Amitai Halevi

Born USA, 1922

Ph-D thesis:
University College London, 1952

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

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

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

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

Major departmental responsibilities:
Chairman, Department of Chemistry 1983-85

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

Field of Research

Physical and Theoretical Organic Chemistry

Experimental and computational studies of isotope effects on molecular properties, equilibria and kinetics.

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

Amitai Halevi

(April 2008)
Isotopes, Symmetry and Music

1. Roots

Obeying the Red King's instructions[24], I will begin at the beginning, go on till I come to the end, and then stop. Accordingly: I was born on May 22, 1922 in Brooklyn, N. Y. to Rose *née* Taran and Mordecai Halevi. Both of my parents were born in the Ukraine, then part of imperial Russia. My mother immigrated to the United States as a teen-ager in 1908 with her father, mother, two brothers and four sisters. They settled in Rochester, N.Y., where her father, Moshe Taran, who had been ordained to the rabbinate but disliked its practice, chose to engage in business instead – with moderate success. After qualifying as an accountant, my mother moved to New York City with her older sister Rae.

My father's route to the U.S. was less direct. In 1911, soon after being drafted into the Czar's army, he deserted, embarked for Palestine and enrolled in the Teacher's Seminary in Jerusalem. When the First World War broke out in 1914 he was interned by the Turkish authorities as an enemy alien, was redeemed by a suitable payment of baksheesh, and made his way to Egypt. There he enlisted in the Jewish Legion of the British army and soon found himself at the Dardanelles, a Corporal in the Zion Mule Corps under Captain Joseph Trumpeldor. After the ill-fated Gallipoli campaign, the Corps was shipped back to Alexandria and disbanded, whereupon my father eventually made his way to the United States. He settled in New York City, where he met and married my mother and began making a living as a Hebrew teacher. However, when the United States entered the war, he joined Yitzhak Ben Zvi and David Ben-Gurion in organizing the American Jewish Legion for Palestine, enlisted once more and trained with the Legion in Canada, but illness intervened and WW1 was over before he could get to the front[25].

2. Childhood

[24] Lewis Carroll, "Through the Looking Glass", Chapter 3.

[25] My father's autobiography would evidently have been much more interesting than mine can possibly be.

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

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

An ardent Zionist, who had every intention of returning to Palestine with his family, my father insisted on speaking to me only in Hebrew. For my first year or so, my mother went along with him, but then gradually slid over into English[26]. My grandmother insisted on speaking Yiddish – to me and everyone else, so I picked up a smattering of that language as well. My father was deeply steeped in traditional Jewish learning but completely non-observant. His passion for the Hebrew language and his inborn talent for teaching are probably what made him persist as a poorly paid Hebrew teacher in an educational system that put its emphasis on inculcating reluctant American-Jewish children with the rudiments of Jewish religious ritual. The job called for some dissemblance on my parents' part, as illustrated by the following anecdote, told to me years later:

Several members of the Center's board of directors used to practice their Hebrew on me. One Yom Kippur, when I was about three years old, one of them asked me in Hebrew, "What did you have for breakfast this morning? I answered, "bread and butter, an egg and a glass of milk." My questioner followed up with, "and what did

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

your father eat?". I answered indignantly, "Don't you know that grown-ups fast on Yom Kippur?"

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

The idyll ended abruptly on May 9, 1928. Several days after the birth of my brother Nadav,[27] my mother died of post-partum complications. Somewhat precocious, I was already a schoolboy, having been enrolled in the first grade of the Center Academy at the start of the school year. My father's predicament as a single parent of an infant and a six-year-old was eased somewhat by the proximity of four maternal aunts, who took turns looking after us. He was thus able to carry on nearly as before, teaching, looking after the summer camp, and earning his B.S. degree at Columbia University's Teacher's College.

The next blow came with the collapse of the American economy following the Wall Street Crash of 1929. Support of the Center from its affluent members dropped, so the teachers had to accept a salary cut in order to keep their jobs. It would be an exaggeration to say that we lived in poverty, but things were tight. Camp Schroon Nahar kept going for another couple of years, but eventually attendance dropped, investors began calling in their loans, and the camp went bankrupt. My father could no longer afford to pay my tuition at the Center Academy, even at the reduced rate for the children of employees, so I transferred to a public school for my general education and received a thorough course of Hebrew studies from my father. The switch did not trouble me greatly. After school, I spent most of my time with my cousins, attending baseball[28] and soccer[29] games , among other activities.

Meanwhile, my father completed his studies at Teachers' College towards the M.S. degree under the supervision of Prof. William H. Kirkpatrick, a strong advocate of

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[27] At present Professor Emeritus of Economics at the Hebrew University, Jerusalem.

[28] The Dodgers had not yet moved from Brooklyn to Los Angeles.

[29] Soccer was a minor sport in America, but two of my uncles were avid fans of the New York Hakoah soccer team, composed largely of ex-members of the legendary Vienna Hakoah.

John Dewey's theory of progressive education: the "child-centered school", a fact that was to have important consequences for us.

3. Pardes Hana

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

Two years later, in the summer of 1934, we boarded ship for what promised to be a permanent move to Palestine.[30] My father had been offered the position of head teacher in a new agricultural high school being established in Pardes Hana under the auspices of the Farmer's Association. In contrast to existing agricultural high schools like Mikveh Israel and Kadoorie, it was to include an academic secondary curriculum leading to full matriculation. Dr. Arthur Biram, the legendary founder and principal of the Reali School in Haifa, was appointed principal of the school in Pardes Hana as well, effectively making it a branch of the Reali. Dr. Biram was a Teutonic-style educator of the old school. The stated aims of the Reali at its foundation in 1913 were: "Zionism, *discipline* [my emphasis _*A.H.*] and self-fulfillment – national and social".[31]

As the permanent site of the school in Pardes Hana was still under construction, its temporary quarters were set up in a homestead in Meged, between Pardes Hana and Karkur. Dr. Biram was way up North in Haifa, so my father was the *de facto* principal,

[30] David Ginsburg, aged 14, was also on board. Our friendship was to last until his death 53 years later.

[31] It is characteristic of Dr. Biram that when WW1 broke out in 1914 he resigned to enlist in the German army, and resumed his position as principal of the Reali School in 1919.

whereas a taciturn, one-eyed Yugoslav agronomist named Zalustcher[32] was in charge of the still rudimentary physical plant, as well as of agricultural education. About half of the pupils were bussed in from neighboring communities, notably Hadera; those from more distant communities – me included – were boarded in a dormitory, presided over by a house mother. The first of these, was Miss Bodenheimer, a dour maiden lady whose stated educational philosophy was that anything that gave a child pleasure was *ipso facto* harmful to the development of his/her character. Her disciplinarian attitude was too much for my father to take, so she was eventually replaced by Mrs. Harifai, the widowed mother of my classmate Ben-Shahar and six-year-old Zaharira,[33] a suitable playmate for seven-year-old Nadav.

The school began functioning with a single class and added a second in the following year, which turned our do be our last, so the number of pupils was small,[34] as was the teaching staff. My father taught all of the Hebrew subjects and Zalustcher introduced us to the elements of farming. Our science teacher was Shmuel Duvdevani, whose independent research on dewfall led to the eventual invention of the Duvdevani Optical Dewmeter, which is still in universal use.

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

Though my father's relations with Dr. Biram remained cordial, their irreconcilable differences on educational policy made it increasingly difficult for him to stay on as head teacher under Biram's authority. Meanwhile, my brother and I had acquired a stepmother,

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

[33] Zaharira Harifai became a prominent actress on the Israeli stage.

[34] Two of them, Aharon (Ahrele) Yariv and Meir (Memi) De Shalit, were to make major contributions to the security of the State of Israel.

a successful Haifa dentist named Yehudit Yoshpe, whom my father had met on a brief trip to the Soviet Union. Thus, our augmented family found itself back in the United States, where, soon thereafter, my father accepted an offer from the Board of Jewish Education in Greater New York to serve as Supervisor.

4. To and from

We resettled in the same Brooklyn neighborhood, where my brother and I resumed close contact with our cousins. Though there were other high schools nearby, I chose to enroll in Thomas Jefferson High School, several stations away on the Subway[35] in Brownsville, a neighborhood that is now mostly black and Hispanic, but was then largely populated by less affluent and more Orthodox Jews. The reason was purely practical: TJHS gave credit for Hebrew, so I could fulfill my "foreign language" requirement without effort. Moreover, my studies in Pardes Hana earned me sufficient credit that I was able to graduate in two years. I did well enough in all my subjects, but had no particular preference for any. In addition, I decided to learn how to play the violin, necessitating a subway ride to the Manhattan School of Music twice a week. I took the violin quite seriously, and was playing in the both the music school- and high school orchestras within a year, but did not delude myself that I was talented enough to consider becoming a professional musician.

The family embarked for Israel in the summer of 1938 and settled in my stepmother's house in Bat Galim – then quite a fashionable suburb. I enrolled at the Technion, where my performance as a first year student was less than satisfactory, to put it mildly. At 16, I was too young to adapt myself to the system: Every student was equipped with a red booklet, called the *Index,* which each of his/her teachers would sign once a semester, confirming his/her attendance at lectures. Attendance was not obligatory and was never checked, a circumstance of which I took full advantage. In some cases, absenteeism could be justified: Chemistry, for example, was taught by the elderly, bearded Professor Ilioff, a recent arrival from Russia, who – knowing no Hebrew – had his lectures translated into Hebrew, transcribed into Cyrillic letters, and read phonetically to the class. It was more difficult to escape laboratories and exercise sessions. Chemistry and Physics were no problem, but my mediocre draftsmanship made Machine Parts, let alone Descriptive Geometry – taught by the formidable Luisa Bonfiglioli – a nightmare.

[35] The fare throughout the underground railway system in New York City was still a nickel, i.e., five cents.

All in all, I spent less time on my studies than I did on music, resuming the violin under Zvi Haftel, concertmaster of the Palestine (not yet Israel) Philharmonic Orchestra, and studying musical theory with the brilliant pianist and harpsichordist Frank Peleg. Then, of course, there was the Hagana. We spent much of our time demonstrating against the White Paper of 1939[36] on the one hand and – on the other – against the Etzel (the Revisionist Irgun Tzvai Leumi), who had taken up arms against the British. Come summer, I postponed most of my exams to *Moed Bet* (second opportunity) and attended a course for squad commanders at Juara, near Ein Hashofet, where I realized that my qualifications for command were less than impressive. Meanwhile, my father, having failed to find suitable employment, left for the United States, returning briefly to take my brother back with him after war broke out in September 1939. I stayed on a few months longer, until ordered back to the United States with the other American citizens in Palestine, not having gotten around to taking the exams.[37]

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

It is not altogether clear to me why I chose Chemistry as my major subject. My interests were broad; any area that was intellectually stimulating might have captured my attention. However, I was suffused with the current Zionist ideology that rejected scholarship for its own sake in favor of practical pursuits. My year at the Technion convinced me that I was not cut out to be an engineer, and science was the next most "practical" field of study. Physics must have seemed too abstract and Biology too descriptive – so Chemistry it was. Once again I took up the violin. I spent two summers as a counselor, life-guard and

[36] In it, the British government imposed severe restrictions on Jewish immigration to Palestine just when the Jews of Europe were facing catastrophe.

[37] The Mediterranean was closed to shipping shortly thereafter, so my stepmother – who stayed on a bit longer, had to fly to India and continue to the U.S. on a Dutch freighter.

swimming instructor in a Jewish summer camp in the neighboring state of Michigan. I joined Avukah, a Zionist Student Organization with a leftist orientation, strongly influenced by Hashomer Hatzair, an ideology that I took rather lightly. Mostly, I enjoyed making friends with like-minded students from other universities, and meeting several prominent emissaries from Israel.[38]

1941 was in my senior year in college. I was aware, of course, of the war in Europe and the threat to Jewish Palestine from the advancing German army in North Africa, but the war was still far away. Things changed drastically on December 7, when the Japanese attacked Pearl Harbor and America too was at war. I was called up for military service, but my induction was deferred on medical grounds: they had found a spot on one of my lungs – a finding that a private check-up could not confirm. I was therefore free to complete my undergraduate studies at the University of Cincinnati with the degree of B.A. (Honors in Chemistry),[39] and enroll as a graduate student at the University of Michigan.

In Ann Arbor, I took up residence in a Co-op House[40] most of whose members were either Jewish or black. The studies were much more demanding than at Cincinnati. Algebra was taught by the brilliant topologist Norman Steenrod, then still a largely unrecognized young man. I took Analytical Chemistry with H. H. Willard, I. M. Kolthoff's competitor, who insisted that I unlearn everything that I had learned from the latter's textbook and begin again by memorizing the Periodic Table. My Physical Chemistry teacher was L.O. Brockway, who had built the first Electron Diffraction Spectrometer as Linus Pauling's graduate student. I ambitiously registered for an advanced Classical Mechanics course with the eminent spectroscopist, D.M. Dennison, but had to drop out; it was too much for me. My most memorable teacher was a truly great scientist, Kasimir Fajans,[41] who taught an elective course in Thermodynamics. In 1942 he was only 55 years old, but

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[38] These included the then already legendary Mania Shochat, "Mother of the Kibbutz Movement", and Enzo Serenyi, who later parachuted into occupied Europe and was captured and killed by the Nazis. .

[39] At that time the University of Cincinnati granted Bachelor of Science degrees only to Graduates in Engineering; science graduates had to settle for the degree of Bachelor of Arts

[40] The Cooperative Movement in the United States – agricultural and urban – antedates the Kibbutz Movement by a century or so.

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

had already begun wasting the remainder of his scientific career on his discredited "quanticule theory".[42] Nevertheless, his lectures on thermodynamics were insightful.

My enjoyable and intellectually stimulating stay at Ann Arbor lasted just one semester. The U.S. Army decided – quite correctly – that my lungs were fine after all, and I was inducted into the Army Air Force. My entire military service can be concisely summed up as zigzagging westward across the United States, moving from airbase to airbase: After basic training at Keesler Field, in Biloxi. Mississippi, a town that was ravaged by Hurricane Katrina in 2005, I was adjudged to be "officer material" and sent off to Southern Illinois University in Carbondale, Illinois for pre-flight training. My stay there was pleasant but brief. I actually had a number of flying lessons in a two-seater Piper Cub before the program was terminated. Courses for the training of commissioned air-crew, *i.e*. pilot, navigator or bombardier, were temporarily over-subscribed. Consequently, we were all packed off to air-gunnery school at Brownsville, Texas, on the Mexican border. As I was literate and reasonably articulate, it was decided that I would be trained as a gunnery instructor in Tonopah, Nevada – in the desert half way between Reno and Los Vegas. From there I was sent to Mountain Home, Idaho, where my duties consisted of sitting in a B24 (Liberator) bomber aircraft and seeing to it that the crew's gunners were competent enough to be sent off to combat. I was becoming embarrassed. Most of the other instructors, including an acquaintance from Cincinnati who became my partner in our off-duty soldierly pursuits, were veterans of at least one tour of duty in Europe, and here I was waiting out the war in comfort and safety.[43] Eventually I volunteered for combat and was transferred to Laredo, Texas, where I was assigned to a Liberator crew as waist-gunner. The war in Europe having ended , we were being prepared for service in the Pacific, but atomic bombs was dropped on Hiroshima and Nagasaki and the war with Japan was over before our crew was considered fit for combat. I headed northwest to Walla Walla, Washington, where I waited for release while people who had entered service earlier were demobilized under the "first in first out policy". These included medical personnel, so – in consideration of my training as a chemist – I was co-opted to the medical laboratory, where I spent the time until my release doing blood- and urine analyses. Finally, In April 1946, Sergeant Emil Amitai Halevi was restored to civilian

[42] See J. Hurvic, "Reception of Kasimir Fajans's quanticule theory of the chemical bond. A tragedy of a scientist", *Journal of Chemical Education* (1987), **64**(2), 122-3.

[43] Say rather in relative safety. There were several plane crashes – with gunnery instructors aboard – during my stay at Mountain Home.

status and started on a leisurely hitchhike across the United States to Cincinnati, making frequent stops on route, including one in Kansas City to attend a recital by the brilliant violinist Giula Bustabo.[44]

5. Jerusalem

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

A brief biographical summary: Ada was born and educated in Sopot, the seaside resort of Danzig – then a Free City in the Polish Corridor between East and West Prussia – to Sala (*née* Karo) and Leon Rauch, who had just moved there from the Polish city of Kaliscz with their young daughter Ella. In 1937, 15-year-old Ella immigrated to Israel with the Halutz movement and Ada hoped to follow her as soon as possible. The Nazi stranglehold on Danzig was soon complete, culminating in the Krystallnacht riots in November 1938, in which the synagogues were burned, Jewish-owned shops were ransacked, and the Jewish men were rounded up and beaten. Through the window of her home Ada saw the Temple across the street go up in flames. It was decided that Ada's father , mother and younger brother Natek would join their extended family in Kaliscz,[45] whereas she would remain in Danzig in order to leave for Palestine with the next Youth Aliya group. Unfortunately, the projected

[44] I did not know at the time that, having been charged with being a Nazi sympathizer, her career as a virtuoso would soon be over.

[45] Ada never saw them again. Natek died in the Flössenberg concentration camp; the fate of her parents is unknown.

sailing never took place. In consequence, on March 3, 1939 – two weeks after her 15th birthday – Ada travelled across Europe by rail to Reni, Roumania with a group of Danzig Jews, and set sail for Palestine in the illegal immigrant ship Astir, run by the Zionist-Revisionist organization, Beitar. The voyage, which was notorious for its length – four months at sea – and for the brutality of the Beitar crew towards the passengers, ended in shipwreck on the Gaza-Ashkelon coast on July 7. After a spell in the Sarafand interment camp, Ada was briefly reunited with her sister at Tel Hai before being assigned to a Youth Aliya group in Kibbutz Givat Hayim. Then, enlistment in the British Army, and service until the war's end as a nurse in Egypt and field hospitals in the North African desert. Soon after arriving in Jerusalem she was employed as a nurse in the eye clinic of the already legendary Dr. Ticho.[46]

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

I was given freedom to choose a thesis supervisor. My main interest was in chemical theory, which in this period was largely qualitative. I was intrigued by Ingold and Hughes' studies of the mechanism of solvolysis and wished to prepare myself to join their research group at University College London in due course. Therefore, it was only natural that I apply to work on my M.Sc. thesis in the laboratory of Professor Max Frankel, who taught the course on "Theoretical Organic Chemistry". There were two brilliant young scientists, the Katchalsky (Katzir) brothers, Aharon and Ephraim, in Frankel's laboratory, on the ground floor of the Chemistry Building on Mount Scopus, but it did not enter my mind to ask either of them to be my supervisor.[47] They were working on polymers, not my idea of "Theoretical Organic Chemistry". In any

[46] Now the Ticho House Museum, featuring the art of Dr. Ticho's wife Anna.

[47] Dov Ben-Ishai was Ephraim Katchalsky's graduate student at the time.

event, I probably would not have been accepted by either; they had more pressing concerns. As I learned later, as soon as Frankel left the lab for the day, they switched to military-oriented research, working on into the night. Both were founding members of Hemed, the newly organized Science Corps of the Hagana. When I heard stories about how shabbily Frankel treated his own research students and how little they learned from him, I realized that I had made a bad mistake. I quickly opted out and moved upstairs to the laboratory of Professor Moshe Weizmann, earning Frankel's undying animosity.

Moshe Weizmann, brother of the eminent Zionist leader and President-to-be, Chaim, was a charming elderly gentleman who had once been a capable industrial chemist. He used to come up to his laboratory every morning at eight to chat with us for an hour or so, and then go to Café Alno on Ben Yehuda Street to play chess with his cronies.[48] The running of the lab was left to his senior assistant, Shalom Israelashvili (Sarel), who had recently earned his doctorate with Prof. Felix Bergmann at the Sieff (subsequently Weizmann) Institute in Rehovoth. Moshe Weizmann had hopes that orange peel could be used as a raw material in the manufacture of perfume, an area in which he had once been active. Accordingly, the research project assigned to me was the peroxide-catalyzed addition of iodoform to limonene and subsequent hydrolysis of the product di-iodide to the aldehyde, which would then be the substrate for further syntheses. Progress was slow, not entirely due to my limited competence as a synthetic chemist. The equipment was inadequate and we had no analytical instruments at all. Virtually the only way of characterizing a product was by melting point. Besides, there were distractions.

On January 29, 1947 Ada and I were married in the presence of two witnesses by Rabbi Kurt Wilhelm, a Conservative rabbi whose right to perform the marriage ceremony was not challenged by the Jerusalem Rabbinate. The reception was held at the home of Levi Riklis,[49] an old friend of my father. There were chairs for Moshe Weizmann and Yehuda Sandler, Farkas' senior assistant; most of the other guests sat on the floor. We settled in a rented room in the Givat Shaul section of Jerusalem. Many members of the militant groups Etzel and Lehi were reputed to be residents of Givat Shaul, so every once and a while a British army patrol would round up the men

[48] This point is in dispute. Mordecai Rubin claims that it was Café Atara.

[49] Father of Shalom Ronli Riklis, conductor of the Israel Philharmonic Orchestra,. and the biochemist Emanuel (Emi) Riklis.

to check their identification and search them for hidden weapons. Once, in our absence, they searched our room. According to our landlady, they walked in, saw a picture of Ada in her British army uniform and one of me in the uniform of the U.S. Army Air Force, and walked out.

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

6. Military Science

The "Water Law" was never passed. After November 29, 1947, when the United Nations General Assembly voted for partition of Palestine into a Jewish state and an Arab state, the Mandatory Government began winding down. In parallel, the Arab uprising began, with minimal interference from the British army, which was preparing for withdrawal. It was clear that there would soon be no use for the English section of the Shai, so I was transferred to Hemed, the newly formed scientific corps. At the University, all pretence of normal academic activity ceased and virtually all of the students and younger faculty members were co-opted to the Hagana. Meanwhile, two new American students had joined our laboratory: Mordecai Rubin and Moshe Perlstein. Mordecai also joined Hemed; Moshe, the more recent arrival, was inducted into an infantry unit made up largely of students and died shortly thereafter in the ill-fated "Convoy of 35".[50]

[50] A convoy of forty men set out on foot from Jerusalem on January 15, 1948 to relieve the siege of the four "Etzion block" settlements. Five were sent back; the thirty-five who continued were ambushed and massacred.

Our training in military science – such as it was – took place on the Mount Scopus campus. When the regular bus route stopped operating, we went up by bus convoy for longer periods. These convoys continued until April 13, when one was ambushed *en route* with the loss of 78 lives, including a number of our friends. The main headquarters of Hemed in Jerusalem was set up in the Schneller Complex, originally a German Lutheran mission. I was assigned the task of making detonators, to be used in a variety of home-made weapons, including the fabled Davidka mortar. Emanuel Goldschlag – a deaf mute who loved explosions because they were the only sound that he could "hear", Yoram Avidor[51] and I set up our laboratory in the deserted Arab village of Lifta a couple of kilometers to the West. Communication between Tel-Aviv and Jerusalem by land was becoming increasingly difficult, as convoys along the main road were under constant attack.[52] With the University stockrooms beyond reach and supplies from the coast unavailable, we began scouring the pharmacies of Jerusalem for formaldehyde, acetaldehyde, phenol, hydrazine and nitric acid, and commandeering them whenever they were found. The main explosive charge in our detonator was pentaerythritol tetranitrate (PETN) which had to be desensitized by dilution with paraffin – 15%, if I remember correctly – to prevent its going off prematurely. Our task was eased somewhat when a cache of PETN bricks of uncertain origin was found in a cave and delivered to us. Since we had no idea of the amount of paraffin in them, we painstakingly leached all of it out with gasoline and then put back the required amount. The PETN was covered by a layer of lead azide, which also had to be desensitized by dilution with gelatin. To set off the azide we added a thin layer of diazodinitrophenol (DINOL) which was ignited by a spark. We made the detonators by hand in batches of 20. For quality control, we tested one detonator in each batch, to Goldschlag's manifest delight. It was a fine product! Having perfected the process, we handed over our stock to the appropriate authorities, wrote up the procedure in detail for Taas, the Ministry of Defense's budding unit for the manufacture of military supplies, and closed shop. Sad to say, as the result of negligence (not ours!) our efforts were almost entirely wasted. The fruit of our labor was stored in a house in the center of Jerusalem along with other explosives and incendiaries; a fire broke out and destroyed the lot. Moreover, as we learned later, the

[51] Yoram Avidor became the Technion's first Professor of Biochemistry. Originally a member of the Department of Chemistry, he and Ruth Ben-Shai seceded in 1971 to found the Department of Biology.

[52] The alternative "Burma Road" being paved to the south of the main road was not completed until June.

Taas engineer to whom the detonator project had been entrusted disregarded our instructions to leach out the indeterminate amount of paraffin present in the PETN bricks and replace it with the specified quantity, but used the PETN as it came. The result: an explosion with one fatality and suspension of the project.

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

The four-week truce, beginning June 11, allowed us a welcome break, including a week of gourmandizing in Tel-Aviv. More importantly, as military supplies could now be brought up from the coast, the improvisatory work of Hemed in Jerusalem lost much of its urgency and the unit in Jerusalem started to wind up its operations. Many of its members were transferred to “the Hill”, the main Hemed base in the Tel-Aviv area. Ada and I remained in Jerusalem until a second, longer truce was brokered in mid-July, when both of us were transferred to the newly-formed Air Force and moved to Tel-Aviv for reassignment. As the senior Israeli officers had served in the RAF, and many – if not most – of the air crew in the early days were American, British and South African volunteers, all Air Force business was carried out in English. There were an excess of volunteer air-gunners with combat experience for the few planes with guns, so – being bilingual and having done intelligence work in the Shai – I was sent off to the Ramat David Air Base as Intelligence-Operations Officer. After a short course as code and cipher operator, Ada too was posted to Ramat David, where we

were housed in family quarters and remained in relative comfort until the final armistice in 1949.

7. A student once more

On release from the IDF, we paid "key money" for a one-room penthouse in Talpiot. Originally the seat of Jerusalem's intellectual aristocracy (Agnon, Klausner, Ben Yehuda, etc…), Talpiot's proximity to the no-man's land southeast of Jerusalem made the rent affordable. As sending me back to the lab was not an option, my incomplete M.Sc. thesis was written up and accepted as is,[53] but I still had to prepare myself for the final oral exams while supplementing my income by making detonators (What else?!) for Taas. Despite Frankel's having grudgingly given me a minimally passing grade in his course, my average was good enough to earn me a scholarship from the British Friends of the Hebrew University for work towards the doctoral degree at any British university that would have me. My first choice was, of course, University College London, the "Mecca" of Physical Organic Chemistry and – to my delight– I was accepted.

In the fall of 1950, after a brief visit to the family in Philadelphia, where my father was now director of Jewish Education, we sailed for England. We rented a room in Swiss Cottage, a short ride by Underground from University College's old chemistry building on Gower Place, where I set to work in earnest. My scholarship was for one year, once renewable, so I had to complete my work for the Ph.D. in two years – or else. This was not common in the UCL Chemistry Department, where a student working in Physical Organic Chemistry was expected to be both a physical chemist and an organic chemist, *i.e.,* to synthesize his or her own substrates and then carry out the required physical measurements on them. Fortunately, my main substrate did not have to be synthesized; my research topic might have been defined whimsically as "The kinetics of the nitration of water".

Ingold and his collaborators had filled half an issue of the Journal of the Chemical Society with a series of papers proving the existence of nitronium ion (NO_2^+) by cryoscopy, characterizing it by UV, IR and Raman spectroscopy, and proving by

[53] M. Weizmann, S. Israelashvili**,** E.A. Halevi and F. Bergmann, "Peroxide-catalyzed addition of iodoform to olefins", *J. Amer. Chem. Soc.* **69**, 2547 (1947).

reaction kinetics that it was the active reagent in aromatic nitration.[54] The mechanism with strong acid in a *non-aqueous* solvent was shown to be as follows:

$$H^+ + HNO_3 \overset{K_{eq}}{\rightleftharpoons} H_2ONO_2^+ \overset{k_1}{\underset{k_{-1}}{\rightleftharpoons}} H_2O + NO_2^+$$

$$H_2O + NO_2^+ + ArH \xrightarrow{k_2} Ar\overset{H}{\underset{NO_2}{<}}{}^{+} + H_2O \xrightarrow{fast} ArNO_2 + H_3O^+$$

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

My assignment was to measure the kinetics of the exchange of oxygen atoms between nitric acid and water enriched in O^{18}. This would give us the rate of dissociation of $H_2ONO_2^+$ to nitronium ion and water, equal under equilibrium conditions to the rate of their recombination (nitration of water), which we could then compare with the rate of aromatic nitration. One young faculty member, D.R. Llewellyn, had been assigned the task of constructing and operating a distillation column for separating the isotopomers of H_2O, in friendly competition with Israel Dostrovsky, who was setting up a similar column at the Weizmann Institute in Rehovoth. Another, C.A. Bunton – Bunny to his friends – who had just returned to London from Rittenberg's laboratory at Columbia University and set up a mass spectrometer, was my immediate supervisor. Ingold's towering intellect dominated the department. His wife, Hilda, the departmental secretary, protected her husband's privacy and kept his office off limits. Nevertheless, he and his close associate E.D. Hughes kept a keen eye on everything

[54] C.K. Ingold *et mult. al., J. Chem. Soc.* **1950**, 2400-2696.

that went on in the Department. Ingold would come into the lab unannounced, ask how things are going and perhaps make a suggestion, always cogent and often valuable, as to how to proceed. The younger faculty members were more accessible; in addition to Bunton, they included D.P. Craig, Alan Maccoll, D.J. Millen, R.J. Gillespie, Charles Vernon and Peter De la Mare. My contemporaries, including several, *e.g.,* John Ridd, Gabriello Illuminati, Jim Gowan and Gerry King, who were to have distinguished academic careers, exchanged ideas freely with me on topics ranging from our individual research projects to politics and religion.

Well within the allotted two-year period, my problem was solved: The rate of oxygen-exchange was measured as a function of acid concentration and found, in any given set of conditions, to be more rapid than the nitration of the most reactive substrate than had been used to date. I studied the nitration kinetics of several more reactive aromatic compounds, having actually worked out a synthetic method for their preparation,[55] and confirmed the hypothesis that formation of NO_2^+ is indeed rate limiting for nitration in aqueous acid, as it is in non-aqueous media.[56] All in all, the UCL Chemistry Department was an exciting and stimulating place. I would have loved to stay on and continue with the study that I had begun on the catalysis of nitration by nitrous acid.[57] Moreover, London had gotten over the blitz and the subsequent period of austerity, and its cultural riches – theater, music and the visual arts – were there to be enjoyed. During our two-year stay, we formed friendships, both chemical and non-chemical, several of which were to last a lifetime. However, much as we would have liked to prolong our stay, our time in London had run out. I wrote up my thesis, Ada typed it and I submitted it. After the oral exam and visit to the nearby pub for the customary farewell beer with my lab-mates, my student days were over.

8. Back to Israel

Before boarding ship in Marseilles, we bought a carton of eggs. In 1952 food was still rationed in Israel and we knew that our friends in Jerusalem, especially those with

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[55] C.A. Bunton and E.A. Halevi, "The preparation of alkanesulphonic acids", *J. Chem. Soc. 1***952,** 4541.

[56] C.A. Bunton, E.A. Halevi and D.R. Llewellyn, "Oxygen exchange between nitric acid and water, Part I." *J. Chem. Soc.* **1952,** 4912-4916; C.A. Bunton and E.A. Halevi, "Oxygen exchange between nitric acid and water, Part II. A correlation of oxygen exchange with aromatic nitration", *J. Chem. Soc.* **1952,** 4917-4924.

[57] C.A. Bunton**,** E.A. Halevi and D.R. Llewellyn, "Oxygen exchange between nitric acid and water, Part III: Catalysis by nitrous acid", J. *Chem. Soc.* **1953,** 2653-2657.

young children, would welcome a gift of a dozen "almost fresh" eggs. We returned to our penthouse in Talpiot, Ada got a clerical job in the State Comptroller's office and I began fulfilling my three-year commitment to the Hebrew University. I was appointed Assistant[58] in the Department of Physical Chemistry, then situated on Mamilla Road, which was blocked off from the Jaffa Gate by a high barrier designed to prevent sniping from the Old City wall.

There was no Head of Department when I arrived. Professor Farkas had died in a plane crash several years earlier and his senior co-worker, Yehudah Sandler emigrated to the United States when he was not offered the position. The lectures in Physical Chemistry were delivered by Aharon Katzir – then still Katchalsky, who – having agreed to head the Department on a trial basis – came up from the Weizmann Institute twice a week. Aharon was a spellbinding lecturer; students would leave his lectures under the often illusory impression that they had grasped everything he said perfectly. I was assigned the duty of conducting the exercise sessions. My job, in addition to distributing numerical problems and checking their solutions, was to review the concepts raised in Katchalsky's lectures and ensure that they were properly understood. The students were an exceptionally talented group, many of whom eventually became prominent figures on the Israeli – and international – scientific scene;[59] the task of teaching them was both demanding and rewarding.

In order to continue experimental research with stable isotopes, I needed a mass spectrometer, so I undertook to construct one with the assistance of Uriel Vogel, a capable technician. Meanwhile, I was constrained to "paper chemistry". At UCL I had measured the rate of isotope exchange between H_2O, a molecule with one O-atom and HNO_3, which has three. I noticed that no-one had derived a general formula for the kinetics of isotope exchange between molecules with different numbers of exchangeable atoms, so I took it on myself to rectify the omission. It required more knowledge of statistical mathematics than either Bunton or I possessed, so we called on David Craig for assistance. Having put the problem aside while writing my thesis, I was now free to complete it in correspondence with my collaborators in London.[60]

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[58] This rank, one below Lecturer, no longer exists in Israeli Universities.

[59] I will not mention their names in order not to offend anyone by inadvertent neglect.

[60] C.A. Bunton, D.P. Craig and E.A. Halevi, "The kinetics of isotopic exchange reactions", *Trans. Faraday Soc.* **51,** 196-204 (1955).

My experimental program was a non-starter. Soon after my arrival, Aharon Katchalsky resigned and was replaced by Gabriel (Gabor) Stein as Head of Department. Personally quite congenial, Stein's firm opinion was that the line of research carried out in the Department should be determined by its Head. Stein's interest was in homogeneous reactions, in particular free radicals and trapped electrons, whereas I – a product of the Ingold school – was interested in heterogeneous reactions, *i.e.,* of ions and polar molecules. As a lowly Assistant, I had to comply. The only result of our collaboration was a study of the bromination of naphthalene that was published several years later.[61]

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

During the academic year of 1954-55 my physical presence was in Jerusalem but my heart was in Haifa. One event that I remember fondly was Linus Pauling's visit to Jerusalem *en route* to the Nobel Prize ceremony in Stockholm. He gave a fascinating lecture at the University on the helical structure of proteins; he took a long, heavy key-chain out of his pocket, twirled it, and there was the helix. David, who had invited Pauling to lecture at the Technion, asked me to accompany him. Accordingly, Pauling, his wife Ava, Ada and I boarded a limousine and traveled North with a motorcycle police escort, as befits a Nobel laureate. When we were under way, Pauling asked the driver to make a stop at Pardes Hana, where he and his wife had friends. These turned out to be the Duvedvanis, who had become friendly with the Paulings during the former's sabbatical in Pasadena. I thus got to meet my old biology teacher for the first time since my school days. This trip was of scientific value to me

[61] E.A. Halevi. I. Loeff and G. Stein, "The mechanism of the vapor-phase bromination of naphthalene", J. *Chem. Soc.* **1957,** 5088-5091.

as well. I had already become interested in secondary isotope effects and was wondering whether they could be related to isotopic polarity differences, such as the reportedly larger dipole moment of DCl than HCl[62] than and that of ND_3 relative to NH_3,[63] which were presumably due to slight differences in mean geometry due to vibrational anharmonicity. I mentioned this to Pauling, who suggested that I look at infrared intensities. Following up on his suggestion, I computed both dipole moment differences; my results were in fortuitously good agreement with the reported experimental data.[64]

9. Technion – Part One

After our move to Haifa in the autumn of 1955 we stayed put for over half a century. It can be divided into two unequal periods, differing both in our life style and in the nature of my scientific activities. In the first fifteen years or so, during which I was climbing the academic ladder at the Technion, we lived in four rented apartments in different parts of town, not to mention our temporary quarters abroad during two sabbatical leaves and several shorter visits abroad.

Our first home was in Shikun HaTechnion on Horev Road. Ada found a job in the Zim Maritime Company downtown and I ensconced myself in the laboratory on the Hadar Hacarmel campus that David Ginsburg had set aside for me. My lab was spacious and relatively well equipped. Without having been asked, David obtained a used, home-built, mass spectrometer for me from the Weizmann Institute, which I hardly ever used, as my principal interest had shifted from the isotopes of "heavy" atoms like oxygen to those of hydrogen, for which more convenient analytical methods were available. In the first few years he steered the brightest students toward me for their fourth year undergraduate research project, which could then be continued toward the M.Sc. degree.[65]

Our experimental work centered on hydrogen isotope effects, specifically *secondary isotope effects*. Up to a couple of years earlier, it was thought that the rate of a reaction can be modified by isotopic substitution, say D for H, only if a bond to the

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[62] R.P. Bell and I Coop, *Trans. Faraday Soc.*, **34**, 1209-14 (1938).

[63] J.M.A. De Bruyne and C.P. Smyth, *J. Amer. Chem. Soc.* **57,** 1203 (1935).

[64] E.A. Halevi, "Polarity differences between deuterated and normal molecules", *Trans. Faraday Soc.***54,** 1441-1446 (1957).

[65] Two of them, Arza Ron and Shammai Speiser eventually became Professors in our Faculty.

relevant hydrogen atom was broken or formed in the reaction. It was then shown independently by E.S. Lewis and V.J. Shiner,[66] that *beta*-deuterated alkyl groups reduced the rate of solvolysis of alkyl halides. This secondary isotope effect was ascribed to less effective hyperconjugation of CD- relative to CH bonds. Recalling the isotopic dipole moment differences referred to above, I wondered whether there could be a more general effect, differential electron release from CH and CD bonds, due to the fact that – as a result of anharmonicity – the mean length of a CD bond is shorter than that of a CH bond. Using a differential potentiometric method of my own devising, we measured the effect of deuteration in the a-alkyl group on the strength of carboxylic acids. We observed that the deuterated acids were slightly weaker than the normal acids. As hyperconjugation cannot be invoked here, this result was ascribed to more effective electron release from CD- than from CH bonds due to their shorter average length.

Our preliminary findings were published[67] and presented in the summer of 1957 at the IUPAC General Assembly in Paris, at which the Israeli contingent included David and Hemda Ginsburg, Eli and Rachel Loewenthal, as well as Ada and me. In the course of the next decade we investigated what became known as the "inductive effect of deuterium" from several angles. It was shown to increase the basicity of nitrogen bases as well as to decrease the acidity of carboxylic acids,[68] and to oppose the hyperconjugative effect in charge transfer complexes, where both were operative.[69] Having made the claim that secondary isotope effects can be used as criteria of mechanism,[70] we went on to prove, on the basis of a combination of primary, secondary and solvent isotope effects, that in nitration of substituted anilines

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[66] E.S. Lewis and C.B. Boozer, , *J. Amer.* Chem. Soc. 74, 6306 (1952); V.J. Shiner, J. Amer. Chem. Soc. 75, 2925 (1953).

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

[68] E.A. Halevi, M. Nussim and A. Ron, "Secondary hydrogen isotope effects, Part I: Strengths of α-deuterated carboxylic acids and amines", J. Chem. Soc. 1963, 866-875.

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

70 E.A. Halevi, "Secondary isotope effects as a criterion of mechanism", Tetrahedron 1, 174-175 (1957).

at the nitrogen atom, the rate limiting step can be shifted from attack by NO_2^+ to the subsequent proton abstraction.[71]

In order to repeat the old work on isotopic dipole moment differences, I recruited an electrical engineer, Naftali Haran, and persuaded the Faculty of Electrical Engineering to grant him credit towards the Ph.D. degree for the design of an instrument for the direct measurement of differences in electric polarization as a function of temperature.[72] The dipole moment and polarizablity differences that we obtained for DCl vs. HCl and ND_3 vs. NH_3 with our differential modification of the venerable Debye procedure were consistent with published infrared and Raman intensities, as well as with the results of new spectroscopic methods, that had begun to appear.[73]

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

An unfortunate consequence of trying to carry out experimental research along several lines at once with a very small group of associates is that it is not always possible to follow each line to its proper conclusion. It may happen that a student has completed his or her work for the M.Sc. or Ph.D. thesis, but the results are not publishable without further refinement. In a large research group, another student completes the project, which is then published with both as co-authors. In a small

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[71] E.A. Halevi, A. Ron and S. Speiser, "Secondary hydrogen isotope effects, Part III: The mechanism of N-nitration", J. Chem. Soc. **1965**, 2560-2569.

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

[73] E.A. Halevi, "Polarization and polarizability differences between isotopic molecules", *Internat. J. Quantum Chem.* (*Symp. No. 5*), 367-370 (1971); C. Scher, B. Ravid and E.A. Halevi**,** "Deuterium isotope effects on the dipole moment and polarizability of HCl and NH_3", *J. Phys. Chem.* **86,** 654-658 (1982).

group working on numerous projects it may happen that the work is not completed and the results are never published. To my regret, this situation – which is clearly unfair to the students involved – occurred several times in my group, as in the case of Chava Fink's investigation of proton exchange in aromatic systems, the endeavor of Hana Loewenschuss (now Frauenknecht) to establish an acidity function for concomitant protonation and hydration, and Zvi Ganot's determination of the dipole moment and polarizability differences between CD_3F and CH_3F. Not as serious but still depressing is a finding that is sufficiently novel to be worth publishing as a preliminary communication but cannot be followed up and published in full, as in the case of Zafra Margolin's observation of a temperature-dependent secondary isotope effect.[74] The frustrating inability to maintain a sufficiently large research group was a factor in my eventual decision to abandon my experimental program.

In parallel with the experimental work, I undertook – with the guidance and collaboration of Ruben Pauncz and the participation of Arza Ron – a computational study of the effect of methyl deuteration on the properties of conjugated molecules.[75] Differences in the mean geometry due to anharmonicity of the symmetrical bending and stretching modes affected the mean electron distribution in ways that could be interpreted as genuine – though very weak – inductive and hyperconjugative effects operating in tandem or in opposition.

The physical organic community found it perfectly reasonable to consider substituent groups like CD_3- and CH_3- to have slightly different electronic properties, in much the same way as ethyl differs from methyl and chloro from bromo. To "genuine" physical chemists, however, the idea was anathema because, to them, the term "electronic" was limited to properties – such as the electronic energy – which are identical in isotopomers, whereas isotope effects arise from differences in the vibrational spectrum. The apparently contradictory points of view were reconciled in the summer of 1959 by Max Wolfsberg, Ruben Pauncz and me in Lidingö, an island in the Stockholm archipelago. It could be shown using first order perturbation theory

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

[75] R. Pauncz and E.A. Halevi, "The effect of deuteration on electron distribution and energy of conjugated molecules, Part I: LCAO-AO treatment of toluene", *J. Chem. Soc.* **1959,** 1967-1974; E.A.—Halevi and R. Pauncz, Part II LCAO-AO treatment of toluene CD_3", *J. Chem. Soc.* **1959***,* 1974-1980; A. Ron, E.A. Halevi and R. Pauncz, Part Ill: LCAO-AO treatment of ethylcarbonium ion and its methyl-deuterated analog", J. *Chem. Soc.* **1960,** 630-636.

that the average potential energy of interaction between a charge and an anharmonically oscillating dipole is isotope-dependent. I eventually summarized this analysis, which we had unexplainably failed to publish, on pages 133–142 of my review of secondary isotope effects.[76] Ruben and I, accompanied by our respective wives and children, had come to Lidingö to attend Per Olov Löwdin's second Summer School on Quantum Chemistry. Ruben, who attended the first Summer School the previous year, impressed Löwdin so much that he was invited back as a teacher. I, of course, came as a student. I was exposed to group theory for the first time in a beautiful course by Laurens Jansen and learned quite a bit of quantum chemistry but not as much as I would have liked, Our two-year-old son, Gadi, had come down with dysentery, which he may well have contracted in Israel, so – for an hour or two of every day over a couple of weeks – I had to relieve Ada from her vigil at his bedside in the Carolinska Hospital in Stockholm.

From Stockholm we proceeded to Cornell University for my first sabbatical, the only one in which I spent a full academic year in one institution. My host Franklin A. Long sent me back to the laboratory bench for the last time in my career. Long was interested in solvent isotope effects*, i.e.,* determination of the mechanism of a reaction – in my specific case the deprotonation of phenylacetylene – by comparing the reaction rate in D_2O with that in H_2O.[77] He was also interested in a more general theoretical problem: Can any mechanistic information be derived from a series of experiments in H_2O/ D_2O mixtures of varying isotopic composition? The idea, which had been broached independently in the 1930's by Paul Gross and A.V. Butler, had recently been revived. The Gross-Butler equation, contains a cubic term in the denominator, derived from the presence of three protons in the hydronium ion (H_3O^+), is purely statistical, and ignores the fact that the hydronium ion is hydrated and that H_2O and D_2O have significantly different solvent properties. We showed that, if these factors are taken into account, the experimental dependence on isotopic composition can be reproduced by assuming any arbitrary number of H-atoms in the hydrated hydronium ion on the one hand and in the water clusters on the other; we concluded that no useful mechanistic information can be derived from experiments in H_2O/ D_2O

[76] E.A. Halevi, "Secondary Isotope Effects", in S.G. Cohen, A. Streitwieser, Jr. and R.W. Taft, *Progress in Physical Organic Chemistry,* Interscience, 1963, pp. 109-222.

[77] . E.A. Halevi and F.A. Long, "The base-catalyzed exchange of phenylacetylene-t_l in aqueous solution", *J. Amer. Chem. Soc.* **83,** 2809-2814 (1961).

mixtures.[78] Our paper was much ignored, prompting me to review the topic in greater detail a decade later.[79] To no avail; an inversion of the Gross-Butler treatment called *proton inventory* was adopted enthusiastically by physical biochemists and is still being used to estimate the number of protons present in the transition state of enzyme reactions.

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

In 1964 I was promoted to the ultimate academic rank, characterizes by Agnon as *professor gamur* (Finished Professor). My critical review of all of the work done to date on secondary isotope effects[53] was well received. I felt that I could now allow myself some time to pursue my non-chemical interests. I took up the violin again and, with friends, was given lessons in string-quartet playing by the violinist, violist and peace activist, Joseph Abileah,[80] who also introduced me to the viola. I soon felt confident enough to register on the international network of amateur chamber music players as "2nd Violin: Grade B; Viola: Grade C", and was thus able to find a group of amateurs of about my level of competence nearly anywhere I might find myself. Ada, who had given up her job at Zim after she could not obtain adequate care for our two year old daughter Dalia either at home or in a day nursery, renewed her childhood hobby of drawing.

It is a truism that good personal relations among scientists make for good scientific collaboration. Soon after his accession as Head of the Chemistry Department, David Ginsburg invited three young faculty members of the Organic Chemistry Department in ETH in Zurich, Jack Dunitz, Albert Eschenmoser and Edgar Heilbronner to visit the Technion. I persuaded him to add a fourth, Heinrich Zollinger, from the ETH's Department of Industrial Chemistry, who had been using isotope effects in his

[78] E.A. Halevi, F.A. Long and M. Paul, "Acid-base equilibria in solvent mixtures of deuterium oxide and water", *J. Amer. Chem. Soc.* **83,** 305-311 (1961).

[79] E.A. Halevi, "Solvent isotope effects: Second thoughts about an old problem", *Israel J. Chem.* **9**, 385-395 (1971).

[80] Father of our own Professor Efrat Lifshitz.

research on of the mechanism of diazo coupling. The four, later joined by a fifth – Duilio Arigoni, maintained lasting contact with our Department; all of them became my personal friends. My family and I spent the summer of 1964 in Zurich, where I helped Zollinger clear up a particularly thorny mechanistic problem.[81] Ada and I were once invited to dinner at the home of Jack and Barbara Dunitz; the other dinner guests were John C. (Jack) and Ann Decius, with whom we immediately found a common language. I had already become interested in molecular symmetry, whereas Jack, Professor of Chemistry at Oregon State College, was co-author of the definitive book on molecular vibrations, in which molecular symmetry is a major factor. At this encounter I decided that I would like to take my next sabbatical in Oregon.

On our way to Oregon, we spent the summer of 1966 at the Brookhaven National Laboratory on Long Island, in New York State, where Max Wolfsberg and I carried out isotope effect calculations on solvolysis, which were eventually correlated with those performed in Jack Shiner's group.[82] Contact with my research group was maintained by mail, though I recall driving 150 miles from Brookhaven to Bayonne, New Jersey and back, in order to ask Mordecai Rubin – who was about to return to Haifa – to help one of my students overcome a difficulty that she had run into. We spent the first two "quarters" of the 1966-7 academic year at Oregon State University (OSU) in Corvallis with Jack Decius and the third at the University of Oregon (UO) in Eugene with Richard M. Noyes. It was a very pleasant year, during which my scientific activities were confined to teaching, learning, and maintaining contact with my students by mail. One non-scientific highlight was a 90 mile drive to Portland, to attend a Stravinsky concert in which the venerable composer conducted one of his works. At Corvallis I was flattered – but not tempted – by the offer of a professorial appointment under conditions substantially better than those I had at the Technion. It took me a few minutes longer to consider a split appointment, similar to positions that several Israeli professors of chemistry had accepted at American universities, but was daunted by the prospect of being away from the family for several months of each year. It was a good year for Ada as well. In OSU, she attended art classes and painted under the instruction of Nelson Sandgren, a professor in the Department of Fine Arts

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[81] M. Christen, L. Funderburk, E.A. Halevi, G.E. Lewis and H. Zollinger, "Mechanism of the Suckfiill-Dittmer-synthesis of two azo compounds", *Helvet. Chim. Acta,* **49,** 1376-1349 (1966).

[82] V.J. Shiner, M.W. Rapp, E.A. Halevi and M. Wolfsberg, "Solvolytic α-deuterium isotope effects for different leaving groups", *J. Amer. Chem. Soc.* **90,** 7171-7172 (1968).

and a well known West Coast artist. On our move from Corvallis to Eugene, she continued her studies with Sandgren's own mentor, Andrew Vincent, who was approaching retirement as Professor of Art at UO.

At about 3 o'clock in the morning of June 6, 1967, someone knocked at the door of our home in Eugene. It was our neighbor, a U.S. Marine officer whom we knew as the father of one of Dalia's playmates. He apologized for disturbing us so late at night, but said, "I was sure you would want to know that there is a war on in your country". The local TV channels were all dead at this hour, but we managed to tune into an all-night news station in Los Angeles with our transistor radio. The announcer said that a correspondent in Tel Aviv had asked a senior Israeli officer how things were going and received the response, "Fantastically", but he didn't believe it. I did; in those distant days the news reports released by the IDF spokesman were generally reliable. A few hours later I phoned a friend in Boston who, in view of the time difference between the east and west coasts, had already ascertained that *yes*, the Egyptian air force had been destroyed on the ground and *no*, there is no transportation to Israel for anyone but military and medical personnel. Nevertheless, I informed my hosts that we would be leaving a few days early, packed our belongings on the roof of our ancient third- or fourth-hand Mercury, and started to drive east the following day. Before we reached Minneapolis it was clear that the war would be over within days and there was no need to hurry home, so we continued to the Argonne National Laboratory in Illinois, where I had a summer appointment. We arrived at Argonne safely after nearly losing a wheel of our car – and our lives – on the freeway somewhere in Wisconsin. On our arrival I found a letter from Heini Zollinger. Convinced that Israel was doomed, he was offering me a temporary position in his group with a salary sufficient to support my family until I could find permanent employment in Switzerland. I phoned to thank him for the generous offer that, as he already knew, was no longer relevant.

My host at Argonne was J.J. (Joe) Katz, an eminent inorganic chemist who had switched to biochemistry. He had devised an elegant technique for following enzymatic reactions, the *isotopic mirror* technique. Two experiments are carried out in parallel, one in D_2O with the normal substrate, the other in H_2O with the perdeuterated substrate, i.e., in which all of the hydrogen atoms have been replaced by deuterium. In the absence of isotope effects, the distribution of deuterium atoms in the product molecule in the first experiment will be the same as the distribution of

protium atoms in the product of the second experiment; it this case it can be concluded that no bonds to hydrogen atoms had been made or broken in the rate determining step of the reaction. If the isotopic distribution in the product molecule is different in the two molecules, the difference may pinpoint the step/steps along the reaction path that is/are kinetically significant. In the course of the previous two years, Joe Katz, with his biochemist colleague Henry Crespi and Swiss post-doc Wolfgang Sauer, had been applying this method to an investigation of the fermentation of hexoses – specifically glucose and mannose – to ethanol, and had amassed a great deal of data on the distribution of hydrogen isotopes in the methyl and methylene groups of the product ethanol . We carried out the analysis of the data in two steps: 1. a steady state treatment based on the Emden-Meyerhoff glycolysis pathway, taking into account possible primary, secondary and solvent isotope effects on its various steps; 2. refinement of the results by computer, the latter being carried out largely by Dan Peterson, a bright teen-ager who was spending his summer at Argonne under an undergraduate research scholarship.[83]

This investigation turned out to be my last incursion into the life sciences, but it might not have been. The following year, in the course of a brief visit to Zurich, I paid my respects to Professor Vlado Prelog and found him in the company of a distinguished visitor, David Rittenberg. I sat listening respectfully as the two *Grand Old Men* complained half seriously to one another that neither of them had received – or would probably ever receive – the Nobel Prize.[84] Rittenberg then turned to me, told me that he was impressed by our fermentation papers, and asked me whether I would care to spend a few months in his group at the Columbia University's College of Physicians and Surgeons. Needless to say, I agreed enthusiastically and was disappointed when I never heard from him again. Several years later I met Rittenberg's erstwhile collaborator, Laura Ponticorvo, who assured me that he was preparing to send me a formal invitation when his health deteriorated and he died several months later.

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

[84] Rittenberg certainly deserved the prize for his pioneering work with Schönheimer on deuterium as a tracer of metabolites, and would no-doubt have received it had not Schönheimer committed suicide in 1941. Prelog was awarded the Nobel Prize in 1975.

10. Technion – Part Two

Before returning to my professional activities, let me depart from my chronological narrative and sum up the personal aspects of the next 37 years very briefly. At just about this time, the Gerard Swope Foundation set aside funds for low cost loans to members of the Technion faculty for home purchase. We took advantage of the opportunity and bought an apartment in a duplex in Danya, then still a comparatively inexpensive suburb, which was our home for the next 37 years. I continued playing chamber music whenever I could find partners and acquired a new hobby, Talmud. For over twenty years I had a weekly session with Professor Zvi Kurzweil, then Head of the Department of General Studies, until his death in 1992. Subsequently, I collaborated with Eliot Shimoff, Professor of Psychology at the University of Maryland (Baltimore Campus) in running a Talmud Study List in English on the Internet from 1998 until Shimoff's death in 2004.[85] Ada continued with her painting, had two solo exhibitions and several group shows. The children grew up, did their military service and went their own ways.

On my return from Argonne, I turned my attention to reinforcing my dwindling research group, but academic politics intervened. There was tension between the old, well established Engineering Faculties and the newer "upstart" Faculties of Science. In particular, the acquisition by the student-poor Faculty of Chemistry[86] of a large modern building, dubbed David's Palace, had aroused considerable resentment. More importantly, the policy of the Graduate School was heavily weighted in favor of applied research. As the Dean of the Graduate School was also the Deputy Vice President for Research, whose prerogative it was to dispense internal research funds, this had serious financial implications for us. I was asked by my colleagues to stand for election in the Senate against the incumbent Dean, and was elected. The President of the Technion promptly cut the Gordian Knot between the *elected* Dean of the Graduate School and the *appointed* Deputy Vice President for Research, leaving the latter function in the hands of the retiring Dean. Nevertheless, as of January 1969, I had three calendar years in which to adapt the academic policies of the Technion's Graduate School to the requirements of a modern research university. By the end of

[85] http://userpages.umbc.edu/~shimoff/tlmd_grp.htm

[86] We had attained Faculty status, but preferred to call ourselves a Department, headed by a Chairman rather than a Dean.

the two following three-year terms, which were held respectively by the physicist Asher Peres and Chemistry's Frank Herbstein, the Engineering Faculties had accepted the new standards and the ill-will between the Engineering and Science Faculties had largely dissipated. As for me, once having been drawn into the Technion's administration, there was hardly a year in which I was not co-opted into one committee or another.

As temporary relief from my administrative duties, I agreed to accept the invitation of H.C. Brown[87] to Purdue University for the summer session of 1969. I earned my not insignificant salary by teaching undergraduate physical chemistry, but what Herb Brown required of me was a series of seminars on secondary isotope effects for his research group; I was happy to oblige, as it meant that I was being regarded as an authority on the subject. I began to feel, however, that – in view of my administrative duties in the Technion, the minuscule size of my research group, and the fact that work on secondary isotope effects was now being carried out in an increasing number of laboratories – my own contribution to the field was reaching the point of diminishing returns.

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

In 1972, IUPAC finally acceded to the repeated requests of the physical organic community to sponsor a series of biennial conferences on the subject. The first International Conference on Physical Organic Chemistry (ICPOC 1) took place in

[87] Nobel Prize in Chemistry, 1979.

Crans sur Sierre, Switzerland in September, 1972. Zollinger and Heilbronner were co-chairmen and I was a member of the international organizing committee. My most vivid memory of this conference has nothing to do with science. I recall sitting for hours with Gunther Wilke in our hotel's television room on the night of September 5, hoping against hope that the Israeli athletes kidnapped by Palestinian terrorists at the Munich Olympic Games, would be rescued; they weren't.

Another unpleasant circumstance, less harrowing but annoying nonetheless, arose at ICPOC 1. All of the four scheduled speakers from the Soviet Union, including a plenarist, were prevented from coming, and there two non-scientists present who did not try to hide the fact that they were there to monitor the comings and goings of the attendees from the Soviet bloc. After the conference I wrote to E.L. Mackor, chairman of the organizing committee of ICPOC 2, scheduled to take place in Noordwijkerhout, Holland in the spring of 1974, expressing my displeasure at how the Soviet contingent was selected and suggesting that no lecturers should be invited from the Soviet Union unless the Soviet scientific establishment ensured that they would be allowed to attend. My letter raised a storm of protest from other Committee members: Paul Bartlett accused me of proposing a boycott of our Russian colleagues and injecting politics into science; Petr Zuman, a Czech who had immigrated to the United States, understood my position but argued that any action taken by IUPAC would do more harm than good. I was castigated by Ernst Bergmann and David Lavie, President of the Israel Chemical Society – who had been sent copies of Bartlett's letter but not mine – for provoking a counter-boycott of Israeli scientists. The storm subsided after I sent an explanatory letter to Prof L. Bénard, President of IUPAC, who responded: that in view of my explanation, "... je considère, par consèquent cette affaire come définitivement reglée". Two years later IUPAC approved the formation of a Commission on Physical Organic Chemistry, but my name was conspicuously absent from its list of members.[88]

The publication of Wodward and Hoffmann's "The Conservation of Orbital Symmetry", as a review article in Angewandte Chemie in 1969 and in book form the following year, was greeted with great enthusiasm by the international organic chemistry community. They had made it possible to consider the reaction pathways of cycloadditions and cyclizations, which had hitherto been regarded as "no mechanism"

[88] Zollinger wrote that my name had been proposed but that I was voted down on the spot as a troublemaker. Nevertheless, I was eventually elected to the Commission in 1981 and served on it for ten years.

reactions and their predictions were beginning to be tested experimentally in several laboratories. At a conference in England early in 1972, I was approached by a German organic chemist, Ernst Koerner von Gustorf, from the Max Planck Institute für Strahlenchemie in Mülheim-Ruhr. He asked me to settle a dispute between him and an American colleague as to the interpretation of the conflicting isotope effects that each of them had observed in their respective studies of [2+2]-cycloaddition, a reaction for which Wodward and Hoffmann had predicted a concerted antarafacial pathway. The isotope effects indicated a stepwise pathway; the discrepancies between the effects observed in the different laboratories were presumably due to the timing of its various steps. I proposed a series of experiments that would settle the matter and, after a protracted correspondence, Koerner undertook to carry them out. I paid a two-day visit to Mülheim on September 15, 1975 en route to my third sabbatical in the United States. The results obtained so far were most encouraging but a few more experiments were required to make them conclusive. Tragically, on September 18, Koerner died of a heart attack at the age of 43 and our joint investigation was never completed. My ill-fated collaboration with Koerner was significant in two different ways: 1. It was the first of my many visits to Germany and initiated an extended series of scientific and personal contacts with German chemists, several of whom became my close friends.[89] 2. It convinced me that the Woodward-Hoffmann Rules, which were almost universally regarded as infallible, should be addressed more critically.

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

[89] I cannot refrain from mentioning two of them: Rolf Huisgen, who took Ada and me on numerous guided tours of Munich's art museums, and Gerhard Quinkert, who insisted that I give a departmental colloquium on "The Talmud and its logic" at Frankfurt.

be possible to go a step farther than merely characterizing a reaction as *allowed* or *forbidden*. A reaction that is *forbidden* because an occupied orbital of the reactant does not correlate with an occupied orbital of the reactant may become *allowed* along a reaction path of lower symmetry, in which all of the occupied orbitals of the reactant and product are in correlation.

I formulated a procedure for determining the symmetry of the reaction coordinate that would direct the system along the appropriately desymmetrized pathway, named it *Orbital Correspondence Analysis in Maximum Symmetry (OCAMS),*[90] wrote it up in detail and naïvely submitted it to *Angewandete Chemie*, but not before sending copies off to Woodward and Hoffman for comment. Woodward did not answer, Hoffman replied that we should discuss the matter at ICPOC 2, and *Angewandete* replied that they cannot publish it as a primary article, but would be happy to publish a review of the subject after the primary paper had been published elsewhere. Thereupon, I trisected the article and submitted three *Communications to the Editor* of *JACS*, who rejected them forthwith. One referee (Woodward?) wrote briefly that there was nothing new in any of them; the other (Hoffman?) analyzed all three at length, arguing that – while the idea behind it was interesting – the method was flawed by fundamental errors of theory. At Noordwijkerhout, Hoffmann avoided me until the last day of the conference, when he approached and said the he believes we had something to discuss. When I reminded him of the draft that I had sent him, he replied that many approaches to the topic have been suggested and that the viable ones will no-doubt be sorted out in the literature – or words to that effect. By then, however, I had had a long discussion with Edgar Heilbronner, in which I convinced him that the "errors" raised by the second referee were more apparent than real. Though he agreed that *OCAMS* is theoretically sound, Edgar doubted whether it would ever be accepted by an American journal, and suggested that I should join the Swiss Chemical Society and submit it to *Helvetica Chimica Acta*. I took his advice and published three papers in rapid succession: the primary paper in *Helvetica*, a formal presentation of the method and its conceptual basis – for which collaboration with Jacob Katriel was indispensable, and the discursive review that *Angewandete Chemie* had promised to

[90] He reader will appreciate my modesty. I refrained from stressing the razor sharpness of my method by naming it *Orbital Correspondence and* ***Coordinate*** *Analysis in Maximum Symmetry (OC****C****AMS)*!

publish.[91] Let me add parenthetically that though I often served as a referee for *JACS* over the years, I never again submitted a paper without an American co-author to that journal.

Of the molecules that I had been using as examples in the application of *OCAMS,* the one that intrigued me most was the simplest, carbene (CH_2), the two non-bonding electrons of which can be either unpaired in the triplet ground state or paired in the lowest excited singlet state. I had just participated, with the Chemistry Department's genuine theoreticians, in a computational study of the process by which triplet carbene was produced directly by thermal decomposition of diazomethane (CH_2N_2), which has a singlet ground state.[92] In substituted carbenes, notably diphenylcarbene, the singlet excited state and triplet ground state were much closer, and I wondered whether suitable substitution in the phenyl rings and/or bridging between them could reverse the sequence. In a final attempt to revive my moribund experimental program, John Metcalfe, a post-doctoral fellow with experience in photochemistry, and I tried to build a stop-flow pulsed laser system to study the formation and properties of diphenylcarbene and its derivatives. Unfortunately, I had bitten off more than the two of us could chew. We had many problems with the electronic equipment and, when the instrument was ready after months of work, it failed to perform reliably. We finally gave up and settled for a computational study, in which we identified the geometric changes required to bring the singlet below the triplet; our prediction was confirmed by Staab several years later.[93]

Collaboration with Carl Trindle, which began while he was spending several months at the Technion, soon led to the extension of *OCAMS* to spin-forbidden processes.[94]

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[91] E.A. Halevi, "Orbital correspondence analysis in maximum symmetry", *Helvet. Chim. Acta* **58,** 2136-2151 (1975); J. Katriel and E.A. Halevi, "Orbital correspondence analysis in maximum symmetry: formulation and conceptual framework", *Theoret. Chim. Acta* **40**, 1-17 (1975); E.A. Halevi, "Orbital correspondence analysis in maximum symmetry", *Angew. Chem. Internat. Edn. (Enqlish)* **15,** 593-607 (1976); German version: *Angew. Chem.* **88,** 664-679 (1976).

[92] E.A. Halevi, R. Pauncz, 1. Schek and H. Weinstein, "Multiplicity change during thermal dissociation", *Jerusalem Symposia on Quantum Chemistry and Biology,* 167-182 (1974).

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

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

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

From here on and until my retirement I accepted no more graduate students. All of my subsequent research was theoretical: either qualitative or computational, and – with the exception of one undergraduate summer student[95] – all of my collaborators were senior academics. There was thus nothing to limit my travel aside from my teaching duties. My membership in the IUPAC Commission on Physical Organic Chemistry[65] took me to a different venue every two years. I attended numerous conferences in the United States, Europe and Japan. Except for one full semester at the University of California at Irvine in 1975-6 [96], I split my remaining sabbaticals into one and two-month segments: the first (1980-1) in Germany (U. of Munich; U. of Frankfurt; U. of Heidelberg; M.P.I., Mülheim) and the second (1986-7) spread more widely (U. of Graz; U. of Paris (Orsay); Clarkson U., Potsdam, N.Y; Cal. Tech.).

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

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

[95]. E.A. Halevi and R. Rom, "The stereochemistry of the Cope rearrangement: Qualitative theory (*OCAMS*) and computation (AM1)", *Israel J. Chem.* **29** (2-3), 311-20, (1989).

[96] A highlight was the first – perhaps only – performance of classical music at the Chemistry Department's Christmas party: Marlene (wife of physical chemist Ed) Lee – piano, organic chemist Hal Moore – clarinet, and I – viola played Mozart's Kegelstadt Trio.

chemistry,[97] was followed up by several studies in collaboration with theoretical chemists using a variety of computational methods.[98] The qualitative features of *OCAMS* were extended in collaboration with the theoretical chemists at the Max Planck Institute in Mülheim.[99] Brief incursions were made into both transition metal and main group inorganic chemistry.[100] Lunch with John Pople produced a mnemonic algorithm for determining the number of independent coordinates for any given molecule[101] and my participation in an IUPAC committee charged with the production of a Glossary of Stereochemical Terminology prompted me to suggest a modification of Kurt Mislow's hierarchy of levels of prochirality.[102]

In 1984 I was called upon to become Chairman/Dean of the Department/Faculty of Chemistry, a job that I had successfully avoided until then. I agreed to accept it for a single two-year term; it was difficult enough. The student body had dropped drastically and funds were low. Pressure from the Technion administration to reduce the size of our technical staff and to give up space in our building to other academic units had to be fought off. Moreover, an international review committee, consisting of Professors R.A. Raphael (Cambridge U.), H.B. Gray (Cal. Tech.) and B.S. Rabinovich (U. of Washington) had been invited to evaluate our Department; under the circumstances, their report was surprisingly favorable. If I survived my term as

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[97] E.A. Halevi, "Qualitative quantum chemistry as a guide to the construction of potential energy surfaces", *Internat. J. Quantum Chem.* **12** (S1), 289-298 (1977).

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

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

[100] E.A. Halevi and R. Knorr, "Orbital symmetry analysis of intersystem crossing during thermal isomerization of tetrahedral to planar nickel (II) complexes", *Angew. Chem. Suppl.* **1982,** 622-634. (1982); E.A. Halevi, and R. West, "Orbital symmetry analysis of the reaction of silylenes with acetylenes and the dimerization of i-silacyclopropenes", *J. Organomet. Chem.* **240,** 129-141 (1982); E.A. Halevi, H. Bock and B. Roth, "On the non-observability of cubic P_8**",** *Inorganic Chemistry* **23,** 4376-4378 (1984).

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

[102] E.A. Halevi, "The level of prochirality: The analogy between substitutional and distortional desymmetrization", *J. Chem. Research* **5,** 206-207, (1985).

Chairman in reasonable shape, it was due in great measure to the support of the Department's indefatigable Administrative Assistant, Kochava Reznik.

In 1989, a year before my retirement, I received a letter from Dr. Rainer Stumpe, Chemistry Editor of Springer Verlag, asking whether I was still interested in their publishing my book. It seems that it had not been rejected because it treated Woodward-Hoffman dogma critically, but because the reviewers had convinced the previous editor, Dr. Ebel, that it was too sophisticated for organic chemists and would not sell, whereas Dr. Stumpe believed that this was no longer true – if it ever was. I rewrote the first four chapters and spent the next year bringing the book to completion. When I agreed to accept the Chairmanship of the Department, I extracted the concession that whatever sabbatical leave was still due me could be taken after my retirement. Therefore, most of the writing was done abroad in the following two years, in Heidelberg, Vancouver, Madison and Irvine. After the manuscript was vetted by Jacob Katriel for theoretical accuracy, my book, dedicated to the memory of my mentor Christopher Ingold and my friend David Ginsburg, finally appeared in print.[103] It was quite well received, but would have had a greater impact had I been able to publish it a decade earlier.

During my second stay at Irvine, Max Wolsfsberg and I produced a paper combining the three areas that had occupied me throughout my career: secondary isotope effects, orbital symmetry and computational chemistry.[104] An invitation to present the results of this study at the 1991 Gordon Conference on Isotopes, 32 years after my first invited lecture at the 1959 Gordon Conference, rounded out my career as an isotope chemist nicely.

Of my many non-chemical duties during my career at the Technion, one – performed just before my retirement – is worth reporting. Three Technion professors; Joseph Hagin – Agricultural Engineering, Ehud Lenz – Mechanical Engineering, and I, were sent to Brazil in order to advise Gilberto Mestrinho, the recently elected Governor of the State of Amazonas, on the establishment of an Institute of Technology in the capital, Manaus. We studied the situation for a week, during which we were treated like royalty, after which, speaking for the delegation, I extended the Technion's greetings to the Governor orally (in Portuguese), presented him with an album of the

[103] E.A. Halevi, *Orbital Symmetry and Mechanism*, Springer Verlag, Heidelberg, 1992.

[104] E.A. Halevi and M. Wolfsberg, "Cycloaddition of acrylonitrile to allene: Computed reaction path (AM1) and intramolecular isotope effect", *J. Chem. Soc. Perkin Trans. 2* **1993,** 1493-1496.

Technion and several bottles of Israeli wine, and promised to send him a detailed report (in English) soon after our return to Haifa. Our recommendation was to go slow: shore up the science departments of the woefully under funded and under equipped University of Amazonas and gradually add engineering departments under the guidance of an international advisory committee, which we undertook to help organize. Meanwhile, however, the Governor and his retinue had been invited to M.I.T., where he received a counter-proposal: to set up an Institute of Biotechnology under M.I.T. supervision, a project that – in our opinion – was doomed to failure for lack of an adequate scientific and technical infrastructure. We were thanked profusely for our efforts and informed that, while the Governor was tending toward the M.I.T. proposal, he would welcome one of us to return and argue our case; we declined. It appears that we had not taken Brazilian politics into account. Mestrinho had been elected for a four year term, after which he suspected – correctly, as it turned out – that he would be succeeded by his predecessor and rival, Amazonino Mendes. If he "went slow", as we suggested, Mendes – not he – would get the credit, so he opted for the M.I.T. project which promised to yield more immediate returns. Three years later, I was invited to speak at a conference in Florianopolis,[105] after which Ada and I took a tour of Brazil, including a visit to Manaus. I was not surprised to learn that the M.I.T-sponsored Institute of Biotechnology never got off the ground.

For quite a few years after my retirement, I continued to attend conferences on isotope effects and reaction mechanism in the capacity of *tribal elder*; once, at an ESOR symposium in Dubrovnik, I presented an "*OCAMS cum* computational" study that I had run on my PC.[106] Though I am neither a technologist nor an ethicist, but do know something about Judaism, I took an active role in a conference on "Technology and Ethics" held at the Technion in collaboration with the University of Judaism (now the American Jewish University in Los Angeles) and accepted the task of co-editing its Proceedings.[107] To keep occupied, I worked for several years as English copy editor for Springer Verlag. In 2007 Ada and I moved to a retirement home in Regba. As of this writing (April 2008), the Technion is still within reach; there are books and the

[105] E.A. Halevi, "[2+2]-Cycloaddition: symmetry, concertedness and secondary isotope effects", *Atualidades de Fisico-quimica Organica,* 1995**,** 215-232.

[106] E.A. Halevi, "Formation and isomerization of C_{120}: Considerations of symmetry, kinetics and thermodynamics ", *J. Phys. Org. Chem.* 15**,** 519-523 (2002).

[107] "Technology and Ethics", E.A. Halevi and D. Kohn *editors,* S. Neaman Institute Press, Haifa, 1993.

Internet; the violin and viola have been largely replaced by the CD player and an occasional concert. I have no complaints.