewhat pretentiously called the Talmud Torah; the Center Academy, a private day school that included Hebrew and Jewish studies along with the regular secular curriculum; a small synagogue for children's services on Shabbat and holidays; a gymnasium and swimming pool; a banquet hall; and what not. My parents shared a roomy two-story house with my grandmother, within easy walking distance of the Center. Our home soon became a haven for itinerant Hebraists from Palestine.

An ardent Zionist, who had every intention of returning to Palestine with his family, my father insisted on speaking to me only in Hebrew. For my first year or so, my mother went along with him, but then gradually slid over into English<sup>16</sup>. My grandmother insisted on speaking Yiddish – to me and everyone else, so I picked up a smattering of that language as well. My father was deeply steeped in traditional Jewish learning but completely non-observant. His passion for the Hebrew language and his inborn talent for teaching are probably what made him persist as a poorly paid Hebrew teacher in an educational system that put its emphasis on inculcating reluctant American-Jewish children with the rudiments of Jewish religious ritual. The job called for some dissemblance on my parents' part, as illustrated by the following anecdote, told to me years later:

Several members of the Center's board of directors used to practice their Hebrew on me. One Yom Kippur, when I was about three years old, one of them asked me in Hebrew, "What did you have for breakfast this morning? I answered, "bread and

<sup>&</sup>lt;sup>16</sup> One of several other children brought up bilingually in Hebrew and English in New York at he time was David Ginsbuurg, two years my senior. He grew up in another part of town, so we did not meet until years later. However the Hebraist community in New York was small. There is a family legend that David's uncle Yekutiel competed unsuccessfully with my father for my mother's hand.

butter, an egg and a glass of milk." My questioner followed up with, "and what did your father eat?". I answered indignantly, "Don't you know that grown-ups fast on Yom Kippur?"

About this time, most likely influenced by my more business-minded mother, my father established a Hebrew-oriented – if not quite Hebrew-speaking – summer camp in partnership with another Hebrew teacher. It was situated on Schroon Lake, in the Adirondack Mountains of upper New York State. It was called Schroon Nahar, I suppose because Schroon Agam would have been less euphonious. In the first few years it did very well. I recall my pre-school years, the summers in the Adirondacks in particular, as idyllic.

The idyll ended abruptly on May 9, 1928. Several days after the birth of my brother Nadav,<sup>17</sup> my mother died of post-partum complications. Somewhat precocious, I was already a schoolboy, having been enrolled in the first grade of the Center Academy at the start of the school year. My father's predicament as a single parent of an infant and a six-year-old was eased somewhat by the proximity of four maternal aunts, who took turns looking after us. He was thus able to carry on nearly as before, teaching, looking after the summer camp, and earning his B.S. degree at Columbia University's Teacher's College.

The next blow came with the collapse of the American economy following the Wall Street Crash of 1929. Support of the Center from its affluent members dropped, so the teachers had to accept a salary cut in order to keep their jobs. It would be an exaggeration to say that we lived in poverty, but things were tight. Camp Schroon Nahar kept going for another couple of years, but eventually attendance dropped, investors began calling in their loans, and the camp went bankrupt. My father could no longer afford to pay my tuition at the Center Academy, even at the reduced rate for the children of employees, so I transferred to a public school for my general education and received a thorough course of Hebrew studies from my father. The switch did not trouble me greatly. After school, I spent most of my time with my cousins, attending baseball<sup>18</sup> and soccer<sup>19</sup> games , among other activities.

<sup>&</sup>lt;sup>17</sup> At present Professor Emeritus of Economics at the Hebrew University, Jerusalem.

<sup>&</sup>lt;sup>18</sup> The Dodgers had not yet moved from Brooklyn to Los Angeles.

<sup>&</sup>lt;sup>19</sup> Soccer was a minor sport in America, but two of my uncles were avid fans of the New York Hakoah soccer team, composed largely of ex-members of the legendary Vienna Hakoah.

Meanwhile, my father completed his studies at Teachers' College towards the M.S. degree under the supervision of Prof. William H. Kirkpatrick, a strong advocate of John Dewey's theory of progressive education: the "child-centered school", a fact that was to have important consequences for us.

#### 3. Pardes Hana

It had always been clear to us that, sooner or later, we would move to Eretz Yisrael. In the summer of 1932, my father, brother and I took a brief trip to Palestine, during which Nadav and I were periodically deposited in the homes of several of our father's old friends in Tel-Aviv, Jerusalem and Ein Harod while he went around investigating opportunities for our eventual relocation. One incident on this early visit stays in my memory: We were traveling by bus from Tel-Aviv to Haifa via Jerusalem, Nablus, Jenin and Afula; the coast road had not yet been paved. At one point in the middle of nowhere, we stopped to greet a bus coming in the other direction. When we got off for a stretch, a buzz went up: one of the passengers on the other bus was Chaim Nachman Bialik; it was the only glimpse I ever had of our national poet. I was taken to the Habima theater to see Hana Rovina in Hadybbuk and Aharon Meskin in Hagolem, and was impressed by Yehoshua Bertonov's dramatic reading of Ezekiel's vision of the resurrection of the dead bones. It was an intensive introduction to current Israeli culture.

Two years later, in the summer of 1934, we boarded ship for what promised to be a permanent move to Palestine.<sup>20</sup> My father had been offered the position of head teacher in a new agricultural high school being established in Pardes Hana under the auspices of the Farmer's Association. In contrast to existing agricultural high schools like Mikveh Israel and Kadoorie, it was to include an academic secondary curriculum leading to full matriculation. Dr. Arthur Biram, the legendary founder and principal of the Reali School in Haifa, was appointed principal of the school in Pardes Hana as well, effectively making it a branch of the Reali. Dr. Biram was a Teutonic-style educator of the old school. The stated aims of the Reali at its foundation in 1913 were: "Zionism, *discipline* [my emphasis \_*A.H.*] and self-fulfillment – national and social".<sup>21</sup>

 $<sup>^{20}</sup>$  David Ginsburg, aged 14, was also on board. Our friendship was to last until his death 53 years later.

<sup>&</sup>lt;sup>21</sup> It is characteristic of Dr. Biram that when WW1 broke out in 1914 he resigned to enlist in the German army, and resumed his position as principal of the Reali School in 1919.

As the permanent site of the school in Pardes Hana was still under construction, its temporary quarters were set up in a homestead in Meged, between Pardes Hana and Karkur. Dr. Biram was way up North in Haifa, so my father was the *de facto* principal, whereas a taciturn, one-eyed Yugoslav agronomist named Zalustcher<sup>22</sup> was in charge of the still rudimentary physical plant, as well as of agricultural education. About half of the pupils were bussed in from neighboring communities, notably Hadera; those from more distant communities – me included – were boarded in a dormitory, presided over by a house mother. The first of these, was Miss Bodenheimer, a dour maiden lady whose stated educational philosophy was that anything that gave a child pleasure was *ipso facto* harmful to the development of his/her character. Her disciplinarian attitude was too much for my father to take, so she was eventually replaced by Mrs. Harifai, the widowed mother of my classmate Ben-Shahar and six-year-old Zaharira,<sup>23</sup> a suitable playmate for seven-year-old Nadav.

The school began functioning with a single class and added a second in the following year, which turned our do be our last, so the number of pupils was small,<sup>24</sup> as was the teaching staff. My father taught all of the Hebrew subjects and Zalustcher introduced us to the elements of farming. Our science teacher was Shmuel Duvdevani, whose independent research on dewfall led to the eventual invention of the Duvdevani Optical Dewmeter, which is still in universal use.

I look back at my two years in Pardes Hana with nostalgia. While by no means the "child-centered school" that my father might have liked to make it, the atmosphere in school was relaxed and much of the teaching was informal. Under Duvdevani's tutelage, I became quite expert at classifying the local flora, treating it as a game. I also enjoyed the agricultural activities, such as milking cows, grooming horses – even the arduous task of uprooting weeds with a hoe, but the idea of becoming a farmer never crossed my mind. My father took every opportunity to expose his charges to cultural events. We, the in-house contingent, would often walk the few kilometers to Hadera in order to attend a

<sup>&</sup>lt;sup>22</sup> Zalustcher was not one-eyed when we came; he had an accident during which an eye was poked out. He picked it up and drove himself to the hospital hoping it could be put back. Everyone said: "Eizeh `hevreman (*What a guy*)!"

<sup>&</sup>lt;sup>23</sup> Zaharira Harifai became a prominent actress on the Israeli stage.

<sup>&</sup>lt;sup>24</sup> Two of them, Aharon (Ahrele) Yariv and Meir (Memi) De Shalit, were to make major contributions to the security of the State of Israel.

theater performance or concert. Recitals by two world-famous violinists, Misha Elman and Josef Szigeti, kindled my interest in classical music – the violin in particular.

Though my father's relations with Dr. Biram remained cordial, their irreconcilable differences on educational policy made it increasingly difficult for him to stay on as head teacher under Biram's authority. Meanwhile, my brother and I had acquired a stepmother, a successful Haifa dentist named Yehudit Yoshpe, whom my father had met on a brief trip to the Soviet Union. Thus, our augmented family found itself back in the United States, where, soon thereafter, my father accepted an offer from the Board of Jewish Education in Greater New York to serve as Supervisor.

### 4. To and from

We resettled in the same Brooklyn neighborhood, where my brother and I resumed close contact with our cousins. Though there were other high schools nearby, I chose to enroll in Thomas Jefferson High School, several stations away on the Subway<sup>25</sup> in Brownsville, a neighborhood that is now mostly black and Hispanic, but was then largely populated by less affluent and more Orthodox Jews. The reason was purely practical: TJHS gave credit for Hebrew, so I could fulfill my "foreign language" requirement without effort. Moreover, my studies in Pardes Hana earned me sufficient credit that I was able to graduate in two years. I did well enough in all my subjects, but had no particular preference for any. In addition, I decided to learn how to play the violin, necessitating a subway ride to the Manhattan School of Music twice a week. I took the violin quite seriously, and was playing in the both the music school- and high school orchestras within a year, but did not delude myself that I was talented enough to consider becoming a professional musician.

The family embarked for Israel in the summer of 1938 and settled in my stepmother's house in Bat Galim – then quite a fashionable suburb. I enrolled at the Technion, where my performance as a first year student was less than satisfactory, to put it mildly. At 16, I was too young to adapt myself to the system: Every student was equipped with a red booklet, called the *Index*, which each of his/her teachers would sign once a semester, confirming his/her attendance at lectures. Attendance was not obligatory and was never checked, a circumstance of which I took full advantage. In some cases, absenteeism could be justified: Chemistry, for example, was taught by the elderly, bearded

<sup>&</sup>lt;sup>25</sup> The fare throughout the underground railway system in New York City was still a nickel, i.e., five cents.

Professor Ilioff, a recent arrival from Russia, who – knowing no Hebrew – had his lectures translated into Hebrew, transcribed into Cyrillic letters, and read phonetically to the class. It was more difficult to escape laboratories and exercise sessions. Chemistry and Physics were no problem, but my mediocre draftsmanship made Machine Parts, let alone Descriptive Geometry – taught by the formidable Luisa Bonfiglioli – a nightmare.

All in all, I spent less time on my studies than I did on music, resuming the violin under Zvi Haftel, concertmaster of the Palestine (not yet Israel) Philharmonic Orchestra, and studying musical theory with the brilliant pianist and harpsichordist Frank Peleg. Then, of course, there was the Hagana. We spent much of our time demonstrating against the White Paper of 1939<sup>26</sup> on the one hand and – on the other – against the Etzel (the Revisionist Irgun Tzvai Leumi), who had taken up arms against the British. Come summer, I postponed most of my exams to *Moed Bet* (second opportunity) and attended a course for squad commanders at Juara, near Ein Hashofet, where I realized that my qualifications for command were less than impressive. Meanwhile, my father, having failed to find suitable employment, left for the United States, returning briefly to take my brother back with him after war broke out in September 1939. I stayed on a few months longer, until ordered back to the United States with the other American citizens in Palestine, not having gotten around to taking the exams.<sup>27</sup>

Back in New York, I enrolled in City College, where nearly all of the students with whom I came in contact were Jewish, intellectual, leftist – ranging from Socialist *via* Stalinist to Trotskyite – and firmly pacifist. I promptly shocked my new acquaintances by enlisting in the Reserve Officers Training Corps (ROTC). The war in Europe was far away and there seemed little prospect that America would ever enter it. My motive in joining was to increase my military competence for the time when I would return to Eretz Israel and rejoin the Hagana. However, my studies at CCNY and the associated ROTC training were short lived. When my father was offered the position of Director of the Bureau of Jewish Education in Cincinnati, the family moved west, and I enrolled at the University of Cincinnati, which did not have an ROTC program.

<sup>&</sup>lt;sup>26</sup> In it, the British government imposed severe restrictions on Jewish immigration to Palestine just when the Jews of Europe were facing catastrophe.

 $<sup>^{27}</sup>$  The Mediterranean was closed to shipping shortly thereafter, so my stepmother – who stayed on a bit longer, had to fly to India and continue to the U.S. on a Dutch freighter.

It is not altogether clear to me why I chose Chemistry as my major subject. My interests were broad; any area that was intellectually stimulating might have captured my attention. However, I was suffused with the current Zionist ideology that rejected scholarship for its own sake in favor of practical pursuits. My year at the Technion convinced me that I was not cut out to be an engineer, and science was the next most "practical" field of study. Physics must have seemed too abstract and Biology too descriptive – so Chemistry it was. Once again I took up the violin. I spent two summers as a counselor, life-guard and swimming instructor in a Jewish summer camp in the neighboring state of Michigan. I joined Avukah, a Zionist Student Organization with a leftist orientation, strongly influenced by Hashomer Hatzair, an ideology that I took rather lightly. Mostly, I enjoyed making friends with like-minded students from other universities, and meeting several prominent emissaries from Israel.<sup>28</sup>

1941 was in my senior year in college. I was aware, of course, of the war in Europe and the threat to Jewish Palestine from the advancing German army in North Africa, but the war was still far away. Things changed drastically on December 7, when the Japanese attacked Pearl Harbor and America too was at war. I was called up for military service, but my induction was deferred on medical grounds: they had found a spot on one of my lungs – a finding that a private check-up could not confirm. I was therefore free to complete my undergraduate studies at the University of Cincinnati with the degree of B.A. (Honors in Chemistry),<sup>29</sup> and enroll as a graduate student at the University of Michigan.

In Ann Arbor, I took up residence in a Co-op House<sup>30</sup> most of whose members were either Jewish or black. The studies were much more demanding than at Cincinnati. Algebra was taught by the brilliant topologist Norman Steenrod, then still a largely unrecognized young man. I took Analytical Chemistry with H. H. Willard, I. M. Kolthoff's competitor, who insisted that I unlearn everything that I had learned from the latter's textbook and begin again by memorizing the Periodic Table. My Physical Chemistry teacher was L.O. Brockway, who had built the first Electron Diffraction Spectrometer as Linus

<sup>&</sup>lt;sup>28</sup> These included the then already legendary Mania Shochat, "Mother of the Kibbutz Movement", and Enzo Serenyi, who later parachuted into occupied Europe and was captured and killed by the Nazis.

<sup>&</sup>lt;sup>29</sup> At that time the University of Cincinnati granted Bachelor of Science degrees only to Graduates in Engineering; science graduates had to settle for the degree of Bachelor of Arts

 $<sup>^{30}</sup>$  The Cooperative Movement in the United States – agricultural and urban – antedates the Kibbutz Movement by a century or so.

Pauling's graduate student. I ambitiously registered for an advanced Classical Mechanics course with the eminent spectroscopist, D.M. Dennison, but had to drop out; it was too much for me. My most memorable teacher was a truly great scientist, Kasimir Fajans,<sup>31</sup> who taught an elective course in Thermodynamics. In 1942 he was only 55 years old, but had already begun wasting the remainder of his scientific career on his discredited "quanticule theory".<sup>32</sup> Nevertheless, his lectures on thermodynamics were insightful.

My enjoyable and intellectually stimulating stay at Ann Arbor lasted just one semester. The U.S. Army decided – quite correctly – that my lungs were fine after all, and I was inducted into the Army Air Force. My entire military service can be concisely summed up as zigzagging westward across the United States, moving from airbase to airbase: After basic training at Keesler Field, in Biloxi. Mississippi, a town that was ravaged by Hurricane Katrina in 2005, I was adjudged to be "officer material" and sent off to Southern Illinois University in Carbondale, Illinois for pre-flight training. My stay there was pleasant but brief. I actually had a number of flying lessons in a two-seater Piper Cub before the program was terminated. Courses for the training of commissioned air-crew, *i.e.* pilot, navigator or bombardier, were temporarily over-subscribed. Consequently, we were all packed off to air-gunnery school at Brownsville, Texas, on the Mexican border. As I was literate and reasonably articulate, it was decided that I would be trained as a gunnery instructor in Tonopah, Nevada - in the desert half way between Reno and Los Vegas. From there I was sent to Mountain Home, Idaho, where my duties consisted of sitting in a B24 (Liberator) bomber aircraft and seeing to it that the crew's gunners were competent enough to be sent off to combat. I was becoming embarrassed. Most of the other instructors, including an acquaintance from Cincinnati who became my partner in our off-duty soldierly pursuits, were veterans of at least one tour of duty in Europe, and here I was waiting out the war in comfort and safety.<sup>33</sup> Eventually I volunteered for combat and was transferred to Laredo, Texas, where I was assigned to a Liberator crew as

<sup>&</sup>lt;sup>31</sup> Fajans, and – independently - Frederick Soddy, formulated the rules of radioactive disintegration. Fajans introduced the concept of ionic polarization, explaining its role in chemical bonding. He was the leading candidate for the 1924 Nobel Prize in both Physics in Chemistry. Unfortunately, the decision was leaked prematurely, and the members of the Nobel Committee were so infuriated that they granted the Physics prize to Siegbahn instead and did not award the Chemistry prize to anyone.

<sup>&</sup>lt;sup>32</sup> See J. Hurvic, "Reception of Kasimir Fajans's quanticule theory of the chemical bond. A tragedy of a scientist", *Journal of Chemical Education* (1987), **64**(2), 122-3.

 $<sup>^{33}</sup>$  Say rather in relative safety. There were several plane crashes – with gunnery instructors aboard – during my stay at Mountain Home.

waist-gunner. The war in Europe having ended, we were being prepared for service in the Pacific, but atomic bombs was dropped on Hiroshima and Nagasaki and the war with Japan was over before our crew was considered fit for combat. I headed northwest to Walla Walla, Washington, where I waited for release while people who had entered service earlier were demobilized under the "first in first out policy". These included medical personnel, so – in consideration of my training as a chemist – I was co-opted to the medical laboratory, where I spent the time until my release doing blood- and urine analyses. Finally, In April 1946, Sergeant Emil Amitai Halevi was restored to civilian status and started on a leisurely hitchhike across the United States to Cincinnati, making frequent stops on route, including one in Kansas City to attend a recital by the brilliant violinist Giula Bustabo.<sup>34</sup>

#### 5. Jerusalem

Having been honorably discharged from the American armed forces, I was free to continue my studies in the educational institution of my choice with the support of a generous monthly stipend under the "G.I. Bill of Rights". I chose the Hebrew University in Jerusalem, Consequently, in mid-July 1946, I sailed for Israel on the converted troopship Marine Carp, in the company of several other young American and Canadian Jews, all headed for Jerusalem. While on board, the news that the King David Hotel had been bombed reminded us that all was not peace and quiet in the Holy City. A few weeks after my arrival in Jerusalem, one of my ex-shipmates suggested that I join her at a concert at the YMCA. To my surprise, but not to my displeasure, she was accompanied by an attractive young lady whom she introduced to me as Ada Rauch. Ada had been released from the British Army Medical Corps in Egypt so recently that she was still in uniform. She and I were soon inseparable and were married several months later – but that is getting ahead of my story.

A brief biographical summary: Ada was born and educated in Sopot, the seaside resort of Danzig – then a Free City in the Polish Corridor between East and West Prussia – to Sala (*née* Karo) and Leon Rauch, who had just moved there from the Polish city of Kaliscz with their young daughter Ella. In 1937, 15-year-old Ella immigrated to Israel with the Halutz movement and Ada hoped to follow her as soon

<sup>&</sup>lt;sup>34</sup> I did not know at the time that, having been charged with being a Nazi sympathizer, her career as a virtuoso would soon be over.

as possible. The Nazi stranglehold on Danzig was soon complete, culminating in the Krystallnacht riots in November 1938, in which the synagogues were burned, Jewishowned shops were ransacked, and the Jewish men were rounded up and beaten. Through the window of her home Ada saw the Temple across the street go up in flames. It was decided that Ada's father, mother and younger brother Natek would join their extended family in Kaliscz,<sup>35</sup> whereas she would remain in Danzig in order to leave for Palestine with the next Youth Aliya group. Unfortunately, the projected sailing never took place. In consequence, on March 3, 1939 – two weeks after her 15<sup>th</sup> birthday - Ada travelled across Europe by rail to Reni, Roumania with a group of Danzig Jews, and set sail for Palestine in the illegal immigrant ship Astir, run by the Zionist-Revisionist organization, Beitar. The voyage, which was notorious for its length - four months at sea - and for the brutality of the Beitar crew towards the passengers, ended in shipwreck on the Gaza-Ashkelon coast on July 7. After a spell in the Sarafand interment camp, Ada was briefly reunited with her sister at Tel Hai before being assigned to a Youth Aliya group in Kibbutz Givat Hayim. Then, enlistment in the British Army, and service until the war's end as a nurse in Egypt and field hospitals in the North African desert. Soon after arriving in Jerusalem she was employed as a nurse in the eye clinic of the already legendary Dr. Ticho.<sup>36</sup>

At this time – though it was soon to change – the first degree offered by the Hebrew University was Master of Science. Therefore, my B.A. (Honors) degree from Cincinnati and my semester as a graduate student at Michigan notwithstanding, I was enrolled as an undergraduate. I was exempted from most of the undergraduate lab courses but required to take a number of lecture courses, several – but not all – of which were enjoyable. The world famous physicist Giulio (Yoel) Racah was a wonderful lecturer; the lectures of the eminent physical chemist Ladislaus Farkas were almost impossible to follow; those of the biochemist Andor Fodor had to be taken down and memorized, so that they could be recited verbatim at the oral examinations.

I was given freedom to choose a thesis supervisor. My main interest was in chemical theory, which in this period was largely qualitative. I was intrigued by Ingold and

<sup>&</sup>lt;sup>35</sup> Ada never saw them again. Natek died in the Flössenberg concentration camp; the fate of her parents is unknown.

<sup>&</sup>lt;sup>36</sup> Now the Ticho House Museum, featuring the art of Dr. Ticho's wife Anna.

Hughes' studies of the mechanism of solvolysis and wished to prepare myself to join their research group at University College London in due course. Therefore, it was only natural that I apply to work on my M.Sc. thesis in the laboratory of Professor Max Frankel, who taught the course on "Theoretical Organic Chemistry". There were two brilliant young scientists, the Katchalsky (Katzir) brothers, Aharon and Ephraim, in Frankel's laboratory, on the ground floor of the Chemistry Building on Mount Scopus, but it did not enter my mind to ask either of them to be my supervisor.<sup>37</sup> They were working on polymers, not my idea of "Theoretical Organic Chemistry". In any event, I probably would not have been accepted by either; they had more pressing concerns. As I learned later, as soon as Frankel left the lab for the day, they switched to military-oriented research, working on into the night. Both were founding members of Hemed, the newly organized Science Corps of the Hagana. When I heard stories about how shabbily Frankel treated his own research students and how little they learned from him, I realized that I had made a bad mistake. I quickly opted out and moved upstairs to the laboratory of Professor Moshe Weizmann, earning Frankel's undying animosity.

Moshe Weizmann, brother of the eminent Zionist leader and President-to-be, Chaim, was a charming elderly gentleman who had once been a capable industrial chemist. He used to come up to his laboratory every morning at eight to chat with us for an hour or so, and then go to Café Alno on Ben Yehuda Street to play chess with his cronies.<sup>38</sup> The running of the lab was left to his senior assistant, Shalom Israelashvili (Sarel), who had recently earned his doctorate with Prof. Felix Bergmann at the Sieff (subsequently Weizmann) Institute in Rehovoth. Moshe Weizmann had hopes that orange peel could be used as a raw material in the manufacture of perfume, an area in which he had once been active. Accordingly, the research project assigned to me was the peroxide-catalyzed addition of iodoform to limonene and subsequent hydrolysis of the product di-iodide to the aldehyde, which would then be the substrate for further syntheses. Progress was slow, not entirely due to my limited competence as a synthetic chemist. The equipment was inadequate and we had no analytical instruments at all. Virtually the only way of characterizing a product was by melting point. Besides, there were distractions.

<sup>&</sup>lt;sup>37</sup> Dov Ben-Ishai was Ephraim Katchalsky's graduate student at the time.

<sup>&</sup>lt;sup>38</sup> This point is in dispute. Mordecai Rubin claims that it was Café Atara.

On January 29, 1947 Ada and I were married in the presence of two witnesses by Rabbi Kurt Wilhelm, a Conservative rabbi whose right to perform the marriage ceremony was not challenged by the Jerusalem Rabbinate. The reception was held at the home of Levi Riklis,<sup>39</sup> an old friend of my father. There were chairs for Moshe Weizmann and Yehuda Sandler, Farkas' senior assistant; most of the other guests sat on the floor. We settled in a rented room in the Givat Shaul section of Jerusalem. Many members of the militant groups Etzel and Lehi were reputed to be residents of Givat Shaul, so every once and a while a British army patrol would round up the men to check their identification and search them for hidden weapons. Once, in our absence, they searched our room. According to our landlady, they walked in, saw a picture of Ada in her British army uniform and one of me in the uniform of the U.S. Army Air Force, and walked out.

Needless to say, by then I had already rejoined the Hagana. I was enlisted in the Shai (Sherut Yediot) the seed from which all of the various Israel intelligence services eventually grew. In view of my command of English, I was assigned to the "English branch". My job was to obtain information on policy from various Jewish employees of the Mandatory Government and pass it on. My major coup was the "Water Law", a proposal to reallocate the water resources of Palestine to the detriment of the Jewish agricultural settlements. My contact typed an extra carbon copy of the proposal and passed it on to me. I had it on my person when Ada and I were riding home on the bus to Givat Shaul. As luck would have it, a British Army patrol stopped the bus and ordered all the men out to be searched. I quickly slipped the papers to Ada, alit, and watched with concern as the officer got on the bus to check the remaining passengers. I needn't have worried; as he approached Ada, she said, "Why were we stopped? The bus was checked just a few minutes ago". He answered, "Really? Sorry.", got off and allowed us to proceed.

#### 6. Military Science

The "Water Law" was never passed. After November 29, 1947, when the United Nations General Assembly voted for partition of Palestine into a Jewish state and an Arab state, the Mandatory Government began winding down. In parallel, the Arab

<sup>&</sup>lt;sup>39</sup> Father of Shalom Ronli Riklis, conductor of the Israel Philharmonic Orchestra,. and the biochemist Emanuel (Emi) Riklis.

uprising began, with minimal interference from the British army, which was preparing for withdrawal. It was clear that there would soon be no use for the English section of the Shai, so I was transferred to Hemed, the newly formed scientific corps. At the University, all pretence of normal academic activity ceased and virtually all of the students and younger faculty members were co-opted to the Hagana. Meanwhile, two new American students had joined our laboratory: Mordecai Rubin and Moshe Perlstein. Mordecai also joined Hemed; Moshe, the more recent arrival, was inducted into an infantry unit made up largely of students and died shortly thereafter in the ill-fated "Convoy of 35".<sup>40</sup>

Our training in military science – such as it was – took place on the Mount Scopus campus. When the regular bus route stopped operating, we went up by bus convoy for longer periods. These convoys continued until April 13, when one was ambushed en route with the loss of 78 lives, including a number of our friends. The main headquarters of Hemed in Jerusalem was set up in the Schneller Complex, originally a German Lutheran mission. I was assigned the task of making detonators, to be used in a variety of home-made weapons, including the fabled Davidka mortar. Emanuel Goldschlag – a deaf mute who loved explosions because they were the only sound that he could "hear", Yoram Avidor<sup>41</sup> and I set up our laboratory in the deserted Arab village of Lifta a couple of kilometers to the West. Communication between Tel-Aviv and Jerusalem by land was becoming increasingly difficult, as convoys along the main road were under constant attack.<sup>42</sup> With the University stockrooms beyond reach and supplies from the coast unavailable, we began scouring the pharmacies of Jerusalem for formaldehyde, acetaldehyde, phenol, hydrazine and nitric acid, and commandeering them whenever they were found. The main explosive charge in our detonator was pentaerythritol tetranitrate (PETN) which had to be desensitized by dilution with paraffin - 15%, if I remember correctly - to prevent its going off prematurely. Our task was eased somewhat when a cache of PETN bricks of uncertain

<sup>&</sup>lt;sup>40</sup> A convoy of forty men set out on foot from Jerusalem on January 15, 1948 to relieve the siege of the four "Etzion block" settlements. Five were sent back; the thirty-five who continued were ambushed and massacred.

<sup>&</sup>lt;sup>41</sup> Yoram Avidor became the Technion's first Professor of Biochemistry. Originally a member of the Department of Chemistry, he and Ruth Ben-Shai seceded in 1971 to found the Department of Biology.

<sup>&</sup>lt;sup>42</sup> The alternative "Burma Road" being paved to the south of the main road was not completed until June.

origin was found in a cave and delivered to us. Since we had no idea of the amount of paraffin in them, we painstakingly leached all of it out with gasoline and then put back the required amount. The PETN was covered by a layer of lead azide, which also had to be desensitized by dilution with gelatin. To set off the azide we added a thin layer of diazodinitrophenol (DINOL) which was ignited by a spark. We made the detonators by hand in batches of 20. For quality control, we tested one detonator in each batch, to Goldschlag's manifest delight. It was a fine product! Having perfected the process, we handed over our stock to the appropriate authorities, wrote up the procedure in detail for Taas, the Ministry of Defense's budding unit for the manufacture of military supplies, and closed shop. Sad to say, as the result of negligence (not ours!) our efforts were almost entirely wasted. The fruit of our labor was stored in a house in the center of Jerusalem along with other explosives and incendiaries; a fire broke out and destroyed the lot. Moreover, as we learned later, the Taas engineer to whom the detonator project had been entrusted disregarded our instructions to leach out the indeterminate amount of paraffin present in the PETN bricks and replace it with the specified quantity, but used the PETN as it came. The result: an explosion with one fatality and suspension of the project.

Several days before May 15<sup>th</sup>, my brother and a number of other American volunteers embarked for Palestine. On that historic date they celebrated the establishment of the State of Israel on board ship, but – on the 19<sup>th</sup> – they were taken off in Beirut, where they enjoyed Lebanese *hospitality* for six weeks, while the U.S. government negotiated their release from internment. As for Ada and me, we were no longer members of the Hagana but of the Israel Defense Forces. However, the transition was automatic and hardly noticeable; for a while the IDF remained an informal, voluntary organization. With the Egyptian army moving up from the South and the Jordanian Arab Legion to the North and East, Jerusalem was truly under siege. Ada and I now lived in a rented room in Beit Hakerem, from where we would walk each morning, taking shelter whenever there was shellfire from Nebi Samuel in the Northwest or Mar Elias in the Southeast, to our posts: Ada to the Signal Corps in Romema and I to Lifta or Schneller, as required. We took our frugal meals in an Army canteen in Romema and walked home to Beit Hakerem in the evening, unless the shelling made it advisable to spend the night within the protective walls of Schneller.

The four-week truce, beginning June 11, allowed us a welcome break, including a week of gourmandizing in Tel-Aviv. More importantly, as military supplies could

now be brought up from the coast, the improvisatory work of Hemed in Jerusalem lost much of its urgency and the unit in Jerusalem started to wind up its operations. Many of its members were transferred to "the Hill", the main Hemed base in the Tel-Aviv area. Ada and I remained in Jerusalem until a second, longer truce was brokered in mid-July, when both of us were transferred to the newly-formed Air Force and moved to Tel-Aviv for reassignment. As the senior Israeli officers had served in the RAF, and many – if not most – of the air crew in the early days were American, British and South African volunteers, all Air Force business was carried out in English. There were an excess of volunteer air-gunners with combat experience for the few planes with guns, so – being bilingual and having done intelligence work in the Shai – I was sent off to the Ramat David Air Base as Intelligence-Operations Officer. After a short course as code and cipher operator, Ada too was posted to Ramat David, where we were housed in family quarters and remained in relative comfort until the final armistice in 1949.

#### 7. A student once more

On release from the IDF, we paid "key money" for a one-room penthouse in Talpiot. Originally the seat of Jerusalem's intellectual aristocracy (Agnon, Klausner, Ben Yehuda, etc...), Talpiot's proximity to the no-man's land southeast of Jerusalem made the rent affordable. As sending me back to the lab was not an option, my incomplete M.Sc. thesis was written up and accepted as is,<sup>43</sup> but I still had to prepare myself for the final oral exams while supplementing my income by making detonators (What else?!) for Taas. Despite Frankel's having grudgingly given me a minimally passing grade in his course, my average was good enough to earn me a scholarship from the British Friends of the Hebrew University for work towards the doctoral degree at any British university that would have me. My first choice was, of course, University College London, the "Mecca" of Physical Organic Chemistry and – to my delight–I was accepted.

In the fall of 1950, after a brief visit to the family in Philadelphia, where my father was now director of Jewish Education, we sailed for England. We rented a room in Swiss Cottage, a short ride by Underground from University College's old chemistry

<sup>&</sup>lt;sup>43</sup> M. Weizmann, S. Israelashvili, E.A. Halevi and F. Bergmann, "Peroxide-catalyzed addition of iodoform to olefins", *J. Amer. Chem. Soc.* **69**, 2547 (1947).
building on Gower Place, where I set to work in earnest. My scholarship was for one year, once renewable, so I had to complete my work for the Ph.D. in two years – or else. This was not common in the UCL Chemistry Department, where a student working in Physical Organic Chemistry was expected to be both a physical chemist and an organic chemist, *i.e.*, to synthesize his or her own substrates and then carry out the required physical measurements on them. Fortunately, my main substrate did not have to be synthesized; my research topic might have been defined whimsically as "The kinetics of the nitration of water".

Ingold and his collaborators had filled half an issue of the Journal of the Chemical Society with a series of papers proving the existence of nitronium ion (NO<sub>2</sub><sup>+</sup>) by cryoscopy, characterizing it by UV, IR and Raman spectroscopy, and proving by reaction kinetics that it was the active reagent in aromatic nitration.<sup>44</sup> The mechanism

with strong acid in a non-aqueous solvent was shown to be as follows:

$$\mathbf{H}^{+} + \mathbf{HNO}_{3} \stackrel{K_{eq}}{\longrightarrow} \mathbf{H}_{2}\mathbf{ONO}_{2}^{+} \stackrel{k_{1}}{\longrightarrow} \mathbf{H}_{2}\mathbf{O} + \mathbf{NO}_{2}^{+}$$
$$\mathbf{H}_{2}\mathbf{O} + \mathbf{NO}_{2}^{+} + \mathbf{ArH} \stackrel{k_{2}}{\longrightarrow} \mathbf{Ar} \stackrel{\mathbf{H}_{+}}{\overset{\mathbf{H}_{+}}{\longrightarrow}} + \mathbf{H}_{2}\mathbf{O} \stackrel{fast}{\longrightarrow} \mathbf{ArNO}_{2} + \mathbf{H}_{3}\mathbf{O}^{+}$$

They proved that  $NO_2^+$ , rather than  $H_2ONO_2^+$ , is the nitrating agent by demonstrating that if the aromatic substrate (ArH) is made increasingly reactive by electron releasing substituents ( $k_2 >> k_{-1}$ ), there is a limit beyond which the rate of nitration cannot be increased, *i.e.*, the rate of formation of  $NO_2^+$  has become rate limiting. In strong *aqueous* acid, however, increasing the reactivity of ArH increased the rate apparently without end. The question was whether, in the presence of water in bulk, the concentration of  $NO_2^+$  is reduced to such an extent that the hydrated ion  $H_2ONO_2^+$  has become the nitrating agent, or whether here too the reactive species is the minute concentration of  $NO_2^+$  in equilibrium with  $H_2ONO_2^+$ . If the latter, the limiting behavior was not observed because the most reactive aromatic substrates used were not reactive enough to compete with water for the nitronium ion: ( $k_2 << k_{-1}$ ).

My assignment was to measure the kinetics of the exchange of oxygen atoms between nitric acid and water enriched in  $O^{18}$ . This would give us the rate of dissociation of

<sup>&</sup>lt;sup>44</sup> C.K. Ingold et mult. al., J. Chem. Soc. 1950, 2400-2696.

 $H_2ONO_2^+$  to nitronium ion and water, equal under equilibrium conditions to the rate of their recombination (nitration of water), which we could then compare with the rate of aromatic nitration. One young faculty member, D.R. Llewellyn, had been assigned the task of constructing and operating a distillation column for separating the isotopomers of H<sub>2</sub>O, in friendly competition with Israel Dostrovsky, who was setting up a similar column at the Weizmann Institute in Rehovoth. Another, C.A. Bunton -Bunny to his friends – who had just returned to London from Rittenberg's laboratory at Columbia University and set up a mass spectrometer, was my immediate supervisor. Ingold's towering intellect dominated the department. His wife, Hilda, the departmental secretary, protected her husband's privacy and kept his office off limits. Nevertheless, he and his close associate E.D. Hughes kept a keen eye on everything that went on in the Department. Ingold would come into the lab unannounced, ask how things are going and perhaps make a suggestion, always cogent and often valuable, as to how to proceed. The younger faculty members were more accessible; in addition to Bunton, they included D.P. Craig, Alan Maccoll, D.J. Millen, R.J. Gillespie, Charles Vernon and Peter De la Mare. My contemporaries, including several, e.g., John Ridd, Gabriello Illuminati, Jim Gowan and Gerry King, who were to have distinguished academic careers, exchanged ideas freely with me on topics ranging from our individual research projects to politics and religion.

Well within the allotted two-year period, my problem was solved: The rate of oxygenexchange was measured as a function of acid concentration and found, in any given set of conditions, to be more rapid than the nitration of the most reactive substrate than had been used to date. I studied the nitration kinetics of several more reactive aromatic compounds, having actually worked out a synthetic method for their preparation,<sup>45</sup> and confirmed the hypothesis that formation of NO<sub>2</sub><sup>+</sup> is indeed rate limiting for nitration in aqueous acid, as it is in non-aqueous media.<sup>46</sup> All in all, the UCL Chemistry Department was an exciting and stimulating place. I would have loved to stay on and continue with the study that I had begun on the catalysis of

<sup>&</sup>lt;sup>45</sup> C.A. Bunton and E.A. Halevi, "The preparation of alkanesulphonic acids", *J. Chem. Soc.* **1952**, 4541.

<sup>&</sup>lt;sup>46</sup> C.A. Bunton, E.A. Halevi and D.R. Llewellyn, "Oxygen exchange between nitric acid and water, Part I." *J. Chem. Soc.* **1952**, 4912-4916; C.A. Bunton and E.A. Halevi, "Oxygen exchange between nitric acid and water, Part II. A correlation of oxygen exchange with aromatic nitration", *J. Chem. Soc.* **1952**, 4917-4924.

nitration by nitrous acid.<sup>47</sup> Moreover, London had gotten over the blitz and the subsequent period of austerity, and its cultural riches – theater, music and the visual arts – were there to be enjoyed. During our two-year stay, we formed friendships, both chemical and non-chemical, several of which were to last a lifetime. However, much as we would have liked to prolong our stay, our time in London had run out. I wrote up my thesis, Ada typed it and I submitted it. After the oral exam and visit to the nearby pub for the customary farewell beer with my lab-mates, my student days were over.

#### 8. Back to Israel

Before boarding ship in Marseilles, we bought a carton of eggs. In 1952 food was still rationed in Israel and we knew that our friends in Jerusalem, especially those with young children, would welcome a gift of a dozen "almost fresh" eggs. We returned to our penthouse in Talpiot, Ada got a clerical job in the State Comptroller's office and I began fulfilling my three-year commitment to the Hebrew University. I was appointed Assistant<sup>48</sup> in the Department of Physical Chemistry, then situated on Mamilla Road, which was blocked off from the Jaffa Gate by a high barrier designed to prevent sniping from the Old City wall.

There was no Head of Department when I arrived. Professor Farkas had died in a plane crash several years earlier and his senior co-worker, Yehudah Sandler emigrated to the United States when he was not offered the position. The lectures in Physical Chemistry were delivered by Aharon Katzir – then still Katchalsky, who – having agreed to head the Department on a trial basis – came up from the Weizmann Institute twice a week. Aharon was a spellbinding lecturer; students would leave his lectures under the often illusory impression that they had grasped everything he said perfectly. I was assigned the duty of conducting the exercise sessions. My job, in addition to distributing numerical problems and checking their solutions, was to review the concepts raised in Katchalsky's lectures and ensure that they were properly understood. The students were an exceptionally talented group, many of whom

<sup>&</sup>lt;sup>47</sup> C.A. Bunton, E.A. Halevi and D.R. Llewellyn, "Oxygen exchange between nitric acid and water, Part III: Catalysis by nitrous acid", J. *Chem. Soc.* **1953**, 2653-2657.

<sup>&</sup>lt;sup>48</sup> This rank, one below Lecturer, no longer exists in Israeli Universities.

eventually became prominent figures on the Israeli – and international – scientific scene;<sup>49</sup> the task of teaching them was both demanding and rewarding.

In order to continue experimental research with stable isotopes, I needed a mass spectrometer, so I undertook to construct one with the assistance of Uriel Vogel, a capable technician. Meanwhile, I was constrained to "paper chemistry". At UCL I had measured the rate of isotope exchange between  $H_2O$ , a molecule with one O-atom and HNO<sub>3</sub>, which has three. I noticed that no-one had derived a general formula for the kinetics of isotope exchange between molecules with different numbers of exchangeable atoms, so I took it on myself to rectify the omission. It required more knowledge of statistical mathematics than either Bunton or I possessed, so we called on David Craig for assistance. Having put the problem aside while writing my thesis, I was now free to complete it in correspondence with my collaborators in London.<sup>50</sup>

My experimental program was a non-starter. Soon after my arrival, Aharon Katchalsky resigned and was replaced by Gabriel (Gabor) Stein as Head of Department. Personally quite congenial, Stein's firm opinion was that the line of research carried out in the Department should be determined by its Head. Stein's interest was in homogeneous reactions, in particular free radicals and trapped electrons, whereas I – a product of the Ingold school – was interested in heterogeneous reactions, *i.e.*, of ions and polar molecules. As a lowly Assistant, I had to comply. The only result of our collaboration was a study of the bromination of naphthalene that was published several years later.<sup>51</sup>

Early in 1954, David Ginsburg, with whom I had maintained friendly relations over the years, informed me confidentially that he had been invited to head the Department of Chemistry at the Technion with a view to converting it from a service unit that was mainly devoted to teaching elementary chemistry to engineering students to a genuine research department. He asked me whether I was prepared to join him in the endeavor and I was delighted to accept, but my commitment to the University still had more than a year to run. As soon as David took up his appointment, I informed Gabor Stein that I would be moving to the Technion the following year. He asked whether I

<sup>&</sup>lt;sup>49</sup> I will not mention their names in order not to offend anyone by inadvertent neglect.

<sup>&</sup>lt;sup>50</sup> C.A. Bunton, D.P. Craig and E.A. Halevi, "The kinetics of isotopic exchange reactions", *Trans. Faraday Soc.* **51**, 196-204 (1955).

<sup>&</sup>lt;sup>51</sup> E.A. Halevi. I. Loeff and G. Stein, "The mechanism of the vapor-phase bromination of naphthalene", J. *Chem. Soc.* **1957**, 5088-5091.

would stay on in Jerusalem if I were promoted to Lecturer, but David immediately countered with an appointment as Senior Lecturer, a rank higher than was justified by my original research up to that time.

During the academic year of 1954-55 my physical presence was in Jerusalem but my heart was in Haifa. One event that I remember fondly was Linus Pauling's visit to Jerusalem en route to the Nobel Prize ceremony in Stockholm. He gave a fascinating lecture at the University on the helical structure of proteins; he took a long, heavy key-chain out of his pocket, twirled it, and there was the helix. David, who had invited Pauling to lecture at the Technion, asked me to accompany him. Accordingly, Pauling, his wife Ava, Ada and I boarded a limousine and traveled North with a motorcycle police escort, as befits a Nobel laureate. When we were under way, Pauling asked the driver to make a stop at Pardes Hana, where he and his wife had friends. These turned out to be the Duvedvanis, who had become friendly with the Paulings during the former's sabbatical in Pasadena. I thus got to meet my old biology teacher for the first time since my school days. This trip was of scientific value to me as well. I had already become interested in secondary isotope effects and was wondering whether they could be related to isotopic polarity differences, such as the reportedly larger dipole moment of DCl than HCl<sup>52</sup> than and that of ND<sub>3</sub> relative to NH3,53 which were presumably due to slight differences in mean geometry due to vibrational anharmonicity. I mentioned this to Pauling, who suggested that I look at infrared intensities. Following up on his suggestion, I computed both dipole moment differences; my results were in fortuitously good agreement with the reported experimental data.54

#### 9. Technion – Part One

After our move to Haifa in the autumn of 1955 we stayed put for over half a century. It can be divided into two unequal periods, differing both in our life style and in the nature of my scientific activities. In the first fifteen years or so, during which I was climbing the academic ladder at the Technion, we lived in four rented apartments in

<sup>&</sup>lt;sup>52</sup> R.P. Bell and I Coop, *Trans. Faraday Soc.*, **34**, 1209-14 (1938).

<sup>&</sup>lt;sup>53</sup> J.M.A. De Bruyne and C.P. Smyth, *J. Amer. Chem. Soc.* **57**, 1203 (1935).

<sup>&</sup>lt;sup>54</sup> E.A. Halevi, "Polarity differences between deuterated and normal molecules", *Trans. Faraday Soc.***54**, 1441-1446 (1957).

different parts of town, not to mention our temporary quarters abroad during two sabbatical leaves and several shorter visits abroad.

Our first home was in Shikun HaTechnion on Horev Road. Ada found a job in the Zim Maritime Company downtown and I ensconced myself in the laboratory on the Hadar Hacarmel campus that David Ginsburg had set aside for me. My lab was spacious and relatively well equipped. Without having been asked, David obtained a used, home-built, mass spectrometer for me from the Weizmann Institute, which I hardly ever used, as my principal interest had shifted from the isotopes of "heavy" atoms like oxygen to those of hydrogen, for which more convenient analytical methods were available. In the first few years he steered the brightest students toward me for their fourth year undergraduate research project, which could then be continued toward the M.Sc. degree.<sup>55</sup>

Our experimental work centered on hydrogen isotope effects, specifically secondary isotope effects. Up to a couple of years earlier, it was thought that the rate of a reaction can be modified by isotopic substitution, say D for H, only if a bond to the relevant hydrogen atom was broken or formed in the reaction. It was then shown independently by E.S. Lewis and V.J. Shiner,<sup>56</sup> that *beta*-deuterated alkyl groups reduced the rate of solvolysis of alkyl halides. This secondary isotope effect was ascribed to less effective hyperconjugation of CD- relative to CH bonds. Recalling the isotopic dipole moment differences referred to above, I wondered whether there could be a more general effect, differential electron release from CH and CD bonds, due to the fact that – as a result of anharmonicity – the mean length of a CD bond is shorter than that of a CH bond. Using a differential potentiometric method of my own devising, we measured the effect of deuteration in the a-alkyl group on the strength of carboxylic acids. We observed that the deuterated acids were slightly weaker than the normal acids. As hyperconjugation cannot be invoked here, this result was ascribed to more effective electron release from CD- than from CH bonds due to their shorter average length.

<sup>&</sup>lt;sup>55</sup> Two of them, Arza Ron and Shammai Speiser eventually became Professors in our Faculty.

<sup>&</sup>lt;sup>56</sup> E.S. Lewis and C.B. Boozer, , *J. Amer.* Chem. Soc. 74, 6306 (1952); V.J. Shiner, J. Amer. Chem. Soc. 75, 2925 (1953).

Our preliminary findings were published<sup>57</sup> and presented in the summer of 1957 at the IUPAC General Assembly in Paris, at which the Israeli contingent included David and Hemda Ginsburg, Eli and Rachel Loewenthal, as well as Ada and me. In the course of the next decade we investigated what became known as the "inductive effect of deuterium" from several angles. It was shown to increase the basicity of nitrogen bases as well as to decrease the acidity of carboxylic acids,<sup>58</sup> and to oppose the hyperconjugative effect in charge transfer complexes, where both were operative.<sup>59</sup> Having made the claim that secondary isotope effects can be used as criteria of mechanism,<sup>60</sup> we went on to prove, on the basis of a combination of primary, secondary and solvent isotope effects, that in nitration of substituted anilines at the nitrogen atom, the rate limiting step can be shifted from attack by NO<sub>2</sub><sup>+</sup> to the subsequent proton abstraction.<sup>61</sup>

In order to repeat the old work on isotopic dipole moment differences, I recruited an electrical engineer, Naftali Haran, and persuaded the Faculty of Electrical Engineering to grant him credit towards the Ph.D. degree for the design of an instrument for the direct measurement of differences in electric polarization as a function of temperature.<sup>62</sup> The dipole moment and polarizablity differences that we obtained for DCl vs. HCl and ND<sub>3</sub> vs. NH<sub>3</sub> with our differential modification of the venerable

<sup>&</sup>lt;sup>57</sup> E.A. Halevi and M. Nussim, "Electron release from carbon-hydrogen and carbon-deuterium bonds", *Bull Research Council Israel* **5a**, 263-264 (1956); E.A. Halevi, "Secondary isotope effects as a criterion of mechanism", *Tetrahedron* **1**, 174-175 (1957). Embarrassingly, the reported  $pK_a$  differences obtained by my potentiometric method were subsequently found to be too large, and were eventually revised, using a differential NMR technique: Y. Bary, H. Gilboa and E.A. Halevi, "Secondary hydrogen isotope effects, Part V: Acid and base strengths. Corrigendum and addendum", *J. Chem. Soc. Perkin II* **1979**, 938-942.

 $<sup>^{58}</sup>$  E.A. Halevi, M. Nussim and A. Ron, "Secondary hydrogen isotope effects, Part I: Strengths of  $\alpha$ -deuterated carboxylic acids and amines", J. Chem. Soc. 1963, 866-875.

<sup>&</sup>lt;sup>59</sup>E.A. Halevi and M. Nussim, Secondary hydrogen isotope effects, Part II: Association constants between methylbenzenes and chloranil", *J. Chem. Soc.* **1963**, 876-880; E.A. Halevi and B. Ravid, "Secondary isotope effects on π-complex formation", *Pure Appl. Chem.* **8**, 339-346 (1964).

<sup>60</sup> E.A. Halevi, "Secondary isotope effects as a criterion of mechanism", Tetrahedron 1, 174-175 (1957).

<sup>&</sup>lt;sup>61</sup> E.A. Halevi, A. Ron and S. Speiser, "Secondary hydrogen isotope effects, Part III: The mechanism of N-nitration", J. Chem. Soc. **1965**, 2560-2569.

<sup>&</sup>lt;sup>62</sup> E.A. Halevi, E.N. Haran and B. Ravid, "Direct measurement of small differences in dipole moment and polarizability", *Trans. Faraday Soc.* **67**, 44-55 (1967); E.A. Halevi, E.N. Haran and B. Ravid, "Dipole moment and polarizability differences between NH3 and ND3", Chem. *Phys. Lett.* **1**, 475-476 (1967).

Debye procedure were consistent with published infrared and Raman intensities, as well as with the results of new spectroscopic methods, that had begun to appear.<sup>63</sup>

The modern techniques referred to above soon rendered our method obsolete. Nevertheless, our studies yielded an unexpected dividend. In order to be consistent with the spectroscopic results, our dipole moment differences had to be calculated with the Debye-Van Vleck equation, that takes account of rotational excitation, as calculation with the classical Debye equation yields incorrect results and – in the case of hydrogen chloride – actually reverses the direction of the isotope effect. The data for ammonia had to be corrected for vibrational excitation as well. I reported the results orally at the Symposium on Quantum Chemistry, held in January 1971 on Sanibel Island, Florida in honor of J.H. Van Vleck, in his presence and that of such luminaries as Mulliken, Slater, and Teller. After my presentation, Van Vleck commented that he had been waiting forty years for someone to confirm his equation experimentally.

An unfortunate consequence of trying to carry out experimental research along several lines at once with a very small group of associates is that it is not always possible to follow each line to its proper conclusion. It may happen that a student has completed his or her work for the M.Sc. or Ph.D. thesis, but the results are not publishable without further refinement. In a large research group, another student completes the project, which is then published with both as co-authors. In a small group working on numerous projects it may happen that the work is not completed and the results are never published. To my regret, this situation - which is clearly unfair to the students involved – occurred several times in my group, as in the case of Chava Fink's investigation of proton exchange in aromatic systems, the endeavor of Hana Loewenschuss (now Frauenknecht) to establish an acidity function for concomitant protonation and hydration, and Zvi Ganot's determination of the dipole moment and polarizability differences between CD<sub>3</sub>F and CH<sub>3</sub>F. Not as serious but still depressing is a finding that is sufficiently novel to be worth publishing as a preliminary communication but cannot be followed up and published in full, as in the case of Zafra Margolin's observation of a temperature-dependent secondary isotope

<sup>&</sup>lt;sup>63</sup> E.A. Halevi, "Polarization and polarizability differences between isotopic molecules", *Internat. J. Quantum Chem. (Symp. No. 5)*, 367-370 (1971); C. Scher, B. Ravid and E.A. Halevi, "Deuterium isotope effects on the dipole moment and polarizability of HCl and NH<sub>3</sub>", *J. Phys. Chem.* **86**, 654-658 (1982).

effect.<sup>64</sup> The frustrating inability to maintain a sufficiently large research group was a factor in my eventual decision to abandon my experimental program.

In parallel with the experimental work, I undertook – with the guidance and collaboration of Ruben Pauncz and the participation of Arza Ron – a computational study of the effect of methyl deuteration on the properties of conjugated molecules.<sup>65</sup> Differences in the mean geometry due to anharmonicity of the symmetrical bending and stretching modes affected the mean electron distribution in ways that could be interpreted as genuine – though very weak – inductive and hyperconjugative effects operating in tandem or in opposition.

The physical organic community found it perfectly reasonable to consider substituent groups like CD<sub>3</sub>- and CH<sub>3</sub>- to have slightly different electronic properties, in much the same way as ethyl differs from methyl and chloro from bromo. To "genuine" physical chemists, however, the idea was anathema because, to them, the term "electronic" was limited to properties – such as the electronic energy – which are identical in isotopomers, whereas isotope effects arise from differences in the vibrational spectrum. The apparently contradictory points of view were reconciled in the summer of 1959 by Max Wolfsberg, Ruben Pauncz and me in Lidingö, an island in the Stockholm archipelago. It could be shown using first order perturbation theory that the average potential energy of interaction between a charge and an anharmonically oscillating dipole is isotope-dependent. I eventually summarized this analysis, which we had unexplainably failed to publish, on pages 133-142 of my review of secondary isotope effects.<sup>66</sup> Ruben and I, accompanied by our respective wives and children, had come to Lidingö to attend Per Olov Löwdin's second Summer School on Quantum Chemistry. Ruben, who attended the first Summer School the previous year, impressed Löwdin so much that he was invited back as a

<sup>&</sup>lt;sup>64</sup> E.A. Halevi and Z. Margolin, "Temperature dependence of secondary isotope effects on aqueous alkaline hydrolysis", *Proc. Chem. Soc.* **1964,** 174. Zafra, now Zafra Lerman, is a professor in Columbia College, Chicago. She has received numerous awards for her contributions to chemical education and public service.

<sup>&</sup>lt;sup>65</sup> R. Pauncz and E.A. Halevi, "The effect of deuteration on electron distribution and energy of conjugated molecules, Part I: LCAO-AO treatment of toluene", *J. Chem. Soc.* **1959**, 1967-1974; E.A.—Halevi and R. Pauncz, Part II LCAO-AO treatment of toluene CD<sub>3</sub>", *J. Chem. Soc.* **1959**, 1974-1980; A. Ron, E.A. Halevi and R. Pauncz, Part III: LCAO-AO treatment of ethylcarbonium ion and its methyl-deuterated analog", J. *Chem. Soc.* **1960**, 630-636.

<sup>&</sup>lt;sup>66</sup> E.A. Halevi, "Secondary Isotope Effects", in S.G. Cohen, A. Streitwieser, Jr. and R.W. Taft, *Progress in Physical Organic Chemistry*, Interscience, 1963, pp. 109-222.

teacher. I, of course, came as a student. I was exposed to group theory for the first time in a beautiful course by Laurens Jansen and learned quite a bit of quantum chemistry but not as much as I would have liked, Our two-year-old son, Gadi, had come down with dysentery, which he may well have contracted in Israel, so – for an hour or two of every day over a couple of weeks – I had to relieve Ada from her vigil at his bedside in the Carolinska Hospital in Stockholm.

From Stockholm we proceeded to Cornell University for my first sabbatical, the only one in which I spent a full academic year in one institution. My host Franklin A. Long sent me back to the laboratory bench for the last time in my career. Long was interested in solvent isotope effects, *i.e.*, determination of the mechanism of a reaction - in my specific case the deprotonation of phenylacetylene – by comparing the reaction rate in  $D_2O$  with that in  $H_2O$ .<sup>67</sup> He was also interested in a more general theoretical problem: Can any mechanistic information be derived from a series of experiments in H<sub>2</sub>O/ D<sub>2</sub>O mixtures of varying isotopic composition? The idea, which had been broached independently in the 1930's by Paul Gross and A.V. Butler, had recently been revived. The Gross-Butler equation, contains a cubic term in the denominator, derived from the presence of three protons in the hydronium ion  $(H_3O^+)$ , is purely statistical, and ignores the fact that the hydronium ion is hydrated and that H<sub>2</sub>O and D<sub>2</sub>O have significantly different solvent properties. We showed that, if these factors are taken into account, the experimental dependence on isotopic composition can be reproduced by assuming any arbitrary number of H-atoms in the hydrated hydronium ion on the one hand and in the water clusters on the other; we concluded that no useful mechanistic information can be derived from experiments in  $H_2O/D_2O$ mixtures.<sup>68</sup> Our paper was much ignored, prompting me to review the topic in greater detail a decade later.<sup>69</sup> To no avail; an inversion of the Gross-Butler treatment called proton inventory was adopted enthusiastically by physical biochemists and is still being used to estimate the number of protons present in the transition state of enzyme reactions.

 $<sup>^{67}</sup>$ . E.A. Halevi and F.A. Long, "The base-catalyzed exchange of phenylacetylene-t<sub>1</sub> in aqueous solution", *J. Amer. Chem. Soc.* **83**, 2809-2814 (1961).

<sup>&</sup>lt;sup>68</sup> E.A. Halevi, F.A. Long and M. Paul, "Acid-base equilibria in solvent mixtures of deuterium oxide and water", *J. Amer. Chem. Soc.* **83**, 305-311 (1961).

<sup>&</sup>lt;sup>69</sup> E.A. Halevi, "Solvent isotope effects: Second thoughts about an old problem", *Israel J. Chem.* **9**, 385-395 (1971).

A highlight in this period was transfer of the Faculty of Chemistry from Hadar to its present premises and the symposium held in September 1964 in celebration of the event. Four leading figures in various branches of chemistry: Robert B. Woodward and E. Bright Wilson from Harvard, Lord (Alexander R.) Todd from Cambridge, and Christopher K. (later Sir Christopher) Ingold. As taking care of Ingold on this and his three subsequent visits to Israel was my responsibility, our relationship gradually ripened from one between a teacher and his student to a genuine friendship.

In 1964 I was promoted to the ultimate academic rank, characterizes by Agnon as *professor gamur* (Finished Professor). My critical review of all of the work done to date on secondary isotope effects<sup>53</sup> was well received. I felt that I could now allow myself some time to pursue my non-chemical interests. I took up the violin again and, with friends, was given lessons in string-quartet playing by the violinist, violist and peace activist, Joseph Abileah,<sup>70</sup> who also introduced me to the viola. I soon felt confident enough to register on the international network of amateur chamber music players as "2<sup>nd</sup> Violin: Grade B; Viola: Grade C", and was thus able to find a group of amateurs of about my level of competence nearly anywhere I might find myself. Ada, who had given up her job at Zim after she could not obtain adequate care for our two year old daughter Dalia either at home or in a day nursery, renewed her childhood hobby of drawing.

It is a truism that good personal relations among scientists make for good scientific collaboration. Soon after his accession as Head of the Chemistry Department, David Ginsburg invited three young faculty members of the Organic Chemistry Department in ETH in Zurich, Jack Dunitz, Albert Eschenmoser and Edgar Heilbronner to visit the Technion. I persuaded him to add a fourth, Heinrich Zollinger, from the ETH's Department of Industrial Chemistry, who had been using isotope effects in his research on of the mechanism of diazo coupling. The four, later joined by a fifth – Duilio Arigoni, maintained lasting contact with our Department; all of them became my personal friends. My family and I spent the summer of 1964 in Zurich, where I helped Zollinger clear up a particularly thorny mechanistic problem.<sup>71</sup> Ada and I were once invited to dinner at the home of Jack and Barbara Dunitz; the other dinner guests

<sup>&</sup>lt;sup>70</sup> Father of our own Professor Efrat Lifshitz.

<sup>&</sup>lt;sup>71</sup> M. Christen, L. Funderburk, E.A. Halevi, G.E. Lewis and H. Zollinger, "Mechanism of the Suckfiill-Dittmer- synthesis of two azo compounds", *Helvet. Chim. Acta*, **49**, 1376-1349 (1966).

were John C. (Jack) and Ann Decius, with whom we immediately found a common language. I had already become interested in molecular symmetry, whereas Jack, Professor of Chemistry at Oregon State College, was co-author of the definitive book on molecular vibrations, in which molecular symmetry is a major factor. At this encounter I decided that I would like to take my next sabbatical in Oregon.

On our way to Oregon, we spent the summer of 1966 at the Brookhaven National Laboratory on Long Island, in New York State, where Max Wolfsberg and I carried out isotope effect calculations on solvolysis, which were eventually correlated with those performed in Jack Shiner's group.<sup>72</sup> Contact with my research group was maintained by mail, though I recall driving 150 miles from Brookhaven to Bayonne, New Jersey and back, in order to ask Mordecai Rubin - who was about to return to Haifa – to help one of my students overcome a difficulty that she had run into. We spent the first two "quarters" of the 1966-7 academic year at Oregon State University (OSU) in Corvallis with Jack Decius and the third at the University of Oregon (UO) in Eugene with Richard M. Noyes. It was a very pleasant year, during which my scientific activities were confined to teaching, learning, and maintaining contact with my students by mail. One non-scientific highlight was a 90 mile drive to Portland, to attend a Stravinsky concert in which the venerable composer conducted one of his works. At Corvallis I was flattered – but not tempted – by the offer of a professorial appointment under conditions substantially better than those I had at the Technion. It took me a few minutes longer to consider a split appointment, similar to positions that several Israeli professors of chemistry had accepted at American universities, but was daunted by the prospect of being away from the family for several months of each year. It was a good year for Ada as well. In OSU, she attended art classes and painted under the instruction of Nelson Sandgren, a professor in the Department of Fine Arts and a well known West Coast artist. On our move from Corvallis to Eugene, she continued her studies with Sandgren's own mentor, Andrew Vincent, who was approaching retirement as Professor of Art at UO.

At about 3 o'clock in the morning of June 6, 1967, someone knocked at the door of our home in Eugene. It was our neighbor, a U.S. Marine officer whom we knew as the father of one of Dalia's playmates. He apologized for disturbing us so late at night,

<sup>&</sup>lt;sup>72</sup> V.J. Shiner, M.W. Rapp, E.A. Halevi and M. Wolfsberg, "Solvolytic α-deuterium isotope effects for different leaving groups", *J. Amer. Chem. Soc.* **90**, 7171-7172 (1968).

but said, "I was sure you would want to know that there is a war on in your country". The local TV channels were all dead at this hour, but we managed to tune into an allnight news station in Los Angeles with our transistor radio. The announcer said that a correspondent in Tel Aviv had asked a senior Israeli officer how things were going and received the response, "Fantastically", but he didn't believe it. I did; in those distant days the news reports released by the IDF spokesman were generally reliable. A few hours later I phoned a friend in Boston who, in view of the time difference between the east and west coasts, had already ascertained that yes, the Egyptian air force had been destroyed on the ground and no, there is no transportation to Israel for anyone but military and medical personnel. Nevertheless, I informed my hosts that we would be leaving a few days early, packed our belongings on the roof of our ancient third- or fourth-hand Mercury, and started to drive east the following day. Before we reached Minneapolis it was clear that the war would be over within days and there was no need to hurry home, so we continued to the Argonne National Laboratory in Illinois, where I had a summer appointment. We arrived at Argonne safely after nearly losing a wheel of our car - and our lives - on the freeway somewhere in Wisconsin. On our arrival I found a letter from Heini Zollinger. Convinced that Israel was doomed, he was offering me a temporary position in his group with a salary sufficient to support my family until I could find permanent employment in Switzerland. I phoned to thank him for the generous offer that, as he already knew, was no longer relevant.

My host at Argonne was J.J. (Joe) Katz, an eminent inorganic chemist who had switched to biochemistry. He had devised an elegant technique for following enzymatic reactions, the *isotopic mirror* technique. Two experiments are carried out in parallel, one in  $D_2O$  with the normal substrate, the other in  $H_2O$  with the perdeuterated substrate, i.e., in which all of the hydrogen atoms have been replaced by deuterium. In the absence of isotope effects, the distribution of deuterium atoms in the product molecule in the first experiment will be the same as the distribution of protium atoms in the product of the second experiment; it this case it can be concluded that no bonds to hydrogen atoms had been made or broken in the rate determining step of the reaction. If the isotopic distribution in the product molecule is different in the two molecules, the difference may pinpoint the step/steps along the reaction path that is/are kinetically significant. In the course of the previous two years, Joe Katz, with his biochemist colleague Henry Crespi and Swiss post-doc Wolfgang Sauer, had been applying this method to an investigation of the fermentation of hexoses – specifically glucose and mannose – to ethanol, and had amassed a great deal of data on the distribution of hydrogen isotopes in the methyl and methylene groups of the product ethanol. We carried out the analysis of the data in two steps: 1. a steady state treatment based on the Emden-Meyerhoff glycolysis pathway, taking into account possible primary, secondary and solvent isotope effects on its various steps; 2. refinement of the results by computer, the latter being carried out largely by Dan Peterson, a bright teen-ager who was spending his summer at Argonne under an undergraduate research scholarship.<sup>73</sup>

This investigation turned out to be my last incursion into the life sciences, but it might not have been. The following year, in the course of a brief visit to Zurich, I paid my respects to Professor Vlado Prelog and found him in the company of a distinguished visitor, David Rittenberg. I sat listening respectfully as the two *Grand Old Men* complained half seriously to one another that neither of them had received – or would probably ever receive – the Nobel Prize.<sup>74</sup> Rittenberg then turned to me, told me that he was impressed by our fermentation papers, and asked me whether I would care to spend a few months in his group at the Columbia University's College of Physicians and Surgeons. Needless to say, I agreed enthusiastically and was disappointed when I never heard from him again. Several years later I met Rittenberg's erstwhile collaborator, Laura Ponticorvo, who assured me that he was preparing to send me a formal invitation when his health deteriorated and he died several months later.

#### **10. Technion – Part Two**

Before returning to my professional activities, let me depart from my chronological narrative and sum up the personal aspects of the next 37 years very briefly. At just

<sup>&</sup>lt;sup>73</sup> W.K. Saur, H.L. Crespi, E.A. Halevi and J.J. Katz, "Deuterium isotope effects in the fermentation of hexoses to ethanol by *Saccharomyces cerevisiae*, Part I: Hydrogen exchange in the glycolytic pathway", Biochemistry **7**, 3529-3536 (1968); W.K Saur, D.T. Peterson, H.L. Crespi, E.A. Halevi and J.J. Katz, "Deuterium isotope effects in the fermentation of hexoses to ethanol by *Saccharomyces cerevisiae*, Part II: A steady state kinetic analysis of the isotopic composition of the methyl group of ethanol in an isotopic mirror experiment", Biochemistry **7**, 3537-3546 (1968).

<sup>&</sup>lt;sup>74</sup> Rittenberg certainly deserved the prize for his pioneering work with Schönheimer on deuterium as a tracer of metabolites, and would no-doubt have received it had not Schönheimer committed suicide in 1941. Prelog was awarded the Nobel Prize in 1975.

about this time, the Gerard Swope Foundation set aside funds for low cost loans to members of the Technion faculty for home purchase. We took advantage of the opportunity and bought an apartment in a duplex in Danya, then still a comparatively inexpensive suburb, which was our home for the next 37 years. I continued playing chamber music whenever I could find partners and acquired a new hobby, Talmud. For over twenty years I had a weekly session with Professor Zvi Kurzweil, then Head of the Department of General Studies, until his death in 1992. Subsequently, I collaborated with Eliot Shimoff, Professor of Psychology at the University of Maryland (Baltimore Campus) in running a Talmud Study List in English on the Internet from 1998 until Shimoff's death in 2004.<sup>75</sup> Ada continued with her painting, had two solo exhibitions and several group shows. The children grew up, did their military service and went their own ways.

On my return from Argonne, I turned my attention to reinforcing my dwindling research group, but academic politics intervened. There was tension between the old, well established Engineering Faculties and the newer "upstart" Faculties of Science. In particular, the acquisition by the student-poor Faculty of Chemistry<sup>76</sup> of a large modern building, dubbed David's Palace, had aroused considerable resentment. More importantly, the policy of the Graduate School was heavily weighted in favor of applied research. As the Dean of the Graduate School was also the Deputy Vice President for Research, whose prerogative it was to dispense internal research funds, this had serious financial implications for us. I was asked by my colleagues to stand for election in the Senate against the incumbent Dean, and was elected. The President of the Technion promptly cut the Gordian Knot between the *elected* Dean of the Graduate School and the *appointed* Deputy Vice President for Research, leaving the latter function in the hands of the retiring Dean. Nevertheless, as of January 1969, I had three calendar years in which to adapt the academic policies of the Technion's Graduate School to the requirements of a modern research university. By the end of the two following three-year terms, which were held respectively by the physicist Asher Peres and Chemistry's Frank Herbstein, the Engineering Faculties had accepted the new standards and the ill-will between the Engineering and Science Faculties had

<sup>&</sup>lt;sup>75</sup> http://userpages.umbc.edu/~shimoff/tlmd\_grp.htm

<sup>&</sup>lt;sup>76</sup> We had attained Faculty status, but preferred to call ourselves a Department, headed by a Chairman rather than a Dean.

largely dissipated. As for me, once having been drawn into the Technion's administration, there was hardly a year in which I was not co-opted into one committee or another.

As temporary relief from my administrative duties, I agreed to accept the invitation of H.C. Brown<sup>77</sup> to Purdue University for the summer session of 1969. I earned my not insignificant salary by teaching undergraduate physical chemistry, but what Herb Brown required of me was a series of seminars on secondary isotope effects for his research group; I was happy to oblige, as it meant that I was being regarded as an authority on the subject. I began to feel, however, that – in view of my administrative duties in the Technion, the minuscule size of my research group, and the fact that work on secondary isotope effects was now being carried out in an increasing number of laboratories – my own contribution to the field was reaching the point of diminishing returns.

On completion of my term as Dean of the Graduate School, I took a partial sabbatical leave in the spring and summer of 1972 at University College London; it was a sad homecoming. Ingold, who had invited me during his tenure of a Visiting Professorship at the Technion in 1968, died in December 1970. Hughes, his immediate successor as Head of Department, had died in 1963. The invitation was renewed by the current Head, Sir Ronald Nyholm, who was Visiting Professor in our Department in the spring of 1971, but by the end of the year he too was dead, killed in an automobile accident. Encouraged by my old colleagues, Ridd and Maccoll, I came anyway, but the Department's day as the center of activity Physical Organic Chemistry was clearly over. The main beneficiary of my nostalgic return to London was Ada, who joined me with the children for the summer. She spent the two months studying with Sir William Coldstream, Head of the Slade School of Art at University College.

In 1972, IUPAC finally acceded to the repeated requests of the physical organic community to sponsor a series of biennial conferences on the subject. The first International Conference on Physical Organic Chemistry (ICPOC 1) took place in Crans sur Sierre, Switzerland in September, 1972. Zollinger and Heilbronner were co-chairmen and I was a member of the international organizing committee. My most vivid memory of this conference has nothing to do with science. I recall sitting for

<sup>&</sup>lt;sup>77</sup> Nobel Prize in Chemistry, 1979.

hours with Gunther Wilke in our hotel's television room on the night of September 5, hoping against hope that the Israeli athletes kidnapped by Palestinian terrorists at the Munich Olympic Games, would be rescued; they weren't.

Another unpleasant circumstance, less harrowing but annoying nonetheless, arose at ICPOC 1. All of the four scheduled speakers from the Soviet Union, including a plenarist, were prevented from coming, and there two non-scientists present who did not try to hide the fact that they were there to monitor the comings and goings of the attendees from the Soviet bloc. After the conference I wrote to E.L. Mackor, chairman of the organizing committee of ICPOC 2, scheduled to take place in Noordwijkerhout, Holland in the spring of 1974, expressing my displeasure at how the Soviet contingent was selected and suggesting that no lecturers should be invited from the Soviet Union unless the Soviet scientific establishment ensured that they would be allowed to attend. My letter raised a storm of protest from other Committee members: Paul Bartlett accused me of proposing a boycott of our Russian colleagues and injecting politics into science; Petr Zuman, a Czech who had immigrated to the United States, understood my position but argued that any action taken by IUPAC would do more harm than good. I was castigated by Ernst Bergmann and David Lavie, President of the Israel Chemical Society - who had been sent copies of Bartlett's letter but not mine – for provoking a counter-boycott of Israeli scientists. The storm subsided after I sent an explanatory letter to Prof L. Bénard, President of IUPAC, who responded: that in view of my explanation, "... je considère, par consèquent cette affaire come définitivement reglée". Two years later IUPAC approved the formation of a Commission on Physical Organic Chemistry, but my name was conspicuously absent from its list of members.<sup>78</sup>

The publication of Wodward and Hoffmann's "The Conservation of Orbital Symmetry", as a review article in Angewandte Chemie in 1969 and in book form the following year, was greeted with great enthusiasm by the international organic chemistry community. They had made it possible to consider the reaction pathways of cycloadditions and cyclizations, which had hitherto been regarded as "no mechanism" reactions and their predictions were beginning to be tested experimentally in several laboratories. At a conference in England early in 1972, I was approached by a German

<sup>&</sup>lt;sup>78</sup> Zollinger wrote that my name had been proposed but that I was voted down on the spot as a troublemaker. Nevertheless, I was eventually elected to the Commission in 1981 and served on it for ten years.

organic chemist, Ernst Koerner von Gustorf, from the Max Planck Institute für Strahlenchemie in Mülheim-Ruhr. He asked me to settle a dispute between him and an American colleague as to the interpretation of the conflicting isotope effects that each of them had observed in their respective studies of [2+2]-cycloaddition, a reaction for which Wodward and Hoffmann had predicted a concerted antarafacial pathway. The isotope effects indicated a stepwise pathway; the discrepancies between the effects observed in the different laboratories were presumably due to the timing of its various steps. I proposed a series of experiments that would settle the matter and, after a protracted correspondence, Koerner undertook to carry them out. I paid a twoday visit to Mülheim on September 15, 1975 en route to my third sabbatical in the United States. The results obtained so far were most encouraging but a few more experiments were required to make them conclusive. Tragically, on September 18, Koerner died of a heart attack at the age of 43 and our joint investigation was never completed. My ill-fated collaboration with Koerner was significant in two different ways: 1. It was the first of my many visits to Germany and initiated an extended series of scientific and personal contacts with German chemists, several of whom became my close friends.<sup>79</sup> 2. It convinced me that the Woodward-Hoffmann Rules, which were almost universally regarded as infallible, should be addressed more critically.

Whatever my reservations about the Rules, it was clear that our organic chemistry students would have to learn them; they would have take a course on orbital symmetry and I would have to teach it. While preparing the course, I realized that Woodward and Hoffman's treatise had not been cut out of whole cloth. Their suprafacial-antarafacial dichotomy makes use of orbital topology rather symmetry and their analysis based on HOMO-LUMO interactions did not seem conclusive, even if what they refer to as *secondary interactions* are included. The only aspect of their treatment that I found wholly satisfying was that based on orbital correlation diagrams. It struck me that if the orbitals were given their proper group theoretical labels – as Longuet-Higgins and Abrahamson had done in the communication that confirmed the procedure to be valid at the molecular orbital level of theory – it would be possible to go a step farther than merely characterizing a reaction as *allowed* or *forbidden*. A reaction that is *forbidden* because an occupied orbital of the reactant

<sup>&</sup>lt;sup>79</sup> I cannot refrain from mentioning two of them: Rolf Huisgen, who took Ada and me on numerous guided tours of Munich's art museums, and Gerhard Quinkert, who insisted that I give a departmental colloquium on "The Talmud and its logic" at Frankfurt.

does not correlate with an occupied orbital of the reactant may become *allowed* along a reaction path of lower symmetry, in which all of the occupied orbitals of the reactant and product are in correlation.

I formulated a procedure for determining the symmetry of the reaction coordinate that would direct the system along the appropriately desymmetrized pathway, named it Orbital Correspondence Analysis in Maximum Symmetry (OCAMS),<sup>80</sup> wrote it up in detail and naïvely submitted it to Angewandete Chemie, but not before sending copies off to Woodward and Hoffman for comment. Woodward did not answer, Hoffman replied that we should discuss the matter at ICPOC 2, and Angewandete replied that they cannot publish it as a primary article, but would be happy to publish a review of the subject after the primary paper had been published elsewhere. Thereupon, I trisected the article and submitted three Communications to the Editor of JACS, who rejected them forthwith. One referee (Woodward?) wrote briefly that there was nothing new in any of them; the other (Hoffman?) analyzed all three at length, arguing that – while the idea behind it was interesting – the method was flawed by fundamental errors of theory. At Noordwijkerhout, Hoffmann avoided me until the last day of the conference, when he approached and said the he believes we had something to discuss. When I reminded him of the draft that I had sent him, he replied that many approaches to the topic have been suggested and that the viable ones will no-doubt be sorted out in the literature – or words to that effect. By then, however, I had had a long discussion with Edgar Heilbronner, in which I convinced him that the "errors" raised by the second referee were more apparent than real. Though he agreed that OCAMS is theoretically sound, Edgar doubted whether it would ever be accepted by an American journal, and suggested that I should join the Swiss Chemical Society and submit it to *Helvetica Chimica Acta*. I took his advice and published three papers in rapid succession: the primary paper in Helvetica, a formal presentation of the method and its conceptual basis - for which collaboration with Jacob Katriel was indispensable, and the discursive review that Angewandete Chemie had promised to publish.<sup>81</sup> Let me add parenthetically that though I often served as a referee for *JACS* 

<sup>&</sup>lt;sup>80</sup> He reader will appreciate my modesty. I refrained from stressing the razor sharpness of my method by naming it *Orbital Correspondence and Coordinate Analysis in Maximum Symmetry (OCCAMS)*!

<sup>&</sup>lt;sup>81</sup> E.A. Halevi, "Orbital correspondence analysis in maximum symmetry", *Helvet. Chim. Acta* **58**, 2136-2151 (1975); J. Katriel and E.A. Halevi, "Orbital correspondence analysis in

over the years, I never again submitted a paper without an American co-author to that journal.

Of the molecules that I had been using as examples in the application of OCAMS, the one that intrigued me most was the simplest, carbene (CH<sub>2</sub>), the two non-bonding electrons of which can be either unpaired in the triplet ground state or paired in the lowest excited singlet state. I had just participated, with the Chemistry Department's genuine theoreticians, in a computational study of the process by which triplet carbene was produced directly by thermal decomposition of diazomethane (CH<sub>2</sub>N<sub>2</sub>), which has a singlet ground state.<sup>82</sup> In substituted carbenes, notably diphenylcarbene, the singlet excited state and triplet ground state were much closer, and I wondered whether suitable substitution in the phenyl rings and/or bridging between them could reverse the sequence. In a final attempt to revive my moribund experimental program, John Metcalfe, a post-doctoral fellow with experience in photochemistry, and I tried to build a stop-flow pulsed laser system to study the formation and properties of diphenylcarbene and its derivatives. Unfortunately, I had bitten off more than the two of us could chew. We had many problems with the electronic equipment and, when the instrument was ready after months of work, it failed to perform reliably. We finally gave up and settled for a computational study, in which we identified the geometric changes required to bring the singlet below the triplet; our prediction was confirmed by Staab several years later.<sup>83</sup>

Collaboration with Carl Trindle, which began while he was spending several months at the Technion, soon led to the extension of *OCAMS* to spin-forbidden processes.<sup>84</sup>

maximum symmetry: formulation and conceptual framework", *Theoret. Chim. Acta* **40**, 1-17 (1975); E.A. Halevi, "Orbital correspondence analysis in maximum symmetry", *Angew. Chem. Internat. Edn. (English)* **15**, 593-607 (1976); German version: *Angew. Chem.* **88**, 664-679 (1976).

<sup>&</sup>lt;sup>82</sup> E.A. Halevi, R. Pauncz, 1. Schek and H. Weinstein, "Multiplicity change during thermal dissociation", *Jerusalem Symposia on Quantum Chemistry and Biology*, 167-182 (1974).

<sup>&</sup>lt;sup>83</sup> J. Metcalfe and E.A. Halevi, "Diphenylcarbene: INDO calculations on several geometrical conformations", *J. Chem.Soc. Perkin II* **1977**, 634-639; R. Alt, R., H.A. Staab, H.P. Reisenauer, and . G. Maier, "Diarylcarbenes of unusual steric and electronic structure", Alt, R.; Staab, H. A.; Reisenauer, H. P.; Maier, G. Tetrahedron Letters, **25**(6), 633-6 (1984).

<sup>&</sup>lt;sup>84</sup>. E.A. Halevi and C. Trindle, "Application of orbital correspondence analysis in maximum symmetry to spin-forbidden processes", *Israel J.* Chem. **16**, 283-290 (1977); E.A. Halevi, "Qualitative quantum chemistry as a guide to the construction of potential energy surfaces", *Internat. J. Quantum Chem.* **12** (S1), 289-298 (1977); C. Trindle and E.A. Halevi, "Spin-forbidden pathways in the interaction of singlet and triplet molecular oxygen with acetylene", *Internat. J. Quantum Chem. Quantum Biol. (Symp. No. 5)*, 281-290 (1978); C.D. Duncan,

Here the "allowing" element is not a geometric displacement, but the component of the spin-orbit operator that has the right symmetry to bring a singly-occupied orbital in the reactant into correspondence with a doubly occupied orbital in the product – or *vice versa*. It was now possible to deal within the same formalism not only with thermal reactions between closed-shell reactants and products, but with photochemical reactions of open-shell species and with reactions – thermal and photochemical – in which electron spin is not conserved. It seemed to me that the time had come for me to summarize my ideas on the relation between orbital symmetry and reaction mechanism in book form, so I wrote the first four chapters and sent them to three American publishers for appraisal. The response in all three cases was identical: initial enthusiasm, replaced by disapproval as soon as the reviewer's reports came in: I was too disrespectful of the Woodward-Hoffman canon! I then sent the chapters to two European publishers. Again, an initially favorable response was followed by rejection, so I gave up; there was no point to writing a book that cannot be published.

From here on and until my retirement I accepted no more graduate students. All of my subsequent research was theoretical: either qualitative or computational, and – with the exception of one undergraduate summer student<sup>85</sup> – all of my collaborators were senior academics. There was thus nothing to limit my travel aside from my teaching duties. My membership in the IUPAC Commission on Physical Organic Chemistry<sup>65</sup> took me to a different venue every two years. I attended numerous conferences in the United States, Europe and Japan. Except for one full semester at the University of California at Irvine in 1975-6<sup>86</sup>, I split my remaining sabbaticals into one and two-month segments: the first (1980-1) in Germany (U. of Munich; U. of Frankfurt; U. of Heidelberg; M.P.I., Mülheim) and the second (1986-7) spread more widely (U. of Graz; U. of Paris (Orsay); Clarkson U., Potsdam, N.Y; Cal. Tech.).

My proposal at the quantum chemistry symposium celebrating the 500<sup>th</sup> anniversary of Uppsala University that *OCAMS* could serve as a guide in computational

E.A. Halevi, and C. Trindle, "Rates of spin-forbidden organic reactions: 3. Extrusion of nitrogen from methylenepyrazoline", *J. Amer. Chem. Soc.* **101**, 2269-2275 (1979).

<sup>&</sup>lt;sup>85</sup>. E.A. Halevi and R. Rom, "The stereochemistry of the Cope rearrangement: Qualitative theory (*OCAMS*) and computation (AM1)", *Israel J. Chem.* **29** (2-3), 311-20, (1989).

<sup>&</sup>lt;sup>86</sup> A highlight was the first – perhaps only – performance of classical music at the Chemistry Department's Christmas party: Marlene (wife of physical chemist Ed) Lee – piano, organic chemist Hal Moore – clarinet, and I – viola played Mozart's Kegelstadt Trio.

chemistry,<sup>87</sup> was followed up by several studies in collaboration with theoretical chemists using a variety of computational methods.<sup>88</sup> The qualitative features of *OCAMS* were extended in collaboration with the theoretical chemists at the Max Planck Institute in Mülheim.<sup>89</sup> Brief incursions were made into both transition metal and main group inorganic chemistry.<sup>90</sup> Lunch with John Pople produced a mnemonic algorithm for determining the number of independent coordinates for any given molecule<sup>91</sup> and my participation in an IUPAC committee charged with the production of a Glossary of Stereochemical Terminology prompted me to suggest a modification of Kurt Mislow's hierarchy of levels of prochirality.<sup>92</sup>

In 1984 I was called upon to become Chairman/Dean of the Department/Faculty of Chemistry, a job that I had successfully avoided until then. I agreed to accept it for a single two-year term; it was difficult enough. The student body had dropped drastically and funds were low. Pressure from the Technion administration to reduce

<sup>&</sup>lt;sup>87</sup> E.A. Halevi, "Qualitative quantum chemistry as a guide to the construction of potential energy surfaces", *Internat. J. Quantum Chem.* **12** (S1), 289-298 (1977).

<sup>&</sup>lt;sup>88</sup> E.A. Halevi, F.A. Matsen and T.L. Welsher, "On the fluxional isomerization of cyclobutadiene, *J.Amer. Chem. Soc.* **98**, 7088-7090 (1976); E.A. Halevi, J. Katriel, R. Pauncz, F.A. Matsen and T.L. Welsher, "Sigma participation in electrocyclic reactions - a consequence of symmetry", *J. Amer. Chem. Soc.* **100**, 359-365 (1978); R. Schatzberger, E.A. Halevi and N. Moiseyev, "SCF study of mode selectivity in the unimolecular dissociation of formaldehyde", *J. Phys. Chem.* **89**, 4691-5 (1985); R. Janoschek, A. Sax and E.A. Halevi, "The application of the method of pseudopotentials to hydrides of silicon and their methyl analogs", *Israel J. Chem.* **23**, 58-65 (1983); E.A. Halevi, G. Winkelhofer, M. Meisl and R. Janoschek, "Electronic transitions of polysilanes and their photochemistry", *J. Organomet. Chem.* **294**, 151-61 (1985); E.A. Halevi , and W. Thiel, "The reactive excited state of naphthvalene and its photochemistry: a qualitative and quantitative theoretical investigation", *J. Photochem.* **28**, 373-381 (1985).

<sup>&</sup>lt;sup>89</sup> V. Bachler and E.A. Halevi, "A simple perturbational approach for the determination of favorable reaction pathways", *Theoret. Chim. Acta* **63**, 83-101 (1983); V. Bachler, E.A. Halevi and O.E. Polansky, "A qualitative determination of the favorable nuclear pathway for the ground state decomposition of formyl fluoride", *Theoret. Chim. Acta* **65**, 81-89 (1984).

<sup>&</sup>lt;sup>90</sup> E.A. Halevi and R. Knorr, "Orbital symmetry analysis of intersystem crossing during thermal isomerization of tetrahedral to planar nickel (II) complexes", *Angew. Chem. Suppl.* **1982**, 622-634. (1982); E.A. Halevi, and R. West, "Orbital symmetry analysis of the reaction of silylenes with acetylenes and the dimerization of i-silacyclopropenes", *J. Organomet. Chem.* **240**, 129-141 (1982); E.A. Halevi, H. Bock and B. Roth, "On the non-observability of cubic P<sub>8</sub>", *Inorganic Chemistry* **23**, 4376-4378 (1984).

<sup>&</sup>lt;sup>91</sup> J.A. Pople, Y.A. Sataty and E.A. Halevi, "The number of independent coordinates in symmetrical molecules", *Israel J.* Chem. **19**, 290-291 (1977). Dr. Aviva Sataty, a member of the Department's administrative staff, was my part time collaborator. She lost her life in a traffic accident in 1991.

<sup>&</sup>lt;sup>92</sup> E.A. Halevi, "The level of prochirality: The analogy between substitutional and distortional desymmetrization", *J. Chem. Research* **5**, 206-207, (1985).

the size of our technical staff and to give up space in our building to other academic units had to be fought off. Moreover, an international review committee, consisting of Professors R.A. Raphael (Cambridge U.), H.B. Gray (Cal. Tech.) and B.S. Rabinovich (U. of Washington) had been invited to evaluate our Department; under the circumstances, their report was surprisingly favorable. If I survived my term as Chairman in reasonable shape, it was due in great measure to the support of the Department's indefatigable Administrative Assistant, Kochava Reznik.

In 1989, a year before my retirement, I received a letter from Dr. Rainer Stumpe, Chemistry Editor of Springer Verlag, asking whether I was still interested in their publishing my book. It seems that it had not been rejected because it treated Woodward-Hoffman dogma critically, but because the reviewers had convinced the previous editor, Dr. Ebel, that it was too sophisticated for organic chemists and would not sell, whereas Dr. Stumpe believed that this was no longer true – if it ever was. I rewrote the first four chapters and spent the next year bringing the book to completion. When I agreed to accept the Chairmanship of the Department, I extracted the concession that whatever sabbatical leave was still due me could be taken after my retirement. Therefore, most of the writing was done abroad in the following two years, in Heidelberg, Vancouver, Madison and Irvine. After the manuscript was vetted by Jacob Katriel for theoretical accuracy, my book, dedicated to the memory of my mentor Christopher Ingold and my friend David Ginsburg, finally appeared in print.<sup>93</sup> It was quite well received, but would have had a greater impact had I been able to publish it a decade earlier.

During my second stay at Irvine, Max Wolsfsberg and I produced a paper combining the three areas that had occupied me throughout my career: secondary isotope effects, orbital symmetry and computational chemistry.<sup>94</sup> An invitation to present the results of this study at the 1991 Gordon Conference on Isotopes, 32 years after my first invited lecture at the 1959 Gordon Conference, rounded out my career as an isotope chemist nicely.

Of my many non-chemical duties during my career at the Technion, one – performed just before my retirement – is worth reporting. Three Technion professors; Joseph

<sup>&</sup>lt;sup>93</sup>E.A. Halevi, Orbital Symmetry and Mechanism, Springer Verlag, Heidelberg, 1992.

<sup>&</sup>lt;sup>94</sup> E.A. Halevi and M. Wolfsberg, "Cycloaddition of acrylonitrile to allene: Computed reaction path (AM1) and intramolecular isotope effect", *J. Chem. Soc. Perkin Trans.* 2 **1993**, 1493-1496.

Hagin – Agricultural Engineering, Ehud Lenz – Mechanical Engineering, and I, were sent to Brazil in order to advise Gilberto Mestrinho, the recently elected Governor of the State of Amazonas, on the establishment of an Institute of Technology in the capital, Manaus. We studied the situation for a week, during which we were treated like royalty, after which, speaking for the delegation, I extended the Technion's greetings to the Governor orally (in Portuguese), presented him with an album of the Technion and several bottles of Israeli wine, and promised to send him a detailed report (in English) soon after our return to Haifa. Our recommendation was to go slow: shore up the science departments of the woefully under funded and under equipped University of Amazonas and gradually add engineering departments under the guidance of an international advisory committee, which we undertook to help organize. Meanwhile, however, the Governor and his retinue had been invited to M.I.T., where he received a counter-proposal: to set up an Institute of Biotechnology under M.I.T. supervision, a project that - in our opinion - was doomed to failure for lack of an adequate scientific and technical infrastructure. We were thanked profusely for our efforts and informed that, while the Governor was tending toward the M.I.T. proposal, he would welcome one of us to return and argue our case; we declined. It appears that we had not taken Brazilian politics into account. Mestrinho had been elected for a four year term, after which he suspected – correctly, as it turned out – that he would be succeeded by his predecessor and rival, Amazonino Mendes. If he "went slow", as we suggested, Mendes – not he – would get the credit, so he opted for the M.I.T. project which promised to yield more immediate returns. Three years later, I was invited to speak at a conference in Florianopolis,<sup>95</sup> after which Ada and I took a tour of Brazil, including a visit to Manaus. I was not surprised to learn that the M.I.T-sponsored Institute of Biotechnology never got off the ground.

For quite a few years after my retirement, I continued to attend conferences on isotope effects and reaction mechanism in the capacity of *tribal elder*; once, at an ESOR symposium in Dubrovnik, I presented an "*OCAMS cum* computational" study that I had run on my PC.<sup>96</sup> Though I am neither a technologist nor an ethicist, but do know something about Judaism, I took an active role in a conference on "Technology and

<sup>&</sup>lt;sup>95</sup> E.A. Halevi, "[2+2]-Cycloaddition: symmetry, concertedness and secondary isotope effects", *Atualidades de Fisico-quimica Organica*, 1995, 215-232.

<sup>&</sup>lt;sup>96</sup> E.A. Halevi, "Formation and isomerization of C<sub>120</sub>: Considerations of symmetry, kinetics and thermodynamics", *J. Phys. Org. Chem.* 15, 519-523 (2002).

Ethics" held at the Technion in collaboration with the University of Judaism (now the American Jewish University in Los Angeles) and accepted the task of co-editing its Proceedings.<sup>97</sup> To keep occupied, I worked for several years as English copy editor for Springer Verlag. In 2007 Ada and I moved to a retirement home in Regba. As of this writing (April 2008), the Technion is still within reach; there are books and the Internet; the violin and viola have been largely replaced by the CD player and an occasional concert. I have no complaints.

<sup>&</sup>lt;sup>97</sup> "Technology and Ethics", E.A. Halevi and D. Kohn *editors*, S. Neaman Institute Press, Haifa, 1993.

# Frank H. Herbstein

Born Cape Town, South Africa, 1926



Ph. D. Hebrew University, Jerusalem, 1956.

#### **Post-doctoral position:**

Department of Metallurgy, Massachusetts Institute of Technology, Cambridge, Mass. USA 1952-55

## **Previous Positions**:

Weizmann Institute of Science, X-ray Crystallography Laboratory, 1948-1952 Head of Chemical Physics Group, South African Council for Scientific and Industrial Research, 1955-65

#### **Technion Positions:**

Professor, 1965-94 (Bertha Axel Professor, 1989–1994); Emeritus, 1994. Chairman, Department of Chemistry, 1 January, 1968 to 31 December, 1971. Elected Member, Senate Steering Committee, 1974. Dean of the Graduate School, 1 January, 1975 to 31 December, 1977. Vice-president for Development, (Acting 1 July, 1984 to 30 September, 1984); 1 October, 1984 to 30 November, 1986.

## Sabbatical leaves:

*California Institute of Technology*, Noyes Laboratory of Chemical Physics, 1972–3, 1980–1.

Northwestern University, Department of Chemistry, Visiting Professor, January-March, 1982.

*University of Cambridge*, England (Churchill College), By-fellow, 1 October, 1987 to 31 December, 1987; 1 March to 31 August, 1994.

Royal Institution, London. 1 January, 1988 to 30 September, 1988.

[Israel Academy of Sciences - Royal Society of London Visiting Professor in Britain during 1987–8].

University of the Witwatersrand, Johannesburg. South African Foundation for Research Development Visiting Professor. 1 September 1991 to 31 January, 1992.

*University of Cape Town.* South African Foundation for Research Development Visiting Professor. 1 March to 30 June, 1992.

## Major National/International responsibilities:

Israel Council for Higher Education, Member, 1982–4.

Chairman, Israel Crystallographic Association, 1970–1, 1989–1991.

*International Union of Crystallography*, Member, Apparatus Commisssion, 1963–72 (3 terms); Member, Small Molecules Commission, 1987–1996, Chairman, 1993–1996).

*Member of Editorial Boards*: Inorganica Chimica Reviews, 1968–74; Journal of Crystal and Molecular Structure, 1972–80; Israel Journal of Chemistry, 1972–6; Journal of Inclusion Phenomena, 1981–1992; Crystallography Reviews, 1986–; Acta Crystallographica, Co-Editor, Sections A, B and C October, 1993–February, 2000.

## **Royal Society of South Africa**:

Foreign Associate 19 February, 1992.

## Fankuchen Award of American Crystallographic Association, 2007.

#### **Consulting in Patent Cases:**

1964 (Pretoria): consulted for De Beers in the patent infringement concerning diamond synthesis alleged by the General Electric Corporation.

1976–7 (Tel Aviv): consulted for Beechams in the infringement alleged by Bristol-Myers of Ampicillin patents.

2004 (London): consulted for Apotex in various litigations concerning allegations by SmithKline Beechams that their paroxetine hydrochloride patents were being infringed.

## **Major Fields of Research**

## Chemical Crystallography and Chemistry of the Solid State.

Crystal structures of molecular complexes and compounds, coordination complexes, polyiodides and other low-dimensional systems; twinning in crystals; physical and chemical reactions in crystals, including phase transformations and thermal decompositions; intramolecular hydrogen transfer in the solid state; structures of overcrowded and strained molecules; errors in crystallographic publications.

## **Publications:**

About 170 publications in the fields of crystallography, structural and solid state chemistry, including the book "Crystalline Molecular Complexes and Compounds" two volumes, 1300 pages, 600 diagrams, 200 tables, 3000 references, published by Oxford University Press–International Union of Crystallography at the end of 2005.

# Frank H. Herbstein

(March 2008)

## WANDERINGS IN THE FIELDS OF CHEMICAL CRYSTALLOGRAPHY

I was born in Cape Town in the Union of South Africa (as it then was) on 3 July, 1926. I went to school in Muizenberg, a suburb of Cape Town, and then to the University of Cape Town, came to Israel in July, 1948, went to MIT on a post-doc. in1953, returned to South Africa in 1956, and then returned (permanently) to Israel in 1965 as a Professor of Chemistry at Technion (emeritus beginning October 1994 but I have continued in research). In 1992 I was honoured by election as a Foreign Associate of the Royal Society of South Africa. In 2007 I was honoured by being chosen to receive the Fankuchen Award of the American Crystallographic Association (ACA Reflexions, Spring 2007, 16–17). A much extended version of my talk in Salt Lake City is about to be published in *Crystallography Reviews* (March, 2008) under the revised title "Classification of closed shell TCNQ salts into structural families and comparison of diffraction and spectroscopic methods of assigning charge states to TCNQ moieties".



Figure 1. Frank Herbstein at age 80.

But first some family background. My father was at the time of my birth a struggling barrister at the Cape Town Bar, my mother was the oldest daughter of the Policansky family, prominent in business and Jewish communal affairs in Cape Town. My paternal grandfather Moritz Herbstein had left Romania at age 16, presumably to escape army service and had made his way to Rishon-LeZion, where he worked in agriculture and contracted blackwater fever. From Palestine of the 1880s he moved to Abyssinia at the time of the first Italian conquest, and then further southwards to (what was then) German East Africa. Family lore has it that he made and lost a number of fortunes en route. No record remains of these adventures. The fortunes must have been small as he certainly was not wealthy when he arrived in Port Elizabeth in the eastern Cape Province about 1890. He married a Kurlander girl and moved to the small country town of Graaff Reinet, where he set up an import business and established a family of four daughters (one died young) and two sons. Later he transferred business and family to Cape Town where he died in 1935. Despite the unhappy end to his stay in Palestine, Moritz Herbstein remained a devoted Zionist and one of his proudest possessions was a letter from Herzl confirming his appointment as Chairman of the Graaff Reinet Zionist Society.

My maternal grandfather left Latvia at about the same time and, after a period in Brazil, settled in Cape Town, where he started manufacturing cigarettes by hand (he had no machinery) and soon was able to send for his fiancée from Latvia. He manufactured cigarettes and she peddled them. They prospered enough to move from one substantial dwelling to the next, with first three daughters and then three sons arriving in quick succession. Philip and Rachel Policansky were well known not to turn away any supplicant for help.

My parents moved to Muizenberg at the time I started school (1933). Muizenberg was a suburb of Cape Town some twenty kilometers south of the centre of the city, predominantly Jewish and known for its magnificent beach. Despite service by a rather efficient railway, youngsters in Muizenberg were somewhat isolated and we had to build our own community lives. I had my bar-mitzvah in July, 1939 under the shadow of the impending war. Although Jewish families in Muizenberg were mainly secular, the synagogue, the Hebrew school (after regular school hours), the Jewish Boy Scouts and the Young Israel Society were at the centre of our adolescent lives. The World War, with its unfolding of the terrible events in Eastern Europe, was far away for my generation, but not for the previous, immigrant, generation. However, U-boat sinkings around the Cape and the see-saw battles in the Western Desert, where most of the South African forces were fighting, brought constant reminders of reality.

After graduating from high school at fifteen and a half I spent a year in various jobs and then (1943) enrolled as a student at the University of Cape Town (UCT) for 18 months of physics, chemistry and mathematics. When I turned 18 I volunteered for a highly secret, highly technical unit that turned out to be air force radar; this was done with my parents' permission (although this was not needed). There was no conscription in South Africa for internal political reasons but there was a large volunteer army, partly because of the British connection and partly for economic reasons. My school mates, generally twelve to eighteen months older than I, were mostly 'up north'.

After basic training I was attached as a radar mechanic to the South African Air Force with a squadron due to participate in rolling up the Japanese empire. Hiroshima and Nagasaki put an end to these plans and I returned to UCT in late 1945 convinced that "physics was the future". RW James FRS ("The Optical Principles of the Diffraction of X-rays") was Head of Department and among my contemporaries were two future Nobel Prize Winners—Sir Aaron Klug P-PRS and Alan Cormack. By the end of 1947 I had presented a master's thesis at UCT describing my unsuccessful attempts to solve the crystal structures of *o*-dinitrobenzene and a picric acid complex of an outlandish aromatic hydrocarbon whose name I have forgotten. But the die was cast— *o*-dinitrobenzene succumbed to my colleague Moshe Kapon in 1990 and I have (with others) solved the structures of many charge transfer (CT) compounds of aromatic hydrocarbons.

I remained at UCT for the first few months of 1948 but the eyes of many of my generation of young Jews were turned northwards to the developments in Palestine. South African Jewry, perhaps 100000 in all, was very well organised and sent altogether some 800 volunteers to participate in Israel's War of Independence; their contributions to the Israeli Air Force and the medical services were especially noteworthy. In Cape Town Aaron Klug and I decided that our contribution could be to summarise potential military uses of infra-red (IR) radiation. Our literature search was done with the help of the Library Services of the newly-established South African Council for Scientific and Industrial Research (CSIR), using the excuse of planning new research directions. We delivered our memorandum to Yoel Palgi, the Haganah emissary to South Africa (and one of the parachutists into German-occupied Hungary in 1944). Rather to my surprise, I found, after arriving in Israel later in 1948, that the memorandum had indeed been read and I became myself involved in some applications of IR. The high-priority experienced military experts started to move north early in 1948. My turn came in July 1948. There were about forty young men in the aircraft flying from Johannesburg to Rome. We had strict instructions to ignore one another and these held water for a few hours at best. Those were the days when one flew only in daylight, overnighting in often plush hotels. The stops I especially remember were Entebbe, Khartoum and Wadi Halfa. In Rome we were placed at the Boston Hotel, besieged by small boys crying Sholem Aleichem and peddling anything from P.Arker pens to their sisters. We flew from Rome to Haifa by Alaska Airways, with an emergency stop for repairs in Nicosia. Although this was during the First Truce when entry of military-age young men was embargoed, there were no problems from UN officials or the like; evidently suitable palms had been well greased. On arrival in Tel Aviv I was offered a choice between the Air Force radar unit, where I had many South African friends, and the Scientific Unit of the Army (Hemed). I chose the latter and was sent to the still-unfinished Weizmann Institute, where I made many new friends. Here I dabbled in electronics and, more usefully, assisted (Dr, later Professor) Ernst Alexander in setting up an X-ray generator. Much of my time at Hemed was spent on developing infra-red tools for night vision (echoes of earlier remarks) and I even went, more as a spectator than as a participant, on several military operations (Figure 2). When the War died down in early 1949 I stayed on as one of Gerhard Schmidt's first graduate students. As a schoolboy, Schmidt had been a refugee from Nazi Germany, and now was an Oxford trained organic chemist with interests in biological crystallography, a student of Dorothy Hodgkin and the coauthor (with Dorothy) of just one publication.



**Figure 2**. Hemed days—the author demonstrating a snooperscope built in the Hemed laboratories at the Weizmann Institute in 1948.

He became well known as a pioneer of 'crystal engineering', his burgeoning career cut short by his premature death. I was too early to participate in Schmidt's photochemical studies—I worked on overcrowded aromatic hydrocarbons and the three-dimensional structure of phenazine (Fig. 2).



**Figure 3.** The electron density in the plane of the phenazine molecule ("anthracene with the CHs in the 9, 10 positions replaced by nitrogen"). This is taken from a paper by Schmidt and myself in *Acta Crystallographica* in 1955. The actual calculation was part of my Ph. D. thesis and took some three months, using an electric adding machine; the contours were drawn by hand. Today (2008) such a calculation would take less than a few seconds and no human intervention would be needed.

I was at that time, by my own assessment, the third best crystallographer in Israel; top of the list were Alexander and Schmidt, diplomatically grouped together, I came next—there were no others. After completing my Ph. D. in Rehovoth, I moved to MIT in the early 1950s; B.E. Warren was then the dominating influence in physical crystallography and I learned there that diffraction space extended between and far beyond the Bragg spots. This rather catholic education was later amplified by sabbaticals at Caltech and Northwestern in the United States, Cambridge and the Royal Institution in the UK and in South Africa at UCT and the University of the Witwatersrand.

After MIT I returned to South Africa in 1956 to join the staff of the South African Council for Scientific and Industrial Research (CSIR), an ambitious attempt to combine high-quality academic and applied research. I enjoyed my nine years in Pretoria and published structures of some overcrowded halogenated aromatics and charge transfer (CT) molecular compounds. The great goal of South African research in those days should have been directed towards understanding the mechanism of the strategically important Fischer-Tropsch process for converting low grade coal to petroleum products. At the CSIR our group made some contributions but it was too large a project for our limited resources. I was fortunate enough to have left before the traumatic breakup of the CSIR, in the name of budgetary efficiency, got underway—how difficult to build, how easy to destroy.

A new chapter in my life started when, in 1965, I was invited to add an X-ray diffraction facility to the Department of Chemistry at Technion in Haifa. David Ginsburg was then the moving force at the resuscitated Technion; we had overlapped in Hemed in Rehovoth in 1948 but were not particularly close; however, he was a great admirer of Schmidt and this may have tipped the balance. We became good friends and even published together. To my previous interests I added much more sophisticated approaches to topics that I had touched on earlier. Phase transitions was one of these. Second order transitions were the first to be developed; the simplest (a dangerous word) of these was the second order transition in naphthazarin C, studied in collaboration with Curtin and Paul in Urbana and Mogens Lehmann in Grenoble, which led on to a study of the *cis*-enol system benzoylacetone over a temperature range of 9 to 300 K with Finn Larsen's group in Århus. Just before leaving South

Africa I had worked on a second order phase transition at  $\approx 160$  K in the CT compound pyrene : pyromellitic dianhydride and this work was greatly extended, including a structure determination at 19 K, during a sabbatical at Caltech with R. E. Marsh and Sten Samson. This led on to a review of second order transitions published in *Crystallography Reviews*, (Vol. 5, 181–226 (2000)). The first order transitions in N-anilinophthalimide and N-(N'-methylanilino)phthalimide, first studied in the early years of the 20<sup>th</sup> Century, then attracted our attention; we were able to confirm the earlier results and relate them to structural and thermodynamic properties of the system (*Acta Cryst.* B54, 277–290 (1998)). A comprehensive view of enantiotropic first order transformations is given in the first part of Yuri Mnyuk's book "Fundamentals of Solid State Phase Transitions, Ferromagnetism and Ferroelectricity", and we were able to put some of the phase transition material in a broader and more up-to-date context ("On the mechanism of first-order enantiotropic solid state transitions: from Simon through Ubbelohde to Mnyukh").

Two other consequences of my sabbaticals at Caltech must be mentioned. Following the pioneering studies of Marsh and Duchamp, Marsh and I (and others) were able to put much of the rather intricate crystal chemistry of trimesic acid on a sound basis. And Marsh taught me how to recognize potential space group errors and how to correct them with a smile. Jointly and severally this has led to re-assignment of correct space groups to more than 100 crystals and, indeed, to a recognition that there are errors in the literature not involving space groups and these too need attention.

What is my favorite among the variegated research projects with which I have been associated over the years? Undoubtedly "Spontaneous deformation of protocatechuic acid (3,4-dihydroxybenzoic acid) monohydrate (PCA.H<sub>2</sub>O) crystals: crystallographic aspects" (Agmon & Herbstein, *Proc. Roy. Soc. Lond.* (A**387**, 311– 336 (1983)), despite its being one of the least cited papers in the crystallographic literature. The unusual crystallization behaviour of PCA.H<sub>2</sub>O was first reported in 1890 (and again in 1905) by Otto Lehmann (a pioneer of liquid crystals), followed by a paper in the *Proceedings of the Royal Society of London* (A**197**, 283–294 (1949) by Robert W. Wood (Professor of Experimental Physics in the Johns Hopkins University, author of "Physical Optics" (first published in 1911)). This was Wood's last publication, at age 81, and my guess is that it was in response to his election as a Foreign Member of the Royal Society. Neither Lehmann nor Wood realized that they were dealing with the monohydrate. Ilana Agmon and I were able to confirm their observations and put them into a rational crystallographic framework. And what are my favorite crystals? Here trimesic acid and the various polyiodides (and polyiodines) run neck and neck.

I also produced, in addition to a couple of book reviews, an attempt to assess the value of citation counts in judging scientific standing and an attempt to predict the future of South African universities after removal of racial barriers to student (and staff) intake. My negative view of the reliability of citations counts has not had any effect on their current wide use; the changes in the universities have been even more drastic than I had anticipated.

Producing some autobiographical notes provides a good opportunity to survey what one has done over the years (here almost sixty) and check whether the results are haphazard or fit into some sort of framework. Thus I have taken my 170-odd publications and grouped them into about a dozen themes, each being handled chronologically.

Group I: Structures of Metals and Alloys
Group II: Thermal Vibrations in Solids
Group III: General Crystallography - Methods and Results
Group IV: Twinning in Crystals and Related Phenomena
Group V. Structures of Overcrowded and Sterically-Hindered Molecules.
Group VI: The Structures of Molecular Compounds and Complexes
Group VII: Structures of Inorganic Crystals
Group VIII: Crystallography of Polyiodides and Polyiodines.
Group IX. Reactions in Solids.
Group X. Various Crystal Structures.
Group XI: Corrections to Publications of Other Workers.
Group XII: Miscellaneous.

Group XIII: Book Reviews

I have often contended that we spend too much time on execution of projects and too little time on planning what to do, although I have myself honoured this precept more in the breach than the observance. One piece of advice I remember from my student days with James—always work on at least a couple of projects; when you get stuck with one you can always switch to the other. I confess to having developed an early penchant for writing Reviews; looking backwards and around helps to put looking ahead in perspective. Jack Dunitz encouraged me to write a review on "Crystalline Molecular Compounds: Chemistry, Spectroscopy and Crystallography" in the (lamentably defunct) *Perspectives in Structural Chemistry* and this, after many years, became part of a much broader two volume monograph entitled "Crystalline Molecular Complexes and Compounds (Structures and Principles)" (Figure 4). Indeed, if I search for a principal theme among my rather dispersed list of topics then it would be "Molecular Complexes and Compounds". This allows me to contend that I have kept up with the times because this theme is the foundation of Supramolecular Chemistry for which Pedersen, Lehn and Cram shared a Nobel Prize in 1987. An important lesson from this summary, appreciated by more and more crystallographers, is that much is to be learned from studies over a wide range of temperatures (and pressures)-one needs to cover as much as possible of P-T phase space instead of restricting one's efforts to 298 K, 1 bar. Anyone actively engaged in crystal structure analysis must be impressed by the power of diffraction techniques in revealing the subtleties of Nature, but also by their limitations. The efforts of many people are need to obtain a complete picture of any crystalline system—resonance methods to study dynamics and subtle bonding effects, thermodynamics to study energetics-the list appears to be endless. However, although the cup is only half full, we are further along the road than when we started and, perhaps most important of all, it has been great fun reaching where we are now.


**Figure 4.** View of the cover of Volume 1 of the two-volume "Crystalline Molecular Complexes and Compounds" Oxford University Press, 2005.

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# Sol Kimel

Born Germany, 1928



Ph.D., Chemistry, University of Amsterdam, Netherlands, 1960

#### **Post-doc position**:

Research Associate, Princeton University, Princeton, NJ, 1961-63

# **Past Positions**:

Research Scientist, Weizmann Institute of Science, Rehovot, 1955-66

# **Technion Positions**:

Associate Professor, 1966-77 Professor, 1977-97 Incumbent, Chair of Chemistry, 1990–97 Professor Emeritus, 1997- to date

# Sabbatical leaves:

Senior Fellow, University of Amsterdam, Netherlands, 1972-73 Visiting Professor, University of Bielefeld, Germany, 1979-80 Visiting Professor, University of California, Irvine, CA, 1987-88 Visiting Professor, University of California, Irvine, CA, 1993-94

#### Short-term Positions as Visiting Research Professor:

Université Paris-Sud, Orsay, France	summer 1977
Max-Planck Institute for Quantum Optics, Garching, Germany	summer 1979
National Research Council, Ottawa, Canada	summer 1980
University of Bielefeld, Germany	summer 1981
Martin Weiner Lecturer, Brandeis University, Waltham, MA	summer 1984
Université Paris-Nord, Villetaneuse, France	summers '88, '89 '90
University of California, Irvine, CA	summers '89-'05

**Major departmental responsibilities** (at various times in the period 1967-1997): Responsible for laboratory of physical chemistry; glass blowing workshop; library; safety committee

## Major Technion/ International responsibilities:

Member of committee for promotion/tenure of senior staff; committee for research, student tribunal, academic staff association Chairman, Israel Laser and Electro-optics Society, 1969-72 Member Quantum Electronics Division, European Phys. Soc.,1969-71 Corresponding Member, Royal Netherlands Academy of Arts and Sciences 1989-to date

# **Field of Research**

**Biomedical Applications of Lasers** 

Photodynamic therapy (PDT) as a new modality for cancer treatment is studied in solutions, in cell suspensions and in-vivo. Photosensitizing drugs (porphyrins, porphycenes and phthalocyanines) are graded according to their efficiency to generate singlet oxygen, the phototoxic intermediary. Changes in triplet lifetimes of sensitizers are measured as a function of oxygen content in different environments. Binding of photosensitizers to erythrocytes and to liposomes, differing in composition and surface charge, are studied by absorbance and fluorescence spectroscopy. The efficiency of oxidative damage, induced by PDT in erythrocytes, liposomes and various tumor cell lines, is monitored electrochemically in real time by the depletion of ambient oxygen. Structure-activity relationships for different photosensitizers are derived from the correlation between the efficiency of a sensitizer and its three-dimensional structure. Novel photosensitizers for PDT are evaluated in-vivo using the chick chorioallantoic membrane (CAM) model developed at Technion.

Video microscopy in real time serves to monitor the entire process of tumor growth and tumor regression. Computerized image analysis is used to quantify occlusion of the microvasculature in a tumor, which results in tumor necrosis.

Photochemical (PDT) and photothermal laser-tissue interactions cause morphological modifications associated with vasoconstriction, which is used in tumor therapy and treatment of port wine stains, respectively.

# Sol Kimel

#### (February 2008)

In 1930, when I was one year old, my parents separated. My father stayed in Berlin while my mother and I moved in with her sister who lived in Amsterdam with her husband and children: one boy, Ab, about my age and another boy, Marco, born two years later. My childhood was uneventful in this harmonious, middle class family of six. The older cousin, Ab, and I went for two years to kindergarten and then for six years to an elementary school that belonged to a rather novel system called Montessori, where children were given freedom to study according to their own development; with little discipline and without report cards. This suited Ab and me very well because we were inquisitive youngsters. Years later, when we went to high school, we noted that we were way ahead of the regular school program. One of my class mates was Anne Frank and after the war our school was renamed the "Anne Frank School".

In May 1940, when I was eleven, our idyllic existence was rudely disrupted. Nazi Germany invaded Holland which capitulated after five days of fighting. The first year was relatively calm. Later we realized that the Nazis had used that year for extensive 'data mining' as it would be called today: Inconspicuously they registered population, including religious affiliation, assets in banks, real estate; everything that later turned out to be detrimental to the population as a whole but fatal for the Jews. Being law-abiding citizens, Dutch Jews complied with all directives not realizing then that they were signing their own death warrants.

In June 1941 the first anti-Jewish decrees were published. These included expulsion of Jewish students and teachers from the general education system and setting up a parallel system for the 140,000 Dutch Jews. In this way the Anne Frank School lost about one third of its students. Ab and I moved to a Jewish High School. During the next school year, anti-Jewish proclamations at ever increasing frequency became progressively more cruel: obligatory identity cards for the whole population were stamped with a red J for Jews; Jews were fired from all government jobs from professors or judges to lowly bureaucrats; they had to surrender property, bank assets, jewelry, radios etc; entry to restaurants, theaters, and cinemas was forbidden as were all public places like beaches, swimming pools, parks or even sitting on a street bench.

In trams and buses Jews had to remain standing, travel by train needed special dispensation; they could shop only between 15:00-17:00 h. While a country-wide curfew was in place from midnight to 4 o'clock, Jews had to be inside their own house after 20:00 h. Again, Jews obeyed; defying a decree meant prison or worse.

In spite of these difficulties the school year 1941/42 somehow carried on. Matters turned for the worse when in May 1942 Jews were ordered to wear a yellow patch, marked with the word old (Jood=Jew) on their clothes. It had to be worn at all times when outside, even when standing behind a window when visible from the street. This made all previous restrictions easily enforceable. Now it became clear what the real intention of the registration in 1940 had been, but it was too late - the trap was sprung. In the summer of 1942, Nazis, assisted by Dutch police, started to round up Jews from their houses for forced labor in the East: sometimes those living in a particular street, sometimes according to line of work but usually at random. A frantic search for documents ensued to prove the relative importance for the German war effort of metal workers, diamond cutters or furriers (who had to make jackets for German pilots). The Jewish Council created an elaborate system based on profession to 'save' people from deportation; of course members of the Jewish Council protected themselves. Indeed, at first people with the right papers were released if picked up at home or on the street. Nobody then knew that the Nazis merely wanted to cause the least possible panic by leaving hope for the remaining community. They were only interested in deporting about two thousand Jews per week to extermination camps; in the end, all 'protected' professions were also deported.

That year I attended school for only ten weeks; the atmosphere was dismal. Every morning the roll call showed who was missing. During those ten weeks ten students disappeared, mostly to 'Westerbork', the transition camp from where they were transported in cattle cars to the East, but occasionally into hiding as I found out after the war. We lived in parallel worlds; one ordinary: school, friends, homework etc, and the other hell: family and friends regularly disappearing. In November 1942 my mother was arrested during a chance visit to our family in a street that unexpectedly had been destined to become 'Judenrein', cleaned of Jews. Her papers could not protect her and she was shipped to Westerbork and a short time later to Sobibor, where she perished. The Nazis, in one of their satanic strategies, made sure always to deport families together to 'live' in the East. My uncle refused to accept this make-believe trickery; he did not allow me to join my mother in Westerbork as I was obliged to do. Instead, I became a non-person and had to go into hiding. This was the end of my war-time high-school education.

With the help of a Dutch resistance leader I was placed on a farm. I lived with this family (10 children of whom 3 were married), working on their farm; never leaving the house and farm except for church on Sunday mornings like everybody else. Of course I did not know who, if anyone, of my family was still alive. In the course of this year, neighbors (some of them Nazis) started to gossip about the 'strange' youngster who never left the farm. One night in April 1944, I was taken to a neighboring farm, at 5 minute walking distance. At that farm lived a young family with two little children. I was elated when I discovered that my uncle, aunt, Ab and Marco were hidden there; they had arrived about one year earlier. As a precaution neither they nor I had any knowledge about us being so close. The five of us lived confined to one room (we slept in 2 cupboard beds), never even seeing the sun.

In February 1945 our hiding place was discovered by Dutch Nazis. The farmer was shot. My uncle was interrogated and beaten to death, never divulging that 4 more Jews were hidden in the house. Following both murders a thorough search yielded our emergency hiding place in the cellar. My aunt, both cousins and I were thrown into a prison cell. The next day we were, again, interrogated and transported by horse cart to Westerbork, which meanwhile had been transformed from a transition camp into a concentration camp, where 900 Jews remained because transportation by train to the East was no longer feasible. In April 1945 we were liberated by Canadian troops. After three more months in Westerbork we could return to Amsterdam. It was then that we learned the extent of the holocaust.

Back to education. While people on the street were celebrating the liberation, Ab and I rushed from one tutor to another, making up for three lost years, to get ready for school entrance exams. We were admitted into fourth grade, effectively losing one school year compared to our age group. The rest was standard: matriculation and higher education.

At the science faculty of Amsterdam University one could select one major plus one minor subject or two majors. I chose physics and chemistry for my BSc; and physical chemistry, with specialization in spectroscopy, for my MSc. By and large it was a happy time; I became exposed to the world of science, which after WWII was the most interesting of all. In retrospect I realize that study at the old-fashioned Dutch universities was too broadly based and took way too long. Typically, completing an MSc in science took seven years plus at least four more for a PhD. Nowadays I am happy to note that nowhere are PhD students kept for eleven years after entering university.

During my studies I was active in the Zionist youth movement, Habonim. Israel exerted a push-and-pull action on me: I felt alienated from Holland by memories of the war experiences while being attracted by the notion of a state in 'statu nascendi' that would develop into a just society (little did I foresee how Israel would look 60 years later).

I wrote to Dr. Joseph Jaffe of the Physics Department at the Weizmann Institute of Science in Rehovot, whose work in spectroscopy I had come across during my studies. Since I did not have any publications, he asked me to agree to be interviewed in Holland by a well-known physicist whom he personally knew. After this scientist reported back to Dr. Jaffe, I was offered a position as "Junior A". The Weizmann Institute then knew 6 academic ranks: Junior A, Junior B, Intermediate, Senior, Associate Professor and Full Professor. My aliya in September 1955 was without help or hindrance from the Sochnut. I simply took the train to Marseille and a boat to Haifa. Joe Jaffe met me in Haifa, drove me to Rehovot and installed me in a rented room. The next day I started work in his research group. During the first year I studied intensities of infrared absorption bands of solutes in different solvents and found a correlation between the apparent band intensity and a function containing the refractive index of the medium. Writing my first paper with Joe (and later papers too) was a truly edifying experience. Joe taught me to carefully weigh every word and I had to go through at least ten drafts; in later years my own students were similarly educated (I hope they don't resent it). I also collaborated with Joe and other members of his group building a 12-m long infrared spectrometer-refractometer which was designed to achieve the highest spectral resolution known at that time. Overall it was an exhilarating period. The next year the spectrometer-refractometer was completed. I measured refraction spectra, n(l), through individual rotational lines in the vibration band of HCl at low pressure. The refractive index change, Dn(l), behaves like an odd function: zero at the center of an absorption line, positive and negative on its sides, with maxima depending on the line strength. It served as the subject of my PhD thesis, presented at the University of Amsterdam.

In the fall of 1956 I went back to Holland to marry Bianca whom I had known before my aliya. Shortly after returning to Rehovot we moved from my rented room to an apartment in shikun Anglo Saxim. In spite of its imposing name it was a 37 m<sup>2</sup> hole in the middle of nowhere, where we lived for three years. Our daughter Daphne and son Etan were born there. In 1960, when I had a PhD, a higher rank, and a family with 2 children, we became eligible for housing on the Weizmann campus. Meanwhile, Ab too had joined the Physics Department, as had Bianca's brother, contributing to an already active social life for Bianca and myself.

Based on my first paper on spectral intensities, I then proposed measuring spectral positions as a function of gas pressure, erroneously believing that Dn(l) would distort the line shape: positively on the high index side and negatively on the low side. I started a measurement program and was thrilled to discover a shift, Dl(p), of the line center at l with increasing pressure, p. However, other rotational lines yielded shifts different in magnitude and/or direction; while two lines were not shifted at all. That is to say, individual rotational lines are associated with a specific spectral shift which we could explain by effects of molecular collisions. We had stumbled upon an important discovery, though my initial interpretation was completely wrong.

In 1961 I took up a post-doctoral position with Prof. Donald Hornig at Princeton University, who was a world authority in spectroscopy. The instrumentation was not impressive compared to standards of the Weizmann Institute. Moreover, Hornig had just been appointed as Scientific Advisor of President Kennedy. He never came to the lab and the few times I managed to see him in his office to discuss problems in spectroscopy, he often would be interrupted by phone calls from Washington and discuss lunar orbiting versus earth orbiting (these were the traumatic post-Sputnik years, until the USA re-established predominance in space research by launching the 1969 Apollo mission to the moon). In Princeton we met Arza and Amiram Ron and our friendship continues till today. Arza also worked for Hornig so, naturally, we started to collaborate. I could not imagine then that 5 years later we would again become colleagues.

After two years Bianca and I returned to Rehovot. At the Institute I became interested in simplifying the problem of an HCl molecule interacting with colliding molecules in the gas phase. During a collision, intermolecular distances and orientations change and the interaction has to be calculated by integrating over the trajectory and averaging over the ensemble. Instead, an HCl molecule trapped at low

temperatures in an inert gas matrix such as argon is exposed to a constant, welldefined environment. Surprisingly, HCl rotates in an inert gas cavity but the rotational lines are displaced compared to the gas phase. At that time, Dr. Harry Friedman, recently arrived from Brussels, had joined Joe's research team and we started to collaborate. I was fortunate; he proposed an ingenious approach to explain the shifts in a matrix, based on considering in an HCl molecule, the distance between the center of mass (near the Cl atom) and the electrical center of interaction (near the molecular midpoint). During constrained rotation of HCl in a matrix, its center of interaction remains fixed in the cavity, consequently the center of mass moves to and fro. This we termed rotation-translation coupling; it could explain shifts of rotational lines as well as observation of novel translational lines in a matrix spectrum.

In 1966 I was invited to join the Chemistry Department of the Technion. On the one hand, Bianca and I were reluctant to leave the superb facilities of the Weizmann Institute, where we had family and had made many friends. On the other hand, the Institute's all-embracing life style led to separation from the real world. Moreover, after eleven years I felt a need to change the direction of my research. Though our results were critically acclaimed, and there was a lot more to be done, I was beginning to lose interest in the narrow field of high-resolution spectroscopy of small molecules. I asked for funds to set up the first laser laboratory in any chemistry department in Israel. Technion allocated the modest sum of \$20,000 to buy a ruby laser and thus they 'bought' me too. We never regretted the move. After dusty Rehovot, we liked the open city with its unmatched views and beaches. We started a new life: Bianca taught English at the Reali High School, our children studied at the Reali school and later at Technion; they married Technion graduates; our first grandson is about to become a Technion freshman.

The physical chemistry laboratory comprised a wonderful group of colleagues, above all Arza with whom I immediately reconnected on the scientific and on the social level. My first doctoral student was Shammai Speiser, who measured the interaction of a very short (ns) laser pulse with iodoform dissolved in cyclohexane and benzene. He explained the strange differences between these two solvents by a nonlinear effect called self focusing. Shammai being my first student at Technion, I believed all students were equally gifted. Shammai went on to become a senior member of the chemistry faculty and its dean. Later I realized that at Technion, like everywhere else, students range from brilliant to very good and I was lucky to have both kinds.

The next twenty years passed (too) quickly with doing laser chemistry of one sort or another. It became, again, time to reinvent myself. Looking at spectroscopic properties of porphyrins, I stumbled onto newly discovered biomedical applications. Some porphyrin derivatives have an ability to concentrate in tumors. Upon irradiation with lasers at selected wavelengths, they were shown to have a therapeutic effect on tumors, called photodynamic therapy (PDT). Production of excited oxygen (singlet molecular oxygen,  $^{1}O_{2}$ ) which is a known cytotoxic agent was suspected to be responsible for PDT but little was known about the molecular, let alone biological mechanisms of cell kill. PDT became a major field of interest during the ten concluding years as an active Technion researcher. Together with Prof. Jim Winkelman (a guest researcher from Harvard) and Dr. Varda Gottfried (a postdoc at Technion) we quantified spectroscopically the production of  $^{1}O_{2}$  from ambient  $O_{2}$  when irradiating porphyrins. We were limited to chemical systems in solution, and our results were not convincing to the medical community.

Toward my sabbatical year 1987/88 I applied to the Beckman Laser Institute and Medical Clinic (BLI) at the University of California, Irvine. I proposed to measure generation of  ${}^{1}O_{2}$  (due to irradiation) and its depletion (due to uptake) in cellular suspensions (in vitro) and in live animals (in vivo). The director of BLI agreed to the in vitro part of my proposal but questioned the feasibility of <sup>1</sup>O<sub>2</sub> quantification in vivo, involving fluorescence- and phosphorescence based spectroscopic measurements. I was the first visitor at BLI with an exact science background and I had to interact with biologists and medical doctors. I knew little about growing/handling cells and even less about working with animals. But people at BLI knew little about laser physics or chemical detection of  ${}^{1}O_{2}$ . Collaboration between different disciplines turned out to be synergistic and mutually rewarding. Using an electrochemical capillary device I had brought with me from Technion, we were first to quantify  ${}^{1}O_{2}$  uptake in living cells and to correlate uptake with cell death. The corresponding in vivo problem was indeed more difficult. Here I was fortunate again. A plastic surgeon from Israel (Dr. Arie Orenstein, Sheba Medical Center) happened to spend a sabbatical at BLI to improve surgery procedures. We started to discuss PDT in general and laser generation/depletion of <sup>1</sup>O<sub>2</sub> in vivo in particular. He suggested employing a transcutaneous electrochemical electrode that had been in use for measuring oxygen levels

in newborns, before the introduction of optical technologies. We scavenged hospitals around Irvine and found one department that had stored three units and was willing to share them with us. The last month of our stay at BLI, Arie and I measured, around the clock,  $O_2$  levels in tumor-bearing rabbits: Irradiating a tumor implanted in one ear and measuring  $O_2$  levels by affixing one electrode at the opposite side of that ear, together with two controls at different positions on both ears. The results not only allowed measuring  $O_2$  levels but, in addition, we proved that PDT follows a vascular mechanism:  ${}^1O_2$  does not attack tumor cells but endothelial cells in blood vessel walls, causing a collapse of the vascular bed supplying a tumor with  $O_2$  and nourishment, thus starving the tumor. This was the beginning of a 19-year long collaboration when every summer Bianca and I spent three months in Irvine. In that period, BLI grew from a handful to about one hundred scientists, mostly physicists and chemists, thus vindicating the synergism hypothesis.

At Technion's chemistry department we do not have animal facilities. Luckily, Varda noticed in the Rappaport Medical School an animal look-alike, which we could adopt. The model is called the chicken egg chorioallantoic membrane (CAM). When a ten-day old fertilized egg is opened, a rich vasculature is exposed, showing venules and arterioles which can easily be distinguished under a microscope. This proved to be an ideal model for studying PDT. We could inject a selected porphyrin or apply it topically onto the membrane, irradiate with laser light and monitor, in real time, dosage related vascular effects. The model proved so convenient and elegant that it was also adopted at BLI.

In 1997, after having spent 31 happy years at Technion, I had to retire. This gave me an opportunity to accept a long-standing invitation from Arie Orenstein to become a senior advisor in the Advanced Technology Center (ATC) at the Sheba Medical Center, where I stayed for ten years. The ATC ran on soft money, thus emphasis was on obtaining research grants. This did not interfere with the quality of our research centered on biomedical applications of lasers and spectroscopy. I am happy to note that in several instances ATC could provide real solutions to problems in clinical areas.

Reflecting on the very long time that has passed since my childhood, I can say that apart from the five WWII years, life has been good: Healthy, happily married, with successful children and adorable grandsons, and to top it all a fulfilling position at Technion that provided me with the freedom to select my own research projects. The journey through 50 years of scientific progress, from high-resolution gas-phase spectroscopy and matrix spectroscopy, via laser chemistry to laser medicine has been truly rewarding.

# **Aharon Loewenstein**

Born Germany, 1929



**Ph-D thesis**: Hebrew University Jerusalem, 1958

### **Post-doc position**:

California Institute of Technology, 1958-59 Columbia University, 1959-60

**Technion Position:** 1962-1997

#### Sabbatical leaves:

Oxford University, 1967-68 Oxford University, 1974-75 University Paris-Sud, 1982-83 University Paris-Sud 1996 Cambridge University 1988-89

#### Major departmental responsibilities:

Chairman, Faculty Committee of Graduate and Undergraduate Teaching. Head, Physical Chemistry Section

### Major Technion's responsibilities:

Technion Graduate and Undergraduate Teaching Committee.

# **Field of Research**

Application of Nuclear and Electron Magnetic Resonance spectroscopy to kinetics and structure of simple molecules in the liquid state Studies of liquid crystalline phases Studies of chirality

# **Aharon Loewenstein**

אהרן לוינשטיין (May 2008)

### Scientific Autobiography and a little more.

התנצלות. ראוי היה לכתוב רשימה זו בעברית. התירוצים לכתיבה באנגלית הם: העדר מונחים עבריים התנצלותי. לאוי היה להרבה מושגים מדעיים וחוסר הכישרון שלי להדפסה בעברית. על אלו, התנצלותי.

Background: I was born (31.01.1929) in Verden an der Aller, a small town some 20km south-east from Bremen, Germany. My father, Gustav, (08.08.1899-15.05.1971) was a lawyer. On April 7, 1933, he was dismissed from his profession by the Law of the Restoration of the Professional Civil Service that prohibited lawyers of non Aryan origin to appear in German courts. In June 1934, together with my mother, Hilde, (02.08.1905-16.9.1960) and my sister, Hanna (15.08.1932-) we immigrated to Palestine on board the ship "Polonia", departing from Trieste. My father obtained a 'Capitalist Certificate' (possessing more than 1000 Pounds Sterling) from the British mandate authorities. A few days after our arrival in the port of Jaffa, we traveled by bus (via Petach-Tikva, Qualqilia. Tul-Karem) to Netania. My father, together with my aunt (mother's sister, Herta) and her husband, bought the only existing small hotel in town from an American, Mr. Notkin. The hotel was built in 1933, had 6 guest rooms and was named Gal-Yam by Itamar ben Avi before my father bought it. I attended the local "Bialik" Elementary School from 1934 to 1942.

Much of what I tell below relates to my remote past and may not reflect precisely the true course of events. I trusted my memory and did not check documentation (wherever it exists) except for my list of publications.

My interest in Chemistry began at the Secondary Agricultural High School in Pardess Hanna (1942-47). I was sent to this school because education in Netania terminated with the 8<sup>th</sup> grade of the elementary school. The school was a boarding school, for boys only, though we had several girls in our class (who lived in the neighboring villages). The school had a five year program because in addition to the 'regular' High

School curriculum we studied Agriculture and worked on the school's farm. Thus I 'lost' one extra year at High School. I never felt sorry about the 'lost year' because the school, the teachers and my experience there were very pleasing. Our Chemistry teacher was Dr. Ashkenazi (nicknamed "Shleikes"). He was a pedantic teacher but performed his job very well. His teaching attracted and stimulated us. Together with a classmate and a good friend, Elkana (Kuni) Halevi, we assembled a 'Chemistry suitcase' that was an old suitcase filled with instruments and chemicals which were, legally or illegally, assembled from various sources, including the school Chemistry laboratory. We used to take our 'laboratory' to the center of our small orange grove and perform various chemical experiments. Fortunately, no catastrophe has happened. During my last (5<sup>th</sup>) year at school, Dr. Ashkenazi quit his position and was replaced by the elderly Dr. Rosenberg. Dr. Rosenberg has retired from the Kaddouri Agricultural School and his teaching of Chemistry was centered mostly on "Soil Science", namely the Chemistry and Physics of soils, fertilizers, colloids, etc. He was a dull teacher, reading his lessons while seated. Still, he was a kind person and presented us with the first introduction to topics that attracted me later. Some years later, while I lived in Rehovot working for my Ph.D., I was looking for jobs to supplement my poor income. Dr. Ashkenazi was the Head Master of a local High School (now the Amos Shalit School) and offered me a job to teach a one term course on the History of Science. My course was taken from a booklet containing lectures, written by several authors, in a series on the History of Science presented by the BBC. As far as I recall, my pupils seemed to enjoy my course. Till today I feel shocked by my boldness in accepting this job.

After graduation from my school (matriculation) I spent about two months in Beit Eshel (a very small settlement that existed about three km east from the old Beer Sheva). The reasons why I went to Beit Eshel were: a. I wished to spend a year of "National Service" by guarding the water pipe in the Negev. b. I was interested in soil mapping around that area and c. I had a cousin in Beit Eshel, *Aharon Heidemann*, whom I liked much. My plan could not materialize because I was not (yet) a member of the Palmach. In August 1947 I joined friends in the Tzofim Daled Hachshara and served in the Third Gdud (battalion) of the Palmach till my release in February 1949. The Third Gdud moved along the country during the war of independence (1947-49). Its main stations were: Beit Hashita, Ein Gev, Hulata, Ein Zeitim, Har Knaan,

Sarafend, Ramle, Shaar Hagai, Wadi Dorot, Beit Shean. In the summer of 1948 (July?), I attended a course for military reconnaissance (קורס סיירים) at Juara and that determined my job till the end of the war. I guided groups of people, from Negba to our northern positions in the Negev, through the Egyptian lines.

After the war of independence I became a founding member of Kibbutz Palmachim (April 1949). My knowledge and experience, in some agricultural professions helped me greatly. My main contribution to Palmachim was the establishment of the vegetable garden. A year or so later, I was strongly tempted to quit the Kibbutz and start studies at the Hebrew University. I could not decide as to which Faculty to register. The 'spirit of the time' (through the education at High School and the Palmach) suggested that I should study Agriculture (Agronomy) that was considered as the most useful profession to serve the State and the Kibbutz. On the other hand, Chemistry was my 'old love', so I searched for a reasonable compromise between the two. I was familiar (from my frequent visits to Beit Eshel during the mid 40's, with two young students who worked with Professor Rabikowitz (of the Faculty of Agriculture at the Hebrew University, located in Rehovot), on the project of soil mapping of Israel (at the time, Palestine). This project fascinated and interested me very much. I believe that I consulted them and eventually registered to a course on Soil Chemistry given by Dr Hagin at the Faculty of Agriculture of the Hebrew University (Professor Hagin is presently an Emeritus Professor at the Technion). The Kibbutz was quite antagonistic to this activity and in general to my academic aspirations (quote: "the time will come when we shall send our members to studies which we believe are needed for the Kibbutz"). Nevertheless, I traveled once or twice a week from Palmachim to Rehovot to attend the lectures and the laboratory exercises of Hagin's course. The course strongly convinced me that I should choose Chemistry if I wished to master properly Soil Chemistry (or Physics). The approach to this topic from the Agronomy point of view seemed superficial to me. Today, I doubt whether I had the capacity to make such a judgment and that the wish to learn 'pure' (as opposed to 'applied') Science was most likely the true (subconscious) motive. In the summer of 1950 I quit the Kibbutz and passed concurs examinations (in Mathematics and Chemistry) for the admission to Chemistry studies at the Hebrew University.

The teaching of Chemistry, especially Physical and Biochemistry, at the Hebrew University in those times was rather poor (an understatement). Considerable emphasis was put on practical Inorganic analysis and simple Organic synthesis and much of our time was wasted on these topics. Elderly teachers, who were dominant, followed the German traditions of the 30th. Quantum Mechanics was hardly mentioned and those who wished to learn it properly attended lectures by Racah given in the Physics Department. The mathematical background (needed for *Racah*'s lectures and for other topics) was essentially nonexistent. To overcome these difficulties, those in my class who wished to specialize in Physical Chemistry, got hold of as many books as they could (this was neither simple nor cheap) and assembled in small groups for selfstudy. Thus, to a large degree, we were auto-didactics (which is not ideal). Some of the younger teachers in the Department were much better and outstanding among them were the brothers Aharon and Ephraim Katchalsky (later Katzir). A few years later, when I spent my postdoctoral years at Caltech and Columbia, I was quite shocked by my poor education in Physical Chemistry as compared to the young American graduate and undergraduate students.

The curriculum in Chemistry in those days led directly to the M.Sc. title (without the B.Sc.), after about five years of study. The fascinating lectures of Aharon Katchalsky on monolayers (in a Physical Chemistry course) convinced me to approach him and ask him to supervise my Master's thesis on that topic (another option that I considered, was the study of nitro derivatives of sugars with Yeshayahu *Leibovitz*). In his lectures *Aharon* played with the idea of a physical world reduced to two dimensions. His lectured were flavored with many historical anecdotes e.g. Benjamin Franklin 'experiments' with oil patches to calm the rough ocean waves, Miss Pockels novel technique for "cleaning" surfaces. etc. Aharon, at that time, was mostly interested in the physical chemistry of polyelectrolytes and consequently suggested to me to measure, using the Langmuir Balance, the interaction between a poly-acid (poly acrylic acid?) monolayers and ions dissolved in the water tray. Aharon had no experience with the use of the Langmuir Balance and asked Ora Kedem (who was also inexperienced in this technique) to assist me with the experimental work. Ora did her best to help me and I learned to appreciate her scientific and personal qualities. Poly acrylic acid is somewhat soluble in water and therefore quite difficult to spread on water-air interfaces. Consequently, we were unable obtain reproducible results and most of that academic year (53-54) was spent in futile struggles with the instrument and the reading of most of the relevant published literature (that was still possible in those days). During the summer of 1954 I traveled abroad on a student exchange program to work in the Shell's Research Laboratories (BPM) in Amsterdam. I worked there for six weeks, on the qualitative and quantitative chemical analysis of hydrocarbon mixtures. This was my first true scientific experience. I worked in a real research (application) laboratory and towards the end of my stay was obliged to write my first detailed research report. With Aharon's blessing and recommendations I traveled to visit some research groups working on monolayer films in Utrecht and Cambridge (England). I learned much from these visits, in particular about the experimental aspects of operating the Langmuir Balance. After my return home, I joined the group of Israel Miller at the Weizmann Institute. He has just returned from a Sabbatical leave in the US and was experienced with monolayer work and the Langmuir Balance. He suggested new measurements using poly methyl methacrylate. Though the idea of measuring the electrostatic interactions between a poly-acid and counter-ions was abandoned, I produced some results, which enabled me to complete my Master's Thesis with some dignity.

Aharon agreed to accept me as his Ph.D. student but lacked the funds needed to support me (I believe that it was 200 I.L. per month). Shlomo Alexander, with whom I was acquainted during our study period in Jerusalem (he studied Physics), approached me and suggested that I should join him and Saul Meiboom to work on a new instrument, Nuclear Induction, which they were about to complete building (7kGauss or 0.7T, permanent magnet, ~ 31MHz for protons). Nuclear Induction was the term used at that time for what is now known as nuclear magnetic resonance (NMR). The name Nuclear Induction comes from the title of an article by Felix Bloch and from the fact that changes in the precession of nuclei spins induce currents that produce the resonances (a simplified version). Saul and Shlomo expected a visitor chemist from the US, Ernst Grunwald, (from Tallahassee Florida), who planned to do some chemical experiments with the new 'toy'. At that time Ernie was interested in acidity functions in non-aqueous media and hoped to measure the pKa values for alcohols in strong acid mixtures through NMR chemical shift titration curves. Unfortunately Ernie was unable to prepare the solutions (though he was a first class

experimentalist) because he was temporarily handicapped by a badly broken leg (the accident happened while crossing a street in London on his way to Israel, "Look Right, Look Left"!). Shlomo and Saul were incapable of mixing ethanol with sulfuric acid (too much to ask from physicists!) and thus Shlomo suggested that my duty in the laboratory (half of the time) would be devoted to preparing these solutions. For the other half of my time I should find some chemical problem, suitable for application with the new instrument and earn my Ph.D.. It was a risky decision to take. I did not have the slightest idea what Nuclear Induction was about and what could be done with it. Still, I gave my consent without too much hesitation because I was very impressed by Shlomo and Saul personalities and trusted their intuition and judgment. I started to prepare mixtures of ethanol and sulfuric acid but the measurements did not produce the expected S titration curves. Saul and Shlomo challenged Ernie to prove that chemists know what they were talking about. Ernie had to choose a system where he knew precisely what to expect: an aqueous solution of methyl ammonium chloride. The pKa value for this molecule is well known so that the titration curve (the methyl resonance chemical shift as a function of the pH), must produce the desired S shape curve. This choice determined the fate of my Ph.D. thesis and most of the future of the Nuclear Induction laboratory at the Weizmann Institute.

When we performed our first measurements in aqueous solutions of methyl ammonium chloride, we were quite surprised by the different spectra in acidic and basic solutions. Once we understood what these differences meant and consequently that one could obtain quantitative reaction rates from the different spectral lineshapes, the study of fast chemical kinetics (proton exchange), became the prominent activity topic of our laboratory. Obviously, the 'problem' of a search for a topic for my Ph.D. thesis, was now completely solved. There existed a very large number of interesting chemical systems where the measurements of these kinetic parameters was (and probably still is) important. Our only 'problem' left was where to start! Till today, nuclear magnetic resonance is one of a few techniques for the measurement of very fast (and also very slow) chemical kinetic processes. The unique features of this technique are the application of an "internal clock" (i.e. the inverse chemical shift, spin-spin or dipolar interactions) for the evaluations of the kinetic parameters and the possibility to measure rates at chemical equilibrium, that is with K=1 (e.g.,  $H_2O+H_2O=H_2O+H_2O$ ).

I worked very intensively with *Saul* and *Ernie* (often from noon to next morning,) on the measurements of proton exchange in ammonium salts. Also, in cooperation with *Arie Berger* we measured proton exchange reactions in a simple amide and with *Michael Anbar* we followed proton exchange kinetics in hydrogen peroxide-water mixtures. It was a period where we obtained truly new results in each working session. The more we progressed with our work, the more we realized the enormous potentialities of the technique. This experience hardly reoccurred in years to come. To pay an old 'debt of honor', we eventually also measured the "S" titration curve for methyl ammonium chloride. I might add that during that period I also synthesized a compound (2-methyl 2-nitro, 1.3 propane diol) that *Shlomo* needed for his studies on the non equivalence of certain methylene protons.

During that period I experienced for the first time the potential usefulness of a computer in scientific research. The modified Bloch equations (by Harden McConnell) for the spectral lineshapes, had to be solved for different values of exchange rates, relaxation times etc. This we did manually on electro-mechanical calculating machines before the home built electronic computer, "WEIZAC", became operative. *Pinchas Rabinovich* (a mathematician from the Applied Mathematics Department) assisted us in the programming (machine language). This was (I believe), the first use of a computer for basic research purpose, in Israel. I spent many nights near the huge machine (a room filled with electronic glass tubes that produced much heat) watching and controlling the array of blinking small lights (binary zero-one indicators). I rechecked some of the results manually because I distrusted the machine. *Saul* succeeded in obtaining a grant (after some political difficulties!) from the US navy. With this money we issued a booklet with some of our numerical calculations for the lineshapes of collapsing exchange doublets, triplets and quartets. It became a 'best seller' in NMR laboratories around the world.

After two years (1958) I had more than enough material for a fairly respectable Ph.D. and planned to go to the US for a post-doc position. Two anecdotes related to my Ph.D. thesis: a. While writing it I wished, naturally, to consult *Saul Meiboom* on the text. Since *Saul* was not fluent in Hebrew (my thesis was written in Hebrew), he suggested that I should come to his home one evening and read it to him. *Saul* was

lying on his sofa listening to my reading but within a few minutes was deep asleep. That was the first and last time he 'read' my thesis. b. Another referee, *Gabor Stein* from the Hebrew University, asked me to come and 'discuss' my thesis with him (a very common procedure). When I entered his office he greeted me with: "What an interesting thesis!" then started to look for it on his desk. I pointed out to him the brown envelope hidden in a pile of papers and he pulled it out. It was unopened! My referees for the Thesis were *Israel Dostrowsky, Willy (Zeev) Low* and *Gabor Stein*. Other newcomers to the laboratory, *Zeev Luz, Mati Sheinblatt, David Gill* and *Avraham Szoke*, made further impressive progress with studies of proton exchange in various systems containing N-H and O-H bonds and also important modifications of the experimental techniques such as the well known CPMG pulse sequence. All were much assisted by the very skillful contributions of our electronic technician, *Moshe Sasson*.

My first post-doc position (1958-59) was spent at the California Institute of Technology (Caltech) with Jack (John D) Roberts. I was awarded the Fulbright Travel Fellowship that was very important financially under the circumstances at that time. The fellowship provided me and my wife Rachel with return air tickets to the US (unlimited stopovers) + shipment (by air) of 70 kg personal luggage + health insurance in the US. Rachel and I fully exploited the benefits of this fellowship for an extensive tour through Europe that lasted for about three months. My first meeting with Jack was, on my way to the US, at the Kekule Conference in London. I had hoped to learn from him some Organic Chemistry applications of NMR (he later wrote two small books on NMR applications in Organic Chemistry). Jack however wanted me to do what I was best trained for, namely chemical kinetics. He proposed to study the (nitrogen) inversion process kinetics in methyl aziridine. The study of this system has been attempted earlier, unsuccessfully, in his laboratory and I contributed to its completion. My 'counter' suggestion was to study the ionization pattern of a tricarboxylic acid (e.g. citric acid), by NMR. This project, which I actually started at Caltech was finalized some years later, together with one of my first graduate students. Haggai Gilboa. The year at Caltech was very enjoyable both scientifically and socially. The weekly physical chemistry seminars were excellent and the course on magnetic resonance given by Harden McConnell broadened my scientific education significantly. In Pasadena I met future friends, Cafiero Franconi (Physics),

*Gideon Fraenkel* (Chemistry) with whom I enjoyed scientific cooperation in years to come.

My second post-doc year was spent at Columbia University in New York with Ben Dailey. The main motive of going there was my fascination from the big city that I visited briefly on my way to California. I had hoped that at Columbia I should take an interlude from NMR and learn a completely new (for me) technique: microwave spectroscopy. The circumstances were, however, unfavorable for such an enterprise because it so happened that Ben was not scientifically active during that particular academic year (1959-60). The option to learn microwave spectroscopy from two of his Post-Docs (Turner and Westerkampf) turned out to be impractical since they struggled with too many experimental and theoretical difficulties. Had I known before that such would be the situation at Columbia, I would probably have stayed at Caltech. I 'consoled' myself by returning to the exchange problems in ammonium salts and otherwise enjoyed the immense cultural resources of New York. Together with Tom Connor (a post doctoral fellow from Oxford), we measured the activation energies of proton exchange in all simple ammonium salts (methyl to trimethyl). The rather unexpected results which we obtained were the cause of an intense scientific dispute with Ernie who worked at that time with Saul at Bell Labs (Saul had left the Weizmann Institute and accepted a position at Bell Labs) and also served as a referee of the paper that Tom and I submitted to the JACS. Ernie believed that some of our results made no sense but could not refute the experimental data and could not produce convincing arguments to support this view. I shared some of Ernie's skepticism and challenged him to repeat our experiments. To my best knowledge, these measurements have not been repeated since, except for the trimethylammonium chloride case which was done by Ernie and agreed with our measurements.

I returned to the Weizmann Institute in the summer of 1960, several months earlier than planned because of my mother's terminal illness. The "Nuclear Induction" Laboratory was still mostly busy with work related to proton exchange and was temporarily headed by *Shlomo*. I took his position when he departed for his Post-Doc position (at UBC) and waited for *Saul* to return. I hesitated to start some new projects of my own and consequently continued along the line of my Thesis. With *Avraham Szoke* I measured the activation energy for proton transfer in water and with *Moshe Sasson* I developed a novel (primitive but working) spin echo attachment to the Varian DP60 spectrometer. *Tom Connor* visited me in May 1961 and we wrote a review on proton exchange measurements by NMR. With another guest *Windelt Drenth* (an Organic Chemist from Utrecht, Holland), we studied the NMR spectra of several ethers and thio-ethers.

I accepted an offer from Otto Schnepp and David Ginsburg to join the Chemistry Department at the Technion and moved there in September 1962. The Chemistry Department at that time was located in the Hadar campus of the Technion. The move meant that from now on I am the master of my activities and for the first time I could decide on my research program. I established my laboratory with adequate equipment (a Varian DP60 spectrometer with several attachments). The Chemistry Department, including my laboratory, moved to the new campus in 1964. My basic interest was still focused on the measurements of chemical kinetic processes by NMR. However, I wished to broaden its scope to electron transfer reactions and to a more basic understanding of the mechanisms, especially the role of the solvent in the kinetic processes. Another aspect of NMR which caught my imagination was the study of the "less accessible" (as they were called at that time) nuclei such <sup>14</sup>N, <sup>59</sup>Co, <sup>95</sup>Mo, <sup>53</sup>Cr, <sup>131</sup>Xe and others. The Varian DP60 spectrometer was equipped for these studies though it suffered from all the limitations of the pre-FT NMR period. These topics, chosen partly as "fun" and partly as curiosity to explore novel territories, brought me into the realm of inorganic chemistry. My first graduate students at the Technion were Mordechai Shporer and Haggai Gilboa. With Shporer and in cooperation with Gill Navon (and later also with Gideon Ron) we investigated rather extensively several cyano-metal complexes in aqueous solutions including studies of electron transfer between ferri- and ferro- hexacyanide ions. Together with another student, Yair Egozi. I investigated the electron transfer in the chromate-dichromate system through the <sup>53</sup>Cr NMR measurements. Our studies of <sup>59</sup>Co resulted in an unexpected bonus: due to the very large chemical shifts of <sup>59</sup>Co, we observed some interesting isotope effects. By chance I learned that Paul Lauterbur was also interested in isotope shifts, which resulted in a joint publication with him. With Haggai Gilboa I returned to the study of the ionization pattern of citric acid that I begun in Pasadena with J.D.Roberts. This time however, the issue was treated properly and also extended to several other tricarboxylic acids and their derivatives.

Further on, in cooperation with *Haggai*, *Janina Altman* and *David Ginsburg*, we studied the kinetics of ring inversion in cis-decalins and in a propellane.

I spent two months in the summer of 1963 at the Varian facilities in Zurich by the invitation of its director, Warren Proctor. I tried to convince Varian Associates to develop commercially a spin echo attachment for the DP60 which Moshe Sasson and I have built. Unfortunately, I did not foresee FT NMR and my sole aim was a pulsed spectrometer for relaxation times and translational diffusion measurements. Warren's reaction to my initiative was very favorable. However, Varian headquarters in Palo-Alto ruled against the project on the basis that the market for pulsed spectrometers is rather limited (the argument was that most NMR users are organic chemists who are not interested in pulsed spectrometers). Bruker's success in introducing the first pulsed spectrometer (and consequently its technical readiness for the FT epoch) proved that Varian's decision was a big mistake. During that visit to Varian I became acquainted with Paul Rigny. Paul was sent to Varian by his Thesis supervisor, Lionel Solomon, to become acquainted with practical NMR. Together with Paul Rigny, Atillio Melera and W.Walter (an organic chemist from Hamburg who supplied he materials) we measured the activation energies for rotation barrier around the C-N bond in several thioamides and compared them with similar results for amides. This topic was studied later in great detail, both experimentally and theoretically, by many researchers.

Around 1965-6 I decided that magnetic resonance activities in our Department should be complemented by Electron Spin Resonance (esr) studies and I purchased a Varian esr spectrometer for this purpose. I persuaded my Ph.D. student, *Rafi Poupko*, to work with me on the new spectrometer. Our first project was the study of the kinetics of proton exchange in radicals derived from simple aliphatic alcohols and compare the results to those obtained in their parent alcohols (*Luz*). For the production of the radicals, we used a Ti<sup>+3</sup> salt and hydrogen peroxide in a flow system. Furthermore we derived a large variety of radicals from amino acids, more complex alcohols and other materials. This opened the way to analyze systematically the hyperfine couplings (spin density) in small organic radicals. In 1967 I convinced *Brian Silver* who then worked at University College, London (I knew *Brian* earlier from his stay with *Luz* at the WIS) to join our staff. *Brian* took over most of the esr

research and became a co-supervisor in *Rafi*'s Ph.D. Thesis. It seems that in those years my research directions were rather divergent and covered broad fields. I liked it this way (and still do).

My first Sabbatical (67-68) was spent in Oxford with *Rex Richards*. I was a Visiting Fellow at All Souls College and received scholarships both from the College and the Royal Society. In order to permit me to dine at High Table without paying, I was awarded a M.A. degree from the University of Oxford (cost: 10 Pounds, paid by All Souls). During the year I was well trained in all the bizarre manners of Oxford dons. My time during that year was occupied mostly by the work of my students at the Technion. I persuaded *Peter Atkins* (Lincoln College) to work with me on the problem of the theoretical interpretation of the magnetic relaxation data in liquid ammonia (protonated, deuterated and mixed species) that *Yair Margalit* had measured. This cooperation resulted in a rather complicated model (most probably wrong) for molecular dynamics in liquid ammonia.

Around that period (about 1967), my interest shifted from the kinetics of chemical reactions to molecular dynamics in liquids. I had the impression that the gaseous and solid phases were, at least in principle, pretty well understood. The complexity of molecular motions in the liquid state intrigued me to explore how much related information could be obtained from measurements, by NMR, of the T<sub>1</sub> and T<sub>2</sub> relaxation times and the translational diffusion coefficients. My measurements of relaxation times were mostly confined to quadrupolar nuclei because their interpretation is fairly straightforward while the interpretation of the relaxation data of dipolar nuclei are considerably more complex. With time I acquired a fairly broad view on this topic and realized the inherent limitations and advantages of the application of various NMR techniques. I started with very little knowledge (an understatement!) of the physics of the liquid state. An obvious solution to my problem was to organize a School (Workshop) on the physics of the liquid state and learn from it as much as possible. This I did during two weeks in September 1971 with the financial help from the Bat-Sheva foundation. I invited top experts in the field of physics of the liquid state (Oppenheim, Litowitz, Kivelson, deGennes, Zwanzig, Bratos, Freed, Ben-Reuven, Bixon, and others). Most experimental techniques and related theoretical topics for the study of liquids were presented in the School. I

believe that the School was a success. Particularly through the lectures of *deGennes*, I became familiar with liquid crystals and appreciated their importance for the measurements of the anisotropy of molecular reorientations in the liquid phase. I also realized that measurements of solutes in liquid crystalline media extend considerably the possibilities offered by NMR spectroscopy since in isotropic media dipolar and quadrupolar interactions are 'washed out' (residual values of dipolar interactions in weakly ordered media are nowadays termed RDC).

As mentioned above, my first enterprise in the study of dynamics in the liquid state was with *Yair Margalit*, in cooperation with *Peter Atkins*, through the measurements of <sup>14</sup>N and deuterium relaxation times in liquid ammonia. Later, with *Yair Egozy*, in cooperation with *Brian Silver*, we studied the deuterium relaxation of deuterated benzene dissolved in a nematic liquid crystal. The main purpose of this study was to obtain information on the anisotropy of benzene reorientation in a liquid crystal. This, admittedly, was a rather bold project for its time. Some years later this topic was studied extensively with much improved experimental and theoretical tools by others. These projects signaled a new (slightly more focused) direction in my scientific activity that lasted for about 20 years.

In the course of scientific research it might happen that a sidetrack turns into a new main avenue. This, happened to me when I read a paper by *Saul Meiboom* who discovered that tetramethylsilane is apparently distorted, from its nearly spherical shape, when dissolved in a nematic liquid crystalline media. I was somewhat skeptic about the result but my skepticism antagonized my faith in *Saul's* scientific qualities. Thus, I decided to try it myself. I started with neopentane, then with methane and finally with xenon atoms (!). In all cases the apparent distortion was observed beyond doubt (*Saul* was right again, as always.). I soon realized that, this phenomenon is closely related to that which is observed for symmetric cations and anions (such as sodium, lithium, ammonium etc.) belonging to soap molecules of lyotropic liquid crystals (though there exist some significant differences). I worked on these topics with several coworkers: *Roman Ader* (methanes, silanes and cobalt complexes), *Manuel Brenman* and *Rachel Schwartzman* (Xenon, various cations in lyotropic systems), *Jean Charvolin* and *Joseph Virlet* (cations). A set of experiments on this topic, which unfortunately were never completed, was performed with *Jean Pierre* 

Kinzinger in Strasburg, (measurements performed at the Bruker laboratories near Karlsruhe and in Wissenbourg). The idea was to dissolve cations a non-aqueous media (to eliminate the possibility of non-spherical distribution of water dipoles around the cations (i.e. non-spherical hydration) and then 'distort' them in a non aqueous liquid crystal. The systems chosen were alkali halide salts complexed with cryptates (2,2,2, ; 2,2,1, 211) and dissolved in PBLG/organic solvent liquid crystals. We obtained quadruplar splitting (triplets,) for sodium but none for several other alkali cations. This work still awaits its completion. With time, some progress, theoretical and experimental, has been achieved on the general problem by other groups, mostly by Diehl and, Jokkissari in Basel and Oulu (Finland), Burnell and deLange in Amsterdam and Vancouver and by Lindmann, Wennerstrom and others in Lund. Till these days I am convinced that we do not yet possess an adequate theoretical model needed to interpret many experimental results. The phenomenon is quite unique because, as *David Buckingham* has correctly pointed out to me rather early in the game, NMR is capable of detecting very tiny molecular, atomic or orbital 'distortions' from their shape in a dilute gas phase. These 'distortions' result from the anisotropy of the intermolecular potentials prevailing in ordered media and seem to be rather difficult to calculate. Thus, I believe that the case of the 'apparently oriented spherically symmetric species' still awaits its proper solution.

In the autumn of 73 a Post-Doctoral fellow from the US, *Mike Piliavin*, came to work with me. He was a theoretical Chemist (Physicist?) and proposed to work on the simulation of phase transitions in nematic liquid crystals by the Monte-Carlo technique (this followed a published work by *Lasher*). His suggestion was good but involved enormous programming work. Rather slow computer facilities were available at the Technion and some attempts were performed at the computer of the Weizmann Institute. Year 1973 was quite bad for scientific research because of the war and its after effects. *Mike* ended his stay with me without, unfortunately, getting any meaningful results.

My second Sabbatical (74-75) was spent again in Oxford. I decided to devote it to time resolved esr, in cooperation with *Keith MacLauchlan*. *Keith* developed this technique and had the only laboratories in the world who was equipped to perform such experiments. My idea was to measure the esr spectra of the photo-dissociation

products from duroquinone dissolved in a nematic liquid crystal. We assumed that due to the anisotropic diffusion of the product radicals in the liquid crystalline media, their spectra would enable us to discriminate between the, so called, "radical pair" and "triplet" mechanisms in the process of photo-dissociation. The issue of "radical pair" vs. "triplet" mechanisms in photo-decompositions was rather controversial and no experimental technique was available to distinguish unambiguously between them. In this project I cooperated with Luigi Pasimeni from Padova who spent the year with Keith. We worked very hard with no meaningful results. This caused a very great disappointment to both of us. The reasons for our failure were eventually well understood: At that time, no aliphatic nematic liquid crystals were commercially available and we were obliged to use 'conventional' materials (like EBBA) that contain benzene rings. In such materials most of the light (from the  $N_2$  laser) was absorbed by the solvent. Furthermore, the light intensity was insufficient and the yield of the product radicals was very low. On top of all, Keith's instrument was an elaborate first model and rather difficult to operate. Despaired by the lack of progress, I turned to a project of measuring the magnetic relaxation times of a probe (nitrobenzene) dissolved in a liquid crystal, close to the nematic-isotropic phase transition point. The aim was to detect changes in molecular dynamics in the pretransitional range. This work was performed in cooperation with David Turner who belonged to Ray Freeman's group.

Another interesting sidetrack was the measurements and exploration of quadruple coupling constants (QCC) of deuterium, nitrogen and other quadrupolar nuclei, in small molecules. These so called 'constants' are very sensitive to changes in the environment of the nuclei in the molecules (e.g. hydrogen bonds and specific intermolecular interactions). There exist accurate methods to measure the QCC's in the gas and the solid phases but difficulties arise in the liquid state. In the liquid state, QCC's must be derived from an expression which contains two unknown parameters (one of them being the QCC). NMR can be applied but with some severe limitations that we studied in detail. I have studied several systems: N<sub>2</sub>O and NH<sub>3</sub> (with *Manuel Brenman*), the azide (N<sub>3</sub><sup>-</sup> ion), ammonium nitrate (with *Haggai Gilboa*) and dimethylsulfone (with *Dan Igner*). The latter study was a rather unique study that involved the NMR measurements of <sup>33</sup>S and <sup>17</sup>O nuclei (in very low concentrations) and computational work concerned with finding the direction of the main component

of the electric field gradient tensor of the sulfur atom (performed by *Miri Karni*). I also set out to measure hydrogen (and HD, D<sub>2</sub>) dissolved in a nematic liquid crystal. This is a tricky experiment because of the difficulty in preparing the samples. We were just about ready to start these measurements when an exellent paper by *Burnell* and *deLange* appeared (in JPC), in which they presented a detailed and complete experimental and theoretical work on precisely this problem. I decided to quit. In 1983 I wrote a review (invited by *J.A.S. Smith*) on the subject of the measurements and interpretation of quadrupole coupling constants by NMR. In this review I described in detail the complexity of the problems and the difficulties to solve them.

An interesting, though futile, attempt was attempted in 79-80 together with *David Goldsmith* (a post doctoral fellow from the US): The idea was to measure changes in the NMR relaxation times of liquids (and liquid crystals in particular) while an acoustic perturbation is applied. Rough theoretical estimates (begun by *Peter Atkins*) indicated that under certain circumstances we might measure variations in the  $T_1$  values when an acoustic perturbation (of the right frequency) is applied. The expected effect originates from non- linear density fluctuations in the liquid caused by the acoustic perturbation. Unfortunately we were inexperienced with acoustic techniques and unable to complete a successful experiment. We devoted great efforts to this project.

On the main track, apart from the work mentioned above, I extended the ammonia work to liquid methyamines (with *Ruth Waiman* and *Esther Glaser*) and to nitromethane (with *David Goldsmith*). I believe that the most interesting and exiting results were obtained in the measurements of the anisotropic translational diffusion coefficients of methane dissolved in thermotropic liquid crystals that possess nematic and several smectic phases. Methane is (almost) an ideal probe for such measurements because of its weak interaction with the solvent. This work was performed with *Mike Moseley* using facilities available at the Weizmann Institute. It produced results for the parallel and perpendicular diffusion coefficients in several liquid crystalline phases. These results present a challenge for theoreticians (*Pier-Luigi Nordio* has shown great interest). I regret that we were unable to extend this work.

An interlude: Sometime during 1980 or 1981, through the initiative of *Izhack Oref* and in cooperation with our colleague *Zvi Dori*, we started to discuss the idea of establishing a Science and Technology Museum in Haifa. A self appointed committee for this purpose has existed but did not succeed to materialize the idea. In February 1983 the first exhibition hall was opened on the site of the old Technion campus in Hadar. In the early stages of the Museum, initially named "Technoda", my main activity was the organization of weekly lectures for the general public on scientific themes. I served on executive board of the museum til about 1996.

I spent my third (1982-3) Sabbatical in Orsay (Universite Paris-Sud). It was originally planned to be in the Saclay (CEA, Atomic Energy Commissariat facilities) but could not be materialized due to political reasons (Israeli activities in Lebanon). *Jean Charvolin* has shown me some very preliminary results that he had obtained in a lyotropic liquid crystal composed of a dicarboxylic acid and solvent. The structure and phase behavior of lamellae (or other lyotropic structures) for molecules containing two polar ends attached to an aliphatic chain, intrigued our imagination (e.g., is the chain straight or folded into loops?). I started to work on the subject in Orsay and did most of it after I returned home. Much of the work was performed at the Weizmann Institute with *Helen Gutman* and in cooperation with *Zeev Luz, Rafi Poupko* and *Herbert Zimmermann*. We studied several bifunctional (dicarboxylic acids) lyotropic systems and studied their phase diagrams that included a reentrant isotropic phase (we expected a cubic phase!). We have also studied the perturbations induced by the addition of short-chained mono- and di- acids to the these systems.

During that Sabbatical I have persuaded *Charles deMenorval* who worked in the laboratory of *Jacques Fraissard* at Universite-Paris 6, to cooperate with me on NMR measurements of <sup>129</sup>Xe and <sup>131</sup>Xe binding to water (clathrates) and proteins in aqueous solutions. We measured the chemical shifts for <sup>129</sup>Xe and the line-widths for <sup>131</sup>Xe in the same samples. Preliminary measurements were very promising but unfortunately time ran short and I had to return home before we could complete this study. <sup>129</sup>Xe NMR has recently found many applications, due in particular, to the use of hyperpolarized Xe which enhances tremendously its NMR sensitivity.

My interest in carbohydrate liquid crystals originated in a Colloquium given in our department by the crystallographer, George Jeffrey from Pittsburgh. He presented Xrays results for sugar based carbohydrates (non-ionic soaps) that form various liquid crystalline phases. The interesting part, for me, was the fact that these materials form both thermo- and lyo-tropic liquid crystals. I imagined that manipulations of the systems (e.g. using the  $\alpha$  and  $\beta$  anomers, changing the length of the length of the aliphatic chain attached to the sugar, changing the nature of the solvent or the temperature), would create a broad and interesting panorama of experimental possibilities. Indeed, as I began (in cooperation with Dan Igner) to obtain results with β-octyl-glucopyranoside in water, benzene and mixtures of benzene and/or water, these expectations were materialized. Uri Zehavi (of Faculty of Agriculture at the Hebrew University) kindly assisted with the deuteration of the octyl side chain and Geoffrey Luckhurst and Andrew Emerson cooperated in the calculations of the order parameter profile of the side chain. Further work on homologous series of the octyl-glucopyranoside (all with Dan Igner) revealed the richness of the phase diagrams. Although several other carbohydrate liquid crystalline systems have been studied since, mostly by DSC, X-rays and optical microscopy, the fundamental understanding of the nature of the phase diagrams, "leaves much to be desired". Some of the peculiarities in these systems are truly amazing, for example, the differences noted for the  $\alpha$  and the  $\beta$  anomers, the appearance of cubic phases and many other effects.

Sometime during 92 I received a copy of an obscure Journal, "Molecular Materials", in which one paper (by *Bykov* et al) caught my attention. It concerned a columnar ('chromonic') lyotropic liquid crystal based on aqueous solutions of a red dye, benzopurpurin. I had some previous experience with a well-behaved chromonic liquid crystal, DSCG / water and decided to have a closer look at benzopurpurin / D<sub>2</sub>O mixtures by deuterium NMR. The work was performed with *Manuel Brenman*. It was not an easy task to assign the different phases of chromonic systems by NMR (we tried also X-rays measurements but had limited access to the instrument and too little experience). Our published work leaves many questions unanswered such as the fact that very low concentrations of the dye are producing ordered phases, the extreme

sensitivity of the nature of the phases to the chemical structure of the dye, the structure of the phases, and others.

My latest (presumably last) scientific adventure which could be termed "chirality etc", started in a bizarre manner: I spent the 88-89 academic year on a sabbatical in Cambridge, England, hoping to do some solid state NMR with Jacek Klinowski that for one reason or another came to nothing. Most of that year was devoted to reading and to the cooperation with the Southampton group (Geoffrey Luckhurst, Andrew Emerson) on the interpretation of the side-chain dynamics of carbohydrate liquid crystals. Towards the end of the year I noticed a paper by Jacques Courtieu, from Orsay, on the measurements of <sup>129</sup>Xe chemical shifts in various liquid crystalline phases. I wrote to Jacques, complimenting him on his paper and suggested to discuss some further possibilities of work with him on Xe. In response I was invited to spend a month with Jacques in Orsay (summer 90). I accepted the invitation willingly. Upon my arrival Jacques presented the work currently performed in his laboratory which included the application of 'compensated' cholesteric liquid crystal mixtures with the purpose of measuring different NMR spectra for enantiomers. This project followed very similar experiments performed many years earlier by Saul Meiboom! The mixtures that Jacques used were sensitive to the temperature, composition and mode of sample preparation and did not seem to be suitable for the purpose of obtaining different NMR spectra of enantiomers. Furthermore, Meiboom's experiments could not be reproduced. This was rather amazing to me, recognizing the experimental capabilities of both Saul and Jacques. I suggested to use solutions of poly-y- benzyl-L-glutamate (PBLG) instead of the 'compensated mixtures'. I had some earlier experience with PBLG solutions through my work with <sup>131</sup>Xe as solute and other studies. We tried it immediately and it worked perfectly! Being, both of us, very enthusiastic with this result, we forgot all about our planned Xe project and devoted the next 10 (and more) years to develop a novel and elegant technique for chiral discrimination by NMR by using chiral liquid crystalline solvents. In a sense, this project 'closed a cycle' for me because it started with the attempts of Jacques to elaborate on some early results of Saul Meiboom.

My collaborators and friends in Orsay were Jean Marie Pechine, Jean Pierre Bayle, Isabelle Canet, Abdelkrim Meddour, Philippe Lesot, Willy Smadja, Philippe *Berdague, Denis Merlet* and of course, *Jacques Courtieu*. Other members of the laboratory assisted in many ways over the years. I spent in Orsay two half Sabbaticals and many shorter visits.

Chirality is a fascinating subject. One aspect that is far from being properly understood, is its quantitative evaluation. I made some efforts to correlate our experimental results which are basically probing the intermolecular interactions (through their effect on the ordering), with some quantitative chirality measures (*Avnir*, *Osipov*, *Moreau*, *Gilat* and others). Another important challenge would be to understand the molecular details of the chiral discrimination that is, the interaction between the poly amino acid and the solute. This can be done by designing clever experiments and/or through theoretical computations. I tried to convince (and still am) experts in molecular dynamics to cooperate with me on this problem. I think that such computations, in light of our present results and through newly planned experiments, could shed light on the mechanism of chiral recognition in Biology. The system which we used (PBLG + organic solvent) is on one hand simple enough for physical modeling and on the other hand not too remote from real biological systems.

Hardware: Instrumentation is of tremendous importance in NMR since it determines what you can do and what you can not do. Occasionally I went to perform experiments in other institutions such as TAU, WIS and other places as I was unable to perform them at home. As told above, my first instrument was a home built (mostly by *Saul Meiboom, Shlomo Alexander* and later in part by *Moshe Sasson*) 31MHz (protons), corresponding to 0.7T permanent magnet. This instrument was a modified version of the Bloch - Arnold instrument that existed at Stanford. It was the only NMR instrument in the world that had a built in cigarette lighter, battery operated. *Saul, Shlomo* and I used this gadget quite a bit! My first instrument at the Technion was the Varian DP60 (60 MHz protons, 4T electromagnet). It was a state of the art instrument. Later arrived the Bruker HFX 90 and the BKR322S pulsed spectrometer, both operating with a 2.1T electromagnet, 90MHz protons. Finally I worked with the Bruker WH400 that operated with a 9.4T wide bore superconducting magnet (energized on 31.01.1985).

NMR instruments are technologically complicated gadgets and a talented service person is essential for their proper routine operation. When I started my work at the Technion, I searched an electronic technician and eventually found *Aharon Peer* for this position. *Peer* served the NMR facilities (and other scientific instruments in the Faculty) for several decades with great talent and capability. He solved many difficult technical problems and thereby contributed significantly to many research programs.

My early dreams of investigating the Physics and Chemistry of soils failed to materialize. One of the reasons was that NMR techniques were not yet properly developed, during most of the period, to handle such problems. Towards the end of my career the situation changed. A few years before my formal retirement (1997) I made great efforts, in cooperation with researchers from the Chemical Engineering Faculty and others, to acquire a micro-imaging NMR spectrometer. Such an instrument would have enabled me to study the anisotropic diffusion and several other properties in heterogeneous systems, such as soils. Unfortunately, my efforts did not bear fruits and I was not able to acquire this instrument.

About two decades ago, I became interested in some particular 'non-scientific' themes. Initially my interest was directed to the fate of German Jewish scientists who were expelled from Germany by the Nazis in 1933. The expulsion of my father, who was a lawyer, may have triggered this interest. The first question that interested me was the reactions of the Aryan scientists to the dismissal and expulsion of Jewish scientists. I could not understand how most of the eminent, well educated, intelligent, scientists, did not react or protest in reaction to such malicious acts (that was still possible, to some extend, in early phases of the Nazi regime). The behavior of the German scientific community puzzled me. A possible answer to this question I received from the geneticist, Benno Muller-Hill from Koeln, who attributed the behavior of the Aryan scientists simply to their greed and aspirations to power. This answer, which sounds simplistic, may be the correct one (with a proper admixture of Anti-Semitism). During my meeting with him I also befriended his student Ute Deichmann who was, at that time, writing her Thesis on Biology and the Biologists in Germany during the Nazi regime. Ute has contributed significantly to historical studies of scientists in Nazi Germany and in particular to those of Jewish origin who were compelled to emigrate.

With the years my interests broadened and included the relationship between Scientists and the State or, in other words, the inherent conflict between loyalty to one's moral principles and his obligation to his state homeland. This topic is not specific to totalitarian regimes as the Nazi Germany or the USSR, but relevant also to democratic regimes, such as the United States or England. The development and construction of "non-conventional" weapons (atomic, biological and chemical) by scientists for their homelands, is the case where the conflict between personal morals and national duties is manifested.

To what degree scientists should be politically or socially involved, is another aspect of the same issue. Personally, I was active in the "protest movements" that arose after the Yom Kipur war. Later, I tried two or three times to be politically involved. However, these naïve attempts ended after very short periods, with great disappointment from the way the political system is run.

My interest was centered mostly around the period of the second world war, when the issue of political attitudes of scientists, arose through the development (or attempts to, in the case of Germany) of atomic weapons. A much discussed and well documented case is that of Werner Heisenberg (the "white Jew"). Did he do the best he could to provide the regime with atomic weapons that would lead Germany to victory? Or did he play some clever games just to do the contrary? I believe in the first option. Other examples are the scientists who participated in the Manhattan project at Los Alamos. Almost none of them quit the Manhattan project that was constructed against Nazi Germany, after it became definitely known that the Germans were far behind with their programs or even after the total collapse of Germany in May 1945. A very few exceptions, such as Rotblatt and some others, quit their participation in the construction of the bomb. A few others (L.Szilard, J.Franck, E. Wigner) tried to prevent the bombing of Japanese cities.

One of the early books that described some of the background to these issues was "The making of the Atomic Bomb" by Richard Rhodes (1986). Since then I have read some of the vast literature that is concerned with these topics. This eventually produced a small article ( published in "Jews and Sciences in German Contexts", U. Charpa and U. Deichmann Ed., Mohr Siebeck, 2007) on a somewhat related issue that occurred before WWII (1938).

Teaching: I worked 35 years at the Technion (1962-1997). During that period I spent 5 years on Sabbaticals (Oxford, twice; Cambridge; Paris, twice). It may be appropriate to conclude these notes with a short review of my teaching experience.

In the very early years I considered teaching as a nuisance and an unavoidable perturbation to my research activity. I did not think that it is worth a great effort, in particular to students who were not Chemistry majors. I was inexperienced in speaking to a large audience and on at least one occasion, lost my voice! The Laryngologist whom I consulted called it "the teacher's disease" and advised me to take a course in speech training. I believe that my teaching qualities at that time were rather awkward and that my performance was unfair to the students. With the years the situation has improved much though I never reached the status of a "distinguished teacher" and my marks in the student's Teachers Quality Questionnaire were mostly mediocre. Gradually I learned to appreciate the importance of good teaching and devoted more and more time to its fulfillment. I gave courses on most topics of Physical Chemistry (Thermodynamics, Kinetics, Surface Chemistry, Spectroscopy) to undergraduates and specialized courses on NMR to both graduate and undergraduates. In the early years I gave a course on NMR at what is now known as Ben-Gurion University in Beer Sheva and a course on Fast Chemical Kinetics at the Weizmann Institute of Science. I served for 9 years on the Technion Committee for Graduate and Undergraduate Studies and for about 3 years as Chairman of the Faculty Committee for Graduate and Undergraduate Studies. I admit that I now hold a very different opinion in the matter of teaching from that which I had in the beginning of my career. I enthusiastically participated in dissipating science to broader audiences such as to High School pupils, soldiers and adults. I gave (and still do) popular talks on topics such as MRI, liquid crystals and others.

A final note to my contribution to teaching may be added. Through my initiative Visiting Professors to the Faculty were invited: *Peter Atkins*, Oxford, on "Interaction between Electromagnetic Radiation and Matter", *Peter Diehl*, Basel, on "NMR and Liquid Crystals" and *Jacques Courtieu*, Orsay, on " Modern NMR Techniques and Studies of Chirality".

I am grateful to my friend Izhack Oref for his comments to this manuscript.
## Asher Mandelbaum

Born Poland, 1934



**Ph-D thesis**: Technion-Israel Institute of Technology

### **Post-doc position**:

Massachusetts Institute of Technology, Cambridge, USA 1966-68

### **Technion Position**:

Lecturer, 1963–66, Senior Lecturer, 1966–71 Associate Professor, 1971–80, Full Professor, 1980–03 Professor Emeritus, 2003–present,

### Sabbatical leaves:

University of Cincinnati, Cincinnati, Ohio, USA, 1976 Mass Spectrometry Laboratory, Ciba-Geigy, Basel, Switzerland, 1989 Pierre and Marie Curie University, Paris (summer), 1993 National Institute of Health, Bethesda, MD, USA (summer), 1995 National Institute of Health, Bethesda, MD, USA (summer), 1997 National Institute of Health, Bethesda, MD, USA (summer), 1999

#### Major departmental responsibilities:

Chairman of the Department of Chemistry, January 1986–December 1988

### Research

### Mass Spectrometry

Organic Mass Spectrometry: Ion chemistry of organic gas-phase ions. Stereochemical effects in the fragmentation of gas-phase ions. Mechanistic studies of dissociation processes of gas-phase ions. Structural studies by mass spectrometry.

### Asher Mandelbaum

(February 2008)

I was born in Krakow, Poland, in late December 1934. World War II broke out when I was nearly 5 years old and it changed the entire course of our lives. When the war erupted my father happened to be in Lvov on business. The city was occupied by the Russians and he was exiled to labor camps in Siberia and Uzbekistan. When Krakow was occupied by the Germans (in September 1939) my mother took me and my younger sister Dina to her parents in Koszyce, a small town about 50 km from Krakow.

Edicts against the Jewish community were published a short time after we arrived in Koszyce. The synagogues were closed and it was forbidden to pray together even in private homes. The Jewish school was closed and the education of Jewish children was prohibited in any form or setting. A short time after the occupation Jews became obliged to wear identification tags (a blue Star of David) on their sleeves. Towards the winter of 1941 the occupying forces confiscated all furs belonging to Jews and these were transferred to the German army stationed on the Eastern front. Various limitations were set on the employment of Jews, and the Jewish Committee was often required to pay fines. In the second half of 1942 all Jews residing in the cities and towns were concentrated in ghettos and concentration camps and their final extermination began. The Jewish Committee in Koszyce succeeded in postponing the deportation a couple of times until November 1942 by bribing the German commander.

In the period preceding the deportation, my grandfather, Yaakov Kaminsky, contacted a young Pole, Janek Młynarczyk, from the small village of Górka near Koszyce, who was ready to help save our family. To this day I cannot understand why Janek agreed to assume this dangerous and difficult task. The German authorities declared that helping Jews would result in a death sentence, both for those guilty of providing assistance and for their entire families. I have never received a satisfying answer to this conundrum despite my correspondence with Janek and his wife and a long conversation with them in Poland in 1998. His great appreciation for my grandfather was apparent and he repeatedly declared that he did what he did "because Pan Kaminsky said so". On a night in November 1942 the Jews of Koszyce were gathered in the town square and taken in trucks to a concentration camp, where nearly all of them were killed. The night before, Janek had transferred our family to his uncle's house in the small village of Szczelce. Fifteen people crowded together into a dark cellar: my mother, my 4 year old sister, myself (8 years old), my grandmother and other relatives. My grandfather, who was a member of the Jewish Committee, did not join us. He was taken to the concentration camp with the other Jews of Koszyce, where he died.

After about two weeks in Szczelce, Janek transferred us to the ghetto the Nazis had formed in the town of Bochnia, about 50 km from Krakow, where we spent the winter of 1942/3. I remember that the ghetto was terribly crowded, both in the living quarters and in the streets. The winter was extremely cold and means of heating were very scarce. There was also a constant shortage of food. The most difficult problem was our uncertainty regarding the future. Towards the spring of 1942 rumors abounded about the approaching liquidation of the ghetto.

Despite the dangers Janek succeeded in entering the ghetto and consulting with my grandmother and mother concerning our next moves. In light of the constant rumors, it seemed that it would be dangerous to remain and we must find another solution. They felt that my grandmother, my sister and my grandmother's sister looked sufficiently Polish to enable them to acquire false identities and present themselves as Poles in distant locations, where there was no reasonable chance of meeting acquaintances. Equipped with false documents they moved to Warsaw, where they remained until the partisan revolt in 1944. However my mother and I looked Jewish and could not avail ourselves of this solution so we needed to find a hiding place.

The first stage was to leave the ghetto. Janek succeeded in bribing a German officer (not an easy adventure, but it is beyond the scope of this short summary), and on an evening in May 1943 I left the Bochnia ghetto with my mother in a German army car and we arrived on the outskirts of the village of Szczurowa, between Bochnia and Koszyce. Janek was waiting for us nearby and he led us in the dark to a small wooden hut belonging to his relatives. The hut consisted of only one room with a little attic and it was occupied by three brothers. They used the only downstairs room and we received the attic, which could only be reached by ladder. Janek was aware of the anti-Semitic views of his fellow Poles (and of his own relatives) and he introduced us as political refugees fleeing the occupying forces and warned us to avoid any mark of Jewishness.

One evening, after we had been in this hiding place for about two weeks, my mother heard the brothers discussing us at supper and sensed that they suspected we are Jews. She decided that it would be dangerous to remain there, and when our hosts fell asleep we left the hut. The only person we could trust was Janek and we set out to find him. After walking all night on unknown and unmarked roads, encountering German patrols which luckily did not identify us, we reached Górka early in the morning. We were forced to ask local villagers working in the fields for directions to Janek's home. Our ordeals throughout this day are engraved in my memory as one of the most terrifying days of my life, although it will not be possible to relate everything that happened to us here.

At this stage Janek began seeking a permanent hiding place for us. Over the next three-four weeks, he would find us temporary hiding places in friends' or relatives' barns or cellars for 2- 3 days, where we hid in haystacks or heaps of litter; however I do not remember this period in detail. I remember the fear of our "hosts" (which was justified, considering the death penalty for those caught harboring fugitives and for their families), who brought us food upon our arrival (usually a 3-day supply of bread and water) and hurried to leave home for the duration of our stay. Sometimes we found that mice had eaten from our bread. I also remember the terrible fear of moving from one hiding place to another, in the dark, at the age of  $8\frac{1}{2}$ .

In July 1943 Janek found us a hiding place in the remote village of Rachwałowice, approximately 3 km from Koszyce, at the home of a church organ player, Mr. Elias, who was acquainted with my grandfather. From the beginning of the war Mr. Elias' daughter, son in law and their 3 daughters, the Skowron family, had been staying with him, having fled Warsaw when the war broke out. Mr. Skowron had previously been employed as a civil servant and he had been a member of the fascist Polish National Democratic party (EnDe). During the war he was active in the right-wing partisan Resistance "Armia Krajowa" known for its anti-Semitism, and he stored arms and ammunition for the partisans in a small shed in his father-in-law's house. Discovery of this shed and its contents by the Germans would undoubtedly have brought the death penalty upon the entire family. Therefore Janek assumed that it would be easier to convince them to hide us in the shed. At first Mr. Elias strongly objected, but upon hearing that my mother was the daughter of Mr. Kaminsky he gradually relented. The fee that was promised also contributed to his decision.

I will attempt to describe the designated hiding place. It was about 2 meters long and a maximum of 1½ meters wide. A wide wooden plank slightly less than 2 meters long was placed on two barrels along the length of the shed, covered by a few sacks of straw. This plank, which occupied most of the shed, served as our bed, table, bench, and everything else. Opposite this bed, along the shed's wall, were shelves heaped with arms and ammunition. Above the "bed" Mr. Skowron installed a larger shelf for us, which served us as a table and work surface. He blocked the entrance to the shed with planks and hid it from strange eyes. He also uprooted a few planks from the floor of the dining room, which bordered with the bunker, and he dug a small tunnel under the floor through which it was possible to crawl. This tunnel served for the daily passage of food and water and the removal of our waste. The bunker's "furniture" ("table", "bed" and shelves) and the tunnel opening left hardly any free space to stand or walk.

Our bunker allowed no light. Rachwałowice, similar to most Polish villages at the time, was not connected to the electricity network (there was also no running water nor telephones), and there was a shortage of fuel due to the war. Skowron brought us a carbide lamp, which gave off white light which enabled some activity in these harsh conditions. The operation of this lamp was probably my first chemical experiment:

 $CaC_2 + H_2O \rightarrow HCCH + CaO$ 

### Calcium Carbide acetylene

A tap is opened, causing contact between carbide and water and the formation of acetylene, and when the latter burns it produces an open light. Under normal conditions I would not advise using such a lamp for lighting purposes, certainly not in a small closed room. At the time the carbide lamp was an essential part of our mental survival.

We hid in this bunker for a year and a half, from July 1943 until the second half of January 1945. We had 2 books, the first was the famous classical poem "Pan Tadeusz" by Adam Mickiewicz, and the second was a geographical encyclopedia. I read both books many times, and by the time of our liberation I almost knew "Pan Tadeusz" by heart. Another occupation was knitting sweaters and socks for the Skowron family and for ourselves. I acquired a not insignificant proficiency in this craft. Under Janek's direction Mr. Skowron bought the wool in small quantities in a

distant town in order to avoid attracting the neighbors' attention. He used this method for other shopping as well.

Throughout our prolonged stay in the bunker my mother devoted much time to teaching me as many subjects and topics as she could in the absence of textbooks. She taught me the Hebrew alphabet, and also prayers she knew by rote. During the months of September-October and April she asked Mr. Skowron about changes in the shape of the moon, and accordingly estimated when Yom Kippur and Pesach occurred; she fasted on the former and avoided eating bread during the latter.

Due to Mr. Skowron's membership in the Resistance we received news bulletins with information about the various fronts. I remember our joy on a morning in June 1944, when we heard about the allied forces' landing in Normandy (6.6.1944). From here on the news we heard signaled that the end of the war was near and with it the end to our troubles.

One night we heard the frightening noise of an airplane. Skowron told us that that night a British plane had landed near the cemetery of Rachwałowice, unloaded a shipment of arms and a high-powered transmitter, and left carrying one of the Resistance leaders. Skowron brought some of the equipment to our bunker, luckily not assisted by his comrades. This sign from the free world gave us a wonderful feeling but we were also frightened of possible German searches as they had probably noticed the planes as well.

The Red Army offensive began towards the end of 1944. After waiting for about a year on the eastern bank of the Vistula (Wisla), they crossed the river at a number of points and drew near us. The German army started to retreat, and many trucks passed Rachwałowice on their way west, very close to our hiding place. Skowron reported with concern that the Germans wished to establish their headquarters in Rachwałowice, and that the village chief had offered them our house. For 3 days the Skowrons deliberated how to solve the problem, which could put their entire family (and us too) in risk of facing a firing squad, although we were so close to liberation. Luckily, the German commander thought that the house of the village chief would be a more suitable headquarters, thus solving one of the most serious threats to our survival. But the danger was not yet over. Two German officers were stationed at our house, and the room they chose was the dining room that bordered with our bunker, into which our tunnel opened. It was terrifying to hear the hobnailed boots of our neighbors on the other side of the wall. The Skowrons were frightened too, lest the

officers would discover the tunnel's opening, and reduced their contact with us to bringing water and food and removing our waste.

In early January 1945, close to my 10<sup>th</sup> birthday, the German army began their panicked flight westwards. In our area they suffered a shortage of fuel, and in their flight they used horses and carts confiscated from villages on the way. Although the bunker was impermeable, we heard them prodding the horses past our house for three days and three nights.

Krakow was liberated by the Red Army on January 19<sup>th</sup> 1945, and the Russians arrived in Rachwałowice a few days later. Skowron made a small loophole for us in the blocked opening of the bunker so that we could see the Russian tanks passing through the village. Only the next day did Skowron allow us to leave the bunker for the dining room, and even then only for limited periods of time. Our elation upon our liberation is indescribable and I'm not certain that I can faithfully reconstruct my feelings on this special occasion. Our renewed encounter with daylight after sitting for a year and a half in a sealed bunker was a very special experience. I remember how difficult it was for me to leave the window, the view of the white winter country and the blue sky.

Our joy at the war's ending was mixed with serious questions in regard to our first steps after leaving the bunker, having been completely severed from normal life for such a long time. Janek advised us to wait a couple of days at the Skowrons, to see what would happen, and then to move to Koszyce, at least temporarily. He spoke to the family that lived in my grandfather's spacious house (the Glowacz family), and received their consent for us to stay there for some time. In the meantime the Skowrons did their utmost to keep our existence hidden from their anti-Semitic neighbors, who might have killed them and us all.

A couple of days after the liberation, early on a cold and snowy Sunday, when all the villagers were still asleep, we left on foot for Koszyce, a distance of about 3 km. After a while I could not continue walking. A year and a half in a small bunker without movement had caused my muscles to atrophy, making it very difficult to walk. Skowron carried me for most of the way (I did not weigh much), and thus we arrived that morning in Koszyce. Janek directed my grandmother and my sister (whom he had also cared for throughout that period) to Koszyce as well and took us all by horse and carriage to Krakow, a distance of about 50 km. A few holocaust survivors who had succeeded in remaining alive gathered together – some had survived in hiding, some

with the Resistance or in concentration camps. Immediately after the war, the refugees established a Jewish committee which took care of all their needs, for example living quarters, jobs, reactivating synagogues, founding and operating Jewish schools, etc. At the Jewish school "Tarbut" we studied intensively for three years in order to complete all the material we had missed during the war. In the summer of 1946 my father returned from Russia and our family was reunited after being separated for almost seven years.

Janek's contribution to our rescue was crucial, although, as I have mentioned, to this day we do not know the motivation for his actions. Throughout the entire period he was exposed to daily life-threatening danger in his attempts to find us short- and long-term hiding places, transferring us from one hiding place to the other, by foot or by carriage, and locating money that my grandfather had deposited with neighbors with whom he was befriended, to pay for our monthly lodgings and my grandmother and sister's expenses in Warsaw. Yanek and the Skowrons were acknowledged as righteous gentiles by "Yad Vashem".

In early 1948, a short time after my bar-mitzvah, our family received permission to leave Poland. In March of that year we left Poland for France. After a short stay in Paris we traveled to the sea port of Marseilles, where we joined a Jewish Agency immigrants' camp. We arrived in Israel on the French boat "Providence" as part of the "Aliya D", equipped with a real Polish passport, a real visa to Bolivia (purchased from the Consul in Paris), a false transit visa through Palestine and the address of an imaginary relative in Tel-Aviv, whom we were allegedly going to visit on our way to Bolivia. We were on the deck of the boat on Friday, Iyar 5<sup>th</sup>, May 14<sup>th</sup>, 1948, when we heard over the loudspeakers that the State of Israel had been established. Our joy was subdued; we all knew that a new war was about to begin, three years after the terrible last war had ended. The next day we reached the Port of Haifa, from where we were taken to the immigration camp in the sands of Kiryat Shmuel (present day Kiryat Yam), and about 2 weeks later we moved to a rental apartment in Haifa.

At this point a new chapter began in my life. In the autumn of 1948 I began my studies in Israel at the "Netzah Yisrael" school in Haifa, where I enrolled in the 8<sup>th</sup> grade. During the years 1949-1953 I studied at the religious "Yavneh" high school in Haifa and after my military service during 1953-1955 I began studying at the Technion Faculty of Chemical Engineering.

During the second year of my studies I participated in two courses in organic chemistry given by the late Prof. David Ginzburg. The courses were jointly addressed to students of the Faculty of Chemical Engineering (about 70 students) and students of the Faculty of Sciences – Chemistry (about 10 students). Professor Ginzburg succeeded in transforming the study of organic chemistry into the most fascinating experience, and as a result I decided towards the end of my second year to transfer to the Faculty of Sciences – Chemistry, which later became the Faculty of Chemistry.

In November 1958 my life changed when I married Leah Faber, who was (and still is) my sister's best friend. In 1960 and 1961 our two sons, Moshe and Yitzhak, were born, and in 1967 and 1969 our two daughters, Aviva and Ilana.

In 1959 I finished my undergraduate studies and received my B.Sc. About a year later I received the degree of Masters of science for developing a new method for synthesis of triphenylenes, supervised by Dr. Michael Cais, a new faculty member. Prof. Cais served as the supervisor also for my doctorate thesis (1960-1963) on isolating and determining the structure of the components of the narcotic plant Catha Edulis. In this research we used mass spectrometry to determine the molecular weight and structure of one of the plant's components. Since in 1963 there was no mass spectrometer with the capacity to measure high molecular weights (up to 1000 dalton) in Israel, the mass spectra were measured in the laboratory of Prof. Carl Djerassi at Stanford University in the U.S. Following the success of this method for determining the structure of organic materials, Prof. Ginsburg decided to establish an organic mass spectrometer was purchased, and I was offered to serve as a faculty staff member with the degree of lecturer and as head of the new laboratory.

One of the first projects we performed with the new equipment was investigating the fragmentation of various derivatives of morphine, provided by Prof. Ginzburg from his large collection. Amongst the materials examined were a number of stereoisomers differing in the configuration at position 14 of the morphine system. To our surprise, the mass spectra of the stereoisomers were entirely different, and the differences indicated stereospecific fragmentation mechanisms. The standard opinion at the time was that due to the relatively high energy in the electron ionization process, the ions created undergo isomerization to the most stable structures, and therefore there should be no expectation of differences between the mass spectra of stereoisomers. This view

was disproved by our observation, which later led to many studies performed by the Technion's mass spectrometry group.

Further details of my academic career are documented in my personal file at the Schulich Faculty of Chemistry in the Technion and on its Website.

# **EVGENY E. NIKITIN** Born Russia, 1933



Ph.D., Physics, University of Saratov, 1955-1958

Past Positions:	
Research Scientist Institute of Chemical Physics Moscow	1958-96
Professor of Chemical Physics, Physical Technical Institute Moscow	1980-91
Trolessor of chemical raysies, raysical reennear institute, woscow	1700 71
Technion Positions:	
Barecha Fellow	1991-93
Professor	1993-2001
Professor Emeritus	2001 to date
Sabbatical leaves	
Instituet fuer Physikalische Chemie, Goettingen, Germany	Fall 1994
Eidgenossische Technische Hochschule, Zuerich, Switzerland	Fall 1999
Tohoku University, Sendai, Japan	Summer 2002
Member of Academies and Societies:	
Deutsche Academie der Naturforscher Leopoldina, since 1977	
European Academy of Arts, Sciences and Humanities, since 1983	
International Academy of Quantum Molecular Sciences, since 1987	
Short-term Positions as Visiting Research Professor	
Institut fuer Physikalische Chemie, Goettingen, Germany 19	65, 72, 79,89,90
Department of Chemistry, Universita di Roma, Italy	1978
Laboratoire de Collisions Atomiques, Orsay, France	1989
Department of Chemistry, University of Southern California, USA	1990
Department of Chemistry, California Institute of Technology, USA,	1991
Department of Chemistry, Stanford University, USA	1992, 93
Department of Physics, Universite Paris Nord, France	1992
Department of Chemistry, Stanford University, USA	1993
Department of Physics, Aarhus University, Denmark	1993
Laboratoire de Collisions Atomiques, Orsay, France	1993
Institute for Molecular Science, Okazaki, Japan	1994
Department of Chemistry, University of Bielefeld, Germany	1995

Laboratoire Aime Cotton, Orsay, France	1995
Phillips Laboratory and University of Southern California, USA	1996
Dept. Chem. /Chem. Eng., Universita di Roma, Italy, 1996,98	8,00,02,04,07
Institute for Atomic and Molecular Physics, Cambridge, USA	1996,99
Zentrum fuer interdisziplinaere Forschung, Bielefeld, Germany	1997,98
Laboratoire de Physique des Lasers, Universite Paris XIII, France	1998
Tohoku University, Sendai, Japan	2000
MPI fuer Biophysikalische Chemie, Goettingen, Germany 2000,0	1,02,03,04,05,06
Department of Physics, Oklahoma University, USA	2001
Department of Physics, Latvian University, Riga, Latvia	2002, 04, 07
Research Centre on Bose-Einstein Condensation, Trento, Italy	2003 -
Humboldt Award Fellow, MPI, Goettingen, Germany	2008

# **Evgeny E. Nikitin**

(December 2008)

# Di giovinezza il bel purpureo lume\*

Tasso

(\*The lovely purple light of youth)

Here is a part of my curriculum vitae as it appears in the manpower Department of the Technion:

1. 1950–1955	Saratov University, Department of Physics, Saratov
	Graduated from Saratov University, USSR
2. 1955–1958	Post graduate study at the Institute of Chemical Physics,
	Academy of Sciences, Moscow USSR
3. 1958–1965	Junior Research Fellow at the Institute of Chemical Physics,
	Academy of Sciences, Moscow, Doctor of Sci. thesis.
4. 1959–1961	Assistant Professor, Department of Chemistry,
	Lomonosov State University, Moscow, USSR
5. 1965–1971	Senior Research Fellow at the Institute of Chemical Physics,
	Academy of Sciences, Moscow USSR
6. 1966–1992	Professor of Chemical Physics at the Institute of Physics and
	Technology, Moscow, USSR
7. 1971–1992	Head of a research group at the Institute of Chemical Physics,
	Academy of Sciences, Moscow.

Now the comments follow.

1. Saratov is a big industrial town situated on the Volga river. This river is the largest in Europe and it flows into the Kaspian sea. The Russian expression for that, "Volga vpadaet v Kaspiyskoe more" is an example of a truism that serves as a test for mental ability of a person. I attended high school during the World War II. Saratov was not occupied by Germans, the front line was about 300 km from the city, extending to the North from Stalingrad. Our toys during childhood were splinters from the shells fired at German airplanes and gadgets from the ammunition (rifles, machine-guns, field-guns and tanks) sent to Saratov for repair (if they were Russian)) or for remelting (if they were German). The city was bombed, especially heavy in the industrial area with the biggest European part of the country refineries, and I have witnessed huge fires and explosions. After high school I entered the Department of Physics of Saratov University. That was a golden time for achieving knowledge and we enjoyed brilliant teachers. Our education was quite versatile, and this allowed one

to choose, after graduating, different fields of activity, in education, industry or academia.

2. I have chosen the latter, and moved to Moscow where I became a postgraduate student in the Institute of Chemical Physics of the Academy of Sciences. The opportunities to learn more were immense: from group seminars and colloquiums in the Department of Chemical Kinetics and Combustion in our Institute, headed by Professor V.Kondratiev, to the famous Landau seminars in the neighbouring Institute of Physical Problems. On top of this, we, young scientists, were welcome to attend various conferences and meetings throughout of the country. Kondratiev introduced me to the problem of the energy exchange in chemical kinetics and turned my attention to the work by Landau and Teller on this subject. Let me dwell on this problem since it can be explained in simple wording and example. Experiments showed that a diatomic molecule acquires and looses its vibrational energy in collisions quite ineffectively and that this has very dramatic implications for the gas dynamics, combustion, molecular lasers, etc. For instance, when you pay for your plane ticket, you notice in the price list a separate entry for kerosene. Kerosene is used as a fuel to provide the trust in the engine that pushes the plane forward by throwing gas particles backward. A part of the released energy (as well as a part of your money) is useless since it is stored as molecular vibrations, which does not provide thrust. The situation is even worse: the vibrating molecules survive in the wake of the engine exhaust and are the source of radiation that provides the guideline for a potential missile to be aimed at an airplane. The low efficiency of the vibrational-translational energy exchange is also a source of non-equilibrium effects in gas dynamics, e.g. in the creation of the protecting shield in front of the space vehicle upon its return to the Earth. Nonetheless, let us turn from the practical cases to the purely theoretical problem of energy exchange in molecular collisions. Landau and Teller were the first who have addressed the problem and have explained, in their paper of 1936, the low efficiency of the energy exchange. In the framework of classical mechanics (assuming that the energy release from the vibrating molecule is small compared to the thermal energy of translational motion) they derived the famous expression for the temperature dependence of the energy exchange rate. This expression for non-equilibrium conditions parallels the Arrhenius expression for temperature dependence of the rate coefficient for chemical reactions. Landau and Teller actually found the rate of a process, which proceeds in violation of the famous

adiabatic principle formulated, by Ehrenfest and Einstein in the framework of classical mechanics. Note in passing, that the adiabatic approximation in its quantum formulation by Born and Oppenheimer forms a basis for many theoretical approaches, in particular in computational quantum chemistry.

At this period, my interests were focused on the problem how the low efficiency of translational-vibrational energy exchange may affect the rate of chemical reaction. I have developed a model, which explained the negative temperature dependence of the preexponential factor of the rate. Publication of my paper on this subject in the Journal of Chemical Physics in 1958 brought unexpected consequences: a thick envelope with preprints addressed to Professor E. Nikitin arrived from E. Montroll and K. Shuler in the U.S. with outlines of a similar model. It took some time for Manpower Department of the Institute to find out who this "professor" was, but as a result I was summoned to our Director, Professor N. Semenov, to explain what it was all about. This was the first step in what is now known as a weak-activation mechanism of unimolecular reactions. My findings, together with further developments by other researchers, were summarised in my first book.

3. Slowly, my interest turned to the general theory of nonadiabatic transitions, and of course, to the Landau-Zener so-called semiclassical model. I succeeded to generalize and to develop, with the help of my graduate students, V. Bykhovski and M. Ovchninnikova, a fully quantum version of this model. A funny episode happened during presentation of my work at a conference at the Physics Department of the Leningrad University. At the beginning of my talk, the famous quantum physicists, Prof V. Fock, told in a low voice to a referee of my thesis that he would have tried to solve the problem in his beloved way, via the Airy-function transformation. The referee answered that Nikitin did it exactly in this way, after which Fock switched off his hearing aid and gave me the highest mark.

During my scientific carrier in Moscow I did several, rather sporadic, travelling to Europe and the U.S. where I made acquaintance with renowned scientists. During my first trip to the West, to the Quantum Chemistry Conference in Oxford in 1960 organized by C. Coulson, I met R. Pauncz and J. Jortner. I can not refrain from showing a picture of that time.



In connection with this, Joshua Jortner writes in 2000: "Dear Evgenii, Here is the photograph from the near past. The conservation principle worked and we did not change at all". The reader may amuse himself trying to identify three persons who are now retired members of the International Academy of Quantum Molecular Science. Later on, I have met other persons. Those who visited the Technion and our department as distinguished guests were A. Dalgarno, A. Kuppermann, R. Marcus, J. Troe, R. Zare and A. Zewail. I am also recalling two happenings that amused me. Both are connected with Dudley Herschbach (D.H.) whose sense of humour is quite appealing to me. The first episode is related to the Faraday Discussion Meetting, Cambridge, 1962, where I have presented my generalization of the Landau-Zener model. Dudley writes (Annu.Rev.Phys.Chem. 51, 1 (2000)) "A few weeks later (after Faraday Discussion, Cambridge, 1962), back at Berkeley, I scowled when an FBI agent appeared at my office to ask about what information I might have exchanged with Nikitin at the Faraday meeting. Thinking it best not to risk telling the agent about molecular transitions induced by crossing of potential surfaces, I simply assured him that conversations with Nikitin dealt only with physical chemistry. The agent glowered when I emphasized that Nikitin was a brilliant scientists". The second one refers to my visit to the French Consulate in Boston, in 1969, accompanied by D.H., where I applied for a two-day visa allowing me to stay in Paris on my way back from NY to Moscow. People in the Consulate were stunned to see a real Russian person in front of them (remember: 1969), and their behaviour was quite hectic. D.H.

put the end to the uncertainty declaring in his low voice: *I am his bodyguard*. This solved the problem. My next visits abroad were to Goettingen and Konstanz, Germany, where I have met J.Troe, with whom I am collaborating up to now.

One of the scientific meetings of this period is very memorable. This was a conference "Shape of Spectral Lines" in 1964 on board of the ship that sailed down and up the Enisei river (it flows from the Baikal sea to the North Ocean). There I met Lena, Elena Dashevskaya, who was a researcher in the Institute of Terrestrial Magnetizm and Radio Wave Propagation, Academy of Science, Moscow. Since I have decided not to follow the custom of telling gossipy stories about private life, I simply tell you that from this time we were together and even worked in closely related fields. Lena pushed me to learn more about the density-matrix formalism and to apply it to the collision problems. The latter had a paramount importance in proper understanding of the processes in quantum magnetometers, the devices that are able to measure extremely weak magnetic fields, either from celestial objects using the space vehicles, or from human heart beats measured in hospitals (I want to boast that Lena was a member of the group that sent their magnetometer toward the Moon to measure its magnetic field).

4. My teaching in Moscow was interesting since teaching a big audience of students was a new experience for me. It also exposed me to hard work, teaching and examinations in the University in parallel to research in the Institute of Chemical Physics. It did not though last long, and eventually I moved to the Institute of Physics and Technology, which was closer to my interests and where I could vary my courses.

5. During this period, I actively worked, together with Lena, still at the Institute of Magnetism and Radio Wave Propagation, and with my younger colleagues in the Institute of Chemical Physics in variety of subjects: general theory of nonadiabatic effects, application of the models of nonadiabatic coupling to inelastic atomic and molecular collisions, theory of vibrational-rotational-translational energy exchange, statistical approach to the unimolecular reactions and weakcollision mechanism in non-equilibrium chemical kinetics. I visited a number of conferences abroad, a lot of scientific meetings in Russia and paid several research visits to some other institutions. The most memorable of these were my visits to the Institute of Chemical Kinetics and Combustion, Novosibirsk, being hosted by my friend of old times, Professor Anatoly Burshtein, who is now at the Chemical Physics Department of the Weizmann Institute of Science, Rehovot. 6. Lecturing in the Institute of Physics and Technology, Moscow, has given me the opportunity to invite students for post-graduate study at the Institute of Chemical Physics.

7. During this period my research group at the Institute of Chemical Physics worked in full force and did not require much of my attention. This allowed me to spend more time travelling, and we, Lena and me, even went abroad together (a quite rare occasion, possible only in Gorbachev's period). In this way, we have met Joshua Jortner at the Summer school on Molecular Dynamics in Avila, Spain, and Nimrod Moiseyev in Orsay, France, at my talk there. In 1990, we got an invitation from the University of Southern California (from the Department of Chemistry for me, and from the Department of Physics for Lena). We have combined this visit with our visit to the Institute of Physical Chemistry, Goettingen, where I began my cooperation with Prof. J. Troe in the framework of the Humboldt Award Research Grant.

To conclude this part, by this time I was equipped with Landau-Lifshitz course in theoretical physics as a background, with the knowledge of chemical kinetics and molecular dynamics from my teachers in the Institute of Chemical Physics, Moscow, Prof. V. Kondratiev (Director of the Department of Chemical Kinetics and Combustion) and Prof. N. Semenov (Director of the Institute) and with my experience in the field of the theory of nonadiabatic transitions and non-equilibrium gas kinetics and statistical models of unimolecular reactions. I also had interesting scientific and social contacts with Prof. Y. Zeldovitch. My views and approaches to the problems that were of interest to me are presented in my four books [1-4].

## En vieillissant on devient plus fou et plus sage\*

La Rochefoucauld

(\*As we grow old, we become more foolish and more wise)

One morning in the fall of 1990, in my temporary office in the University of Southern California, I got a telephone call from Prof. Zeev Tadmor, the President of the Technion who invited me to visit the Technion with the perspective to join the Department of Chemistry. In November 1990, I received the formal invitation from Prof. Z. Berk, the Vice President for Academic Affairs, to join the Technion as a "Barecha Fellow" Full Professor. This Fellowship, originated from an unanimous person in Switzerland, provided initial support for the research, as well as a long-term loan, without interest, towards the purchase of an apartment. In this period, I have nice long phone conversations with Joshua Jortner, the President of the Israeli Academy of Sciences, who presumably was behind the whole idea. He urged us, my wife Elena Dashevskaya and me, to visit Israel on our way back to Moscow after our stay in the U.S, notwithstanding the difficulties created by the Gulf War. We succeeded to do thus by cancelling and booking various flights, which finally brought us to Tel-Aviv.

Upon our arrival at Haifa, after visiting Jerusalem, Tel-Aviv and Rehovot Institutions, we were hosted by Prof. Nimrod Moiseyev, who introduced us to the Dean, Prof. Jacob Katriel and arranged all the official visits and also showed us some places in Israel. He also arranged my talk at the departmental colloquium, where I have spoken about the theory of the energy transfer in molecular collisions (mark this point, I will touch it later). During this visit, I contacted my future colleagues of Physics and Chemistry in the Technion and other Institutions, and understood that I can learn a lot of new things from them. At the Department of Chemistry I browsed through the synopsis of lecture courses, which I was supposed to teach, and found them quite appealing; moreover I was informed that I could suggest new courses. I also talked to graduate students, and found them very motivated and interested in that branch of science, which I was fond of - the application of quantum mechanics to atomic and molecular processes. I therefore decided to accept the offer, being strongly supported in this decision by my wife and the family of her daughter, Irina, who already lived in Israel for a number of years. Our move from Moscow to Haifa would bring no harm to the activity of my group in the Institute of Chemical Physics. which was routinely established by that time. My younger colleagues became independent in their research and I felt quite sure regarding their scientific development. It looked quite natural that Dr. S. Umanskii, my former undergraduate and graduate student, took the position of Director of the group.

The formal letter of my appointment to the Technion I got on July 8, 1991 (signed by the President Z. Tadmor), and the confirmation of my permanent position on May 3, 1993. Since then I have enjoyed working at the Department of Chemistry, now the Schulich Faculty of Chemistry, and have taken the opportunity to create easy contacts with the scientific community of the world.

Before I begin to describe my teaching and research projects, I would like to mention the most enjoyable happening during my stay in Technion. These were my scientific and social contacts with Nimrod Moiseyev, whom I met, as I have noted earlier, for the first time at my talk in Orsay in the spring of 1990. He actually appeared as a messenger for the Technion describing in general the activities in the Department of Chemistry. Upon my start at the Technion, he was frequently my adviser on different issues, some of which were quite new to me. I was always welcome to come to his office for scientific discussions, to ask questions and expressi my opinion. His seminars were quite revealing, and the conferences, he organised, were full of the atmosphere of free discussions. I continue this practice up to the present, meeting with pleasure my former undergraduates, now working with Nimrod. I am also happy, that when Nimrod served as a dean of the Department, my status changed from Full Professor to Professor Emeritus.

From the beginning of my work at the Technion, I got a dilemma: either to start learning Hebrew or preparing courses for teaching. I have decided for the latter, and as a result I am still half-illiterate in Hebrew up to now. An interesting suggestion by one of my Israeli friend to overcome threshold in Hebrew with the help of a nice Only-Hebrew-Speaking-Girl-Haver" was rejected by my family. Thus I taught in English, which was OK for graduate courses. The teaching in the Technion (and in Israel in general) is different from what I was used to in Moscow where I normally would stand in front of the blackboard with chalk, and the students would make notes following my presentation. In Technion I can use all the techniques, which help presentation (overhead projections of transparencies or computer files), but the students basically remain spectators, expecting to receive from me printed lecture notes allowed me to get a fresh look at my lectures every new semester. The list of my courses, which were periodically corrected and modified is given below.

Some topics of molecular dynamics Theory of molecular collisions Quantum Chemistry II Quantum Chemistry III Statistical Thermodynamics Chemical Kinetics Winter 1991. Spring 1992 Winter 1992, 93,95, 96, 98 99, 2000 Spring 1993, 95, 97,99, 2000,01 Spring 1994, 96,1997 Winter 1996,1999

Electrode processes	Winter 1997
Collision Processes in Gases and Plasma	Spring 1998, 2000

I complemented my presentations with a list of problems, which I suggested for solution to my students. We had discussed the solutions at the exams. I continued my teaching also as Emeritus. Now I was free in choice so that I decided on something new:

Density matrix formalism	Winter 2001
Theory of fluctuations	Spring 2002

I am passing now to the research and again I begin with a problem. A rather unexpected difficulty arose and my wife could not be in my group though she was working with me all the time since we have met in 1964. A solution to the problem was rather trivial: Lena worked as a researcher within the Shapiro, Giladi and Kamea programs with Prof. I. Oref, and they conducted their work in cooperation with me and published the papers with me as a co-author. Lena's authorship will appear in many of our papers, not necessarily published with I. Oref.

The beginning of my research work in the Technion was related to continuation of my work with scientists from the Institute of Chemical Physics, A. Maergoiz and V. Ushakov, who periodically stayed with Prof. J. Troe in Goettingen. We have conducted the theoretical work on the complex formation mechanism following the idea of the so-called adiabatic channel approach put forward by J. Troe. This activity is vividly described in the autobiographical sketch written by Troe in his Festschrift issue of Physical Chemistry and Chemical Physics [4].

However, my main interest lied in the development of the theory of nonadibatic transitions and energy exchange in molecular collisions. This issue was quite important, and that was a reason that I was invited three times to the Department of Space Engineering in USC and even to the Edwards Air Base (both in California) to discuss problems related to the non-equilibrium effects in gases. Now, what interested me was a passage from classical picture of energy exchange to its quantum counterpart. If found, it would enable to use classical dynamical quantities for calculation of quantum properties. At this moment, I expect a comment from my colleagues and readers that at present state of the art of the computational technique one can directly calculate desired quantities by solving Schroedinger equation, and not bother with classical-quantum correspondence. My answer to this comes from the conviction in the truth of the idea expressed by Zeldovitch, that "desire to know and to understand is a basic feature of the mankind and thus it can not be questioned". I simply wanted to understand the problem, and this is undisputable. Full stop. Maybe I unconsciously followed the Occam's Razor principle (1300 AD): *The scientific and philosophic rule requiring that the simplest of competing theories be preferred to the more complex or that explanations of unknown phenomena be sought first in terms of known quantities* (Webster Dictionary).

Having successfully defended my point, I now describe how it unfolded. During my visit to Stanford University, I succeeded, together with R. Zare, in understanding the quantum/classical relation for the oscillator strengths for diatomic molecules [6]. After some time, during my visit to California Institute of Technology, I have applied, together with A. Zewail and his colleagues, this idea to explain certain trends in the vibrational predissociation rates of van-der-Waals complexes [7]. Later on, we worked, together with my graduate student Y. Karni and then with J. Troe, on a more deep relation between the classical and quantum pictures of vibrational predissociation. Can you imagine, that a simple single-step event in quantum picture (simple, as soon as we believe that we understand Quantum Mechanics), corresponds, with certain caveat, to the complicated multi-step classical picture of chaotic motion? Quantum tunnelling through chaotic classical see – that would be a proper description coming from crossing the sea on the way from Egypt [8]. Though all this was quite interesting and revealing, the problem of energy-exchange in collisions was still pending: how a large amount of energy, suddenly released from a molecule, can be accommodated by a low-energy thermal reservoir? My preliminary thoughts on this subject were presented at the Symposium "Gas Phase Chemical Reaction Systems" [9] ("100 Years after Max Bodenstein"), and in the Proceeding Book I was put on the "Bodenstein family tree" together with my teachers and colleagues.



Still, we continued to contemplate about this topic and wondered why is it that in some experiments involving polyatomic molecules the apparent efficiency of the energy exchange was quite high. And we, helped by my graduate student I. Koifman-Rosenblum, found the answer. The translational energy first transforms into rotational energy of the molecule (an event occurring during a collision) and then the rotational energy is dissipated into the vibrational reservoir (an event occurring during a long time between collisions). This dissipation is basically equivalent to the process that stopped the intrinsic rotation of the Moon and made it to face the Earth with the same side all the time.

Meanwhile, our interests were attracted by a nice cross-beam experiments conducted in Tohoku University, Sendai, Japan. On a quite complicated machine, they were able to measure the energy dependence of an inelastic cross section from the very threshold (which is very rare) in collision of excited mercury atom with nitrogen molecule. We liked the result, since it represented a very rare manifestation of the conical intersection – yet another example of breakdown of the adiabatic approximation. Along with this, we began, with our Japanese colleagues, a careful study of dynamics and spectra of wan-der-Waals complexes of excited mercury atom with rare gases. At that time, we did not yet know that we were bringing up a kitten that later would become a perfect Schroedinger cat (see below).

In order to have a rest from a complicated dynamics of atom-molecule inelastic collisions, we have taken part, with our French colleagues and with my graduate student Berengolz, in solving much simpler problems of inelastic atom-atom collisions (Laboratoire Aime Cotton, Orsay and Laboratoire de Physique des Lasers, Universite Paris XIII). In these events, one can clearly experimentally see the fingerprints of the interference of the matter waves (no question, you can always get interference pattern theoretically!), yet another phenomenon, which later would show up with in the Cat.

Following fashion and to be on top of the wave, I wanted also to learn something interesting from the physics of cold atoms and molecules. I therefore have taken part in the project of a novel source of cold molecules at the Department of Physics, Oklahoma University, USA. Molecules were simply allowed to escape from a reservoir through a bent tube, and those with low velocity were adiabatically guided along the tube by a magnetic field. High-velocity molecules could not be contained by the field and were captured by the tube walls. My role was to estimate nonadiabatic effects following the ideas of E. Majorana, famous Italian Physicist. The stay in Oklahoma was very interesting in another aspect: we revived cooperation with our old friend, Dr. M. Auzinsh from Riga, whom we knew from the time when Latvia was still a part of the Soviet Union.

I now turn to the Schroedinger Cat. Everybody heard about it, many speak about it, few really understand it. We did not belong to the few elite, until we witnessed an experiment and created a simple theoretical description of it during our visit to Tohoku University in 2002. But long before this, I was given an omen that something interesting was going to happen: a person from the Netherlands (I neither



remember his name, nor his home university) sent to me his thesis together with a photo, which you see below. There are so many similar creatures around our Department in the Campus that I though this picture will be appropriate. The Cat was introduced by E. Schroedinger in 1935. In a thought experiment, Schroedinger postulated a sealed vessel containing a live cat and a device triggered by a quantum event, such as the radioactive decay of a nucleus. If the quantum effect occurs, cyanide is released and the cat dies; if the event does not occur the cat lives. The cat could only be said to be alive or dead when the vessel had been opened and the situation inside it had been observed.

In the Tahoku experiment, a live Cat in the vessel was an ensemble of vander-Waals dimers in the region of crossing of a cold molecular beam with a beam of the laser pulse. Instead of release of cyanide they had a second laser pulse, and instead of quantum event (such as the radioactive decay of a nucleus) the controllable time delay between two pulses. The fate of the Cat can be controlled by the time delay, and the Cat can be found (when observed) alive or dead by our will. Let me translate it in a more vulgar way. Assume you get two successive money transfers to your bank account. You expect a double amount, but according to quantum picture (which is of course correct) you may have anything between none and four. Why then we have never experienced this interesting phenomenon, save for the cases when a hacker interfered? Because something occurs between two transfers, which is called dephasing. Every time we simply get the average between zero and four, the averaging occurs as a result of minute variation of timing between two transfers. If bankers could control this timing, as Tohoku people could control time delay between two pulses, we would have a much more interesting life. Anyhow, in Sendai we have seen the Cat manipulated, and having found a simple solution to the respective Schroedinger equation, which agrees with observations, we thought that we understood the phenomenon of the Schroedinger Cat (did we?) and we wrote a paper about it [10]. Anyhow, we were happy to witness our molecular Cat alive or dead when the vessel had been opened (the situation inside it had been observed by way of laser-induced fluorescence).

We have left Sendai with a feeling that we have learned something new, and then decided to go back to our problem: we (stubborn people from Haifa and Goettingen) still wanted to generalize the Landau-Teller results for large energy transfer by using parameters appropriate to small energy transfer. We succeeded in this after creating several papers together with our associate Dr. I. Litvin. Our final results were presented in a paper of 2008 [11], commemorating the 70<sup>th</sup> anniversary of the Landau-Teller paper and the100<sup>th</sup> anniversary of Landau's birthday. In this

paper, we presented the semi-classical three-dimensional generalizations of the classical collinear model of Landau and Teller, and provided the explanations of a strong deviation of the temperature dependence of the vibrational relaxation time from what is known as the Landau-Teller plot. Being at the Department of Chemistry, I can not refrain from drawing a parallel between this effect and the deviation of the temperature dependence of a chemical reaction from the Arrhenius plot due to the tunnelling.

Our other activity after 2001, in joint efforts with people from the J. Troe group, was directed toward low-temperature complex formation, and quantum effects, which modify the traditional temperature dependence of the rate coefficients bringing them to predictions by Bethe and Wigner in the limit of ultra-low temperatures. I am continuing in this direction as a Humboldt Awardees, now with M. Auzinsh, University of Riga, joining our team. During my work at the Technion, I have published about 50 papers and attended numerous scientific meetings. The latest one, in August 2008, was devoted to the Dynamics of Molecular Collisions. And at in all my deeds, scientific and others, I was helped by my wife. Interestingly, she continued to be employed by the Technion for a period during which I already became an Emeritus.

I conclude my contribution to this volume with a funny episode from my life. This episode can happen to anybody and illustrates the old proverb "habent sua fata *libelli* (books have their own destiny: Horace). As you can see from the list of my visits abroad, I have been several times to the University of Rome. There I have a friend, Dr. Francesco Di Giacomo, and we worked together on nonadiabatic effects in oxonium. Besides, we discussed the contribution of E. Majorana to the theory of nonadiabatic transitions and even we have written a comment on this subject [11]. When Francesco decided to teach a course on combustion, he choose the book by Kondratyiev and myself as a basis. This book was out of print and sale, but he succeeded to buy a second-hand copy through the Internet. So far, this is the nice part of the story. The sad and funny part sounds like this. The book bought through Internet by Francesco was nothing else than our present to a Canadian scientist, Professor J. Polanyi, at the time of his visit to Moscow, more than 30 years ago. I wonder whether those who put the book on sale knew that the selling price of this book will be quite high since the book was signed by the authors? *O tempora, o* 

*mores*! (oh the times! oh the manners!) Indeed, "the use of books is endless, and much study is wearisome" (Ecclesiastes, The New English Bible, Oxford, 1976). References:

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- 2. E. E. Nikitin, *Theory of Elementary Atomic and Molecular Processes in Gases*. Oxford: Clarendon Press, 1974.
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# **Izhack Oref**

Born Israel, 1936



**PhD thesis**: University of California, Santa Barbara, California, USA, 1967

### **Post-doc position:**

University of Washington, Seattle, Washington, 1967-68

## **Past Positions:**

Research Associate - Corn Products, Argo Illinois, wide line NMR, 1962. Research Associate - Varian Associates, Palo Alto, California, wide line NMR, 1963-64. Lecturer, University of California, Santa Barbara, California, 1967. Senior Research Associate - Gulf General Atomic Research Center, San Diego, California, 1968-69.

# Technion Position: 1969

### Sabbatical leaves:

Visiting Scientist - Oxford, summer 1971.
Visiting Professor - Department of Chemistry, U. of Washington, Seattle, summer 1973.
Visiting Professor, University of Washington, Seattle, Washington, 1976-77.
Visiting Professor - University of Texas, Austin, 1983-84.
Visiting Professor - University of Arizona, Tucson, Arizona, summer 1984.
Visiting Professor - University of Washington, Seattle, Washington, summer 1986.
Visiting Professor - Brandeis University, Waltham, Massachusetts, summer 1986.
Visiting Professor - Max-Planck Institute, Göttingen, summer 1988.
Visiting Professor - University of Iowa, Iowa City, Iowa, summer 1989.

Visiting Professor - Bowling Green State University, Bowling Green, Ohio, spring 1990.
Visiting Professor - Sydney University, Sydney, Australia, summer 1990.
Visiting Professor - Cambridge University, UK, 1994.
Visiting Professor - University of Texas, Austin, 1998.
Visiting Professor - University of Cordoba, Argentina, 1999.
Visiting Professor - University of Michigan, Ann Arbor, 2001.
Visiting Professor - University of Cyprus, Nicosia, 2004.
Visiting Professor - Stanford University, 2004.

## Major departmental responsibilities

Development of an original Computer Assisted Instruction (CAI) in Chemistry for Technion students 1971-75 Development of Demonstrations in Chemistry 1973-77 Head of Physical Chemistry Laboratory in the Department of Chemistry, 1985-87 Chair, New Building Committee, Department of Chemistry, 1997-2004 Head, Environmental Science Program, 1996–2000

Tread, Environmental Science Program, 1990–2000

Head, New Teaching Wing Building Project, 1999-2004

Dean, Department of chemistry, 2002-03

# Major Technion's responsibilities

Dean of students, 1987

Member, Technion Interdisciplinary Committee of the Department of Science and Technology Education, 1979-83

Member, Computer Committee, 1971-74

Member all-Technion Committee, 1988-89

Member, Technion Executive Committee, 1991-94

Member, Technion Leaves Committee, 1991-94

Member, Technion Academic Guests Committee, 1991-94

Member, Neeman Institute Science Committee, 1990-93

Member, President Search Committee, 1994

Member, Technion Restructure (Ash) Committee, 1994-95

Member, Environmental Forum, 1995-98

Member, Neeman Institute Science Committee, 1996-99

Member, Technion Development Committee, 1996–98

Head, Environmental Science Program, 1996–2000

Member, Technion Select Committee on Academic Appointments and Promotions, 1997-98 Chair, Technion professional committee on Academic Appointments and Promotions, 1999–2001

Member, Technion committee for honorary degrees, 1999

Member, committee for promotion and tenure in the department of Science Education, 2000-04

Member, Research authority Grants committee, 2000

Member, Technion permanent committee for promotion and tenure, 2001

### Research

#### Kinetics and Molecular Dynamics

### Past Research

Experimental studies of the rate of intramolecular energy dissipation (IVR) and intermolecular energy exchange.

Chemical spectroscopy of reactive systems (supercollisions). Unimolecular dissociation and combined ab initio and RRKM calculations. Time dependent rate coefficients of fast unimolecular reactions. Solvent effects on the overtone spectroscopy of OH and NH.

### **Current Research**

*Supercollisions:* Collisions that transfer an inordinately large quantity of energy in a single collision are called supercollisions. They were first found experimentally by us in 1988 and theoretical work has since continued on the subject. The effect of a minor fraction of supercollisions on the rates of chemical reactions is evaluated and their contribution to the overall average energy transfer is calculated.

*Energy Transfer:* Classical trajectory calculations of collisions between bath atoms and molecules and highly excited polyatomic molecules are performed. Using ab initio and assumed inter- and intramolecular potentials, average energy transferred per collision quantities and collisional energy transfer probability density functions are calculated for an assortment of polyatomic-monatomic and polyatomic-polyatomic systems under a variety of initial conditions.

*Scattering:* Scattering of inert gas atoms from highly excited polyatomic molecules are studied using trajectory calculations. The energy transfer results are compared with crossed molecular beam results. *Cluster dissociations:* The dynamics of large molecules with up to 1000 internal modes, as well as cluster dissociation are studied for a variety of initial conditions.

#### קורות חיים-יצחק עורף

נולדתי בתל אביב ב- 30.5.36 לרות ויעקב קרק. אמי, רות, נולדה בעיירה גורדז' שבליטא לחיה ויצחק קצין שעל שמו אני נקרא. למשפחת קצין היו תשעה ילדים וכולם, למעט שניים, היגרו מליטא לדרום אפריקה בשלהי המאה ה-19 ותחילת המאה ה-20. באותה תקופה, באמצע שנות ה-20, עלו גם אמי, אחותה והוריהם ארצה כיון שארץ ישראל היתה המקום היחידי אליו סבי, שהיה רב העירה, הסכים להגר. אבי, יעקב קרק, נולד בקובנה שבליטא לחיה וישראל ניסן קרק, שהיה רב ותקופה מסוימת רב ראשי בקובנה ומראשי תנועת המזרחי שם. במשפחת קרק היו 14 ילדים. הבכורה היגרה לברזיל והשאר היגרו ברובם לארה"ב. אבי למד בישיבות סלובודקה ומיר שבליטה ובגיל 16 הצטרף לקבוצת "אחווה" בתנועת הפועל הצעיר. הוא הפסיק את לימודיו ונסע לעבוד בחוה של חקלאי יהודי בליטא על מנת להשתלם בחקלאות. ב-1919 החליטו חברי הקבוצה לעלות ארצה. הם גנבו גבולות, עבדו בדרך, הושמו בכלא, שוחררו בעזרת קהילה יהודית מקומית והגיעו סוף סוף לארץ ב-1920. קבוצת "אחווה" הצטרפה לגדוד העבודה על שם יוסף טרומפלדור שסלל את כביש צמח-מגדל. בראש הגדוד עמד יצחק שדה (לנדוברג), בעתיד מפקדו הראשון של הפלמ"ח וממיסדי צה"ל. רוב חברי הגדוד הורכב מעליה רוסית שאותה ארגן טרומפלדור ו"הליטאים" הצטרפו כקבוצת מיעוט נבדלת. עם סיום עבודות הסלילה בכביש התפרק הגדוד לקבוצות וגרעינים שהקימו ישובים. קבוצת "אחווה" הפכה לקבוצת בנאים שבנתה, בין השאר, באיילת השחר ובכפר גלעדי. לאחר תקופת הכשרה קצרה בקיבוץ גניגר, יסדה הקבוצה ב-1926 את קיבוץ שריד. בעקבות קרע אידיאולוגי עזב אבא את הקיבוץ ועקר לתל אביב. אחרי שרוב ילדיהם עזבו, סבי וסבתי, הורי אבי, החליטו אף הם להגר מליטה ובסוף שנות ה-20 הצטרפו יחד עם שלוש מבנותיהם אל אבי בתל אביב.

חיי בתל אביב כילד וכנער היו נעימים למרות שמחוץ ל"בועת הילדות" החיים היו קשים. מלחמת העולם השניה הביאה מחסור לפלסטינה. האיטלקים הפציצו את תל אביב ובני דודי ושכני המבוגרים נעלמו מסביבתי כיון שהתנדבו לצבא הבריטי ולבריגדה היהודית. אנו עברנו לגור בגדרה בה היתה לי משפחה מנצר הבילויים. אחרי תום מלחמת העולם התחיל המאבק לעצמאות המדינה ולהעלאת עולים ניצולי השואה. אני זוכר את ימי העוצר הרבים שהוטלו על העיר על ידי הבריטים וכיצד שיחקנו בשדרה (שדרות רוטשילד שם גרתי), למרות האיסור, ואת השיריוניות הבריטיות שעצרו לידינו מידי פעם והחיילים שגערו בנו ושלחו אותנו לביתנו. ב-1947 "כבשו" ה"כלניות" (הצנחנים הבריטיים) את בית הספר העממי בו למדתי והפכו אותו לבסיס הקדמי שלהם. הם השתכנו בחלק מהכתות ובשאר אנו למדנו. באותה עת שבתו התלמידים בבית הספר בדרישה לא ללמוד אנגלית כיון שזאת היתה שפת הכובש ולבסוף הושגה פשרה שבה למדנו "אמריקנית". מלחמת השחרור פרצה וקרבות נגד הערבים התחוללו בגבול ת"א-יפו, מרחק לא גדול מהבית בו גרתי. העיר התמלאה בפליטים משכונות הגבול. בזמנו נבנו בתי תל אביב עם "חדר כביסה" על הגג. אל החדרים האלו, כולל זה שבבית בו גרנו, "פלשו" הפליטים ובית הכנסת בשכונה התמלא אף הוא בפליטים. כל משפחה זכתה במספר מטרים מרובעים של רצפה כאשר בין המשפחות מפרידות מחיצות עשויות שמיכות או סדינים.

כאמור, החיים כילד בעיר "הלבנה" היו נעימים למרות התקופות הקשות בחיי המבוגרים. בתי הבאוהאוס היו חדשים, השדרות היו רחבות והרחובות צרים. לכל בית היתה גינה מטופחת, מספר המכוניות היה קטן והאופנים היו כלי תחבורה מקובל (ולא מטעמים בריאותיים). העיר היתה מוקפת דיונות עליהן החלקנו וקימזצנו ונהר הירקון היה נהר זורם ולא מזוהם משפכים, אם כי כשהייתי בימיה הזהירו אותנו לא להתרחץ בו מפני מחלת הבילהרציה. למען האמת, נפלתי מהסירה לתוך הנהר מספר פעמים ללא תוצאות הרות אסון. ילדותי חוותה תקופה מלאת ארועים בחיי האומה ומלחמת העולם ומלחמת השחרור הטביעו את חותמם על האומה ועלי אישית.

לאחר שרות בצה"ל החלטנו שושנה חברתי (לימים אשתי) ואני לנסוע לארה"ב כדי ללמוד באוניברסיטה. קבלנו מילגות באוניברסיטה של אילינוי והחלטנו לממש אותן. שוש למדה מיקרוביולוגיה ואני למדתי כימיה. בין השנה השלישית והרביעית עבדתי עבודת קיץ בחברת Corn Products שבשיקגו, היום חלק מ- Uniliver, על מכשיר wideline NMR ששימש לבדיקות גנטיות לא פולשניות של תכולת השמן בגרעיני תירס. הרעיון היה לברור את הגרעינים עם תכולת השמן הגבוהה ביותר, לזרוע אותם וכך להשביח את התירס לשמן. העבודה נראתה לי משעממת ולשמחתי ביום הראשון של העבודה נכנס למעבדה המהנדס הראשי של מפעל הגלוקוזה, שנוצרת מפרוק העמילן שבתירס, ובתסכול סיפר שהם מייבשים את מונוהידרט הגלוקוזה בתנורים והמוצר המוגמר נשלח בקרונות רכבת. כשהמשלוח מגיע ליעדו, בקליפורניה למשל, כל הסוכר בקרון המשא הופך לקובית סוכר אחת גדולה, דבר המפריע כמובן לשינוע הסוכר. הבעיה נראתה מענינת ובקשתי רשות לעבוד עליה. בבדיקה ב- NMR, המאפשרת להבחין בין מים חופשיים למים הנמצאים בגביש כמונוהידרט, נראה שהגבישים רטובים למרות תהליך הייבוש בתנור. בדיקת הנתונים הפיסיקליים הראתה שיישנו מעבר פזה בין הידרט לאנהידרט המתרחש בטמפרטורות הייבוש של הסוכר. מחקר נוסף הראה שהתקשות הסוכר קשורה לכמות המים החופשיים. שינוי תנאי הייבוש פתר את הבעיה. חברת Varian, יצרנית ה-NMR, שמעה על המחקר ובהמלצת ראש המעבדה שכרה את שרותי לאחר סיום לימודי. המעבר מהפרריות של אילינוי לפאלו אלטו, קליפורניה, של שנות השישים היה מעבר לפלנטה אחרת, ביחוד לסטודנט עני שלראשונה השתכר משכורת סבירה. החיים ב- Varian היו מענינים וכללו סמינרים בנוכחות פליקס בלוך, שקיבל פרס נובל על גילוי תופעת ה-NMR והצוות שחלקו מופיע על המאמרים שזכו בפרס, כמו כן היו שם גם ריי פרימן וריצ'רד ארנסט, זוכה פרס נובל. כולם היו מטובי מדעני ה-NMR באותם זמנים וכולם היו בצוות המדהים שראסל ווריאן אסף סביבו. אחרי שנה ב- Varian ב הראשוני בספטמבר 1964 לחזור ללמודים והבחירה שלי נפלה לעבוד בקבוצתו של Frederic Wall (UCSB) University of California Santa Barbara ב – Frederic Wall Santa Barbara כימיות ולי זה נראה, מבורות, כדבר העתידי שיפתור שהשתמש במחשבים לתאור הדינמיקה של ראקציות כימיות ולי זה נראה, מבורות, כדבר העתידי שיפתור Quantum Mechanical Calculations of the את כל בעיות הכימיה. נושא המחקר שלי היה: H+H<sub>2</sub> Reaction Calculations of the דינמית. המחקר נעשה העבודה הראשונה במכניקה קוונטית שנעשתה במערכת זו ובכלל על מערכת דינמית. המחקר נעשה על מחשבי 1904 ובכלי היה: הראשונה במכניקה קוונטית שנעשתה במערכת זו ובכלל על מערכת דינמית. המחקר נעשה על מחשבי 1904 ובכלי היה היה מכניס את התוכנה היו מדפיסים על כרטיסים דינמית. המחקר נעשה על מחשבי Berkeley ו- 360 ו היתי מכניס אותם לקורא כרטיסים שהיה הקלט של המחשב. תוצאות ה-Batch היו מתקבלות לאחר מספר שעות במרכז המחשבים. כדי לזרז את העבודה הייתי מריץ תוכניות במקביל ב- UCSB שנות במרץ הייתי נוסע במכונית של האוניברסיטה, ישן במכונית ומריץ הרצה כל ארבע שעות. במרץ 1967 סיימתי את הדוקטורט במסלול ישיר וחגגתי את הערוע בג'ונגלים של מכסיקו.

עבודת הדוקטורט פתחה בפני את עולם הקינטיקה והדינמיקה המולקולרית וגירתה את תאבוני להבין כיצד מתרחשות ראקציות כימיות. היה ברור לי שעבודה תאורטית בתחום עם כח המחשוב הקיים אז היתה בלתי אפשרית. אפילו מערכת ה- H<sub>3</sub>, שלושה פרוטונים ושלושה אלקטרונים, נמצאה בעת ההיא קשה ביותר לחישוב. בנוסף היה לי רצון ללמוד כיצד נמדדות ריאקציות במעבדה - ריאקציות קצת יותר מסובכות מאשר H+H<sub>2</sub>. לאחר בדיקת ההיצע החלטתי לעשות את הבתר דוקטורט בסיאטל אצל מסובכות מאשר Ravinovitch. זאת היתה בחירה מוצלחת מבחינה מדעית, מקצועית ואישית והשהות שם זכורה לי לטובה עד היום. מדוע בחרתי דווקא בקבוצתו של רבינוביץ? לשם כך עלי לתאר את שטח הריאקציות היונימולקולריות, לכאורה הריאקציות הפשוטות ביותר, בשנות ה-50 וה-60.

כאשר מתרחשת הראקציה: CH<sub>3</sub>NC → CH<sub>3</sub>CN מקדם מהירות הראקציה תלוי בלחץ. את הסיבה לכך הסביר לינדמן ב-1922 על ידי המנגנון הבא:

 $CH_3NC + M \leftrightarrow CH_3NC^{\#} + M$ 

 $CH_3NC^{\#} \rightarrow CH_3CN$ 

הלחץ נקבע על ידי מולקולות האמבט M. לינדמן הסביר שישנו פסק זמן במעבר ממגיב לתוצר שבו המגיב נמצא במצב מעורר כתוצאה מההתנגשות ובמשך זמן זה הוא יכול לעבור קירור על ידי ההתנגשות המגיב נמצא במצב מעורר כתוצאה מההתנגשות ובמשך זמן זה הוא יכול לעבור קירור על ידי ההתנגשות נוספת עם M אחר. מה שהוא לא הסביר הוא מה המשמעות של הצורן המעורר המעורר M ומדוע, באותו הלחץ, מקדם מהירות הראקציה תלוי ב- M - באם הוא גז מונואטומי, דואטומי או רב-אטומי. בקיצור, הלחץ, מקדם מהירות הראקציה תלוי ב- M - באם הוא גז מונואטומי, דואטומי או רב-אטומי. בקיצור, ההבנה המולקולרית של ראקציות אלה שאפה לאפס, זה היה המצב במשך מספר שנים. בשנת 1927 ההבנה המולקולרית של האפריהם גמונו אומי או היה המצב במשך מספר שנים. בשנת 1927 ההבנה מהדם המולקולרית של העקציות אלה שאפה לאפס, זה היה המצב במשך מספר שנים. בשנת 1927 ההבנה מקדם ההבנה המולקולרית של האפריהם גמופו בשנת 1932 פיתחו תאוריה סטטיסטית עבור מקדם מהירות הראקציה היונימולקולרי. תאוריה זו, הנקראת תאורית RRK היתה תאוריה קלאסית טהורה שניבאה תוצאות נסיוניות באופן מוגבל ביותר. ב-1951 ו-1954 פרסם רודי מרכוס מאמרים בהם שיפר

את תאורית RRKM (ולכן נקראת התאוריה המשופרת RRKM). לפי RRKM, מתרחשת ראקציה יונימולקולרית כאשר מולקולה מעוררת עוברת למצב קומפלקס משופעל והאנרגיה הפנימית מתחלקת בצורה סטטיסטית בכל דרגות החופש האפשריות. מקדם מהירות הראקציה ניתן לחישוב על ידי מציאת ההסתברות להימצאות אנרגיה בקאורדינטת הראקציה. בדיוק בתקופה שמרכוס פרסם את התאוריה שלו ההסתברות להימצאות אנרגיה בקאורדינטת הראקציה. בדיוק בתקופה שמרכוס פרסם את התאוריה שלו מרסם בינים מרסם את התאוריה שלו ההסתברות להימציה מתחלקת יונימולקולריות בדרך שונה לגמרי. הוא טען בניגוד למרכוס, שהאנרגיה מתרכזת באיזור אחד במולקולה וראקציה מתרחשת כאשר מתקיימת סופרפוזיציה למרכוס, שהאנרגיה מתרכזת באיזור אחד במולקולה וראקציה מתרחשת כאשר מתקיימת סופרפוזיציה של אמפליטודות של התנודות הנורמליות. בדרך זו, קשר מסוים מתארך מעל ערך קריטי וכך המולקולה מתפרקת. בתקופה זו עשיתי את הדוקטורט ובתקופה זו קשר מסוים מתארך מעל ערך קריטי וכך המולקולה ניסיונות קריטיים, למצוא איזו תאוריה מבין השתים נכונה. כשהגיעה העת עבורי להחליט להיכן לנסוע לעשות את עבודת הבתר דוקטורט, הקבוצה של רבינוביץ בסיאטל נראתה לי כאטרקטיבית ביותר בשטח הקינטיקה הכימית.

שנת הפוסט בסיאטל היתה מענינת מבחינה מדעית וחברתית. מרבינוביץ למדתי רבות על הגישה המדעית-ניסיונית ועל השאיפה להבנה של התהליכים הבסיסיים ביותר בכימיה מבלי לפזול ימינה ושמאלה אל האספקט המסחרי של העבודה. אגב, עבודותיו של רבינוביץ הובילו לתובנה שהתאוריה הסטטיסטית של מרכוס נכונה והתאוריה הלוקלית של סלייטר אינה משקפת נאמנה את התוצאות הניסיוניות. עם סיום מרכוס נכונה והתאוריה הלוקלית של סלייטר אינה משקפת נאמנה את התוצאות הניסיוניות. עם סיום הפוסט" עבדתי שנה ב- Golf General Atomic ב- La Jolla שהוא פרוור של סן דיאגו בקליפורניה. תפקידי היה לפתח מודלים מתמטיים של דיפוזיה של חומרי ביקוע בתוך גרפיט בכורים גרעיניים. לבסוף ב – 1969 הצטרפתי לפקולטה לכימיה בטכניון.

הנושא הראשון שבו עסקתי בטכניון היה שוב בדיקה בין שתי תאוריות. Don Bunker איות חגושא הראשון שבו עסקתי בטכניון היה שוב בדיקה בין שתי תאוריות. David Wilson יונימולקולריות בלחצים סופר גבוהים תראה עליה בערך מקדם מהירות הראקציה ואילו צפה כאתגר לא צפה ירידה. העבודה בראקציות יונימולקולריות בפזה הגזית בלחצים סופר גבוהים הוכחה כאתגר לא פשוט אך בסופו של דבר הוכחנו שאין עליה ואין ירידה ומקדם מהירות הראקציה נשאר קבוע כפי שצפוי פשוט אך בסופו של דבר הוכחנו שאין עליה ואין ירידה ומקדם מהירות הראקציה נשאר קבוע כפי שצפוי היה מתוט אך בסופו של דבר הוכחנו שאין עליה ואין ירידה ומקדם מהירות הראקציה נשאר קבוע כפי שצפוי נפונט אך בסופו של דבר הוכחנו שאין עליה ואין ירידה ומקדם מהירות הראקציה נשאר קבוע כפי שצפוי נמוט אך בסופו של דבר הוכחנו שיו גמולים ומקדם מהירות הראקציה נשאר קבוע כפי שצפוי נפונט ליה מתאורית ה- מתאורית ה- מתום הלחצים היה מתאורית היום הלחצים היה מתאורית היום ללחצים סופר גבוהים.

מלחמת יום הכפורים פרצה באוקטובר 1973. נקראתי למילואים בצפון ושהיתי שם מספר חודשים. מלחמה זו היתה ארוע קשה מבחינתי לא רק מהפן המדעי אלא גם מהפן האישי והלאומי. חזרתי מהמילואים בתחילת 1974 עם תובנות חדשות שבעצם מלוות אותי עד היום. השבריריות של הקיום הלאומי והאישי וההכרה שלא ניתן לסמוך על "מומחים" ומנהיגים התגבשה אז והיא חלק מתפיסת עולמי. היבט נוסף של תוצאות המלחמה עבורי היה הצורך במעורבות חברתית. למען האמת אינני יודע כמה מהיבט זה מקורו בחינוך בבית וכמה בארועי המלחמה. דבר אחד ברור לי שהוא בא לידי ביטוי בתום המלחמה הן בהתנדבות מעל ומעבר למסגרת המילואים בצהל והן בפקולטה ובטכניון.

הפרויקט הרציני הראשון שהקמתי בפקולטה היה "הדגמות בכימיה" לתלמידי תיכון. תלמידי כתות ט'-יב' בקרו בפקולטה בימי שישי בצורה מאורגנת ונחשפו להדגמות בכימיה שניתנו בהתנדבות על ידי מורי הפקולטה. ריכזתי את הפרויקט במשך מספר שנים. במהלך קיומו של הפרויקט בקרו בפקולטה כ-70,000 תלמידים והוא נפסק מסיבה שאינה ברורה לי כלל ועיקר. באותה תקופה פנה אלי ד"ר מרדכי לוי מנהל המוזיאון הלאומי למדע ולטכנולוגיה (בהקמה) והזמין אותי להצטרף לוועד המנהל שהחל בתכנון הקמת המוזיאון. לאחר השתתפות בישיבה אחת או שתיים נוכחתי לדעת שחסרה לוועד היכולת להוציא את הרעיון מן הכוח אל הפועל. היו תוכניות, היו דיבורים אך הצד הביצועי היה חלש. הצעתי לוועד המנהל לצרף לשורותיו את אהרון לוינשטיין ואת צבי דורי מהפקולטה שלנו וזה אישר את ההצעה. עם הצטרפותנו חל שינוי משמעותי בתפיסת הוועד המנהל של המוזיאון. במקום לחכות לתרומת ענק להקמת מוזיאון גדול בשטח שהקציבה עירית חיפה, בין האוניברסיטה לטכניון, החלטנו ללכת בקטנות. מר עמוס חורב, נשיא הטכניון דאז, נתן לנו שטח של כ-200 מ"ר שהתפנה בקמפוס הדר הכרמל לאחר עלית הפקולטה להנדסה כימית לקמפוס הנוכחי. צבי דורי התמנה למנכ"ל המוזיאון ואנוכי למנהל המדעי שלו ואת התצוגות הראשונות בנינו בחדר 231 בפקולטה לכימיה בעזרתו של מר אברהם הרטוד שהיה הטכנאי הראשי של כימיה פיסיקלית. המוזיאון נפתח ב- 1982 ולתקופה מסוימת כיהנתי כמנהל הכללי בנוסף לתפקידי כמנהל המדעי. ב- 1986 עבר המוזיאון (טכנודע, כפי שהוא נקרא אז ומדעטק כיום) לבנין הראשי של הטכניון בהדר, בנין ברוולד, ולאחר זמן פרשתי מהפעילות בו. כל תפקידי במוזיאון נעשו בהתנדבות ובנוסף לעיסוקי הרגילים בפקולטה ובטכניון. תקופת הקמת המוזיאון היתה ללא ספק מענינת, מאתגרת ומלאת סיפוק. למדתי איך בונים יש מאין וכיצד מנווטים במבוכי הביורוקרטיה בטכניון ובמשרדי הממשלה. כיום המוזיאון תופס מקום חשוב בחיי התרבות בארץ דבר המביא לי סיפוק ושמחה.

בתקופה זו עבדה קבוצתי על בעיה עקרונית בראקציות יונימולקולריות: כמה מהר זורמת האנרגיה בין דרגות החופש של המולקולה. ניסויים בלחץ גבוה אפשרו לנו למדוד את התופעה ונמצא שפרק הזמן הוא פיקו-שניות. אחר כך עברתי לנושא מעבר אנרגיה בהתנגשויות. קינטיקה, פוטוכימיה ופוטופיסיקה תלויות פיקו-שניות. אחר כך עברתי לנושא מעבר אנרגיה בהתנגשויות. קינטיקה, פוטוכימיה ופוטופיסיקה תלויות פיקו-שניות. אחר כך עברתי לנושא מעבר אנרגיה בהתנגשויות. קינטיקה, פוטוכימיה ופוטופיסיקה תלויות פיקו-שניות. אחר כך עברתי לנושא מעבר אנרגיה בהתנגשויות. קינטיקה, פוטוכימיה ופוטופיסיקה תלויות התנגשויות או ליתר דיוק, כמה אנרגיה עוברת בהתנגשויות זוגיות (בינריות). במספר אבוגדרו של התנגשויות או ליתר דיוק, כמה אנרגיה עוברת בהתנגשויות זוגיות (בינריות). במספר אבוגדרו של התנגשויות, יש כאלה המעבירות הרבה אנרגיה ויש כאילו המעבירות מעט. ישנה התפלגות הנקראת: למעבר כמות אנרגיה מסוימת  $\Delta E$  בהתנגשות בין שתי מולקולות בתנאי התחלה נתונים. ברגע ש למעבר כמות אנרגיה מסוימת שה לסטר ולמצוא את מקדמי מהירות הראקציה ולכן, זוהי פונקציה (P(E,E') ידוע ניתן לפתור משוואת מסטר ולמצוא את מקדמי מהירות הראקציה ולכן, זוהי פונקציה קראנובה ביותר, אך עלומה, בדינמיקה מולקולרית. כדי למצוא את (E,E') פיתחנו שיטה ניסויית שימה באתר שימה ביותר, אך עלומה, בדינמיקה מולקולרית. כדי למצוא את התקדמי מהירות הראקציה ולכן, זוהי פונקציה שקראנו לה "ספקטרוסקופיה כימית". מולקולה קרה עם סף פרוק סם מתנגשת עם מולקולה מעוררת בגרגיה בארגיה בארגיה ב-E > E < 2 מהנגשת אחת אנרגיה מסתבר באנרגיה ב-מתנגשות אחת מנגשות אחת אנרגיה מסתבר הגם שהתגלה היה לא פחות ממדהים עבורנו והתברר שגם עבור הקהילה המדעית. מסתבר שמולקולות ציקלוהקסהדיאן, קאדריציקלן וציקלובוטן קרות מתפרקות כאשר הן מתנגשות במולקולות מסתבר

אזולן מעוררות. פרוש הדבר, שבתהליך מועברים, בהתנגשות אחת, בין 32 ל-42 קילוקלוריות למול. אף אחד לא חשב שניתן להעביר כל כך הרבה אנרגיה בהתנגשות בודדת והתנגשויות אלו זכו לשם מיוחד: Supercollisions. לגילוי זה היתה משמעות רבה בהבנה של ריאקציות כימיות וכיצד הן מתרחשות בפזה הגזית. בין לבין, עסקנו באלקטרוכימיה בפזה הגזית, בפתוח מודלים מתמטיים בראקציות יונימולקולריות הגזית. בין לבין, עסקנו באלקטרוכימיה בפזה הגזית, בפתוח מודלים מתמטיים בראקציות יונימולקולריות ויבליער", "שער", הגזית. בין לבין, עסקנו באלקטרוכימיה בפזה הגזית, בפתוח מודלים מתמטיים בראקציות יונימולקולריות ובנבליעה מולטיפוטונית על ידי לייזר CO<sub>2</sub>, בה הראנו שלמרות שהאנרגיה נכנסת למולקולה ע"י "שער", ויברציה אחת, האנרגיה אינה נשארת בויברציה אלה מתפזרת בצורה סטטיסטית בכל דרגות החופש של המולקולה. דבר זה סתם את הגולל על היכולת לעשות כימיה סלקטיבית ע"י ערור סלקטיבי של וויברציות D.C שנים במולקולה. דבר זה סתם את הגולל על היכולת לעשות כימיה סלקטיבית ע"י ערור סלקטיבי של וויברציות באזורים שונים במולקולה. פעילות ממושכת בשטח וכתיבת Review גדול עבור מויפר ביש לוויברציות Trajectory בי המרחשים בזמן התנגשויות בין-מולקולריות ועלי לחשוב על כיווני מחקר חדשים. ב- 1990 נביקור ארוך באוסטרליה אצל לאט לאט לשינוי כיוון המחקר של מניסיים ופיתוח מודלים אנליטיים בביקור ארוד בין-מולקולריות ועלי לחשוב על כיווני מחקר חדשים. ב- 1990 נביקות לימים המתרחשים בזמן התנגשויות בין-מולקולריות ועלי לחשוב על כיווני מחקר חדשים. ב- 1990 נביקור ארוך באוסטרליה אצל (מו לאט לאט לשינוי כיוון המחקר שלי מניסויים ופיתוח מודלים אנליטיים לחישובי מסלולים בדינמיקה מולקולרית.

במקביל לעיסוקי המדעיים עסקתי גם בפעילות טכניונית כמו כל חבר סגל מן השורה. זמן מה הייתי דיקן סטודנטים. בתקופות אחרות הייתי חבר בוועדות טכניוניות שונות: ועדה מרכזת, ועדות מינויים, ועדה לבחירת נשיא, ועדה לשינוי מבנה הטכניון, הועדה לפיתוח אקדמי, ועוד עיסוקים טכניוניים שונים. הנכונות שלי להשתתף בוועדות טכניוניות ופקולטיות באה לי, אני מוכרח להודות, קצת מאוחר לאחר עבודה של מספר שנים בטכניון. לקח לי זמן להבין שאוניברסיטה הינה מערכת המנהלת את עצמה ואם אני ושאר חברי הסגל נסרב לקחת חלק בפעילות הניהולית, המערכת יכולה להתפורר או לחילופין עלולה, חלילה, להשלט על ידי גורמים לא אקדמיים. בפקולטה עמדתי בין השאר, בראש פרויקט בנין מעבדות ההוראה על שם צייס. זה היה פרויקט מורכב, מסובך ויקר אשר שדרג את הפקולטה בצורה משמעותית. בתקופתי כדיקן קודם נושא הבינוי בפקולטה וזכינו למספר מעבדות מחקר חדשות וחדישות, אגף משרדים חדש והונחה תשתית לשדרוג כל אגף מעבדות המחקר. שינוי מבנה ההוראה בפקולטה נעשה אף הוא בתקופה זו. במקום תפירת קורס יעודי לכל פקולטה הוצעו קורסים אחידים בשתי רמות, בסיסית ומתקדמת, כאשר לכל פקולטה בטכניון ניתנה האפשרות לבחור את הקורסים המתאימים לה. שינוי השיטה איפשר גמישות בהוראה והעלאת הרמה מבלי לגרום להעמסת יתר של המרצים והמתרגלים. ריכוז הכנת הצעת הפקולטה לקבלת תרומה ממר סימור שוליך היה פרק מענין נוסף שבו למדתי כיצד מציגים נושא בצורה ענינית ואטרקטיבית לתורם פוטנציאלי. למדתי גם על הרמה הגבוהה של אגף קשרי הציבור בטכניון.

וחזרה למדע. חישובי המסלולים נעשו על ידי פתרון משוואות התנועה של המילטון תוך שימוש בחישובי ab-initio למציאת הפוטנציאלים האינטר והאינטרה מולקולריים. לפעמים השתמשנו גם בפוטנציאלים
אינטרמולקולריים שנלקחו ישירות מתוך ניסויים. בעזרת חישובי המסלולים ניתן היה בפעם הראשונה להפריד בין מעברי אנרגיה וויברטוריים, רוטטוריים וטרנסלטוריים. מצאנו ש:

- א. בהתנגשות בין מולקולות פוליאטומיות הערוץ העיקרי למעברי אנרגיה בין מולקולריים הינו ויברציה לרוטציה
  לויברציה (V→V) ואילו בהתנגשויות בין אטום למולקולה הערוץ הינו ויברציה לרוטציה (V→T/R)
- ב. האנרגיה הרוטטורית והטרנסלטורית העוברת בהתנגשות הינה קטנה. מאידך, בהתנגשות בודדת יכולה לעבור כמות גדולה של אנרגיה למולקולה המעוררת וממנה. לכן, הממוצע הינו נמוך.
- $V{\rightarrow}R$ ישיר אנרגיה אד המעבר הישיר <br/>  $R{\rightarrow}T$ ו $R{\rightarrow}T$ ו לטרנסלציות לטרנסלציות אד המעבר הישיר אד אד<br/>  $V{\rightarrow}r$ ו אינו יעיל. אינו יעיל.
- ד. כאשר הרוטציות של המולקולה המעוררת קפואות, כמו בקרן מולקולרית, כמות האנרגיה העוברת בהתנגשות פוליאטומית-פוליאטומית גדולה יותר מאשר בהתנגשות בתנאים רגילים כיון שנוצר קומפלקס התנגשות המאפשר מעבר אנרגיה בקלות בין ויברציה לויברציה. בהתנגשויות אטום-מולקולה לעומת זאת העדרות הרוטציה מפריעה למעבר האנרגיה ויעילות המעבר יורדת.
- ה. במולקולות מעוררות בעלות רוטור פנימי כמו טולאן או פרה-קסילן (די-מתילבנזן) יעילות ההתנגשות יורדת כיון שהרוטור המעורר מפריע למעבר האנרגיה ואילו, כאשר מולקולות אלו משמשות כאמבט קר היעילות עולה בגלל התנודות בעלות תדירות נמוכה הקשורות לרוטור הלא פעיל.
- יעילות מעבר האנרגיה בהתנגשויות של מולקולות פוליאטומיות כפונקציה של הטמפרטורה מורכבת כיון שישנו שינוי במנגנון מעבר האנרגיה. בטמפרטורות נמוכות המנגנון הינו דרך קומפלקס התנגשות ואילו בטמפרטורות גבוהות המנגנון הינו דרך התנגשות מהירה בקיר הפוטנציאל הרפולסיבי.
- ז. האנרגיה הממוצעת העוברת בהתנגשויות פוליאטומיות גבוהה בהרבה מהאנרגיה העוברת בהתנגשות אטום מולקולה וזאת עקב תוספת הערוץ V→V.
- ח. חישובי המסלולים הינם כמעט המקור היחידי לקבלת (P(E,E אמין לשימוש בחישוב מקדמי מהירות הראקציה.
- ט. במולקולות מעוררות ישנה וויברצית שער (Gateway mode) שדרכה עוברת מרבית האנרגיה במהלך ההתנגשות למתנגש הקר. ככל שתדירות הויברציה נמוכה יותר יעילות מעבר האנרגיה, בממוצע, גבוהה יותר.
  - י. מצאנו supercollisions גם בחישובי מסלולים ופענחנו את המנגנון בו התנגשויות אלו נוצרות.

לאחרונה פורסמו תוצאות נסיוניות של התנגשויות בקרן מולקולרית בין קריפטון או ארגון לבין אזולן מעורר. השתמשנו בתנאי ההתחלה של המערכת הניסויית והצלחנו לקבל בחישוב חלק ניכר של התוצאות הניסיוניות והסברים בצידן. מצאנו שהתהליך התמים לכאורה של התנגשות אטום דוגמת ארגון עם מולקולה מעוררת דוגמת אזולן הינו תיבה מלאת הפתעות מענינות. A+B+C נושא נוסף בו עסקנו הינו התנגשויות משולשות (תרמולקולריות). ההסתברות ששלושה גופים vogt נושא נוסף בו זמנית הינה זניחה לכן מקובל להניח שהתנגשות כזו מתרחשת בשני שלבים. זהו התהליך יפגשו בו זמנית הינה זניחה לכן A-B+C המתייצב ע"י M, לדוגמה היווצרות אוזון בפזה הגזית בתהליך בראקציות בי-מולקולריות בין A ו- B המתייצב ע"י

לא ניתן לתאר את כל עבודות המחקר שעשינו ואשר פורסמו ב - 105 מאמרים בפרק ביוגרפי קצרצר אך אני מקווה שהתאור דלעיל נותן את טעם המעשה. למעונין קצת יותר אני ממליץ לקרוא שלושה מאמרי סיכום המציינים כל אחד פרק נפרד בראקציות יונימולקולריות ומראי המקום שלהם מצוינים למטה

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כל העבודה המתוארת,ושאינה מתוארת, לעיל לא נעשתה לבד. תלמידי ועמיתי המחקר היו חלק בלתי נפרד מהעבודה וכך היו גם עמיתי בסגל הפקולטה וברחבי העולם. גם בפעילותי הלא מדעית זכיתי לעזרה ושיתוף פעולה מעמיתי בפקולטה ובטכניון. תודתי נתונה לכולם. תרומתו של כל אחד חקוקה על ליבי. על שיתוף הפעולה המדעי ברצוני להודות לעמיתי למחקר הנסויי: צילה גולדשטיין, יעל נמירובסקי, שמואל צרבינסקי, ג'ון מטקלף, רבקה לבין, זינה לייבה, אמנון פשוצקי, סלאח חסון, וולארי בולטוב ואלכס פרלמן ובצד התאורטי והחישובי: נינה גורדון, אורית הרשקוביץ, אווה צידוני, מירי קרני, זויה פבלובסקה, ויקטור ברנשטיין ולנה דשבסקיה. בנוסף, עלי לציין את שיתוף הפעולה המדעי עם עמיתי בפקולטה. ויקטור יבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם ייבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם ייבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם ייבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם ייבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם ייבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי בפקולטה. אליהם אינה יבגני ניקיטין, סול קימל ושמאי שפייזר. כולם היו חלק מהמסע המדעי המעניין שעברתי נפקולטה. איניברסיטת על אינישיי, סול קימל ובעולם איתם שתפתי פעולה במשך השנים. את י. יורטנר,אוניברסיטת תל אינים אינים להוסיף את עמיתי בארץ ובעולם איתם שתפתי פעולה במשך השנים. את י. יורטנר, ובעולם איתם שתפתי פעולה במשך השנים. את י. יורטנר, אוניברסיטת עולה איתם שתפתי פעולה במשך השנים. את י. יורטנר, אוניברסיטת הפעולה המדעי ש להוסיף אביב, אסא ליפשיץ, האוניברסיטה העברית, שנימו פעולה לימשין. עמיז, העמיק, ניקניין, העניין, טרינה, עולה אינים איניים אינים איני אינים איני אינים איני גיינים א