Ruben Pauncz

Born Hungary, 1920

Ph-D thesis:
Szeged University, 1944

Past Positions:
University of Szeged, 1948-55

Technion Positions:
Research fellow, 1956
Senior Lecturer 1957-60
Associate Professor 1960-62
Professor 1962-1968
Professor Emeritus 1968

Sabbatical leaves:
University of Uppsala, 1959-60
University of Florida, 1964-66
University of Basel, 1974
University of Waterloo, 1982-83
University of California at Santa Barbara, 1986-87

Major Technion/international responsibilities:
Member international school for quantum chemistry
Member international academy of quantum molecular sciences
External member of the Hungarian Academy of Sciences

Field of Research

Physical and Theoretical Chemistry

Electronic correlation problem, alternant molecular orbital method, construction of spin eigenfunctions, paired orbital method

Ruben Pauncz

(April 2008)

Many handicaps and a lot of good luck.
A scientific autobiography

This paper is dedicated to Hagit and Shmuli, Yael and Avri

Chapter 1: 1920 to 1948

1.1 Introduction

I never thought that I would write an autobiography, as I thought, in my opinion that my personal history was of no interest to anyone except my close family. However, Professor Ilan Marek very kindly invited me to consider writing down the development of several scientific ideas in which I participated. At first I hesitated, but then I thought perhaps there was something special in how I became a scientist. I still feel that in my story the unusual thing is the combination of terrible handicaps on the one hand and a measure of extraordinarily good luck on the other. In order to understand this I have to tell you my personal story from the beginning and then I can show the scientific steps taken and try to illustrate in each case how I found the problem and how I arrived at the solution.

1.2 My Maternal Grandfather

I was born on August 8, 1920 in Szoreg, Hungary. My father was a veterinary surgeon and grew up in a small village in the Mátra mountains, where my paternal grandfather was a shopkeeper. My father was the only son who went to university, all of his other sons remained in the village as shopkeepers.

My father took part in the First World War, where he was taken prisoner and there he developed a heart condition. He died very young in 1922, he was only 33 years old. I was two years old at the time, so I have no recollection of him. We were quite poor, so my mother had to go to work. She got a job as a clerk supervising children in foster homes, but she had to go to another town. I was left in the care of my maternal grandparents. My maternal grandfather, Dr. Alexander Popper was about 65 at this time. He had retired from the job of district doctor but still had several private patients who came to see him. In spite of the great difference in age, he became a father to me. He was an extraordinary person, a good physician and had a very wide interest in languages, literature, and geography. He liked books very much. He learned French from books, and when Szoreg was occupied by French troops at the end of the First World War, he was able to speak with the French officers.

Fig. 1. My father (Ferenc Pauncz at the time of the First World War)

He learned Hebrew, and knew it so well that when a new translation of the Hebrew Bible was published in Hungarian, he uncovered some translation errors in the first volume. He wrote to the editors and, after which they sent each volume to him before publication and he was able to correct some of the errors in the translation. At the end of the very last volume, the editors thanked many people, most of them Hebrew scholars, but they also thanked Dr. Alexander Popper for his careful remarks.

He knew Latin very well, and when I attended the Gymnasium, we walked along from our house in the street every afternoon, practicing Latin declensions and conjugations. In the higher classes he advised me, against my wishes, to study Greek. We were able to choose between Greek and French, and naturally I wanted French, so he promised to teach me French personally, and so he did. He also practiced stenography with me, and it was very unusual that a grandfather should act in such a capacity as a father. He had tremendous influence on my development and it was from him that I learned to love books. As I already mentioned, we were quite poor, but he still found ways of buying books. This "vice" was one that I certainly inherited from him. I became a bookworm from an early age, devouring the many books that he owned and later buying books on my own. When I wrote my first book I dedicated it to his memory.

Fig. 2. My grandfather - Dr. Alexander (Sandor) Popper

1.3 Elementary School

I attended elementary school from the age of 6 to 10 in Szoreg, my home village. I have very little recollection of my time at school, except for one episode: one of the bullies in the class beat me up saying:"because you killed Jesus". I went home crying and told my grandparents that one of the bullies at school beat me up because I was a Jew, and I remember that they looked very sad.

I had quite a few good friends at the school. When, many years later, I received an Honorary Doctorate from the University of Szeged, Professor Peter Laszlo arranged a meeting in Szoreg with my schoolmates from elementary school. I was very happy to see them, especially one of my old friends, Pavo Árpád with whom I liked to play after school at my home or at his. He now lives in Budapest, about 200 kilometers to the north from Szoreg, but came to Szoreg especially for this occasion. I also met him later in Budapest in my inaugural lecture at the Hungarian Academy of Sciences.

1.4 High School

I started high school (Gymnasium, from the age of 10-18) in the town of Szeged, which is only 5 kilometers away from Szoreg. I used the bus, and sometimes I rode my bicycle. My grandfather chose the Piarist Gymnasium, which was well known for its high standard, especially in Latin and Greek. The teachers were very good.

I remember an episode in my first class, when I was 10 years old, I was taken by the mathematics teacher to the fifth class to solve a problem on the blackboard. Of course this was a punishment for the fifth class students in order to show them that even a first class student can solve the given problem.

In the fifth class I had problems with mathematics, I was afraid of the teacher, who was well known for his terrible temper. I always received the highest grades in each subject, but this year I received only the second best for mathematics. My whole family (including my grandfather) was very worried. We knew that in order to get into university you had to have the highest possible grades. They even considered the possibility for hiring a private tutor for me in mathematics. Very fortunately the following year we had another teacher, a young and sympathetic person and once again I loved mathematics.

During my school years I had many good friends, most of them Jews. But I also had a special friend who had a great influence on my development, Gezi Szadeczky. His father was a librarian in the University. From Gezi I learned the love of literature, of music, and the arts. His father was instrumental in getting me accepted to the University. In spite of my good grades, there was very little chance for me to continue my education because we had *numerus clausus,* which limited the acceptance of Jews to the university.

1.5 University at Szeged

Thanks to the influence of Gezi's father I was accepted to the University as a first year student of chemistry. Gezi also enrolled in the same faculty and we studied together a lot. The mathematics taught at the university was at a very high level; they employed on the academic staff some well known mathematicians like Frederic Riesz, (who was my professor in first year mathematics) and others. I was a little stronger than Gezi in mathematics, and we spent long hours together solving problems. Unfortunately, in the second year Gezi left for the Technical University in Budapest. He decided that the level at our university was not high enough in some other subjects. We still kept in touch during the summers. I had another good friend at the University, Sandorfy Kamill, who came from Budapest. In spite of the fact that his father was a Judge in the High Court, he was not accepted in the University in Budapest because of his Jewish origins. We chose the same professor as doctoral supervisor: Professor Kiss Árpád, a physical chemist. My thesis was about the spectra of Schiff bases. I synthesized several compounds, and measured their absorption spectra. This was the easy part, the difficult part began when we tried to interpret the behavior of the spectra. I arrived at the conclusion that in order to understand this we would have to study quantum mechanical methods.

At that time nobody taught quantum mechanics or quantum chemistry at our University. I had to learn everything from books. In the library of Szent-Gyorgyi, the Nobel Prize winner, I found Pauling and Wilson's: "Introduction to Quantum Mechanics", and studied it very carefully. In our chemistry library I found another book: Hans Hellmann's "Einfuhrung in die Quantenchemie", an excellent monograph, and one of the earliest books written on the subject. I studied this book from A to Z, and even translated it into Hungarian for my personal use. I consider myself to be a Hellmann student, even though I never met him. After the war, when I returned to the University as a lecturer, I continued to use this book. After the Communist takeover, we were "asked" to give lectures about eminent Soviet scientists. I chose Hellmann, I gave a very enthusiastic talk about his scientific achievements. At that time I did not know the tragic fate of this great scientist. He had to leave Germany because his wife was Jewish. He moved to the Soviet Union and became a member of the Karpov Institute, where he was very successful, made many important contributions (Hellmann Feyman theorem, pseudo potentials, and so on). Some of the members of the Department became very jealous of his success and denounced him as a German spy. He was executed in the Soviet prison, at the age of 35. When I gave my talk, I did not know his fate, and very fortunately for me, nobody in the audience knew it either, otherwise I don't think I would have been able to write this autobiography today. Another book which I studied in depth was von Neumann's book ("Mathematische Grundlagen der Quantum Mechanik"). I found this book in the mathematical library. I mentioned earlier the magnificent high level of mathematics in our university. They also had a beautifully equipped library, most of the books they received in exchange for their world famous journal (Acta Methematica). I had a very good friend (a mathematician) Janos Suranyi (later he became a professor in Budapest), he drew my attention to Neumann's book.

1.6 Nazi Times

I have already mentioned the fact that for a Jew it was almost impossible to get into the university. I was very lucky that I was accepted (thanks to my friend Gezi). During my university years several times there were anti-Semitic outburst amongst the university students, mostly among those studying the Law. In my class (we were about 20 students) most of the students were alright, they even warned me if there was to be an anti-Semitic demonstration, so that I could stay at home in order to avoid the possibility of being beaten up. The war started one year after I entered the university, in 1939. We followed with trepidation the advance of the German forces in Europe. In Hungary there were some new laws: Jews were not permitted to serve in the Army, but they had to serve in special units, called work service units. These were very dangerous, many of my friends perished while working in these units. I was extremely lucky that my work service was postponed each year while studying at the University. I even took additional courses (theoretical physics and mathematics and teacher training courses) in order to prolong my studies at the University. By some extraordinary luck I received a postponement each year. By March 19, 1944 the Hungarian Government was tired of the war and with the aid of Horthy Miklos (the head of the country), they decided to withdraw from the German war. The Germans retaliated very quickly, and Hungary was overrun with Nazi forces. I saw German tanks in Szeged arriving on this date. I had to finish my university studies as quickly as possible. Fortunately my doctoral thesis was ready at this time and I took the doctoral examination in early April. My thesis was approved, but I did not receive my doctor's degree. Even on my doctoral exam I had to wear the Yellow Star, and within a week of my taking my exam there was an edict that no Jew could receive a university degree.

Other decrees promptly followed, and in June we had to leave our home and were transferred to a Ghetto in Szeged. In July came the last step, all the Jews in Szeged were to be deported. There were three transports from Szeged, and by some extraordinary luck my mother and I went in the first transport. After spending a day or two cramped with too many people in a cattle car, we arrived in Austria (Strasshof). The two other transports were sent to Auschwitz - though we did not know the name at that time. My grandfather, grandmother and my aunt were sent in the second transport. We were told that they were going to a place where they did not have to work! Fortunately, there was some bombardment by the Allied Forces whilest the train was on its way, and the train was diverted. They also arrived at Strasshof to our great joy. Sadly the third transport went to Auschwitz and very few survived.

We stayed in Strasshof for a month, and then with a small group of people we were sent to Amaliendorf by Gmund. During our stay in Strasshof, my grandfather, who was 80 at the time became ill and very weak. He died on the first day in Amaliendorf and was buried in a cemetery in a neighboring village. Many years after the war together with Kathy my wife, I visited the place. We did not find my grandfather's grave, but found mention of his name on a small plaque.

We were working in a factory which produced pullovers for the German army. We lived in a barrack near to the factory, and my grandmother worked in the kitchen, my mother and aunt in the sewing workshop and I worked in the manufacture of the pullovers. We worked about 10 hours each day, but received very little to eat. At the end of the war I weighed 35 kilos (my usual weight is around 70 kilos). However, we were very lucky to be in this place, in spite of all the hardships. This was a much better place to be in than all the other places where Jews were sent.

In the Spring of 1945 we were sent to a new place: Theresienstadt, which is north of Prague. Then in May 1945 the Russian troops liberated us. I because very ill with fleck typhus and was treated in a Russian field hospital. Fortunately I survived this very dangerous illness and a few weeks later we were sent in another cattle wagon in the direction of Hungary. After travelling for quite a few days we finally arrived home in Szeged.

1.7 After the War

We were very lucky to have survived this terrible Holocaust, but many of our relatives died during this period. Almost my entire family on my father's side (from Matra) perished. Many of my best friends did not return from the war.

The situation in Szeged was relatively better than in other places in Hungary - Szeged did not suffer as much from the war. Nevertheless it was very difficult.

We returned from the deportation and we had to start from scratch. I had to find work and I accepted an offer to work as an analytical chemist in the laboratory of a textile factory in Papa (this was several hundred of kilometers from Szeged). I spent about half a year in the laboratory and I found the work terribly dull. At home I started to read some scientific books (on non eucledian geometry, among others). I decided to leave the work and I wanted to make my way illegally to Israel. I returned home to say goodbye to my family, but somehow my plan did not materialize, as I became ill with tuberculosis. I had to be hospitalized in a Tubercolosis Sanatorium near Gyula, where I stayed for almost 2 years. In the end they found that the tuberculosis was concentrated in parts of my bones (fortunately there was no infection in the lungs) and I had to undergo a very difficult operation in which parts of the bone which were infected were cut out. The operation lasted about five hours, but it proved successful and after a month or so, I was able to leave the Sanatorium and return to Szeged. After being for such a long time in hospital it became quite impossible to leave for Israel.

I started to look for an opening in the University of Szeged, however there was no opening in Chemistry. Again I had a stroke of luck as one of the teaching assistants in the Department of Theoretical Physics decided to leave for France and his job became vacant. The Head of the Department (Professor K. Szell) knew me well from my university years and accepted me.

Chapter 2: 1948 to 1956

2.1 My return to the University

Almost four years had passed since I had left the University. One year when I was deported, more than half a year as an analytical chemist in the texile factory and about two years in the Sanatorium. During this time I almost lost touch with the scientific world, and now my most immediate task was to be a good teaching assistant. However, here I had a serious problem: I had almost never spoken in public, so I started my first class with some trepidation. I had only two students, both of whom are now professors, but nevertheless I had difficulties in presenting the subject. Fortunately this problem was solved in a short time and I discovered the joy of presenting new material to an audience.

My second problem was with the subject of theoretical physics. Although I had studied theoretical physics during my university years - especially towards the end - my basic training was in chemistry. So I had to work very hard, but it was a great joy to be back in University. During the first few months I studied the subjects that I had to teach, but after a while I had free time to look into the literature. My main interest was in the use of quantum mechanics for interpreting the structure of molecules. I began to refresh my knowledge of basic quantum mechanics and I started to read the then current literature in quantum mechanics and quantum chemistry. Unfortunately I did not receive any help from my professor, who was quite old at that time. Perhaps, at least in my eyes, he no longer had an active interest in research, and so I had to begin my own research.

I still had some contact with my previous research advisor, Árpád Kiss and together we were able to publish a paper on my doctoral thesis. Quite soon I was able to write another paper, in which I compared the two methods used in quantum chemical calculations, the molecular orbital and valence bond methods. This drew attention to some mathematical problems which arose when we used these methods.

I worked very hard during my first semester and was quite successful in studying both theoretical physics and the new developments in quantum chemistry. It gave me a great sense of satisfaction to be back in the field of science - I truly felt that this was where I belonged.

2.2 Summer Vacation

It was only half a year since I left the Sanatorium, and I worked very hard during those few months, I now felt that I needed a vacation. Fortunately I managed to find a place in a convalescence unit in the Mátra mountains for two weeks. My family had originally come from the Mátra region. These mountains are not very high, the highest is about 1000 meters high. The convalescence home was maintained by the Joint Distribution Agency, an organization that was helping the Jewish victims who had survived the Holocaust. Most of the other people in the convalescence home were about my age, among the girls I found an especially attractive one, Margit Jakobovits. She had been in some of the worst imaginable places: in Auschwitz, in other camps, and finally in Bergen-Belsen. She also suffered from tubercolosis and was treated for a year in Sweden, later spending two years in a Sanatorium in Debrecen, in Hungary. In the end she had to undergo a very difficult operation, in which one of her lungs was incapacitated and no longer in use.

We fell in love and after three weeks, having prolonged our stay in the Mátra by a further week, we decided to become engaged. Both of our families were astonished. She came from a deeply religious background and her family was not sure that I would be a good husband. My family were surprised that I was able to decide such an important issue in only three weeks. We then returned to our respective homes, Szeged and Fülesd in the north of Hungary. It was more than half a year later that we finally were married, but in the meantime we exchanged letters and visited each other. Her home town was very far from mine, almost a day away by train. The wedding took place in April 1949. Her family wanted to go to Israel and had to leave very suddenly, so it was a very modest wedding.

Fig. 3 Manyi (Miriam) and I at our wedding

It took place in the yard of the home of one of Manyi's best friends with no member of either family present. Manyi was well received by my family. The housing situation in Szeged at that time was terrible, so we had to stay in a two-room flat, which we shared with my mother, aunt and grandmother. Our third room was being used by another family and was only returned to us many years later when our first born (Shmuli) was born in 1954.

2.3 My first research papers

My first piece of research of interest was my encounter with Hellmann's book. Hellmann considers in detail the Thomas-Fermi model of the atom. In Hungary some very detailed research was made using this model. Paul Gombas was the leading theoretician in this field. I first met Gombas when I was a second year student, and Gombas gave lectures in theoretical physics. I was able to make some good notes from his lectures, and when he saw my notes he offered me a job as a teaching assistant. This was in 1940 and unfortunately for me, the following year he left our University for Budapest.

One can calculate the kinetic energy of an electron gas included in a given volume following the derivation given by Fermi. If one compares this result with the one based on a quantum mechanical model (or example, particles in the box), then the Fermi result is much lower than the quantum mechanical one. For some time I studied the origin of this discrepancy and I finally found the answer. In the Fermi derivation, one assumes that the electronic kinetic energy starts from zero and goes up to a maximum value. In the quantum mechanical model, the energy can never be zero (uncertainty relations), as there is a lower energy level. One can translate this into the

Fermi derivation and instead of zero, one starts from the smallest value. The new derivation gave a good agreement with the quantum mechanical results. I then showed this derivation to Gombas and he liked it, presenting a paper on this subject to the Hungarian Academy of Sciences. On his invitation, I spent three weeks at his Institute and made the acquaintance of his coworkers, among them Tibor Hoffman and Rezso Gaspar.

My second research paper was connected to a problem treated by Platt. He discussed the spectra of some linearly condensed aromatic compounds (benzene, naphthalene, anthracene and so on. He showed that one can understand the regularities found in the spectra of these molecules on the basis of the simple planar rotator model. I generalized his treatment, replacing the simple planar rotator by an elliptical rotator. This meant solving a new quantum mechanical problem.

The third paper was a critical analysis of Moffitt's method, which was a very interesting and new approach. He tried to interpret the structure of molecules by using both the homopolar bonds and the ionic structures of the constituent atoms. I chose a case where one can calculate to a greater accuracy, the atomic and ionic states (the hydrogen molecule). The result was very surprising. The use of the more correct ionic function gives a much weaker result. From this, it follows that it is not correct to interpret the ground state of the hydrogen molecule as a superposition of homopolar and ionic states.

For several years I worked completely by myself, but finally I was able to engage a very able coworker. I managed to convince Ferenc Berenc (a high school teacher) to return to the university and from this time on, we had a very fruitful collaboration. We wrote quite a number of theoretical papers dealing with condensed aromatic compounds, calculating their structure, diamagnetic anisotropy and spectra.

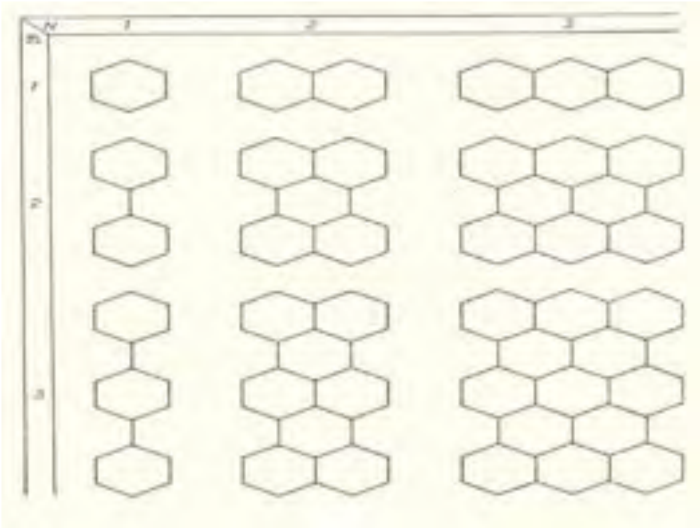


Fig. 4. Series of condensed aromatic hydrocarbons

I had a very interesting encounter with Edgar Heilbronner. While I was studying the condensed aromatic compounds, I came across a new paper by Heilbronner in which he used a brilliant argument. Instead of considering the individual compounds, he (fictitiously) joined them together to form a torus whose higher symmetry provided an easy way to calculate the eigenvalues. After this, he recovered the original molecules by "cutting" the torus in half, i.e. considering those solutions in which the coefficients on the atoms used in building the torus are zero. As we had already calculated some molecules from this series by the conventional method, I was able to verify his results. However, these were disappointing, as Heilbronner's results differed from ours. Then I was able to find the reason for this discrepancy: Heilbronner's results corresponded to molecules which differed from the real ones, in that the Coulomb integrals on some atoms differed from the original ones by + or -1. Following this discovery, I sent Edgar my paper and he received it in a pleasant manner. We met in Haifa some time later, and became friends, and I was privileged to have spent one of my sabbaticals at his Institute a few years later.

In my last paper from this period, I used perturbation methods for the treatment of systems built up from identical units. I was inspired by a paper by C.A. Coulson and G.S. Rushbroke. Their treatment refers to the case when the connection between the repeating units occurs only in one place. I generalized their arguments to the case where there are multiple connections. As an example we considered the series of molecules built up from naphthalene, or anthracene units. I obtained closed form expressions in which I could use the data referring to the basic units and the number of units. In 1955, I was allowed to attend the first Summer School in Oxford arranged by C.A. Coulson. I presented my paper in a seminar and it was favorably received by Coulson.

Fig. 5. Charles Alfred Coulson

Chapter 3: 1956-1970

3.1 Aliya

We had wanted to come to Israel for a long time, but during the communist regime it was impossible to leave. When I was allowed to attend the Oxford Summer School, I had to go by myself - my wife and son remaining at home. However, the situation changed dramatically during 1956 as there was an uprising, the communist regime collapsed and for some time one could cross the border, with little impunity.

It was not an easy decision to make as I had a very good position at the University and I was quite well known in Hungary for giving lectures and in other countries as well. In spite of the great dangers involved (Miriam was 5 months pregnant, Shmuli was only two and a half years old) we decided to take the risk. We crossed the Austrian border during the night and in the morning we were taken to a refugee camp. I phoned the Israeli Embassy in Vienna and told them that we wanted to go to Israel. They sent a taxi for us, giving us passports and within a few days we were on our way to Haifa.

I shall never forget the first view of Haifa from the sea, and we were happy to be in Israel. Within a couple of days I had visited the Technion (at that time it was still in Hadar) in order to look up Chemical Abstracts where all my papers (about 15) were refereed. For the first time I met David Ginsburg, he was the Head of the Department. He offered me a position as a Research Associate. I also visited the Weizmann Institute and had an interview with Aron Katzir, who also offered me a position. At that time there was no teaching at the Weizmann Institute and since I very much enjoyed giving lectures, I accepted David Ginsburg's offer.

We were placed in an Aliya Center for those people with an academic background in Kiryat Hayim. The first years were quite difficult, as we had come on aliya without anything, but we immediately felt at home. My second son (Avraham) was born in April 1957.

I knew only a few words in Hebrew, those which I learnt many years ago from my grandfather. David advised me to attend an Ulpan for an intensive course in Hebrew, but I preferred to start working immediately. I started to learn Hebrew by myself in the afternoons and in the mornings I would work at the Technion. A few months later, I wrote a new paper ('The Structure of Circumanthracene'), and following David's suggestion I submitted it to the Journal of Chemistry (London), where it was immediately accepted. When I felt my ability in Hebrew was sufficiently strong enough, I accepted David's advice and attended a two months intensive Ulpan. The following October, I gave my first course in Hebrew, having only a few students (some of them are now Emeriti Professors in the Technion). The course proved successful and I received tenure as Senior Lecturer.

Fig. 6. At the Technion in 1956

3.2 Collaboration with Amitai Halevi and David Ginsburg

I decided to approach a new field of research which was close to one being undertaken in the department … I was most fortunate that Amitai Halevi was engaged in research to which I was able to contribute. He is an excellent physical organic chemist, his speciality was at that time the investigation of the second order isotrope effects. We managed to have published three joint papers. In the third one, Amitai's student (Arza Ron, who is today an Emeritus Professor in the Department) also contributed. I found the work with Amitai very stimulating, and I found in him a great friend.

I also found a common interest with David Ginsburg. He was a first rate organic chemist and at the same time an excellent head of the department. We had a joint paper in which I made calculations on the structure of some organic molecules. Later, I had many discussions on this subject with Shneor Lifson from the Weizmann Institute.

3.3 The first Summer School of Per-Olov Lowdin

In 1958, I received a personal invitation from Per-Olov Lowdin to attend the first Summer School which he was organizing in Sweden. David arranged for me to receive a travel allowance for this purpose and I was on my way to Sweden. There were 30 participants, each of them had received a personal invitation from Lowdin. I knew him very well from the literature and also sent him the reprints of some of my papers. The School lasted six weeks, it was very intensive and I was able to help the other participants in solving some of the exercise problems. At the end I gave a seminar on my last work in Hungary.

Lowdin was quite impressed and he invited me to come to Sweden for a year and to be one of the main lecturers in the next Summer School.

David found the idea a little premature, and he suggested that I should wait for my first Sabbatical. I explained to him that I had never had a teacher and that this was to be my first opportunity to learn from a famous scientist. He agreed and in 1959 I was on my way to Stockholm. The second Summer School was attended by a great number of scientists (Amitai Halevi, and Joshua Jortner were participants from Israel, Roald Hoffmann was also there, he was a research student at that time). I must admit to having been a little frightened at the prospect of giving lectures in English, but apparently everything went quite well.

Lowdin retained me in all the following Summer Schools (and later Winter Schools in Florida) and I became one of the main lecturers in 35 Schools organized. More than two thousand scientists attended these Schools coming from quite a few countries.

3.4 My "annus mirabilis" in Uppsala

My year in Uppsala surpassed all my expectations. The scientific atmosphere in the institute was very vigorous. I had the good fortune to work with Lowdin and one of his guests (Joop de Heer). When I attended the first Summer School in Oxford, Coulson spoke about a promising new method: "The alternant molecular orbital method" proposed by Lowdin. During my year in Uppsala we worked on the generalization of the method and its applications for alternant conjugated systems. The results were very encouraging. This field of research became one of the central themes in my research for at least a decade. I also had a doctoral student with me from Israel (Zeev Ritter). With Lowdin, de Heer and Ritter we published a large number of papers during this wonderful year. Lowdin asked me to stay another year but I declined as I did not want to stay away from Haifa for such a long time. On the basis of my achievements David proposed me for Associate Professor and this was approved (1960).

3.5 The first Winter School in Florida

Following my return to Haifa, I received an invitation from Lowdin to be one of the main lecturers in the new Winter School in Quantum Chemistry to be held on Sanibel Island, Florida. Lowdin had another position at the University of Florida, Gainesville. I was fortunate to have received permission from David to be away for a couple of weeks (I made up in the number of lectures given to my students here ??). It was the first time that I had visited the States. The Dean of the Graduate School of Florida University visited one of my lectures and he was quite impressed.

One year later I received an invitation from the University of Florida to be one of the Graduate Research Professors. It was a very tempting offer (John C. Slater, one of the founding fathers of quantum mechanics was also a Graduate Research Professor there), but I did not want to leave Israel, so I politely declined the offer. Very soon after this I received the promotion in the Technion to full professorship (1962).

Fig. 7. Per-Olov Lowdin

3.6 My first Sabbatical in Florida

From 1959 to 1964 I had quite a few graduate students for master and doctor degree. We continued to work on the application of the alternant molecular orbital methods for condensed aromatic hydrocarbons and we made several generalizations of the method. In 1964 I was to take my first (true) Sabbatical leave. I spent this in Florida, giving lectures in the Graduate School and continuing to lecture in the Winter School on Sanibel Island. I received an invitation to write a monograph on the Alternant Molecular Orbital Method, and it took me a whole year to finish it, I incorporated most of our research. I received permission from the Technion to stay another year. In the meantime, the Chemistry Department moved from the Hadar site to its new building as at present (David Ginsburg's careful planning) and this meant a great improvement in the research facilities.

3.7 Research activity from 1966 to 1970 - excellent research students

After finishing the book entitled on the 'Alternant Molecular Orbital Method', my research interest turned to other subjects. I was fortunate to have some brilliant research students: with Harel Weinstein (now at Cornell University having a very vigorous research program) we investigated the use of localization methods in quantum chemical calculations and wrote a research paper and a review paper on the subject (together with Maurice Cohen); with Uzi Landman (now at Georgia Tech) we engaged in some very interesting research on some methods. Uzi Landman became one of the leading theoreticians, and he made some very important results in many fields. I was fortunate to have Gabriel Kventsel as a research student. He came from the Soviet Union, where he had made important contributions, but he still needed a degree. I was his supervisor and I had an extremely easy job. He remained in our Faculty, where he has undertaken good research and has an outstanding teaching record. In 2007 he retired, but is still continuing to work in the Faculty. Shalom Srebrenik was another of my research students and was very talented, we managed to work on some good research. I put his name forward (together with Yoram Tal) for postgraduate positions at Richard Bader's department (McMaster University, Canada). Bader was very grateful for my recommendations. Both Srebrenik and Tal made very important contributions to his research program.

With one of my research students (Arie Lemberger) we discovered something very interesting. We showed that the usual interpretation of Hund's rule is not correct. This research was continued by one of my outstanding students - Jacob Katriel, today a Professor Emeritus in our Department). I convinced him to stay at the Department as a lecturer, where he was extremely successful, having his own research program and his students (Nimrod Moiseyev, one of our outstanding faculty members was one of his students). We had some other common research with Katriel and he also collaborated later with Amitai Halevi.

3.8 Academy of quantum molecular sciences

In 1967, there was a meeting of some of the leading quantum chemists in Menton (France) and R. Daudel, A. and B. Pullman, P.O. Lowdin, C.A. Coulson, R.G. Parr, C. Roothaan, J.A. Pople were among the participants. They decided to establish an Academy of Quantum Molecular Sciences with the purpose of furthering advancement in this field, arranging international congresses every third year and honoring young outstanding researchers with a medal. Raphie Levine from Israel received the first medal with Joshua Jortner who followed soon after (both of them were later elected to the Academy). They also coopted some leading theoreticians (John C. Slater, Robert Mulliken, Linus Pauling and some others). The following year was the first election of new members, four well known scientists were elected (E. Huckel, J. Van Vleck, E.B. Wilson and J.O. Hirschfelder), the fifth member to be elected was a much younger person, and that was me.

During the Summer School in 1968, Lowdin told me that I had been elected to the Academy. At first I did not believe him, as I thought that he was pulling my leg. But when I returned to Haifa I found the letter from the President of the Academy (Raymond Daudel) in which he announced my election. I was definitely embarrassed on seeing that some very good scientists in the field whose work I estimated very highly were still not members. Subsequently, I worked very hard during the following election meetings to bring some of my esteemed contemporaries onto the board of the Academy and in many cases I succeeded.

Chapter 4: 1970-1988

4.1 The Spin Eigenfunctions book

In the beginning of 1970 I had a very difficult period. After so many years of creative activity, I suddenly experienced 'a writer's block'. I felt that I had no new ideas. It was a very unpleasant feeling. I still derived much satisfaction from teaching, but my future looked extremely bleak.

A change in this situation came unexpectedly with an invitation to write a monograph on the subject of construction of spin eigenfunctions. I had received a similar invitation many years earlier, but at that time I did not feel ready to tackle the subject. Now I had plenty of experience, including my teaching of part of the subject in the International Winter and Summer Schools. I worked on the book for almost four years, and it had a marvellous influence on me, as I found that I had plenty of new ideas (all connected to the subject of the book). I published a couple of papers, and I had a very pleasant sabbatical leave in Basel with Edgar Heilbronner. The book appeared in print in 1974, it became a very good tool for mastering the subject. I received many compliments from different people who used the book in their research.

4.2 Collaboration with Al Matsen

I met Al Matsen for the first time in the Valadalen Summer School (1958), and from that time on we became good friends. He introduced me to the use of the symmetric group in quantum chemistry, and he also advocated the idea of spin free quantum chemistry. In the 1970's there was a new method in use in the treatment of configuration interaction based on the implementation of the representations of the unitary group. Matsen made the early contributions, and later great progress was made by Joseph Paldus (Waterloo University, Canada) and Isaiah Shavitt (who, at an earlier stage had been my colleague at the Technion). I became interested in the subject (later when I had a sabbatical leave, this was taken with Joe Paldus and Jiri Cizek in Waterloo). I invited both Al Matsen and Joe Paldus to give lectures at the Technion. Finally, Matsen invited me to coauthor a book with him on the use of the representations of the unitary group in quantum chemistry. I spent part of my sabbatical at Matsen's Institute and the book appeared in 1986.

Fig.8. The inscription reads: "To Ruben Pauncz with fond memories of a long and fruitful collaboration. Al Matsen"

4.3 A personal tragedy and a new life

Miriam, my wife became very ill in the beginning of 1983. She suffered from the loss of one of her lungs (a legacy of Auschwitz). After a year of illness she died on February 5, 1984. She was a wonderful wife, the mother of my two sons and the grandmother of the first two grandchildren.

After a year of mourning, I met my future wife: Kathy. Her husband died at about the same time as Miriam. We married in the fall of 1985. We have been very lucky with each other, as it is very seldom that a second marriage can succeed as well as ours.


Fig.9 Kathy and I at our wedding

4.4 Symposium on my 65th birthday

My dear friend (and "scientific grandson") Nimrod Moiseyev arranged a symposium in 1985, he invited all the leading theoreticians in Israel to each give a lecture. Our highly efficient departmental secretary (Mrs. Kohava Reznik) arranged that all my friends from abroad sent congratulations by telegram. David Ginsburg gave the introductory talk with his famous dry humor, and I was deeply honored by the presence of all the leading theoreticians at that time. Among them was Joshua Jortner who had become one of the leading theoreticians recognized internationally.

Nimrod also had another initiative: he was the guest editor of a special issue of the Israel Journal of Chemistry (Vol.31, No.4, 1991) with the title: 'New Trends in Quantum Chemistry' and this special issue was dedicated to me. There were 19 contributions by my former students, and my scientific friends. I was greatly honored by his kind action.

4.5 My last sabbatical

I spent my last sabbatical in two different places. The first sabbatical was spent at Santa Barbara, California, where I was invited by Bernie Kirtman, this resulted in a very fruitful collaboration. I started to look once more at the alternant molecular orbital method and tried to generalize it for arbitrary systems. The final results were a little disappointing, as the method worked very well for conjugated hydrocarbons, but for other systems it was less successful.

The second part of my sabbatical was taken at Waterloo University (Canada) with Jiri Cizek and Joe Paldus. I had previously been there, and this time it was again very pleasant resulting in a couple of publications.

Chapter 5: 1988 to the present time

5.1 Professor Emeritus

I became Professor Emeritus in 1988. Fortunately, I was able to keep my office and I did not feel any great change. I still gave some graduate courses and I continued with my research. I also had more time to read the recent developments in our field. I felt very fortunate that in our Department we conducted such vigorous research in quantum chemistry. Nimrod Moiseyev (Jacob Katriel's student) became a very important member of the group (later he was to become the Head of the Theoretical Group). He had many excellent students, one of them (Uri Peskin) joined the group as an Associate Professor. Nimrod also attracted many other outstanding research students.

5.2 The symmetric group in quantum chemistry

I received an invitation from David Klein to write a monograph on the use of the symmetric group in quantum chemistry. It was a great pleasure to work on this project. This was the first book which I had written using the latex language, which meant that at the end I sent only a disk containing the entire book to the publishers. The book appeared in print in 1995. I am hoping to give a graduate course on the subject of the book in the first semester of the next academic year.

5.3 Construction of spin eigenfunctions - an exercise book

In 1999, I started to work on a new and abridged edition of one of my books which appeared in 1974. I gave a graduate course on the subject in 2000 - and I had some outstanding students on my course. This forthcoming semester I shall once more give this course, and some very good graduate students (together with one postdoctoral fellow and a young talented faculty member) have expressed their interest in participating in this course.

5.4 The medal of the Israel Chemical Society

In 2005, close to my 85th birthday, I received the medal of the Israel Chemical Society. I was deeply moved by this great honor, all the previous and later recipients were great scientists. I felt that it was a recognition of the role which I played in helping to bring quantum chemistry in Israel to such a high level where it is today. I was simply lucky that when I arrived in 1956 this field was still in its infancy. I had the great fortune to introduce it to the Technion, and many years later at the Weizmann Institute of Science I gave a graduate course for several years. I also gave one semester at the Bar-Ilan University.

5.5 My scientific friends

During many years of scientific activity I collaborated with quite a few scientists both in Israel and abroad. Some of them became my personal friends. I cannot finish the reminiscences without speaking about them. I have mentioned already Sándorfy Kamill, Alfred Coulson, Per Olov Lowdin, Al Matsen, Edgar Heilbronner. To my great sorrow they are no longer with us. I shall always treasure their memory.

Roy McWeeny - I met Roy for the first time in Valadalen. I was well acquainted with his work, he was. one of the lecturers there. We have stayed in close touch since then, and I invited him to Haifa where he gave an excellent lecture course. I have stayed with Kathy at their home in Pisa - Virginia and Roy were excellent hosts.

Bob Parr - I spent part of my Sabbatical at his Institute. He is one of the leading theoreticians in our field and he has given lectures at the Technion.

Joe Paldus and Jiri Cizek - I visited Waterloo several times and we had several close collaborations. I invited Joe Paldus to Haifa, where he gave a very interesting course.

Bernie Kirtman - I spent a Sabbatical in Santa Barbara and found Bernie and Tybe to be wonderful hosts.

Joop de Heer - we spent a year together in Uppsala collaborating on the alternant molecular orbital method. Joop later invited me to Boulder Colorado, where I gave a week of lectures. We have remained close friends since then.

Sten Rettrup - he participated in one of the Summer Schools. In one of my lectures I remember that I mentioned an unsolved problem and the very next morning Sten came up with the solution. We have remained friends since then, and he invited me to Copenhagen where I gave a couple of lectures.

Jan Lindenberg - when I arrived in Valadalen in 1958, Jan was one of the graduate students. We have remained close since then, Jan invited me to Aarhus where I gave a short series of lectures.

5.6 Final remarks

I started my recollections by saying that I had terrible handicaps, but I have also had great luck. I feel very lucky that in spite of the handicaps I was able to fulfill many of my aspirations.

I am grateful to Ilan Marek for suggesting that I write these recollections down.

I would like to thank my wife, Kathy and my sons, daughters-in-law and my six grandchildren for their loving help.

Arza Ron

Born Israel, 1934

Ph-D thesis:
Technion-Israel Institute of Technology, 1961

Past Positions:
Princeton University, 1961-63
University of California, 1963-64

Technion Positions:
Research Fellow 1964-65
Senior Lecturer 1965-70
Associate Professor 1971-81
Professor 1981-2002
Professor Emeritus-2002

Sabbatical leaves:
Visiting Research Scientist , TU Berlin Summer 1997
Visiting Research Scientist, Dartmouth College, NH, Spring 1995
Visiting Research Scientist, Bell Laboratories, Murray Hill, NJ, Fall 1994, Summer 1995
Visiting Research Scientist, University of California, Irvine 1983, 1986-1987
Visiting Research Scientist, IBM Yorktown Heights 1982
Visiting Research Scientist, University of California, Irvine 1977-1978
Visiting Research Scientist, University of California, San Diego 1972-1973
Visiting Research Scientist, University of Southern California 1967-1968

Major Technion/international responsibilities:
The senate steering Committee 1996-1999
The Harvey Prize Committee 1996.
The Senate Standing Committee for Promotion and
Appointments of Senior Lecturers and Lecturers, 1988-89
The Senate Standing Committee for Approval of Honorary Degrees, 1990-92
The Senate Committee for Academic Development, 1984-85
Advisory Council of the S. Neaman Institute for Advanced Studies in

Science and Technology, 1985-88
Head of the Centre for Pre-academic Studies, Technion 1979-82
Head of the Solid State Institute, Technion 1990-94, 1997–2001
Member of the Israeli Council for Higher Education (MALAG), 1992–2001
National Higher Committee for Promotion and Appointments of Science and Engineering Professors in all Israeli Colleges 2003

Field of Research

Physical Chemistry, Solid State

Molecular spectroscopy of organic molecules in condensed phases: single crystals, solutions, molecules imbedded in rare gas matrixes and adsorbed molecules on surfaces.

Spectroscopic studies of semiconductors and their quantum structures.

Study of the elementary excitations: excitons, phonons, their dynamics and the interaction between them.

Experimental techniques: luminescence, excitation spectra, resonance Raman, Raman excitation profile, microwave modulated spectroscopy and time-resolved spectroscopy. All measurements are carried out at cryogenic temperatures.

Arza Ron

(April 2008)

I had joined the Chemistry Department as Senior Lecturer in 1965, ten years after its founding. I transformed from one of the youngest faculty members at that time to one with the longest seniority in the Department. My strong links to the Chemistry Department date~~s~~ to the years before its establishment~~,~~ when it was part of the Faculty of Science MADAIIM (Chemistry, Mathematics and Physics) which was founded in 1952.

I was one of the ca 15 students who in 1953 enrolled in the Faculty of Science, and all of us studied together as a class in the first year. It was not clear to me which of the sciences will be my choice. After a long internal struggle I chose Chemistry over Physics. In 1954 David Ginsburg joined the Technion and became the Dean of the new born Department of Chemistry, and I was one of its very few students. I almost complied with David Ginsburg's suggestion to choose Organic Chemistry as my major.

As a compromise I majored (fourth year undergraduate as well as MSc) in Physical Organic Chemistry. During these three years I taught in the physical chemistry lab and recited in the class room Physical Chemistry subjects; one of my students in this period was Zeev Tadmor who became in later years the President of the Technion. My MSc thesis advisor was my favorite teacher, Amitai Halevi (and I his first or second graduate student). The atmosphere in the Laboratory was very pleasant and there was a feeling of freshness, of pioneering. Amitai had brought with him from the Hebrew University in Jerusalem a home-built mass spectrometer, a source for many frustrations, but it gave the Lab the character, seemingly, of a modern instrumental laboratory, and was supposed to impress potential donors (see a picture of Amiai Halevi and I, taken by Technion PR and distributed in a booklet among potential donors). For my thesis I used a relatively modern spectrometer coupled with a 19th century instrument, a dilatometer; the poor reproducibility of the measured results was a source of frustration for me. To our satisfaction the experimental results supported the expected direction of the secondary isotope effect of deuterium. It was my first successful scientific endeavor but not my first publication.

In 1956 a well-known theoretical chemist~~,~~ from Hungary, Reuben Pauncz, joined our faculty. He taught us the secrets of quantum chemistry and the art of chemical computations. Only during the first semester did he lecture in English, but after six months he shifted to Hebrew. For me, his Hungarian English was~~,~~ the best-understood English I had heard until then. In the summer of 1959, after completing my MSc thesis, I did a series of chemical computations under the supervision of Reuben Pauncz using a terribly noisy mechanical calculator; the results of these calculations appeared as my first publication. During that summer, and later while waiting for my PhD advisor Otto Schnepp to return from his Sabbatical leave, I prepared myself for the Comprehensive Qualifying Examination.
This examination was composed of three written sessions, each lasting four hours, covering everything we had ever learned in Organic, Inorganic and Physical chemistry, followed by an oral examination covering the written material and more….

I read what I wrote until this point and felt that it lacks essential facts that can explain my development as a person. So I had a change of heart and decided to try writing my "biography" and also to mention some other scientific figures that are part of the past of the Chemistry Department, such as Otto Schnepp and Yenina Altman.
As a proper disclosure, for whoever decides to read my "life story", let me reveal at the start: I am not a holocaust survivor, not a war hero, not a heroic scientist, I did not fight in the war of independence, contribute scientifically to the war effort, and did not come from a developed country to the help of Israel. I was just born raised and grew up in Israel in its trying and interesting period of the twentieth century (like the Chinese 'blessing' "may you live in interesting times").

Let me start at the beginning:
I was born in Ra'anana in 1934 as the second daughter to parents, who (my father specially)~~,~~ had wished very much for a boy. I hope that as a child, and also later as an adult, I lived up to my father's expectations and compensated for not being born a boy.
Both my parents came as Zionist pioneers to Israel in the early 20th century. My father, a young protégé student in TARBUT high school in Bialystok, Poland, left for Palestine on his own just before graduation. My mother was a talented, experienced

teacher in Sofia, Bulgaria. Both started and ended their lives in Israel as working people who led a very modest life.
When I was born my parents were relatively better off, my father managed a citrus orchard, having a decent monthly income, so they could add a room to the 24 square meter hut they owned. There was no electricity or bathroom inside the wooden hut (ZRIF).
My early childhood, playing with the neighborhood kids, bare foot~~,~~ in the unpaved sandy Rambam street in Ra'anana (like a huge sand box), was very pleasant and uneventful.
All these nice days came to an end with WW 2. My grand father, who visited with us~~,~~ from Poland, looking (too late) for a route to bring his family to Israel, went back to Poland in August 1939, to be~~,~~ in times of war~~,~~ with his wife, my grandmother, and three younger sons, my uncles. Almost no one survived the war.

My father had lost his steady job, the source of a secure income and became a day laborer. The owner of the citrus orchard had to uproot the citrus trees in his orchard. He had lots of expenses and no income from the orchard, since fruits could not be shipped to Europe during the war and his financial reserves dried up.
My father yearned and longed for his mother, whom he had last seen at the age of 17, and now he lost hope of ever seeing her again.
Though I was a relatively young child I followed the psychological and economical crisis in the family. I could not fall asleep at night until I heard my father coming home (around 11) from the work allocation office telling my mother whether or not he got work for the next day. Since that time and until now I feel that next to war, unemployment is the worst thing in the life of people.
To supplement the family income, my parents added one more room to the hut and took children to board with us (up to three at the time). I was jealous of these children, they took away from me~~,~~ a big part of my mother's attention, and coming from well to do families they received presents and beautiful clothing from their parents, who wanted to compensate for sending them away from their home.
The years of WW2 were hard for me as a child. The end of the war was also traumatic. It became clear that from the whole family of my father only one brother, and one cousin had survived; this cousin became a student at the Hebrew University and was killed in Jerusalem in 1948.

In elementary school~~,~~ I was very much appreciated by my teachers, who predicted a “great future” for me, may be even that one day I will become a professor. At that stage I did not make too much of an effort to live up to my teachers' expectations.

The youth movement, MACHANOT HA'OLIM, played a very important role in my life, more than school. I was a devoted member, never missed any activity, three meetings a week, during the school year and during summers working in one KIBUTZ or another, in preparation to join a KIBUTZ or build a new one, in due time.

High school education was not free and there was no high school in Ra'anana.

The school I wished to attend was TICHON HADASH in Tel Aviv. Though it meant almost two hours commuting each direction; tuition and transport fees too were a big economic load for my parents. I started attending TICHON HADASH in September 1947.These four years of high school were among the best years of my childhood life. It was a unique high school, founded as a cooperative by a group of intellectuals who had come from Europe to Palestine in the early thirties. For some of them there were no open academic positions at the University, while for ~~s~~ others education was their life vocation.

The Principle of the School was a German-born lady Tony Halle. Tony (all teachers were approached by their first name) was a progressive and ideological lady who believed in a better world. Most of the teachers were very much appreciated and some were even idolized by the students. Most of the kids in school were members of youth movements, and the high school supported the KIBUTZ ideology. Few of my class mates ended up in a KIBUTZ; however six of us ended in the Academic world, with three at Technion.

My army service was in the NACHAL (still dreaming about future life in a KIBUTZ). But when my group in the NACHAL went to build a KIBUTZ, I followed Amiram (my future husband) to the Technion.

As students we lived in a rented room, together with a nice family in a small apartment and supported ourselves by teaching high school kids. Amiram taught in a KIBUTZ High School 20 km from Haifa, and I was a private tutor of high school kids who needed help in mathematics for their matriculation exam.

In the summer between sophomore and junior year we had our first baby. We had no place to live in~~;~~ and no nearby parents to help with the baby. The solution was to live in a KIBUTZ 20 KM from Haifa which needed a high school teacher. The KIBUTZ provided our whole family with "room", board and baby care in exchange for ~~a~~ full time teaching by Amiram in their school.

If I had to make the hard decision: doing science or having children my choice would be, with no hesitation, the second one. It was great that I did not have to make this choice, and a solution was found to combine these two aspects of life. It was not always easy. Very early in the morning I went to the nursery to nurse my baby. Travel to the Technion was quite difficult. One km by foot and then two busses each direction, three for Amiram who had to get to the Technion campus in Nave Shaanan. In the early fifties, with the large immigration, there was a shortage of busses, and many a time the bus on the main road Tel Aviv - Haifa would just pass by me without stopping. We were a very small group of student in the Chemistry class, only 5 of us graduated in 1957. Yenina Hesheles, later Yenina Altman, was one of my class mates. I chose to write about her because in different circumstances she could also have been an emeritus in the Chemistry Dept. Yenina was born in Poland, and was one of the very few survivors of the Yanovski camp. As a 12 years old child, with no family in a German concentration camp she wrote poetry. The poems she wrote were smuggled to the outside world by Polish partisans. Before the final liquidation of the camp, the partisans managed to smuggle her and a few others out of the camp. She was placed as a servant in a Polish family, where she wrote the Diary, day by day about her life in the camp. This diary with other 3 diaries was published in 1955 under the name "Gone in the Flames" the introduction to the book was written by Arnold Zweig. We, sometimes, studied together, preparing for exams. I could not understand how, after having gone through hell in her life, she could be so anxious about exams- "such a minor thing". I learned from her that if she would have kept the same frame of references as in the war, in the camp, she could not have led a normal life.

Yenina and I took the qualifying exams (these comprehensive exams were given for the first time in 1958 and did not last for many years) and became among the first few PhD students in the Chemistry Dept. Yenina, besides being a very talented organic chemist, is also a talented author, a talent that saved her life. Beside the diary she wrote as a child, she published four books and for one of them which she wrote under the pseudonym Zvi Eaten (the names of her two sons), she was awarded the AKUM prize in 1999. Her last book "The White Rose", I found most fascinating. It is a product of ten years of historical research~~,~~ Yenina had done while working as a visiting scientist in the Organic Chemistry Laboratory at the University of Munich.

The book is a story of Germans in between the two great wars and WWII, two groups of them: German scientists whose life was conducted similar to other Germans. The second group named “the white rose” was a small number of young people, who during the war actively resisted the Nazi regime. Their actions had positive impact on several aspects of the war.

Just before starting to work on my PhD I had my second baby, whom Otto Schnep named “The big boss” (: it is not me the boss in the lab, the real boss here is Arza’s baby).

My PhD research comprised two parts: Spectroscopy of adsorbed benzene on porous glass, with Mordechai Folman and with Otto Schnepp, a pioneering work in the area of spectroscopy of adsorbed molecules. Our effort to look at benzene adsorbed on metallic surfaces was long before it's time, and failed. It was achieved years later in vacuum systems five orders of magnitude better than the one we had in our lab in 1960.

The second part was spectroscopy at very low temperatures of 2,2-paracyclophene single crystals. The spectroscopic results corrected the published crystal structure of this molecule. The spectra were obtained on a photographic plate; I used a densitometer located in the Physics Department to obtain graphic spectra from the plate. A physics graduate student looked over my shoulder, while measuring the plate and said: you are lucky, so many sharp lines, you have already a thesis. It was published and well received, but I did not include it in the thesis. The low temperature was achieved with liquid hydrogen, dangerous to store and dangerous to work with. We would carry the storage tank, with 35 litters of liquid hydrogen four flights up the stairs. Belts were connected to the handles on both sides, and two of us with the belts harnessed to our shoulders carried the storage cryostat to the Lab. Academically I understood the danger, but being young and stupid I did not feel any fear. Several years later, my graduate students in the Technion~~,~~ also used liquid hydrogen I was hysterical and spent many long evening and nights in the Lab when experiments with liquid hydrogen were run. At that time I understood Otto Schnepp's anxiety – which in the past I attributed to his character.

Otto served in the Technion for 10 years 1955 – 1965. He had a decisive role and impact on the character of the Chemistry Department and specifically on Physical Chemistry. As a youngster from a Zionist Jewish family living in Vienna he fled

Europe~~,~~ with his family in 1938 to Shanghai~~s.~~ In Shanghai~~s~~, living in very poor conditions he acquired a high school education and a B.Sc degree. He did his PhD in Berkeley under Donald McClure a well known spectroscopist. While in the States Otto was preparing to realize his Zionist dream to become a scientist in Israel.

He arrived in Israel with a wife and baby and encountered enormous difficulties both in the Lab and at home. These were years with a shortage of food, especially for people who knew no one among the more established Israeli population. Coming from affluent US it was especially hard. The conditions in the Lab were even worse: no money, no basic instrumentation, no administrative help, not even a direct phone line; each call had to go through the central Technion switch board, sometime waiting hours for a connection to some office in Tel Aviv. Otto was a very determined person. He worked enthusiastically, spread his enthusiasm around, invested in his graduate students, and brought a style not known before. I see in Otto together with Mordechai Folman (in his understated style) the founding fathers of Physical Chemistry in the Technion.

During the ten years of his residence in the Technion he ran several research projects financed by American sources; educated four PhD students, introduced modern research techniques ~~,~~(some before their time). In spite of these impossible conditions Otto managed to publish papers which became classics. Later in 1965 Otto went back to the States and left behind him, a nicely equipped lab, two PhD students and two Master students in the first stages of their research. I as a brand new faculty member inherited it all, the Lab, the equipment~~s~~ and the students.

I managed to finish my DSc in two years (at that time the Technion was granting DSc degrees rather than PhD – just like MIT).

In 1961 Amiram and I were ready to try science in the big world. It was a great time for science in the USA. Trying to compete with the Russian sputnik, all doors were wide open for young scientists. All applications of ours were positively answered.

We had to choose between Stanford and Princeton. Our choice was Princeton for a prosaic reason that my loved and loving great uncle was the orthodox rabbi of the nearby town of Trenton.

My going to work, as a mother of two young children, was not viewed well at all by the families in the junior faculty housing where we resided. From the hundred families who lived there only two women worked, I was viewed as a cruel mother.

I worked in Donald Hornig's Lab, who shortly after I arrived became President Kennedy's scientific advisor. He was a very smart and sharp scientist, but his main interest at that time was politics. When I arrived at the Lab, there was no one there who knew the ins and outs of the lab; there was no overlap with the previous post doc, so I was all on my own. I learned the hard way, how one should not run a lab.

I suggested to Hornig to study the vibration overtones of crystalline HCl, anticipating two kinds of overtones: one cooperative (on adjacent molecules – at twice the fundamental frequency) and the regular one. Indeed it was observed for the first time and became one of my better known publications. A few months later another post doc arrived this time from the Weitzman Institute, Rehovot.

With Sol Kimel in the Lab we tolerated very well the absence of Hornig. Sol and I together studied the intermolecular interactions in crystalline methane. In addition to the immediate scientific results, there were many positive implications for my scientific development and that of the Chemistry department of the Technion. Above all it was a beginning of a long cooperation and splendid friendship.

From Princeton, I followed Amiram to the University of California, San Diego. There I worked for the Nobel laureate Harold Uri. I did my best to prove by chemical and physical experiments his theories about the source of cliftonite cubic macroscopic graphite) in meteorites. At the end of that academic year one more baby joined our family. In appreciation and love to Uri the man, we had planned to name the baby Uri, in case it was a boy (it was a girl)

Uri’s science was at a decline and he had to depend solely on his two research fellows, me and Ramah, an Indian post doc. I had lots of empathy toward Uri in his declining years; it was his last active year. Scientifically it was not a very fruitful or useful year for me.

Notwithstanding ample opportunities for work in the States, we felt it was time to go home.

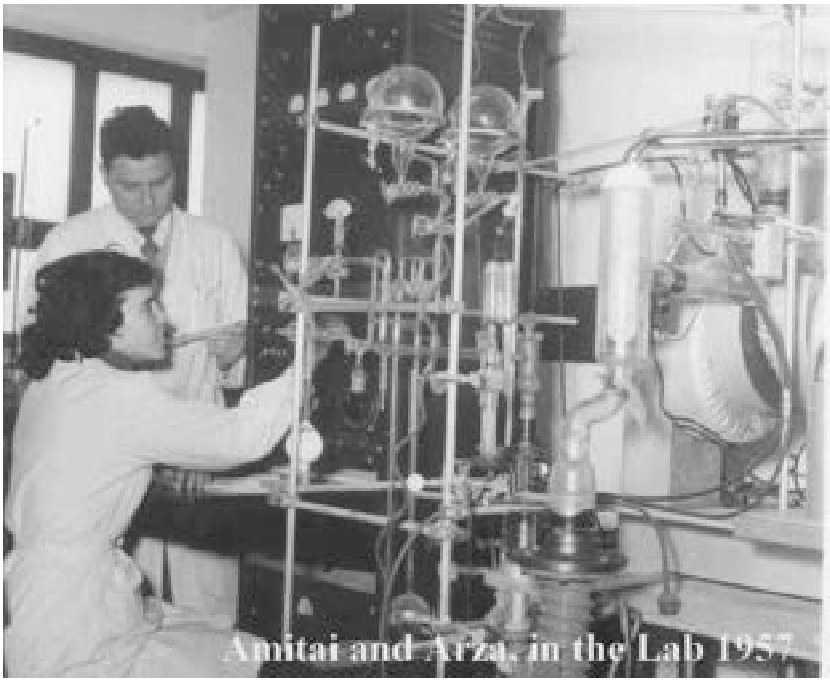
Amiram had a position waiting for him in the department of Electrical Engineering, but he chose to join the “renovated” Physics Department, young in spirit and in new faculty members.

In answer to my application to the chemistry dept in the Technion, I was informed~~,~~ that there was no available slot for a physical chemist. I took a soft money research position offered to me by Otto Schnepp - with whom I loved to cooperate.

On October 1964 we returned to Haifa to our small cozy one bedroom apartment at walking distance from the Technion.
Arriving at the steps~~,~~ of the new chemistry building – now located in Nave Sha'anan, I met Raul Kopelman, a young bright scientist, who had arrived as a lecturer three years earlier, all packed and ready to leave for the States. And in a year's time of my arrival Otto Schnepp also left for the States. From a situation of "no slots in physical chemistry" all of a sudden there were two. I found out that Sol Kimel was willing to consider moving to Haifa, and I, for one, strongly supported his candidacy. That is how Sol Kimel and I in 1966 again became colleagues, this time in the chemistry dept at the Technion, as Associate Professor and Senior Lecturer, respectively.
For 15 years we had a nice pleasant and fruitful cooperation in the subject of molecular spectroscopy in all spectrum range from the far IR to the vacuum UV.

In the early eighties, I joined the Solid State Institute, and moved there a large part of my activity. I attended lectures in solid state at the physics dept and filled missing knowledge in semiconductors. The techniques I used in my research were the same as in molecular spectroscopy. I wanted very much to approach applied science. Dealing with semiconductor samples brought me closer (or so I felt and believed) to the world of high tech, at that time in its very beginning. I studied alloy materials of III-V semiconductors like Ga(x)Al(1-x)As, and later man-made structures of these materials: single quantum wells, multiple quantum wells and complex structures of QWs which form a cavity for light. In the eighties, a large part of the scientific community~~,~~ did not believe that III-V semiconductors would ever play a significant role in the electronic industry. Nowadays, III-V quantum structures play a major role in the electronic industry. I did not get very close to applied scientific problems; however I enjoyed immensely studying the fundamental excitations in these systems: the electron hole pair with energy levels analogues to those of the hydrogen atom, the Trion one hole and two electrons, analogue of the negative hydrogen ion. Our group was among the firsts to observe spectroscopically the Trion in a quantum well.
These twenty years in the Solid State Institute, I worked in very close cooperation collaboration with Elisha Cohen. Elisha was an experienced and very talented physicist, an extreme optimist~~,~~ (enough for the two of us), and above all his generosity knew no limits.

Together we educated several very talented PhD students, some ended in the high tech industry and some in Academe. During my tenure in the Solid State Institute, I had the pleasure and the satisfaction to serve for seven years as the Head of the Institute. It was a pleasure, since all scientists in this interdisciplinary organization, from physics, chemistry, electrical eng. and material science, operated in great harmony. The Technion referred to the SS Institute as a "diamond in the crown" and treated us accordingly; we were well supported and equipped. Foreign dignitaries visiting the Technion, ministers, Academy heads and the like were taken for a visit to the SSI. During these years I took part in several Technion committees, among them the steering committee of the Technion Senate? In the years 1992-2002 I was a member of the Israeli Council for Higher Education (MALAG). I learned about and got involved in fundamental problems of the higher education in Israel. Among others, as a member of a subcommittee of MALAG, I got involved in problems of higher education in the Arab Sector. The subcommittee met for over a year, studied carefully all facts, and came up with proposals, to rectify and/or improve the situation (referring to a small number of issues). Some of these proposals were implemented by VATAT (the sub committee for planning and budgeting) but still there remain many crucial issues that have to be dealt with. In 2002 I was offered the Presidency of the Western Galilee College, a college located in the midst of a large Arab population and one third of its students are Arabs. I saw in this position an opportunity to deal with and contribute to two important issues which were close to my heart: bringing first-rate higher education to the periphery and dealing with higher education and other subjects concerning the Arab sector. It is my personal belief that if we will not live in harmony and equality with the Arab population here in Israel there will be no future for generations to come. I served one term as president of the Western Galilee College, until Dec. 2006. My contribution to the two issues, dear to my heart, was small but not negligible. As of the beginning of 2007 I am an emeritus of two Academic institutes and still trying to push forward and promote the issues I believe in.



Amitai Halevy and I in a picture taken and distributed by Technion Public Relation (1955).

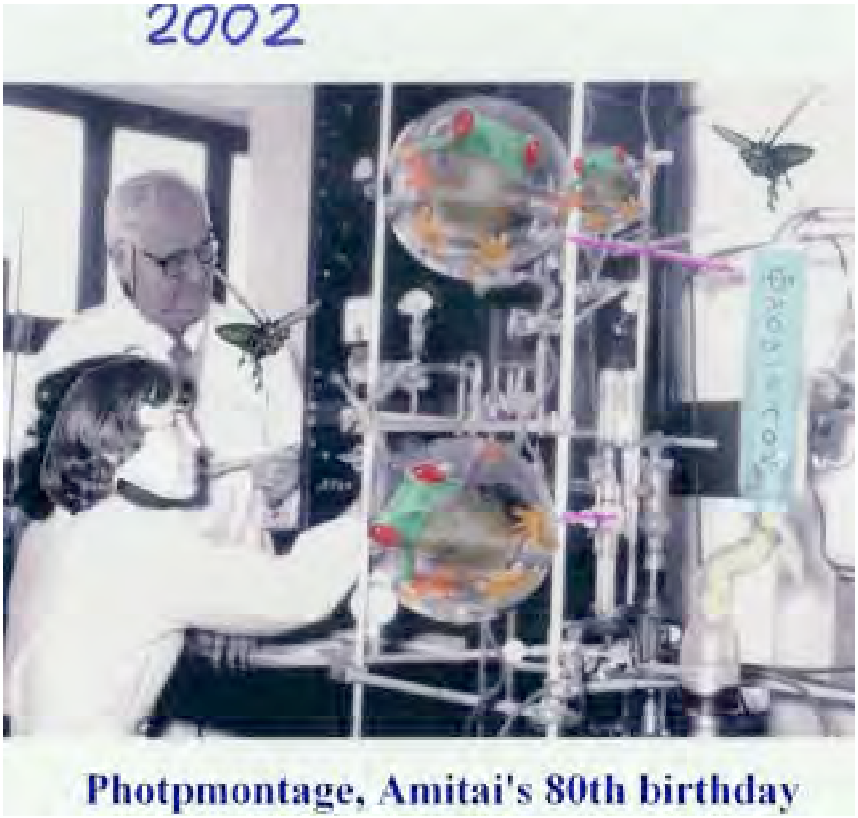


Photo montage: Amitai and I, in 2002, inserted into the 1955 picture, on the occasion of Amitai's 80th birthday (2002).

The Classes of 1956 and 1957, the first and second graduating classes of the Chemistry Department

The 50th Anniversary of the class of 1957.

http://www.potters.org/subject77584.htm

Mordecai Rubin

Born USA, 1926

Ph-D thesis
Columbia University, 1954

Post-doc position
University of Wisconsin, 1956-58

Past Positions
Research Chemist General Foods Corp., 1954-56
Instructor, Assistant Professor Carnegie Institute of Technology 1958-66

Technion Position
Associate Professor 1966
Professor 1972
Professor Emeritus 1994

Sabbatical leaves:
Syva Research Institute, Palo Alto, California 1972-73
Rice University, University of Bordeaux and TU Darmstadt 1978
Australian National University 1982-83
1986 University of Oregon and University of Heidelberg
University of Oregon 1989 and 1994

Research

Photochemistry and History of Chemistry with particular reference to Ozone

Mordecai B. Rubin

(March 2008)

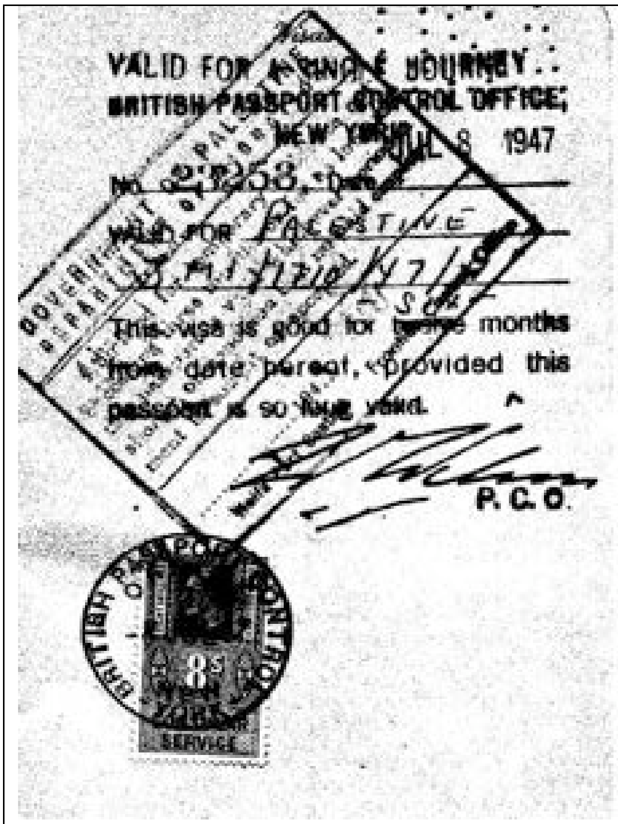
REFLECTIONS ON A LIFE IN CHEMISTRY AT THE TECHNION

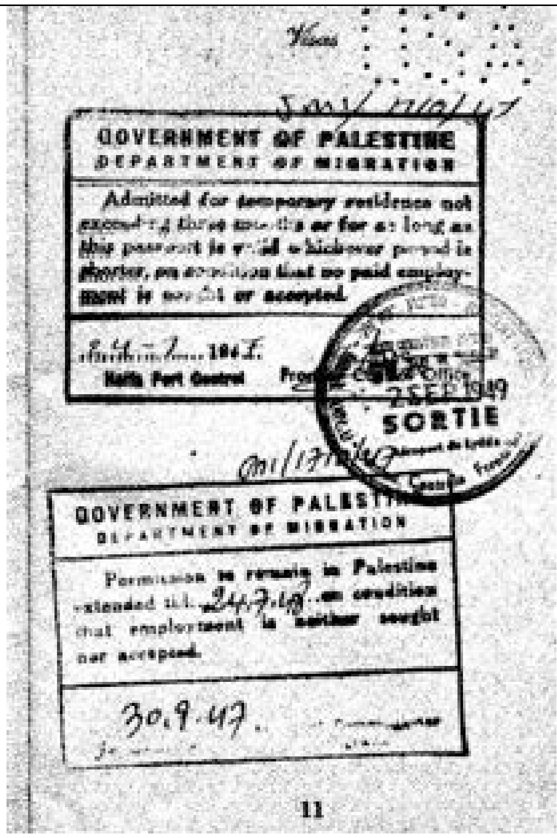
Born Boston, Mass. 1926
Married Ruth Charney 1953
Three children, seven grandchildren

U. S. Army 1944-1946
University of Pennsylvania B. Sc. 1947
Haganah-Zahal 1947-49
Harvard University, Private Assistant to S. M. Kupchan 1949-50
Columbia University, M. A. 1951, PhD. 1955 (F. Ramirez)
General Foods Corp., Research Chemist, 1954-1956
University of Wisconsin, Post-doctoral fellow (W. S. Johnson) 1956-58
Carnegie Inst. of Technology, Instructor, Assist. Professor 1958-1966
Weizmann Institute, Weizmann Fellow, 1964-65
Technion, Assoc. Prof. 1966-72, Prof. 1972-1994, Emeritus Prof. 1994-

Some children are born with a silver spoon in their mouth, I was born with a complete set of Herzl's writings. This was the opening sentence in my 1946 application for graduate study at the Hebrew University when I was finishing my undergraduate degree in chemistry at the University of Pennsylvania. It came about because of the chance overhearing of a conversation. While my unit in the US Army was standing at ease one day before the CO (commanding officer) appeared, I heard Ernie Sokal, originally from Austria but gone before the Nazis arrived, telling his neighbor that he had just learned that the Hebrew University had been approved as a participating university in the GI Bill of Rights. On the spot I decided that I would go there after I finished my bachelor's degree. The GI Bill was a law passed by the US Congress, granting members of the armed forces who had served during WWII certain benefits, one of these was a grant of up to $400 per year for university tuition and the sum of $65/month (later raised to $75) for subsistence for a time period equal to their length of service plus 12 months. My parents had met at a Zionist convention and I had grown up in a home where the latest events in Palestine and the Zionist world were regular topics of discussion. Visitors were often Zionist activists of one sort or another who came briefly to Harrisburg, Pennsylvania, where I lived from the age of 12. Here was my opportunity to see it for real.

I was discharged from the army in July of 1946 and went back to Penn that September to finish my bachelors degree. In those days anyone with $400 available for tuition had no difficulty in being accepted at the Hebrew University. I worked part time at various jobs during my senior year at Penn and was able to pay the $150 required for a ticket on the Marine Carp sailing from New York in July 1947 to Beirut, thence to Haifa, and on to Egypt. The Marine Carp was a former troop ship which had been slightly converted to carry passengers. This was at a time when it was almost impossible for Jews to enter Palestine legally. The attached pages from my passport (no. 71188) show that I was one of the few exceptions There were about 10 more like me on the Marine Carp sailing in early July of 1947, half of us were actually planning to go to the university, the others had found a way to get in to the country and disappeared as soon as we landed in Haifa.





From passport from M. Rubin

When you are 21 and on your way to high adventure together with some like-minded spirits, primitive accommodations and mediocre food cannot detract from your pleasure in life. The two week journey passed quickly. I remember a singer named Sarah Osnat-HaLevi singing Kapitan after dinner and lazy days in the sun. There was a one-day stop in Beirut but we were not allowed off the ship. The next day, July 24, 1947 saw us in Haifa where we were met by Al Yanow, an American who had come to the Hebrew U a year earlier and was the university's liaison to the trouble-making Americans. A night in Haifa at the Carmeliya Court Hotel and the next day we were shepherded to Jerusalem and lodged in the Pension Pax in New Montefiore which was to be home for the next year. Classes were not due to start for some time; not being plump in the pocket I went off to Kibbutz Ginegar for over a month where a group of Americans, some of whom I knew, were organizing to start their own kibbutz (later Gesher Aziv) and spent a month working on the kibbutz.

Kibbutz life was great except for the work; up at 5 AM to pick corn until noon could not compete with chemistry.

And then back to Jerusalem where I began my abortive doctorate. There were two Americans majoring in chemistry at the Hebrew University at that time, myself and a newly married young man named Amitai HaLevi who had arrived in 1946. We were both formally students of Moshe Weizmann, Chaim's brother, a nice old gent who liked to sit in Café Atara or other cafes with his cronies and claimed, at least partly correctly, that Chaim should have stayed in chemistry and he, Moshe, should have become the politician. He liked to say, in his juicy Ashkenazi Hebrew "What has the Third Aliya brought us? The thieves, the prostitutes, and the professors of the Hebrew University." (Ma heviya lonu haaliyo hashlishis? Haganovim, hazonos ve haprofessorim shel hauniversitoh haivris). In fact, our thesis adviser was a surprisingly well-informed young chemist (considering his isolation from the world of chemistry) named Shalom Israelashvili (later Sarel). The conditions for doing research in Jerusalem in those days were very poor. Glassware was not available and had to be made in the glassblowing shop (the boss of that shop was the father of Jackie Rosenbaum, longtime glassblower in our department in Haifa). A very limited selection of chemicals was available and the mandatory government did not give licenses to import anything. My research project involved allyl radicals, free samples of the required allyl halides were available all over the world but it was necessary for me to begin by synthesizing allyl halides from the available allyl alcohol.

This was at a time when relations between the British Mandatory government and the yishuv in Palestine were very bad. The British forces in Palestine were trying desperately to maintain order with a variety of repressive measures. Innocent me was arrested twice by the British. Once my American accent helped me talk my way out of it and once I latched on to the coattails of Al Yanow who was a reserve officer in the US Navy and was released on the strength of that with me following closely behind him. A few months later came the UN decision of November 29 with its wild celebration and a new world.

As fate would have it, the conditions for doing research became irrelevant when it no longer became feasible to go up to the campus on Mt. Scopus every day, 20 years passed before it was possible to return to my lab space there although I was on guard duty one night in November, 1947, together with Amitai. In early November 1947 I was invited to join the Haganah and went through the traditional ceremony in a darkened room with a bible, a candle and a pistol on the table. Various military activities followed, the most memorable being a week at Kiryat Anavim acting as reinforcement for the defense of the kibbutz and simultaneously participating in a one week course on the Schwarz-Lose machine gun. This was a water-cooled Austrian machine gun of the first World War, there were two in Jerusalem and 500 bullets for the two of them. We learned how to take them apart, put them together, etc. etc. The grand finale of the course was the firing of one of the 500 bullets. I left that squad to join Hemed (Hayl Mada) but they later received Czech Besa machine guns and saw some important action.

Somehow I had learned that chemists and physicists were being recruited for a special unit called Hemed (Scientific service) and joined this group after an interview with Aharon and Ephraim Katchalski at Bet HaYahalom in Jerusalem. Our first action was a course in Chemical warfare on Har HaZofim. Our text was a book from the 1930’s which had been found in the chemistry library. We studied from this book and even made a smoke screen (a photograph of the few wisps of smoke we achieved is attached) and then were assigned various tasks in Jerusalem. I was attached to the late Ernst Fischer to assist in the process of constructing a “plant” for the explosive “Cheddite” to be produced in an abandoned Arab house between Montefiore and Bet HaKerem in encircled Jerusalem. Cheddite was an explosive which had been used in the 19th century Franco-Prussian war and been replaced by TNT everywhere except in Jerusalem where there were no sources of toluene or nitric acid. It was a mixture of about 85% potassium chlorate plus rosin, vaseline, paraffin, etc. It was moderately effective when detonated in an enclosed space such as a land mine but quite harmless in the open. An improved version used potassium perchlorate but we did not have an adequate supply of that. My chemistry colleague, Amitai HaLevi, (still a colleague 60 years later) established a facility for making detonators in another abandoned house. One of his partners, name of Goldschlag, was a deaf mute who loved to set off the detonators. He couldn’t hear the explosion but he could feel it. The other was Yoram Avidor, later a colleague in Haifa. Goldschlag appeared some years later at my laboratory in Columbia Univ to try and borrow some money, I was still getting along on the GI Bill’s $75/month and was unable to help.

The Cheddite plant was finished and Ernst Fischer and I were transferred to the Hemed base at the Givah outside of Tel Aviv. Ernst was a fast mover and left Jerusalem immediately, I was slower and was stuck in Jerusalem when it was cut off from the rest of the country in April 1948 until the first truce with the Arabs when I left also via Derech Burmah. I had stopped smoking in Jerusalem for the simple reason that there were no cigarettes but the first stop on the bus was at a kiosk in Rehovot where stupid me went and bought cigarettes. Hemed was very hush-hush and it took me a couple of days to find the base and report for duty. There I remained for the rest of the war of independence living in a tent and going to relatives in Tel Aviv once a week for a hot bath. I don't know where the "Givah" actually was but it cannot have been very far from central Tel Aviv because I used to walk in to town on Saturday nights and, if nothing better offered, see the latest production at the Habimah theater (free entry to soldiers) or a concert at the Ohel Shem auditorium.

Along the way I acquired a new name. For 21 years in the US I had struggled with the name Mordecai, people couldn't pronounce it, never mind spell it, it was thought to be a last name with Rubin as the first, it was mistaken for a girl's name. Finally I came to a place where Mordechai was a perfectly acceptable name and I became Jimmy. First Joe from GI Joe but that was felt to be too impersonal, so Joe became Jimmy. And to this day there are a few people who call me Jimmy. I can still remember the shouts in Hebrew when my brother arrived in the spring of 1948 from the US, “HaAch shel Jimmy ba”.

I worked at various things in Hemed in Tel Aviv, principally the design of a timing device for artillery shells. The State was established, the Haganah became an army and we in Hemed acquired military ranks, I became a Segen, in the American army I had been a Technician 5th grade, called corporal, now I was an officer.

As the War of Independence wound down, people began to think of the future. Just about every chemist in Hemed wanted to go to the US to study. And here was Jimmy with an (expired) US passport and parents in America. I was brainwashed into going back to the US for my PhD although I could have stayed in Hemed, later Raphael. So I got my discharge from Zahal, worked in the Assia pharmaceutical company for the many weeks it took the US to decide whether to renew the passport of this militaristic Rubin and finally, under the auspices of Mahal (mitnadvei hutz la’aretz). flew to Zurich for a few days, then to Paris for a few days and finally to New York where my parents had moved while I was away. In a way the return trip was a joke of fate on me. My program when I left the US in 1947 had been to get my PhD in Jerusalem, spend a year as a post-doc in Zurich followed by a similar year in Paris. I did get to both places but only in passing and returned to the US 2 years older but no further on in my life. I did have the experience of participating in my own small way in the establishment of the State of Israel, of surviving a variety of hazards and proving to myself that I was more man than mouse. I would not trade those two years for anything.

What I did not expect when I returned to the US was to find myself a hero. Anyone who is less of the hero type than me would be hard to find. Very few Americans (about a thousand in all) had come to Israel to participate in the war of independence and here was a real live one. I felt more at ease with non-Jews who couldn’t have cared less.

I went to Boston where my parents’ family had been based and found a job at Harvard with the late S. M. Kupchan who had just become an Instructor. Kupchan had an NIH grant to investigate the Zygadenus alkaloids and hired me as his lab technician. This was classical natural products chemistry, grinding up the plant, extracting it with solvent, and attempting, mainly by chromatography, to isolate pure, crystalline products, It was a great year for me, I sat in on P. D. Bartlett's course in physical organic chemistry, G Kistiakowsky's grad. course in physical chemistry, heard Derek Barton's lectures on conformational analysis, went to colloquia, to seminars and even moonlighted with Ernie Wenkert, then a grad student, to publish a short paper in Nature on some peracid reactions, my first publication. I even obtained about 20 mg. of crystalline Zygadenine, that work was continued by David Lavie from Rehovot working under Kupchan. The following September I moved to my parent's home in NYC and began graduate study at Columbia in September 1950.

During my first year at Columbia, the senior staff in organic chemistry, W. Doering, D. Curtin, and R. Elderfield, all took positions at other universities. I wound up with Fausto Fortunato Arturo Ramirez y Cruz (later known for his work with phosphorus organics) as my research adviser working on the Chinese plant Tan-Shen. This plant was part of the Chinese pharmacopeia, its extract with boiling water supposedly had a variety of beneficial effects. The Austrian chemist von Wessely had worked on the components of this plant before WWII and suggested furanophenanthrenequinone structures. Because of difficulties created by the Korean war, we were not able to obtain a significant amount of the plant from China (my theme song was "can't go on, all my Tanshinone is gone") and began a synthetic program which finally converted into a study of lithium aluminum hydride reduction of unsaturated gamma-lactones. Ramirez was my research adviser but my teacher was Gilbert Stork who had come to Columbia in 1951. In addition to being an outstanding organic chemist, Gilbert, as we all called him, was an educator who was interested in training young people and a role model for many. He had a regular Thursday evening seminar with grad. students which was a model of eleemosynary execution. As it turned out, he was also far more helpful in my search for an academic position than Ramirez.

An important event outside the chemistry department occurred when I met Riffie (Rivka) Charney during the winter of 1951-52. I was chairman of the New York Chapter of the American Veterans of Israel, an organization of individuals who had participated in the illegal immigration of the 1940's or the war of Independence. We met half a dozen times a year at a member's home with a guest speaker, someone closely connected with Israel, often members of the consular staff in New York. Riffie was a second cousin of my stepmother and had just returned from 2 years working in Sarafand hospital. It seemed like a good idea to meet with an American who had just returned from Israel and to hear about life two years after we had all left the country. We were married in June 1953 and together for over 54 years with 3 children and seven grandchildren along the way.

A restaurant in Kussnacht, Switzerland ca 2000. Photo by A. Eschenmoser

Bogis-Bossey, Switzerland (above Nyon) 2001. Photo by C. Jefford

Towards the end of my graduate studies, I wrote to a Mordechai Levy at the Technion about the possibility of a position on the staff. He never answered although

I learned many years later from David Ginsburg that they were looking for young staff members at that time. I also wrote letters to 50 chemistry departments in the US inquiring about teaching positions; I received two offers of a post-doctoral and one letter informing me that my letter had been misplaced and the position they had available had been filled. And so I wound up at the Central Research Laboratories of General Foods Corporation in Hoboken New Jersey working for the Maxwell House Division on the chemistry of coffee aroma. This was a challenging task. I learned many years later on a visit to the Firmenich company in Geneva that General Foods had farmed the problem out to them and that capillary GC had indicated at least 300 compounds in the volatiles from roasted coffee.

It did not take very long for me to realize that the problem was very challenging but that the company expected a solution within a week or two and that it was not possible to do a proper research study. So I resigned and in June 1956 joined the research group of W. S. Johnson at the University of Wisconsin working on the total synthesis of aldosterone. At an early stage of my Wisconsin stay I spent a number of weeks doing some ozonolysis studies, attended an international ozone conference; the history of ozone has been the companion of my later years. A few days at the Upjohn Company in Kalamazoo Michigan taught me the technique of preparative paper chromatography and with the help of this technique I was able to complete the total synthesis, obtaining about 0.2 mg of aldosterone, enough to establish it’s identity by comparison of infrared spectra and by biological activity.

As Bill Johnson’s fair-haired boy at that time I had a number of industrial job offers and two university positions. I chose Carnegie Institute of Technology (now Carnegie Mellon University) in Pittsburgh and we moved there in September 1958 with six month old Betty. David followed in 1959 and Samuel in 1961. A physical chemistry colleague, G. J. Mains, had a 1000 watt, high pressure mercury vapor lamp he was not using and I was able to resume an interest in photochemistry which had been aroused by some work I had done during my doctoral studies. Within a few years my research effort concentrated exclusively in organic photochemistry. It was slow going at Carnegie, the senior staff were not very interested in helping us youngsters but by the time I left my research group numbered eight and I had been able to publish a reasonable number of papers in photochemistry and steroid chemistry.

My interest in coming back to Israel had not waned and in 1962 I began to consider the possibility of spending a leave in Israel. Carnegie did not have a regular sabbatical program so that financial support was necessary . I wrote to Ernst Fischer of Hemed days who had become Assoc. Prof. at the Weizmann Institute, he returned a welcoming but pessimistic reply concerning the possibility of financial support. And then one of those chance events that intrude upon one's life came again. I attended an ACS meeting in New York and went to dinner with a group including Gilbert Stork who casually mentioned during the course of the dinner that he had heard that my old friend of Harvard days, Ernie Wenkert, was going to take over as head of Organic Chemistry in Rehovot. When I returned home, I phoned Ernie who confirmed the rumor and invited me to join him in Rehovot for 1964-65. As easy as that I was awarded a Weizmann Fellowship and in August 1964 the five of us came to the Weizmann Institute. A rich year for the whole family, professionally, socially, touristically, educationally.

In the course of that year I was invited to give a lecture at the Technion and the upshot was that I was offered a position as Associate Prof. In February 1972 I became full Professor. We went back to Pittsburgh in August 1965 and began getting ready to move the family to Haifa. I received a Fulbright Award so that the Technion did not even have to pay me for my first year in Haifa and I earned as much as the Prime Minister did. The comedown was the following year when monthly pay was 800 lirot per month (about $250). We had known the salary situation when we came to Haifa and had decided to come anyhow and hope for the best. We were encouraged by my brother who explained that he managed because of "retroaktivi". The new salary agreement was signed two or three years after the previous one had expired and then one received the differential retroactively for two or three years. One then reduced or eliminated temporarily one's overdraft and began over again. After a few years Rivka began to teach technical English in the General Studies department, at first part time and then, when the children were older, full time and our financial situation improved. The chemistry department had moved two years earlier into its new buildings under the benevolent dictatorship of David Ginsburg. The first years at the Technion were very productive, the students were first rate, there was plenty of money for chemicals and equipment, and I managed to get some research grant money as well. In those days, it was not necessary to have any outside funds for research, the resources of the department were adequate even, for example, for the purchase of a Varian T-60 nmr spectrometer for the routine nmr work of the organic and inorganic chemists. Within a couple of years I had a good set-up for doing photochemical research including two rooms for organic chemical work, two darkrooms, one equipped with a Cary 15 UV-Vis spectrophotometer and a Bausch and Lomb apparatus, an Aminco-Bowman apparatus for fluorescence and phosphorescence studies, and darkroom space for sample preparation and manipulation. I had amassed a collection of light sources and was well set-up for photochemical research. Excellent glassblowing and machine shop services allowed construction of a variety of special equipment for photochemical work, much of which concentrated on α-diketones and vic-polyketones..

I never had any ambition for administrative jobs but I tried to do my part in the running of the department. My philosophy was to volunteer for a job which I liked, such as chairman of the department colloquium which I did for many years, so as to be in the position of being able to decline less welcome chores on the grounds that I already was doing my part. Nonetheless, I served at various times as faculty adviser to the technical services, to the glassblowing shop, and the library, head of organic chemistry, and on (too many) various committees. I also served on a Technion committee to examine the condition of the libraries; one of the sad aspects of my time at the Technion has been to witness the continuing downgrading of our library.

There was also a great deal of intellectual stimulation. During my first year in Haifa, R. B. Woodward spent several days in Haifa and received an honorary doctorate and Selman Waksman was here for the dedication of the new biochemistry building (biochemistry was part of chemistry in those days). The list of visiting professors who came for a month or more to present a special course from 1966-70 included D. Arigoni, A. Dreiding, J. Dunitz, A. Eschenmoser, D. Everett. E. Heilbronner, C. K. Ingold, N. Leonard, V. Prelog, and P. Skell. We kept a guest book at home all the years in Haifa, it contains a large number of names of distinguished chemists who were guests in our home for one of Rivka's delectable dinners. Much battered after 40 years of use, it occupies a place of honor in our house. Many of the visitors became friends as well as colleagues. Columbia, Harvard, Stanford and the ETH undoubtedly outdid us in the number and quality of visitors but we were not far behind. Entertaining the many visitors was very much a cooperative faculty activity which was part of the congenial atmosphere in the department.

At the time of writing this, October 2007, I have been at the Technion for 41 good years, 13 of them as emeritus. The emeritus status is like a permanent sabbatical. Looking back, there have been ups and downs (particularly a drastic shortage of students at one time) but I cannot imagine a better choice of career for me. My successes and failures have been my own (and my co-workers), a not onerous teaching load has allowed most of my time to be spent doing the things I really wanted to do and being stimulated by colleagues in Haifa and colleagues from all over the world who have come to Haifa or whom I have visited in their own habitats. There have been joint papers with Technion colleagues David Ginsburg, Frank Herbstein, Menachem Kaftory, Moshe Kapon, Shammai Speiser as well as Bill Resnick from Chemical Engineering (he called it comical engineering). And with colleagues at other institutions including P. Bowers (Simmons College), J. Cannon (U. of Western Australia), J. Freeman (Oxford), R. Gleiter (Heidelberg), D. Levy (Chicago), H.-D. Martin (Düsseldorf), O. Mills (Manchester), R. M. Noyes (Oregon), W. Philipsborn (Zurich), Z. Rapoport (Jerusalem), W. Sander (Braunschweig, Bochum), and H.-D.Scharf (Aachen).

Sabbatical leaves in Australia, Texas, Oregon, Bordeaux, Dusseldorf, Darmstadt and Heidelberg have offered much intellectual stimulation and the opportunity to travel and interact with so many people. Keren Hishtalmut has made many short trips possible, these have combined sightseeing, personal contacts and science in a most pleasurable way. One of the delights has been the many visitors who have come to Haifa and offered stimulating discussion and the pleasure of their company in our home. Wherever I go in the world, I have friends who are eager to spend time with me, some of them far greater chemists than I could ever claim to be. Rivka and I felt that we wanted to make our contribution to assure that the visitor program would continue and we set up a fund to which we contributed from 1996 to 2003, the proceeds to be used to bring a visitor to the chemistry department for each of two years and then to the General Studies department (where Rivka taught for 20 years) for one year and so on.

מטכ"ל / אכ"א / מח"ל

№ 00810

תאריך 31.8.1949

רשיון מיוחד ליציאה

הנדון: RUBIN MORDECHAI

מס. אישי 14004

הנ"ל רשאי לעזוב את מדינת ישראל ל ארה"ב

תוך 30 יום מתאריך הוצאת תעודה זו.

כוחה של תעודה זו יפה רק בצירוף דרכון אמריקאי מס. 71188

ראש מח"ל

1947l

Permission to leave Israel, 31 August, 1949

THE OREGON CONNECTION - Beginning in 1986 I established an invaluable connection with the University of Oregon chemistry department which resulted in our spending 15 extended periods in Eugene, most of them during the summer months when Haifa is hot and humid but Eugene is (mostly) cool and comfortable. It all began because my uncle Meyer Rubin did not know enough geography. Boston born and bred, after completing his rabbinical studies, he was offered a position in Portland only three hours by train (he thought) from dear Boston. Unaware of its existence, he had agreed to a position in Portland, Oregon, and unable to wiggle out of his commitment, he spent several years there eventually returning to Boston. In between he married a charming lady, my Aunt Nell, who was always singing the praises of God’s country, Oregon. So I decided to take my 1986 half sabbatical in Oregon. I was granted a half sabbatical so as to take advantage of a half year break between the end of Betty's military service and the beginning of David's, half sabbaticals later became an accepted thing.

Richard (Dick) M. Noyes, the youngest member of the Noyes chemical dynasty, had been a junior faculty member at Columbia when I was a grad student and then moved to the University of Oregon in Eugene where he was full professor and a member of the US National Academy. He had developed the field of oscillating reactions and I was interested in the possibility of observing this phenomenon in photochemical reactions. I wrote to him and in short order had an appointment from February to June as visiting fireman in Eugene. The trip to Eugene was a great success; the work went very well with two papers appearing in due course, we made friends, enjoyed a lot of music and theater, traveled around scenic Oregon, had an outing to Palo Alto, and enjoyed life in general. Also, an invaluable result was the decision not to investigate photochemical oscillating reactions. Three years later we came back on another half sabbatical, then again in 1992 when Riff retired and we began renting private homes rather than semi-furnished apartments. We were very comfortable, the chemistry continued to prosper, and we enjoyed Eugene and the surroundings very much. So much so that we returned almost every year thereafter for a total of 15 stays. In the early years I also gave a course on Photochemistry and one on the history of chemistry. After my retirement and Dick's incapacitation in 1994 (he died in 1997), the chemistry department began the practice of giving me a courtesy appoint every year. The department provided me with an office equipped with computer, etc, the Science Library, where I did much of my literature work on ozone, also provided an office, and life in Eugene continued to provide many pleasures. In return, I smiled at my colleagues in the chem. department who annually professed great delight in seeing me again and then proceeded to ignore me. I did make a small donation to the department each year as a token of appreciation. There certainly was a lot to appreciate. A good fraction of my ozone work was done in Oregon.

OZONE – THE COMPANION OF MY LATER YEARS. I have racked my brains but cannot recall the process which brought me to the point of a major interest in the history of ozone. I know that it began in 1999, perhaps a year or two earlier and my first, prize- winning paper on the subject appeared in 2001. Ozone chemistry goes back to 1839, most of the journals of the 19th century are not available in Haifa and often not in Israel at all so my literature searches provided a good reason to travel. These travels brought me to the ETH Zurich, the University of Basel, the University of Heidelberg, the Australian National University, and the University of Oregon. Librarians all over the world have been most helpful, they seem to thrive on finding esoteric items. Ozone has proved to be a marvelous choice, its development parallels the development of chemistry and it promises to keep me busy for as long as I am able to be busy. Some day, if the spirit moves me, I many write a book.

FORTY-THREE OF RESEARCH

Mordecai Rubin

When I began my graduate research in 1951, the only instruments we used, if at all, were a Cary UV-Vis spectrophotometer, a polarimeter, a refractometer, a thermometer, and a balance. One year later the department at Columbia acquired a Baird Atomic double beam recording infrared spectrophotometer and all the graduate students quickly learned how to use this instrument and to interpret spectra. Suddenly it wasn't possible to work if the machine were down, which it often was. Separation techniques available were crystallization, distillation and column chromatography. Period. Except for the use of electrical heating, our laboratory techniques were not significantly different from those of our scientific fathers and grandfathers. One tried, whenever possible, to obtain crystalline compounds or crystalline derivatives which could be purified by crystallization and sent for C,H, and maybe N analysis, the holy arbiters of structure. One of the reasons I began to teach a course in the history of chemistry was my desire to transmit to students what it was like to have done research one generation before them.

By the time I assumed a postdoctoral position at Wisconsin in 1956, two great advances had taken place. 60 MHz Nmr machines were in use, usually operated by an individual with special training for these highly temperamental instruments, and paper chromatography had migrated from biochemistry to organic chemistry. Now progress was impossible if the nmr spectrum was not available. Thin-layer-chromatography came along a few years later and gas chromatography and so on. We may have heard of X-ray crystallographic analysis but it had nothing to do with us organic chemists. We didn't dream of 500 MHz nmr machines or 2D spectra and so on. We didn't need big budgets, either.

Organic chemistry was very much an art; some people were blessed by the Deity with good lab technique, heaven help the poor slob whose technique was not good, he had better run to physical or theoretical chemistry at maximum speed.

When I came to my first independent position at Carnegie Inst of Technology in 1958, I had a number of research directions which I was interested in pursuing. These included various aspects of steroid chemistry (steroids were almost invariably highly crystalline when pure) concentrating on stereochemical aspects and potential photochemistry, nonbenzenoid aromaticity with particular reference to oxepin, and a natural products project involving special species of ants which secrete (hopefully) biologically interesting compounds. This latter project derived from a contact of W S. Johnson with a zoologist at a college near Philadelphia who had collected kilogram quantities of ant species which secreted compounds which preserved their nests from disease.

This latter project never got of the ground because potential research students took one look at my 2 liter bottle of ants, went Ugh, and asked about other possible projects. I did a little bit of extraction myself but was never able to interest anyone in doing a serious examination for biological activity of ant extracts of any sort. I had also wanted to continue some work with ozone deriving from my experience in Wisconsin but Carnegie did not have an ozone generator so ozone lay dormant for 40 years until I took up its history after retirement.

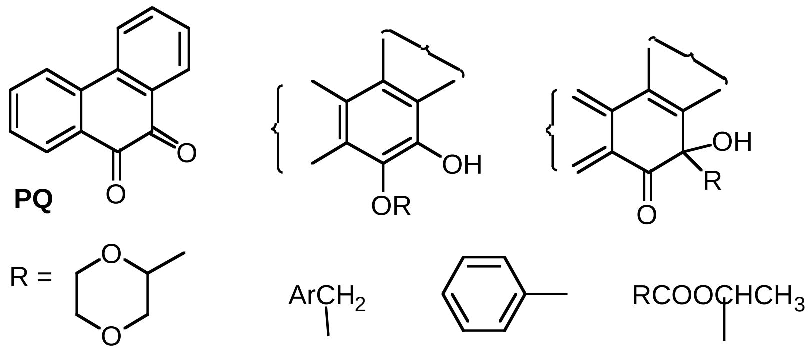
Projects involving steroids brought forth a reaction opposite to that of ants. Those were the days when cortisone was in its heyday and steroids were

the salvation of humanity. I established a connection with the Squibb Institute for Medical Research, they even gave me an unrestricted, annual research grant, on condition that any new steroids we prepared would be sent to them for biological screening and that they would have first refusal in the event of patent possibilities. The first compound I sent them was 17α-progesterone which I had prepared with my own hands; it turned out to have truly remarkable properties which were ultimately attributed to a serious goof on the part of the individual at Squibb who performed the screening. But the research money came in every year until I moved to Haifa and I visited Squibb annually and enjoyed the company of Oskar Wintersteiner who had been a pioneer in steroid applications, Gus Fried and others. Grad students in those days pictured themselves as heroes who would bring manifold blessings to mankind by virtue of the new steroids they would synthesize.

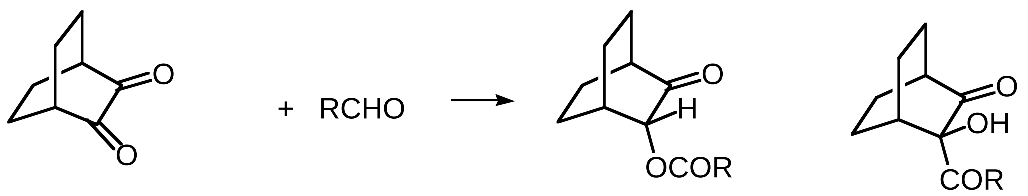
During my graduate school years I had synthesize two compounds by literature photochemical reactions. These were delightful reactions to perform, you put a solution of a compound near a commercial sunlamp and came back the next day, removed the solvent and obtained a nice yield of crystalline material. I couldn't understand the mechanism and filed this away for future reference. As I have noted, my colleague at Carnegie, G. J. Mains, had a 1000 watt mercury lamp he was not using which he gave to me on permanent loan. Eventually dimerizations of steroidal dienones and 2 + 2 cycloaditions of alkenes to enones and dienones occupied us. I was writing up our dimerization photochemistry for a communication to JACS when the student working on the problem came into my office to tell me that a paper by Oskar Jeger on exactly that subject had appeared in the issue of Helvetica which had just arrived in the Library. Fortunately, there was enough difference between that work and ours to allow publication of our work. I wrote to Jeger who never answered but he did mention me in the course he gave here in Haifa as the first of the visiting professors. This helped when I became a candidate for a position in Haifa a short time later. One of his co-authors was Duilio Arigoni who later became a good friend.

However, my first foray into photochemistry in Pittsburgh would never have happened had there been any sort of a textbook on the subject in 1958. There was so little known about organic photochemistry at that time that you could take almost any organic compound off the shelf and be a pioneer. I knew that simple ketones eliminated CO on photolysis, cyclopentanone giving cyclobutane for example, and I had the harebrained notion that one could photolyze 9,10-phenanthrenequinone (PQ) and obtain biphenylene by elimination of 2 CO. Using substituted quinones would then allow preparation of all sorts of substituted biphenylenes whose synthesis was not simple by other means. PQ is a pretty insoluble compound, dioxane is about as good a solvent as any so I set up an irradiation of a gram of PQ in 50 cc of dioxane, forgot about it when I went home and came in the next day to find a colorless, fluorescing solution in the irradiation vessel. Evaporation of the dioxane gave white crystals. A chemist's dream. The product resulted from 1,4-addition of a C-H bond of dioxane across the diketone system of PQ, under the appropriate conditions the quantum yield was unity, the chemical yield was quantitative, and five isosbestic points could be observed in the UV-Vis spectrum if a degassed solution was irradiated at 436 nm. A gift from heaven to any photochemist. A by-product was the thermolysis of the photoadduct which provided a simple route to dioxene. This PQ photochemistry was the beginning of a career-long romance with the chemistry and photochemistry of quinones and vicinal polycarbonyl compounds. Later, a Masters student working with Bill Resnick in Chemical engineering used this reaction to develop a model pilot plant.

We then investigated the reaction of PQ with p-xylene and found, to our surprise, that this reaction gave a different type of product resulting from the 1,2 addition of the hydrocarbon across one of the carbonyl groups. The reaction had been investigated by Waters and the product assigned the wrong structure. So here we were with a puzzle. In the course of investigating this we found that under irreversible conditions (light absorbed by PQ only and not by products) a mixture of both kinds of products was formed with toluene or substituted methylbenzenes and that their ratio depended on the substitution of the methylbenzene used.



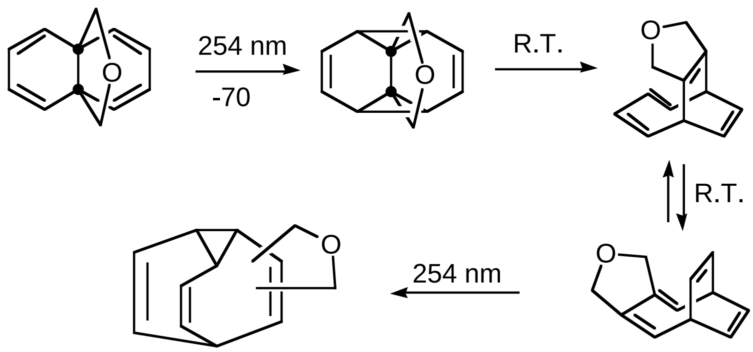
We proposed an explanation based on radical polarity for these results, explored reactions of PQ with a variety of other compounds, even benzene reacts, and entered the area of alicyclic α-diketones in a search for similar behavior. Investigation of various aspects of chemistry and photochemistry of these compounds occupied my research for over 30 years and only ended when retirement put a stop to experimental work. I reviewed the subject twice, in 1969 and in 1985. Cyclic α-diketones are lovely crystalline compounds whose colors range from pale yellow to deep blue, they can often be synthesized easily and can be purified readily by vacuum sublimation. Their UV-Vis spectra have at least two absorption bands, one of which is in the visible. The position of the latter absorption maxima is highly dependent on molecular structure, selective irradiation of mixtures is perfectly feasible. They exhibit both fluorescence and phosphorescence. Their only fault is a marked tendency to undergo hydration. We were able to obtain a variety of such compounds from other laboratories. Over the years we investigated a number of aspects of the photochemistry of such compounds including photoadditions analogous to those observed with PQ but we found only one example in which the two modes of addition occurred simultaneously. This was in the reaction of cyclic α-diketones with aromatic and aliphatic aldehydes which gave both types of addition products. After considerable confusion because products of type A rearranged extremely readily to type B, we were able to establish that two different mechanisms were involved, unlike the PQ reactions.



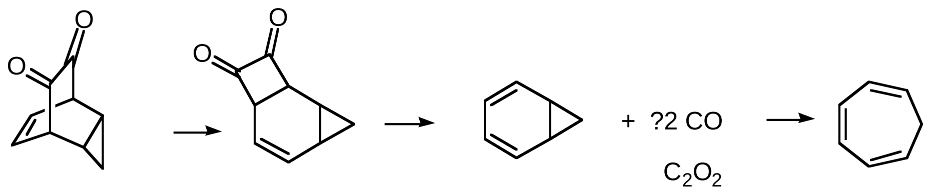
A particularly advantageous source of new diketones was the laboratory of the late H.-D. Scharf in Aachen, Germany. His group had developed a procedure for preparing unsaturated diones via Diels-Alder addition of dichlorovinylene carbonate to cyclic dienes of all sorts. Scharf supplied us with gram quantities of about 10 different compounds and even had an additional amount of one of these prepared specially when our supply gave out. We investigated their photochemistry in some detail and found that, unlike ordinary α-diketones which hang on to their oxygen atoms vigorously, the unsaturated compounds rearrange to cyclobutanediones which, in a second photochemical reaction, bisdecarbonylate to form dienes. We looked very hard, even at 10 K in the laboratory of W. Sander, for ethylenedione, C_2O_2, without success.

A chance encounter at an ACS meeting I attended just before leaving the US for Israel resulted in a research grant from the US Army for my first two years in Haifa. I saw a friend in a crowd and went over to say hello. He introduced me to his companion and excused himself. “What do you do?”, asked the companion. “Photochemistry,” said I, wondering how I had got stuck with this peculiar guy. “Just what I need,” said the companion, who turned out to be the director of anti-malarial research at Walter Reed (U.S. Army) hospital in Washington, DC., and later director of research for Merck. They had some very effective anti-malarial drugs which, however, caused skin sensitivity on exposure to sunlight which persisted for as long as one year. They were interested in a photochemical study of these substances. The upshot was that I received a research grant from the US Army to study the photochemistry of a variety of substituted quinoline methanols. Each summer for two years I traveled to Washington to report on our progress. We managed to establish some of the photochemistry and published a short paper but by that time the war in Viet Nam, where malaria was a problem, had ended and there was no interest in further study.

About a year after I arrived in Haifa, when the photochemistry lab was off and running, David Ginsburg approached me with a suggestion for a collaborative effort involving the photochemistry of tetraenic propellanes. David would supply the compounds and the co-workers, I would supply the photochemical know-how. A typical compound of the type in question is the tetraenic ether shown below which has characteristic light absorption in the ultra-violet. The system turned out to be a playground of photo- and thermal reactions where combinations of light of appropriate wavelength and the correct choice of reaction temperature resulted in selective formation of a variety of interesting isomers. The results are summarized in the diagram below. Results of the various equilibria involved varied with the nature of the third ring of the propellane. This was a system in which the combination of temperature and wavelength could provide excellent control of reactions. We later learned that a Prelog co-worker had investigated this system before us and been unable to unravel the chemistry. Thus, for example, if the first photochemical reaction is not performed at low temperature, rearrangement followed by a second photochemical reaction occurs resulting in a complex mixture of produces. Later Masemune observed analogous results with the unsubstituted cis-9,10-dihydronaphthalene.



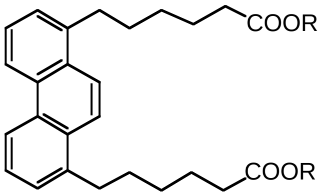
My favorite paper appeared in JACS in 1981, The Photolysis of Two Tricyclic Nonenediones. Direct Observation of Norcaradiene. Graduate students had never been abundant in our department, research groups of 3 or 4 were considered sizable. But in the 1980s the number of new graduate students came very close to zero. I gave some thought to leaving Israel at that time but didn't. I did continue doing bench work on my own and the 1981 paper is an example. Some years earlier we had developed low temperature (liquid nitrogen or liquid air) photochemical capability and variable temperature equipment, all constructed in the machine shop at the Technion. Appropriate irradiation of the unsaturated diketone (one of Scharf's compounds) led to formation of the cyclobtanedione, short wavelength irradiation at liquid nitrogen temperature produced norcaradiene and I was able to obtain the ultraviolet spectrum of norcaradiene. It was also possible to jump the temperature and measure the kinetics of the first order reaction of norcaradiene to cycloheptatriene. The chemistry was later studied at 10 K. A number of other examples of the norcaradiene system were prepared, including the one related to Vogel's methanonaphthalene using the same method



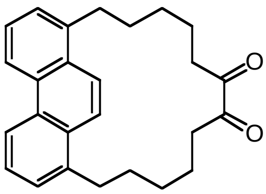
Another little paper which I liked described a procedure for differentiating between cage and non-cage reactions using photochemistry to generate a pair of radicals within a solvent cage and to generate the same radicals separately outside the cage. This appeared at about the same time as the new CIDNP procedure and could not compete with such a sophisticated, simple method. We also were able to show that light intensity could be an important factor in photochemical reactions even in the classical benzophenone photochemistry and investigated a number of aspects of ketyl radical chemistry.

Another direction in α-diketone photochemistry began in 1975 when a bearded, kipah wearing chemist from Raphael who had already done a Masters degree (Ginsburg) at the Technion appeared in my office. He was Shmuel Welner who worked at Raphael and had a one year sabbatical coming to him; he was interested in getting a doctoral degree. He had some idea of working on the types of photochromic systems developed at the Weizmann Institute but that seemed too derivative to me.

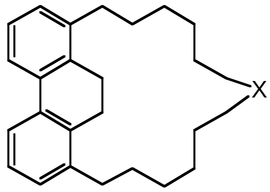
Instead I proposed that he explore the possibility of photochromism in PQ systems having an appropriate bridge, such as **A** where intramolecular reactions would be possible. The initial problem was a synthetic one and Welner was an experienced organic chemist so that progress was good until the Yom Kippur war came along when all of my research students were mobilized for close to one year and graduate work stopped completely,



B



C



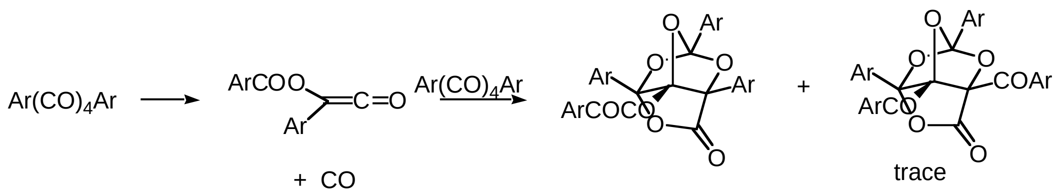
A

Welner did not surface again for several years. His work involved the synthesis of compound of types **B** and C as precursors of **A**. Long wavelength irradiation of **A** could give a product of intramolecular reaction which could regenerate **A** at shorter wavelengths. At the time of the outbreak of the Yom Kippur war he was very close to completing the synthesis of **C** but the worked stopped with its outbreak and after the war he was kept full time on Raphael projects. I was convinced that the project was dead. until he showed up several years later with an arrangement for part-time work on his doctorate. I remember clearly telling him that if he could complete the synthesis of **C**, he would surely have a molecule of sufficient interest to guarantee him a good doctoral thesis. And he did, obtaining some tens of milligrams of a waxy, yellow solid which was pure **C** by all the usual criteria. Compounds of type A fell by the wayside and I never had the manpower to engage in their synthesis.

Welner also determined spectroscopic properties and saw that he had an unusual molecule. He took his results to Shammai Speiser who very quickly realized that we had a special molecule in hand. Excitation of the phenanthrene moiety resulted in emission from both choromophores. Partial energy transfer had occurred from excited states of the phenanthrene moiety in C to the diketone moiety. Energy transfer and electron transfer in bi-chromophoric molecules is a subject of considerable interest. Over the years we studied many aspects of the energy transfer process, synthesizing molecules with varying steric relationships between the chromophores and looking at other systems. The BSF provided support for this work over a 12 year period. Collaborators from the US were Saul Cohen, Colin Steel, George Atkinson, and Don Levy. It was particularly appropriate that this work was done in Haifa because one of the very first studies of excitation energy transfer had been reported by Schnepp and Levy in Haifa using compounds synthesized in the laboratory of David Ginsburg and containing naphthalene and anthracene moieties joined by varying hydrocarbon bridges. We returned to these compounds with modern tools in the course of our work. There were over 15 joint publications from Haifa over the years plus some independent efforts by Shammai.

I used the enforced quietness of the Yom Kippur war to gratify a long standing curiosity, if α-diketones are so good to the chemist, how about tri-, tetra- and so on. In the absence of ongoing research, I wrote a review article on the chemistry of vicinal polyketones which appeared in 1975 and was followed 25 years later by a joint effort with Rolf Gleiter. Synthetic procedures for tri and tetraketones were well-known, Gleiter succeeded in preparing vicinal pentaketones, higher carbonyls have been reported erroneously, none are known.

The photochemistry of these compounds was the last project in my laboratory before retirement. This was a joint effort with Rolf Gleiter at Heidelberg supported by GIF for four years. We found vicinal triketones to be very uninteresting photochemically, quantum yields for their reactions in solution were exceedingly small although the derived radical anion was of interest. We were never able to establish a satisfactory explanation for the low reactivity. Diaryltetraketones were another story. They reacted with modest quantum yield and high chemical yield to give products – derived from two molecules of tetraketone minus one molecule of carbon monoxide. The structure was eventually determined by a young M. Kaftory. We postulated that the ketene shown was an intermediate, matrix isolation studies by W. Sander supported this view of a ketene intermediate and confirmation came from synthesis of the ketene which was reacted with tetraketone to give a product identical with the photoproduct from tetraketone. A happy day when the IR spectra of the product of the photochemical reaction of tetraketone and the thermal reaction of synthetic ketene with tetraketone were compared.



Pentaketones were expected to react analogously to tetraketones but experiments in Haifa and in Heidelberg never succeeded in obtaining identifiable products. Degassing carefully, drying super carefully, varying wavelength and temperature, none of these experimental variations gave results.

CONSULTING

In early 1970 I was invited to be a consultant for Haifa Chemicals, Ltd as part of a team consisting of Bill Resnick (Dean of Chemical Engineering), Alex X (a practicing chemical engineer who ddropped out after a short time) and myself. Haifa Chemicals had been set up to produce potassium nitrate, a premium fertilizer, by a brilliantly simple processed developed at Israel Mining Industries (TAMI). Potassium chloride from the Dead Sea and nitric acid produced on location from ammonia by a classic process were mixed with amyl alcohol (imported); the hydrochloric acid dissolved in the organic phase and potassium nitrate precipitated from the aqueous phase. As simple as that. The amyl alcohol was recycled after washing with water to remove the hydrochloric acid which was used for production of food grade phosphoric acid from phosphate rock. Excess nitric acid was destroyed by addition of formaldehye. The key to success of the process was separation of the immiscible layers but production had slowed drastically because of emulsion formation which made the separation slow and inefficient. The plant was approaching the point where production would not even cover debt servicing. It was said sotto voce that we consultants were hired to act as arbiters in apportioning blame for the failure.

In the event, I realized that the plant was a closed system with no facility for removal impurities which meant that these accumulated and were probably responsible for the emulsion problems. At the consultant's suggestion, a facility for removing a fraction of the solvent, purifying it by distaillation, and returned the purified solvent to the plant was constructed and went into operation after the entire charge of amyl alcohol had been replaced by fresh solvent. From then on the energetic young staff continually improved plant operation and eventually the plant became highly profitable and was enlarged. A second facility was constructed elsewhere and Haifa Chemicals became the world's major supplier of potassium nitrate.

I received a research grant from Haifa chemicals to investigate the chemistry involved in the plant operation with particular reference to the amyl alcohol solvent.

The first result we obtained was that the chemists and chemical engineers involved had not known that the solvent being used was not, as supposed, a mixture of amyl and isoamyl alcohols but a mixture of amyl alcohol and 2-methyl-2-butanol produced by the oxo process from 1-butene and formaldehyde. During the entire development process, the people involved had never known what they were actually working with. We established the chemistry of the alcohol solvent, in particular that formation of acetals between formaldehyde and alcohol solvent could be suppressed by careful monitoring of the amount of formaldehyde added.

Consulting continued until I went off to Palo Alto, California in the summer of 1972 on my first sabbatical. By that time the plant was running well and some minor consulting events occurred afterwards when unexpected problems turned up. Bill Resnick and I shared a gratifying feeling of achievement. A useful lesson for consulting and many other human activities was that you can make recommendations but you have to let the people on the spot make their own decisions. And another lesson, the main function of a consultant is to listen and keep people honest.

My second consulting function was with MALAT (Merkaz LeMechkar Taasiyati) on the Technion campus from 1974 until it shut down (because of intragovernmental politics) in 1977. I functioned as the house organic chemist on a variety of projects.

THE UNIVERSITY OF OREGON. A MINI-CAREER IN PHYSICAL CHEMISTRY

I also had another personality, that of a physical chemist; it was confined to the University of Oregon and began as follows in the following letter which is self-explanatory:

April 14, 1985

Dr. R. M. Noyes
Department of Chemistry
University of Oregon
Eugene, Oregon 94703
USA

Dear Dr. Noyes,

Many years ago, 35 to be approximate, you were a young staff member at Columbia and I was a graduate student interested in natural products chemistry. A great deal has happened since then including my move, twenty years ago, to Haifa where I am professor of chemistry. Also a change of interest to organic photochemistry with considerable emphasis on reaction mechanisms. I have found your work on oscillating reactions very intriguing although I have not done any work in this area.

I have a one-semester sabbatical coming up in the spring of 1986 which brings me to the point of this letter. Would it be possible to spend some part of this period in your laboratory at Eugene. My thought is to get in the lab and do some experimental work; a catastrophic shortage of graduate students here has required that I keep my hand in as an experimentalist. For me it would be an opportunity to learn new aspects of theory and experiment. For you it might be worthwhile to have additional manpower.
Since I will have sabbatical support, money is not a major consideration although some modest financial support would be helpful. My colleague, Amitai Halevi spent a sabbatical period divided between Eugene and Corvallis many years ago and still sings the praises of science and life in general in your part of the world.

Sincerely yours,

Mordecai Rubin

Noyes responded positively within a very short time and even offered financial support. So I went off at the end of January, 1986, and Rivka followed a couple of weeks later after her English exams had been given and graded. We were to remain in Eugene for 4 months, go home for a couple of weeks, and spend the rest of our half sabbatical in Heidelberg supported by a grant from the DAAD.

My initial project in Eugene concerned the nitrogen gas oscillator, a system in which nitrogen being formed by gentle stirring of aqueous mixtures of nitrous acid with ammonium sulfate (the diazotization of amines in its simplest form) escaped in bursts, periods of quiescence being followed by vigorous gas evolution, followed by quiescence, and so on. Dick Noyes thought that this might be due to the fact that the escaping gas removed some catalyst which then had to build up again until the rate of nitrogen formation became sufficiently great. I was able to eliminate this possibility by a variety of approaches. The conclusion was that the system became supersaturated with nitrogen until it reached a limiting value which then escaped in a violent burst, built up to limiting value again, and so on.
The whole process could be controlled nicely by appropriate stirring. I also found that the supersaturation could be discharged extremely rapidly by application of ultrasound at appropriate time intervals. Noyes lab was a collection of ancient

equipment from his Columbia days and the latest modern equipment including a pressure transducer of extraordinary sensitivity which could be connected to a chart recorder, eventually the whole set-up was computerized. Calibration of the transducer allowed one to determine the amount of supersaturation.

We found that increasing the rate of reaction, i.e. the rate of formation of nitrogen, resulted in a limiting value for supersaturation which could not be exceeded. The same procedure could be applied to any gas for which a chemical reaction giving a sufficiently rapid rate of gas formation could be achieved; we were able to determine limiting supersaturations for nitrogen, oxygen, hydrogen and carbon dioxide. Two publications in the Journal of Physical Chemistry in 1987 resulted from my brief stay in Eugene and I qualified as a bona fide physical chemist.

My original idea in coming to Eugene had been to learn something about oscillating reactions and see what could be done in the way of photochemical ones but nothing ever came of this. However, what I did had been fun experimentally and productive; many questions remained unanswered. In 1989 after Betty had recovered sufficiently from her automobile accident, we went back to Eugene, this time for 5 months, and picked up where I had left off. This time I had a more distinguished status and also gave a one-semester course in organic photochemistry. We first applied the earlier method to determination of supersaturation of nitrogen in various solvents. The experimental set-up we had also allowed one to determine the rate of transfer of gas between the liquid and gas phase and we determined these values for a range of gases, a subject of considerable controversy. Two more publications resulted from this work. And another visit to Eugene in 1992 involved a detailed study of carbon dioxide behavior in aqueous systems, including “artificial” sea water with the usual standard two papers. Since the oceans are the world’s largest reservoirs of dissolved CO_2, this study has some relevance to present problems.

Unfortunately Dick Noyes suffered multiple strokes in the early 90’s and died in 1997, the possibility of joint further research ended. I had done considerable work on the rates of evaporation and condensation of various low-boiling liquids but this never reached publication because of Noyes’ condition. The connection with Oregon has continued, I gave a course in the History of Chemistry one year and since my retirement at the Technion have had an annual appointment as courtesy visiting research associate at the University of Oregon. They give me an office with computer, etc and full faculty privileges which include, most importantly, the library.

In return I smile at my colleagues and enjoy the facilities of the University. This has been of enormous help in my work on the history of ozone. For reasons which nobody at the University has been able to explain to me, the University of Oregon has an excellent library with many journals available back to their origin. Until well after WWII it was far from being a first- or even second-rate chemistry department but I wish we had a comparable library in Haifa.

Eugene, Oregon, and the surrounding area is a lovely part of the world in the summer time. Rivka and I spent 15 extended visits there most enjoyably. I wish it were not so far away.

RETIREMENT ACTIVITIES – HA'OLAM GALGAL SOVEV

As with most retirees, the period immediately following retirement in 1994 was spent in clearing off my desk, publishing papers which had been postponed until there was time available, writing one more review article, etc. The chem. dept also involved me in a triennial moving game proving the old adage that the world is a turning wheel. From 1966 until 1994 I had sat, like almost all of my colleagues, in a tiny 9 m^2 room (513) with a mini desk and one chair for a visitor. In 1994 that space plus my laboratories was given to Yoav Eichen, a talented new addition to the department, and a 6 x 2 m^2 area at the end of the hall on the 5th floor (514C) was converted into an office for me. After about 3 years, that space was needed for other purposes so I moved up to the 6th floor to a 5 x 3 m^2 space formerly occupied by the late Dan Becker. And then, about 3 years later, I moved once again because the 6th floor was being redone. Back to my original spot now numbered 512 and including the 9 m^2 area which had once comprised the office of Magda Ariel plus my old 513, 18 m^2 in all. The largest office in the department except for the chairman, where would this periodic enlargement end? I now sit ***exactly*** where I did in 1966, the only difference being that I face in the opposite direction in order to connect with the Ethernet connection but not for long. In 1966 there was a door on the opposite wall leading to the research lab. I had the opportunity to move once more in 2006 when the new office wing was opened but enough is enough. UNHAPPILY THE CHEMISTRY DEPARTMENT HAS SEEN FIT TO MOVE ME ONCE AGAIN. ANY SPACE OCCUPIED BY RUBIN IS OBVIOUSLY OF ESSENTIAL NEED TO THE DEPARTMENT AND SO I AM NOT ALLOWED TO REST IN PEACE BUT HAVE TO MOVE AROUND THE CORNER TO ONE OF THE NEW OFFICES.

OZONE

Without knowing exactly how it happened I became involved in the late 90’s with the history of ozone. This was probably due to my earlier experience with ozone at Wisconsin combined with the presentation of a course in the history of chemistry. I discovered that E. Hagenbach, a colleague of C. F. Schönbein, the Basel professor who discovered ozone in 1839, had written a memorial volume including a full bibliography of Schönbein’s publications. In January 1997 I wrote to Fabian Gerson who was professor at the Univ. of Basel and had been a visitor in Haifa and a guest in the Rubin’s home, asking if it would be possible to obtain a copy of the Hagenbach book. Back came a bulky photocopy of the book followed by a second copy from the Basel library. Two years later, en route to the Burgenstock stereochemistry conference, I stopped in Basel for a few days and photocopied all of Schönbein’s papers on ozone, many of them published only in the local publication of the Basel chemical society. Ozone was off and running and has been going every since. My database on ozone, from its early days, through its discovery and until the end of WWII now numbers about 4000 references. Current interest in ozone is so great that about 2000 references appear each year and the late 40’s is a good stopping point.

Conducting a search of the literature for the period mid- to late 19th century was a daunting task. Scifinder did not exist and would have been of no use, the Centralblatt was not available and the relevant journals, J. Praktische Chemie, Poffendorff’s Annalen (Annalen der Physik und Chemie), Liebigs Annalen, Comptes Rendus, etc, were not available in Haifa or elsewhere in Israel. It was necessary to travel and so I went to Heidelberg, an old University, and sat in the library there going through the indexes of the journals year by year. This continued during the following summer in Eugene and in Basel as noted above. The library at the Australian National University in Canberra was also very helpful. Eventually I found an 1879 bibliography on ozone by A. R. Leeds. Life was made more difficult by the fact that authors often gave minimal literature citations in their papers of the mid-19th century. Nowadays many of the old journals are available on the Internet so that travel is not so necessary but Riff and I always liked to travel so that was no hardship.

“The History of Ozone. The Schönbein Period, 1839-1868” appeared in 2001 in the Bulletin for the History of Chemistry and has been followed by 4 additional papers with no. VI accepted for pubication and no. VII close to completion. The first paper received an ACS award which was very gratifying at age 76. The second paper covered the period 1869-99, later papers have dealt with specific aspects of ozone history such as the isolation of pure ozone and determination of its physical propertied. The choice of history of ozone couldn't have been more successful had it been planned, which it was not. It was just a matter of following up some curiosity and becoming more and more involved. It has provided a lot of intellectual stimulation, kept me active, given a good excuse for travel, enormously improved my reading ability in German and French, and been very pleasurable. There have also been some fascinating detective episodes such as a nine month search ending successfully in Sweden for a photo of Ernst Hermann Riesenfeld who was responsible for the first isolation of pure ozone. Some day, if the spirit moves me I will write a book.

Shammai Speiser

Born Israel, 1941

Ph. D. Thesis:

Technion, 1969

Technion Position:

Lecturer, 1973

Senior Lecturer, 1976

Associate Professor, 1981

Professor, 1986

Incumbent, Freund Chair of Chemistry, 1962

Major Administrative Positions:

Head of Technion’s Youth Relations, 1989-1990

Dean, Faculty of Chemistry, 1961-1964

Dean, Dvision of Continuing Education and External Studies, 2002-2008

Shammai Speiser

Chemistry and Beyond

1. Life in Haifa, 1941-1959

I was born in Haifa, on December 26, 1941, only child to my parents, Gina and Zvi, who emigrated in 1936 from Ternopol, then Poland now Ukraine, to Palestine.

My early childhood memories include curfew imposed by British soldiers in red barrettes, (nicknamed “KALNIYOT”, anemones in Hebrew) my kindergarten teacher and friends, my first day in school, the breakout of the War of Independence, and the liberation of Haifa in 1947, on Passover night. On September 1 1947, I strated my first year at “Hugim” school. Hugim then was a comprehensive school, lasting 12 years of studies from elementary school, through junior high school all the way to high school. It had a very liberal and open- minded approach to education that encouraged free debate and students’ self-expression, I am sure that it had shaped my personality and my professional career. I have graduated from the mathematical-physical class (MEGAMA REALIT), emphasizing study of exact sciences, without, however, neglecting liberal arts and social sciences. My favorite teacher was Arie Rocker who taught chemistry in a very inspiring manner that convinced me to make it my lifelong subject. I still maintain strong contacts with many of my schoolmates, many of which are very close friends.

Life out of school were very active, street games at my neibourhood, activities at the youth movement (Hashomer Hatsair), all kinds of sport activities, especially football (soccer), which became my favorite sport. I was a rather good football player, and made it later to the Technion's selection team. I liked painting, which I abounded at the age of 14, resumed only later upon my retirement in 2010. I became enthusiastic about classical music at an early age, joining my father in listening to music radio broadcasting. I later acquired taste to jazz and to other music genres, it became a passion of mine ever since.

2. Student at the Technion, starting a family, 1959-1964, and army service, 1964-1967

After graduating from high school in 1959, I have started my army service in the Academic Reserve (ATUDA AKADEMIT) and started my studies, for the four years B.Sc. program, at the Chemistry Department within the Faculty of Sciences at the Technion. My favorite subjects were many aspects of physical chemistry, taught by Professor Otto Schnepp, in particular spectroscopy and photochemistry. In the second semester of my third year studies, I have chosen a project in the advanced physical chemistry laboratory. The project, supervised by Professor Amitai Halevi, involved investigation of the mechanism of oxidation of iso-propanol utilizing kinetics methods and deuterium isotope effects. I was very impressed from Halevi's approach to the project, which for me was the first encounter with the methodology of scientific research. When I had to choose my final year graduate research work, Professor Halevi's laboratory was my obvious priority.

Amitai suggested that I would investigate the mechanism of N-nitration. This project followed the M. Sc. Work of Arza Ron[1] that showed that deuterium substitution increases the basicity of nitrogen bases as well decreasing the acidity of carboxylic acids due to a secondary isotope effects, thus suggesting using these effects as criteria of mechanism. My work proved, utilizing a combination of primary, secondary and solvent isotope effects that in the nitration of substituted anilines at the nitrogen atom, the rate-limiting step, in contrast to the established mechanism of aromatic nitration, can be shifted from attack by NO_2^+ to the subsequent proton abstraction.[1]

Being a soldier in the Academic Reserves required special training activities in the summer academic breaks of 1960 and of 1961. In the summer of 1961, I finished the officers' course, while in summer of 1962 we had a break from any army activity. After graduation, I should have started my army service in August 1963; this meant that I was unable to finish my research project. Amitai wrote a letter to the Army authorities requesting delaying my service for another year to allow completing my research and to write my M.Sc. thesis. Luckily enough the army approved my request so that by September 1964 I finished this stage of my academic chemistry education. My M. Sc. Thesis: “The mechanism of N-nitration”, was eventually published[2], my very first scientific publication.

It is important for me to mention all this period at the Technion took place at the historic building of the Technion (now hosting the National Science Museum) and surrounding sheds and buildings. Conditions for study and for research were poor and Spartan. For example, our student laboratory of organic chemistry was located, in Haifa Bay, in an industrial structure, not fitted for the task requiring long journey by bus. Yet working with Amitai was a very inspiring experience for me both scientifically and personally. I admired his sense of humor, his kindness and his being a true Renaissance scholar. We found many common fields of interests beyond the obvious chemical issues, such as history, literature, music and football. Amitai was my first chemistry mentor becoming also my friend.

In the summer break of 1962, I met Ruth Friede at a dance evening at the Technion's student club. Ruthie, who was then a soldier, also studied to become a teacher in the Army Teachers College. It eventually lead to our getting married on March 17, 1964.

In October that year, I started my army service in The Nuclear Research Center, "KAMAG" near Dimona. **Our son, Erez, was born on February 20, 1966**. I then transferred from KAMAG to serve at the Naval Officers Academy in Haifa.

During that year, I started planning to continue my chemistry education towards a Ph. D. degree (then D. Sc. Degree) at the Technion. During my fourth year at the Technion and in KAMAG, I liked my advanced courses in spectroscopy, photochemistry, quantum chemistry and statistical mechanics, and decided to do my doctoral research in these disciplines. I thought of joining the research group of Professor Otto Schnepp that was involved in molecular spectroscopy. Much to my disappointment, I discovered that Schnepp had left the Technion to take up a position in the US. I consulted with Amitai that told me that in addition to Arza Ron who took over Schnepp's group, two new Faculty members have just joined the Department (now the Faculty) of Chemistry, Professors Mordecai Rubin and Sol Kimel. I went on to see both of them. Rubin told me that his interests are mostly in synthetic organic photochemistry and much less in the physical chemistry aspects of it, which did not appeal to me. I then met with Sol Kimel, this actually started my academic career.

3. My Ph. D. work, 1967-1970

Sol outlined his scientific background and gave me a short review of his research on infrared spectroscopy of matrix-isolated molecules, done at the Weizmann Institute before joining the Faculty at the Technion. He went on to say that while he is still interested in some theoretical aspects of this subject he intends to establish a laser based spectroscopy research at the Technion. He was very frank in telling me that he has no experience in this field but is confident that it is the future field of research in molecular spectroscopy. He suggested that I would do some literature survey in order for me to decide whether I would like to join his group as his first student. The laboratory main equipment was a newly purchased ruby laser. It was supposed to arrive at about the same time of my discharge from the army, in March 1967. The subject that I were to explore was specific physical chemical aspects of the Stimulated Raman Effect. At that time there was not even one textbook about laser physics that I could have used to learn the basics of the field. It took me more than a month of intense reading of the literature to realize that this subject is being thoroughly investigated by leading groups (one was headed by Bloembergen that later was awarded the Nobel Prize in physics for his work on this subject) which did not leave much for us to contribute. However, my readings suggested other possibilities in the newly emerging discipline of nonlinear optics. One of the subjects of investigation in this field was two photon absorption processes that obey different selection rules, compared to conventional one photon processes. I came back to Sol telling him of my conclusion and suggested that I will investigate two photon induced photochemical processes that may lead to different reaction patterns due to these unique selection rules. Sol told me that he asked around about me and received enthusiastic recommendations about my academic performance, so much so that he trusts me with leading a research project as I suggested. Our joint experience turned out to be highly rewarding, mainly due to Sol's personality. It was an odd collaboration between a real educated European Gentleman, with a great sense of humor, and a rough going, opinionated Israeli born. I appreciated very much the freedom and independence given to me by Sol to conduct my research, and enjoyed our daily scientific discussions and each other company. We soon found out that we had many common interests also in non- scientific subjects. Sol was my mentor and eventually became a very good friend.

I found out that iodoform undergoes photolysis, when excited at a 350nm to yield, in a chain reaction, molecular iodine. One molecule of iodine is produced for each quanta absorbed by iodoform that has a broad absorption band at this excitation wavelength, which corresponds to excitation of two quanta at the ruby laser 694nm wavelength. This made iodoform an ideal candidate for quantitative determination of the ruby laser induced two-photon photolysis, by measuring the quantum yield of the produced iodine.

The six days war in June 1967 had delayed the arrival of our laser system. When it finally arrived in October, we had to learn the basics of setting up a laser spectroscopy laboratory. We got very useful advice from Professor Shaul Yatsiv and his coworkers from the Department of Physics at the Hebrew University in Jerusalem. They had already a lot of experience in laser physics and laser techniques, especially in the study of nonlinear optics of metal vapors and in particular the study of the Stimulated Raman Effect in potassium. By the end of November, we were ready for our first experiments after setting up our laboratory with the skillful assistance of our departmental mechanical and electronic services. We developed a methodology for measuring the photolysis yields as function of excitation laser intensity. We have established the two-photon characteristics of the laser induced photolysis of iodoform and were able to determine the **absolute** value of the two-photon absorption cross section, a first time measurement of its kind. However, our most exciting discovery was a new kind of nonlinear optical solvent effect that increased the efficiency of the two-photon absorption due the effect of self-focusing of laser beams in solvents possessing a high optical Kerr constant, similar to the manifestation of this effect in enhancement of Stimulated Raman intensities. We finally went on to develop a novel chemical method for measuring the self-focusing length in a variety of solvents. I finished writing my D.Sc. thesis: “Photochemistry of molecules excited by ruby laser; two-photon photolysis of iodoform”, and submitted it in December 1969. It was a real pioneering work in the very young field of laser-induced chemistry, the first done in Israel and one of the very few worldwide. Our publications of this work [2,3] were well received by the scientific community.

Earlier in 1969 Professor Adam Heller, then the head of laser research at the “Sylvania” group of “General Telephone and Electronics, (GT&E)”, visited us. He told me that he likes our research, and offered me to do my post-doctoral work with him, exploring his new idea of developing a novel kind of liquid lasers. I accepted his offer, when less than a month before my scheduled departure to the US, on August 1969, I received a cable informing me that GT&E had closed down its all its research activity in “Sylvania”, meaning of course, that I needed to find a different place for doing my post-doctoral studies. Sol has suggested that I will apply for a temporary position of a Research Fellow at the Technion, which will give more time for securing an alternative post-doctoral position. I followed his advice, and my application for that positon was approved. Thus, I became a Research Fellow all through the academic year of 1970/71.

Our second son, Allon, was born on February 27, 1970, just 2 days before I defended my D. Sc. Thesis.

The year I spent as a Research Fellow proved to be rather fruitful. I collaborated with Sol and his new student, Oded Kafri, on number of projects. We extended our nonlinear photochemical studies on iodoform to consider theoretical aspects of multiphoton induced photochemistry[4-6]. Together with Itzhak Oref, we investigated the two-photon induced photolysis of gas phase azoethane.[7] Review of our work on nonlinear photochemistry, published in Nature, described it as a pioneering work and a breakthrough in this emerging field.[8] With Oded we also developed a novel theory for describing laser Q-switching by a rotating mirror, based on the Doppler Effect.[9]

In April 1971, Professor Jan Kommandeur from the University of Groningen, The Netherlands, visited our laboratory. Jan was Sols' acquaintance from their joint studies at the University of Amsterdam. We had a very intense discussion of my work and Jan came up with the suggestion that I will join him in Groningen as a Research Fellow. After consulting with Ruthie, I accepted this offer and in August 1971, we traveled to Groningen for a one year of post –doctoral studies that , eventually was extended to a second year.

4. Post- Doctoral period in Groningen, 1971-1973

Traveling to Holland was our first trip ever abroad. We were assisted by Jan, members of his research group and above all by his wife, Lizzie, that made our adjustment to life in Groningen smooth and pleasant. We rented an apartment not far from the new campus of the university. Erez started to go the kindergarten in our neighborhood and after some difficulties, including speaking fluent Dutch adjusted well to his new environment. Ruthie who stayed at home with Allon was excited in exploring Groningen that proved to be a pleasant, though a provincial quiet town. She soon got her way around spending time with some of our new friends, including two

Israeli families also associated with the university. Overall, we liked being in Groningen, however, we never got used to Dutch weather, and I believe that most locals shared our sentiment.

I started working in the Laboratory for Physical Chemistry, which was in the center of Groningen and was about to be relocated in the new campus. I got used to riding my bikes to the laboratory and to the social interaction with laboratory members, both research students and Faculty. Jan first suggestion for a research project was to investigate radiation less transitions of the vibrational molecular manifold due to infrared excitation by a CO_2 laser. The idea was to build this laser utilizing the excellent technical skills of the Laboratory workshop. We consulted with Jan's colleague in the Technical University in Enschede who had a lot of experience in building CO_2 lasers and realized that it will not be an easy task and may not be worth the effort, especially since the same process could be studied using optical excitation in the UV-visible spectral range. We only had a ruby laser system, similar to the setup that I have used at the Technion, which was not suited to the proposed project that needed a tunable laser source. Jan issued budget request from his funding agencies to purchase a dye laser pumped by nitrogen laser, which was the suitable system. While we were waiting for approval this budget request and eventually to receiving the laser system, Jan suggested that I would get involved with the research program of his Ph.D. student, Gerard Makkes van der Deijl. Gerard was studying photoionization of biphenyl radical anion induced by ruby laser excitation. He observed a kind of saturation effect for the intensity dependence of the process. He varied the laser intensity by changing the pumping energy of the ruby rod. Based on my own experience and that of others I knew that this procedure results in uncontrolled changes in the temporal substructure of the laser pulse leading to uncontrolled laser intensities, not correlated with the pulse temporal envelope. Adopting my suggestion Gerard repeated his experiment, now changing the laser intensity by a series of neutral density filters, while keeping the flash pump energy fixed. The intensity dependence turned out to be linear, as it should for single photon excitation. Temperature dependence of the photoionization process indicated the existence of a higher excited state reached by consecutive two-photon absorption, however, the ultrafast non-radiative decay of this state, reduces the apparent quantum yield at higher excitation energies. This explained the two different laser dependencies that we observed.[10]

We soon realized that the pattern observed for the biphenyl photoionization is not limited just for this system and should effect any molecular excitation process of the first excited singlet state, where at higher laser excitation intensities, the fluorescence or the photochemical yield originated from this excitation will be quenched by further excitation to a upper singlet state. We called this general phenomenon "photoquenching". Together with Jan's Ph.D. student, Rennie van der Werf, we studied the various aspects of photoquenching and came up with a very useful expression in terms of a Stern-Volmer like intensity dependence of the observed quantum yield.[12] Together with Rennie I was involved in building up a new laser laboratory based on nitrogen laser-pumped dye laser system. It was part of a new project to study radiationless transitions in isolated molecules by monitoring their time resolved fluorescence decay. Unfortunately, we have finished building the laboratory just before my post-doctoral period ended.

During my second year in Groningen Sol took up a sabbatical leave at the University of Amsterdam. He was looking for a project that could make use of the laser flash spectroscopy set up that was available. Together with a graduate student we studied the flash spectroscopy of iodoform and iodine and were able to prove that the primary process in iodoform photolysis is the formation of atomic iodine.[11]

During 1972, I applied for an academic position at various Israeli Universities. I have soon realized that in-breeding practice of favoring a local university graduate dictated the responses I received. Michael Ottolenghi from the Hebrew University told me that secure their next appointment for Yehuda Haas that was about to go for his post-doctoral studies in California. At Tel Aviv University Danny Huppert was actually sent to the laboratory of Peter Rentzepis for a post-doctoral period in which he learned how to set up picosecond lase laboratory upon his return to Tel Aviv. In Weizmann Institute Yehiam Prior that has just left for his post-doctoral period was due to take up a position in the Chemical Physics Department.

I received nominations for a Lecturer position at Ben Gurion University and at the Technion. Some family considerations made me to favor the offer from the Technion. Thus in August of 1973 we ended our stay in Groningen and returned to Haifa.

5. Faculty member at the Faculty of Chemistry at the Technion, 1973-1979

On September 1, 1973, I came to the Faculty of Chemistry to start my work as a Lecturer. The Dean, Professor Michael Cais greeted me. He told me not to expect any set up money to establish my research laboratory due to severe budget cuts taken by the Technion-“you will need to prove yourself without this” were his exact words. He went on to tell me that Sol offered to share his laser laboratory with me and have assigned a fourth year undergraduate, Hedva Zipin, to start my research group. While I felt at home coming back to my old laser laboratory and even to the same small office I had, I was frustrated that my research plans for setting-up a new laser laboratory based on tunable laser for time resolved photophysical studies of molecular systems, will have to wait until I would be able to secure the necessary funds. The breakup of the Yom Kippur war in October resulted in another delay of almost half a year before normal activities at the Technion have resumed. Our laboratory engineer, David Jacobs was killed in Sinai; it had shocked all of us, and was a blow to our research capabilities.

This new situation forced me to change my research strategy. I had to rely on projects suited for ruby lase excitation, to initiate novel theoretical studies and to try and build a nitrogen laser based tunable laser system utilizing the Faculty machine shop.

Together with Sol and Arza Ron we studied the theoretical aspects of isotope separation utilizing stimulated Raman gain differences of isotopes[13]. Hedva Zipin studied the two-photon photolysis of the ferrioxalate actinometer system[15]. I have extended investigating photoquenching as a limiting factor for laser-pumped dye lasers[14,16], and initiated collaboration with Jacob Katriel to study the coherent aspects of multiphoton transition proababilities[17]. **On August 22, 1975 our 3rd son, Oren was born**, just when I received my first major research grant from the Israel Academy of Sciences.

Together with Eliezer Weiss, a 4th year undergraduate student, we have investigated the prospects of utilizing energy transfer as means of extending due lasers tunability and minimizing photoquenching effects[18].

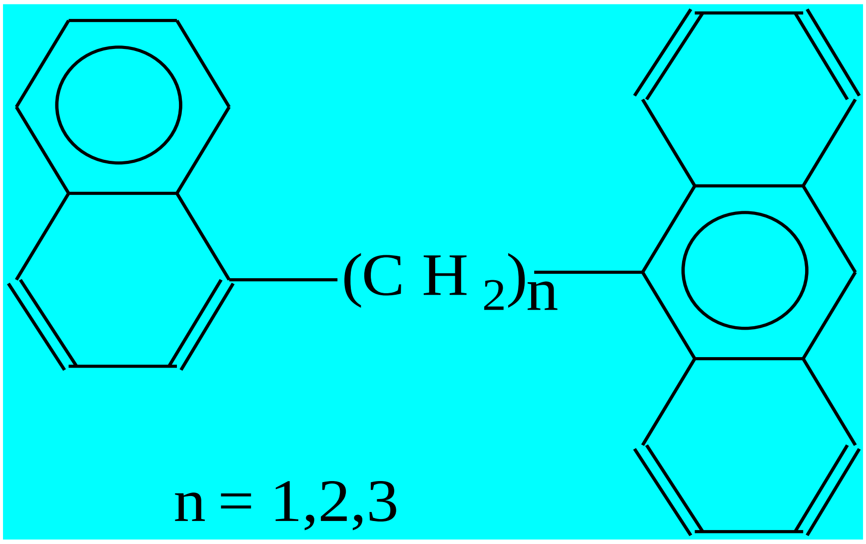
During the summer break of 1976, I stayed at the Physical Chemistry Laboratory in Groningen. Together with Rennie van der Werf we resumed our project of investigating radiationless transitions in small isolated molecules. We soon observed what seemed to be quantum beats as expected from theory. However, to our great disappointment we realized that electronic noise interfering with our detection system was the main cause of our observation. Thus, experiment had to be modified and required some work at the workshop. That meant that I was unable to finish the project during my brief stay. Professor Joshua Jortner from Tel Aviv University was also staying for the summer in Groningen. We had several scientific discussions about my research results where I have discovered a limiting 3/2 power law for multiphoton laser induced photochemical processes.[6] Joshua suggested a theoretical study to formulate a general explanation to this power law due to absorption saturation effects. Our studies showed that indeed a 3/2 power law holds for multiphoton processes over broad laser intensity range' once above a critical intensity which is readily reached at the focal region of a focused laser beam.[19]

Upon returning to Haifa, Sol and I finished writing a comprehensive review on "Lasers and Chemistry" for Chemical Reviews. Its publication in 1977 was well received by the chemical community and was used for many years as a major reference source for researchers in laser chemistry.[20]

My first graduate student, Reuven Katraro, joined me in October 1976. My previous work on energy- transfer- dye- lasers (ETDL) convinced me that the field of electronic energy transfer (EET) is challenging. We soon have succeeded in establishing the unique temperature dependence of long range electronic energy transfer observed in donor acceptor pairs in PMMMA matrix.[21,22] By the end of 1976 I received my tenure and was promoted to the rank of Senior Lecturer. Reuven became our laboratory engineer after finishing his M. Sc. Work. During that period we extended our studies of ETDL and other aspects of EET.[23-25] Our success enabled us to receive a NRF research grant which was mainly used to purchase nitrogen laser pumped dye lase system together with a data acquisition system. We were now in a position to study time resolved laser spectroscopy and photophysical prossseces.[26]

For a long time I was interested in investigating the mechanism of intramolecular EET (Intra-EET). Schnepp and Levy did the pioneering work in this field. They investigated the bichromophoric molecule, **I**, where excitation of the naphthalene moiety resulted in fluorescence from the anthracene chromophore. Schnepp and Levy interpreted the observation as an efficient intra-EET process. However, given the experimental conditions they could not establish the process mechanism.

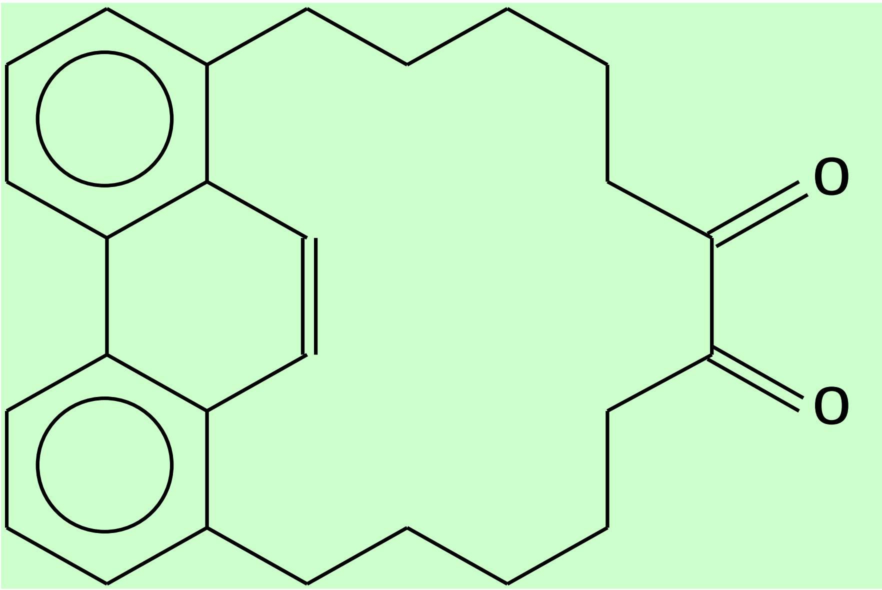
I



In the fall of 1978 I was asked by Professor Mordecai Rubin to help his Ph. D student' Shmuel Welner to measure the fluorescence spectrum of the bichromophoric molecule**, II**, that Welner synthesized. I immediately recognized that this molecule will be ideal for investigating the mechanism of Intra-EET process. This was proved in a series of measurements that were rapidly published.[27] This study marked the beginning of more than 30 years of collaboration with Professor Rubin for studying all aspects of Intra-EET.[28]

In February 1979, Nasser Shakour joined our laboratory as our engineer, replacing Reuven Katraro.

II



6. Sabbatical Year, 1979-1980

In August 1979, I took up a sabbatical year with Professor Ernest (Ernie) Grunwald at Brandeis University. Ernie was interested in infrared multiphoton induced photochemistry utilizing a pulsed CO_2 laser. I suggested investigating the excitation and photolysis of hexafluorobenzene (HFB) by following the time resolution of the excitation by monitoring the evolution of hot bands formation in the electronic spectrum of HFB. This became the Ph.D. Project of Mike Duignan. We indeed were able to show just that[29], but, in addition, we noticed fluorescence emission in the visible spectral region. I suspected that this emission can be attributed to reverse radiationless transitions, as predicted by Jortner and co-workers giving rise to electronic excitation of the pumped molecule. Time resolved spectral analysis revealed a short-lived emission corresponding to the expected HFB fluorescence[30], accompanied by a broad structure-less emission band, spanning the entire visible region, with two distinct peaks corresponding to C_2 and C_3 fragments produced by the HFB photolysis. We showed that this band is characteristic of black body radiation for hot body at 3500K, attributed without mass spectrometry analysis to "highly carbonated" molecular fragment[31], that might have been superheated C_{60}, as shown , much later, by Kolodney, when C_{60} became a major researched molecule.

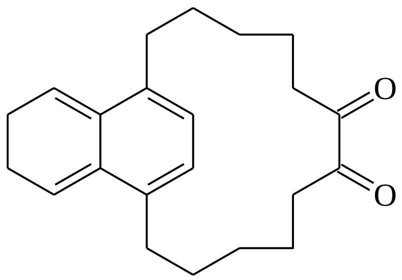
My father died on February 1980, which required my travel to Haifa during my stay at Brandeis.

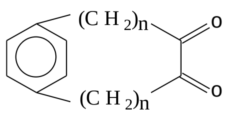
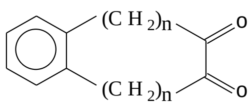
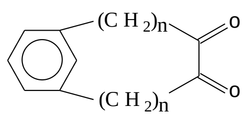
7. Technion, 1980-1986

In 1981, I was promoted to the rank of Associate Professor. I continued my old project on photoquenching and on ETDL systems. We have measured photoquenching parameters that determine RTDL performance.[32,33] Our results were used in defining laser dyes in Kodak catalogue for laser dyes. Mordecai Rubin and I decided to launch a comprehensive research project for the elucidation of the mechanism of Intra-EET in solution. Our strategy was to synthesize a family of bichromophoric molecules with various lengths of the inter-chromophore bridge. The molecules chosen comprised of an aromatic molecular chromophore, such as benzene and naphthalene connected to a biacetyl moiety **III, IV** , similar to **I**.[34,35]

III

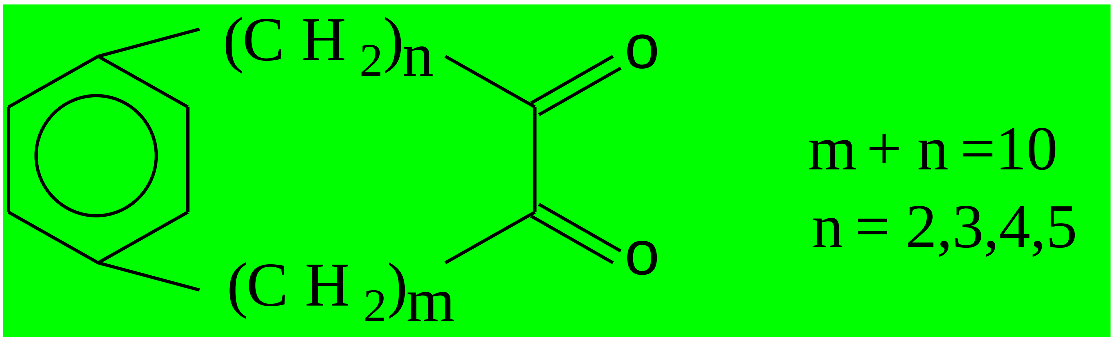
1,4-Naph-5,5



P-n,n	**O-n,n**	**M-n,n**
		
n = 4,5,6	n = 2,3,4	n = 3,4

Scheme 2.

IV



Together with Professor Colin Steel of Brandeis University we received our first US-Israel BFF grant for studies of Inra-EET. Our continued efforts in this field resulted finally in proving the essential role that Dexter type exchange interaction plays in singlet-singlet short range Intra-EET processes.[36-38] . My achievements received recognition by awarding me the Raymond and Miriam Klein Prize in 1984.

In addition, I continued to investigate molecular non-linear optical effects.[39] Together with Jacob Katriel and my Ph.dD student, Meir Orenstein we have investigated optical bistabilty by developing a novel eikonal theory suitable for non-linear optical media in general and to molecular systems in particular.[40-42]

In summer of 1982, I began collaboration with George Atkinson, first at Syracuse University and in the following years at the University of Arizona in Tucson on various aspects of spectroscopy and photochemistry of isolated molecules. This resulted in series of publications[43-45] and in receiving our next BSF grant with George.

In August 1983, while in Syracuse , my mother died and I had to cut short my stay with George.

8. Sabbatical year, 1986-1987, Technion 1987-1991

In September 1986, I accepted the invitation by Dr. Jim Yardley, head of the photochemical research group of Allied Signal in New Jersey, to join his efforts in launching a project of molecular electro-optics. He was in particular interested in our recent results on optical stability and in our ideas in molecular electronics. We were successful in defining the experimental condition for observing optical bistability in nonlineraly absorbing dyes and its implications to spatial light modulation. These efforts continued in 1987-1989 when I continued my association with Jim as a consultant.[46-50].

During my sabbatical leave, I was promoted to the rank of Full Professor. Upon my return to the Technion, in September 1987 I resumed my research on Intra-EET and in molecular electro-optics and molecular electronics.[51-55] We were awarded a special grant to purchase a Nd-YAG laser coupled to a tunable dye laser allowing us to conduct laser induced spectroscopy and photochemistry in the 220-700 nm spectral range. We built a supersonic jet expansion valve that enabled us not only to study Intra-EET processes in solution,[56-59] but also to start a new project in investigating these processes under the conditions of super-cooled isolated molecules.[59]

In 1988, I became the Head of Technion's Division for Youth Activities, I resigned from this position after I was elected as the Dean of Faculty of Chemistry in October 1990.

9. Dean of Faculty, 1991-1994

I started my tenure as Dean when the academic standing of our Faculty at the Technion was in question. We had very few students almost no new Faculty members and eight Faculty members that were about to retire within the next 2 years. The sentiment at the Technion management was that a major change involving even a possible shutdown, is needed. During the period in which I prepared myself to take office in January 1991, I have drafted a “white paper” that I have sent to the President, analyzing the situation and outlining a plan of action for reviving the Faculty. I stressed the need to replace all of the positions that will become available due to the anticipated retirements by judiciously chosen young recruits that will receive significant start-up funds to insure their academic success. To my pleasant surprise, President Tadmor accepted my white paper and instructed his administration to help us achieving the goals set by me. In the course of my four years as Dean we recruited nine new Faculty members, we initiated new courses and established collaborations with the Faculties of Materials Engineering and of Biology. In addition, the management accepted our demands to start rehabilitation of the deteriorating infrastructure of the Chemistry Building. This enormous project continues to this day.

In 1992, I became incumbent of the Fruend Chair in Chemistry.

In spite of dedicating much of my time to carry out my administrative duties, I managed to keep an active research. I established collaboration with Professor Don Levy from the University of Chicago (and Editor of Journal of Chemical Physics). Together we received new BSF grants, to work on our EET jet and solution studies.[61-66] We have also received an EEC grant to start a new project on photoquenching in laser dyes. [67] In addition, I have expanded my projects on molecular electronics[68-72] to the field of photoconductivity in polymers.[73-79] In 1993, I received The New England Academic Award, in recognition of my achievements in EET research.

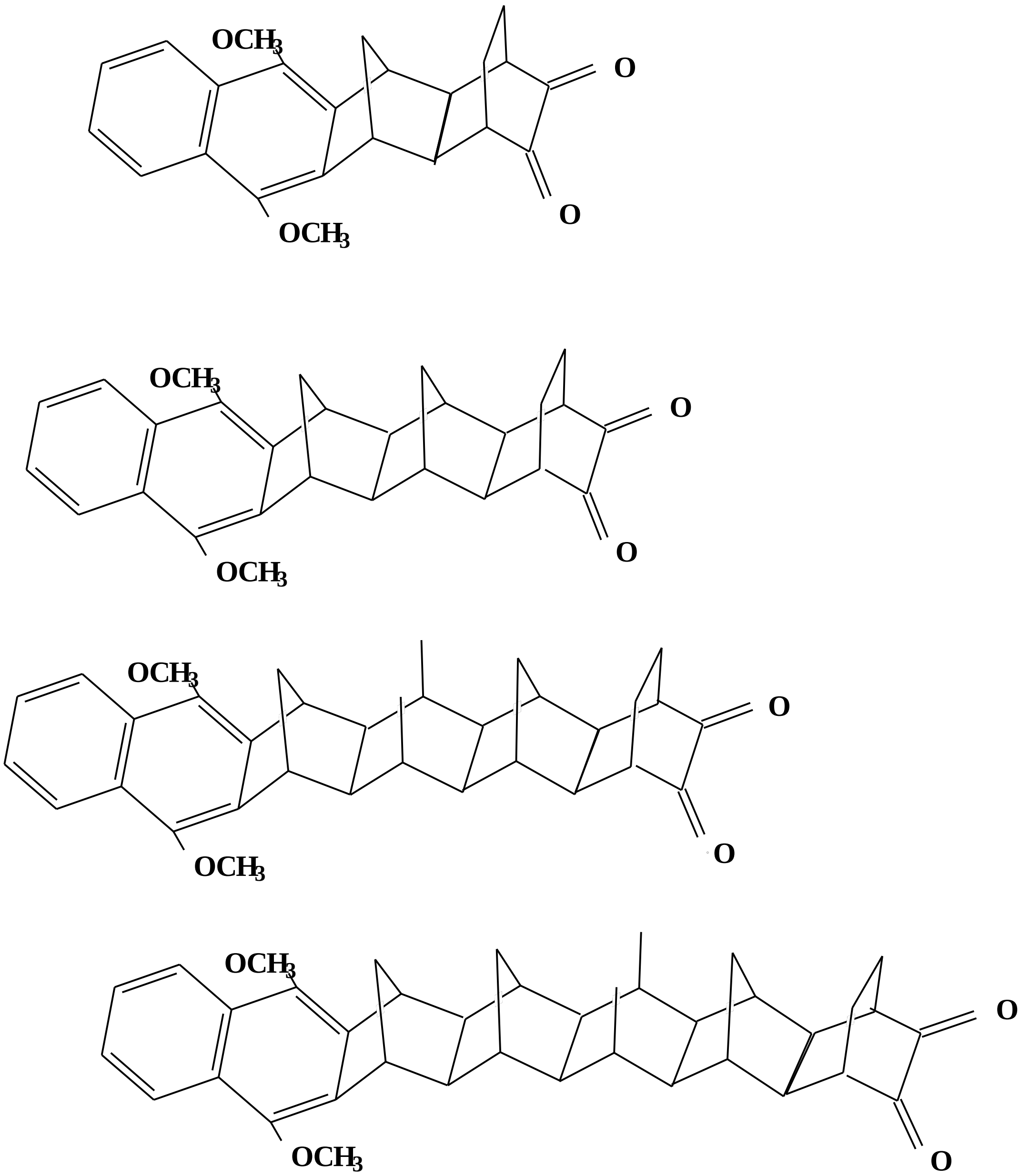
10. Sabbatical leave, 1994-1995

After finishing my tenure as the Dean of Faculty of Chemistry I took a sabbatical leave. I spent the first part of my sabbatical at the Laboratory of Physical Chemistry in Groningen. I devoted that time to finish writing few papers,[81,82] and an invited review on EET[82] .

In February 1995, our first grandchild, Yotam, was born. Certainly the most significant event of that year for me.

In the second part of my sabbatical leave, I stayed at the University of Melbourne Australia, collaborating with Professor Ken Ghiggino. I suggested studying the role played by the inter-chromophore bridge, in bichromophoric molecules, by comparing the rate of Intra-EET in rigid bridges and in flexible bridges. The work of Ghiggino on electron transfer suggested that rigid bridges are efficient due to through-bond interaction in enhancing the process. I suggested approaching Professor Paddon-Row from university of Sydney to synthesize for our project a series of molecules based on his previous work with Ken based on the rigid norbornene bridges. It took another year after I left Australia for him to finish the synthesis of the desired series of molecules, **V**. The results were spectacular we showed that rigid bridges enhance Intra-EET by a factor of up to 10^5 compared to flexible structure such as **III** and **IV**.[90] This was considered as a major breakthrough in understanding the mechanism of Intra-EET.[91]

V



11. Technion, 1995-2010

My research projects continued to involve Intra-EET in solution, supersonic jets and in stretched polymers films.[92-97]Collaboration with Japanese groups yielded interesting results in revisiting the pioneering work on EET in the naphthalene-anthracene bichromophoric molecule**, I**. By utilizing ultrafast laser excitation, we were able to prove that the observation made by Schnepp and Levy was a genuine Intra-EET process as they have argued.[98]

In 2000, after short sabbatical leave at the university of Vienna and at Humboldt University in Berlin, I started an ambitious project in which I aimed to explore the potential of using EET in combination with photoquenching effects in molecular electronics.[99]

I collaborated with Yoav Eichen in synthesizing and characterizing the molecular systems that were needed for this project [100-102] , and with Uri Peskin for establishing the theory needed for understanding controlled transfer prosseces.[103,104] Another important collaboration started with Professor Raphael Levine from the Hebrew University , on the concept of molecular logic.[105] We soon realized, that our approach of combining Intra-EET with photoquenching, in specially designed bichromophoric molecule, could be utilized for making a full-adder, molecular-scale logic circuit.[106-111]

In 2001, In recognition of my achievements in the field of molecular electronics, I received the Henry Taub Prize for Excellence in research.

In 2003, I was asked to act as the President of the Israel Chemical Society, I served for three years but did not agree to a second term as it conflicted with my new duties at the Technion as the Dean of the Division of External Studies and Continuing (E&C) Education. During my tenure of six years as Dean of ES&C, I managed to expand the Division's activities and to increase its revenues by 400%.

An additional area of research I was involved with, just before retirement, was photo induced proton transfer. I collaborated on it with Menahem Kaftory and our efforts resulted in several joint publications.[111-116]

I received the Medal of Honor from the Claude Bernard-University Lyon 1. In 2009, after finishing my tenure as Dean of the Division I took my last sabbatical leave at Ecole Normale Superiuer in Paris and at Columbia University in New York.

12. Life as an Emeritus Professor

Retirement In October 2010 meant that I should give up my laboratory, thus I will not be able to pursue any experimental project. I had no problems with this regulation and had different plans for my retirement. I was surprised, however, to discover upon returning from my sabbatical, that without my consent, that the laser equipment was taken by the new Faculty Analytical Facility, and faculty members took all of my other laboratory equipment, and threw away what was left of it. No one offered any compensation for it, although I purchased it out of my research funds. I had a long-standing promise from all serving deans that they will renovate my office and even that promise was never fulfilled, the excuse given was always lack of funds, which in view of “confiscating” my laboratory while I was still an active faculty member left me bitter and highly disappointed. The only consolation for me was the fact that a new talented Faculty member, Lilac Amirav, took over my old laboratory, thus keeping it as part of spectroscopy and laser interaction laboratories.

I rather enjoy my retirement. It gives me time to enjoy our eight grandchildren, travel a lot. I still go to scientific conferences, mostly as an invited speaker. I teach a course in thermodynamics at the International School of the Technion, and enjoy the freedom to attend only seminars that interest me.

I started to paint, a childhood passion of mine, and enjoy the results. I study many aspects of classical music and even give popular lectures on music and art for interested parties.

Frank Sinatra, reflecting about the end, claimed that he did it his way, using the lyrics of his friend Paul Anika, who in turn took it from the original song: “comme l’habitude” (“as usual”), by the French pop star, Claude Francois. I feel that “I did it my way”, however, without Sinatra’s pessimism.

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