Walter Kohn

Personal Stories and Anecdotes Told by Friends and Collaborators

MATTHIAS SCHEFFLER
PETER WEINBERGER
Editors

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On the Occasion of the
80th Birthday of Walter Kohn
Walter Kohn receiving his Prize from the hands of His Majesty the King, Stockholm, December 1999 (© 1999 The Nobel Foundation, photo: Hans Mehlin)
All of Walter Kohn’s personal friends readily agree that it is almost unthinkable that he is turning 80 in March 2003. Considering all his activities — from human rights to theoretical physics — it is indeed wonderful to see that “age” has not changed his radiant lifestyle. He still seems to be one of the most frequent flyers, whether to Cuba or, as his next destination, to India and/or China, not counting his various annual transatlantic flights between Santa Barbara and Europe. He has never stopped talking to people in the same personal way, whether they are heads of state or graduate students. It is probably his open, convincing smile that brushes away all possible obstacles in communicating with fellow human beings …

Born in Vienna into a rather well-to-do Jewish family — his father and his uncle ran a famous art-postcards shop — he grew up in the learned atmosphere of an assimilated Jewish stratum that once carried to a large extent the cultural outburst of fin-de-siècle Vienna. There was no question that he would attend the “Akademisches Gymnasium”, not because it was close by — in the same direction as the “Eislaufverein” (an ice-skating rink next to the main concert hall), where he often went on briskly cold winter afternoons — but because it was simply considered to be “the” best.

Although Austria was ruled at that time by a clerico-fascist regime whose political antisemitism created a cultural atmosphere in its image, dramatic changes in Walters’s life only took place after the Nazi occupation in 1938. He was forced out of his beloved gymnasium and eventually was transferred to the (Jewish) Chajes-Gymnasium, where the number of students diminished on a daily basis. There, he also met two outstanding teachers who intellectually stimulated him for the rest of his life.

Fortunately he escaped to England on one of the last “Kindertransporte” (“children transports”) — a 16-year-old boy transplanted into a foreign country who had left behind his parents. In England he was put up as an apprentice in horticulture and was later deported as “enemy alien” to Canada, where he worked as a lumberjack. But the learned atmosphere could not be banished
even within the fenced-in camps: an ad hoc school took care of the semi-adult kids that Hitler Germany had uprooted.

Eventually he managed to be accepted at Toronto University. His first publication was written in the same spirit as his former teachers’ at the Chajes Gymnasium in Vienna and dealt with contour integrations, a mathematical technique he still loves. If there was anything he rescued from the old continent, from his parents’ home, something he always carried along in his mind, then surely enough it was this learned atmosphere, intellectual enlightenment and the feeling that curiosity-driven science leads to new perceptions.

This is not the place to cite or even comment on his by-now-famous papers – on the “Kohn anomalies”, just one example of his various contributions to modern solid state theory, or on what is now well-known as “Density Functional Theory”, for which he was awarded the Nobel prize in chemistry in 1998. Here the focus is on Walter’s personal contacts – with colleagues, collaborators, friends – which were always intense, and not only with those who ended up co-authoring works with him. Some of his scientific friendships, with Luttinger or Friedel, for example, were a permanent source of new ideas. Looking back it has to be noted that – very typically for Walter – none of his friends in physics were narrow-minded scientists; all of them also had broad “other” interests.

Walter’s other interests are a most rewarding aspect of meeting him, besides, of course, the notoriously sizzling discussions about certain aspects of theoretical solid state physics. It is a real pleasure to go with him to an
exhibition, a gallery, to talk about fictional literature; his enthusiasm for the exceptionally new, for the yet-unknown old, seems to be unlimited. His enthusiasm extends from certain styles of Greek vases, discovered during a scientific trip to Crete, to drawings by Egon Schiele to be seen at a special exhibition in the capital of an Austrian province.

One cannot write about Walter without also writing about Mara, his wife, perhaps the only fixed point in the space of his vibrant activities. Mara and the house in Santa Barbara – which contains an incredible collection of precious old books and the archives of her father, Roman Vishniac, many of whose famous photographs hang on the walls – constitute the Kohn’s home. The house, with its staggering view of the coastline – sometimes the Channel Islands seem to pop up only few miles away – seems an appropriate resting place for Walter before he goes off again, touring the world in matters of science or human relations. Mara and her soft, quiet voice – a friendly but decisive “Walterchen” has the ability to momentarily cut the thread of his fascinating stories about people he met or one ought to meet – occasionally give the urgently needed reminder that even a Walter Kohn needs moments of creative quietness.

Quite a few stories collected in this book give account of the circumstances of working with Walter, of being a collaborator of his, of being accepted as a friend of the Kohns. There is no doubt that these stories also cover a certain chapter in the history of science in a very personal manner: a kind of “oral history” of modern theoretical physics. Besides physics, besides stories about
physics, however, they all speak to Walter Kohn’s enduring humanity – if only by recalling his infectious, giggling laugh, which he must have saved from his early childhood.

Berlin, Vienna, January 2003

Matthias Scheffler

Peter Weinberger

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Much of life is luck. A member of the class of 1955 in Mechanical Engineering at MIT, I discovered that, because I had talked my way out of English and Chemistry, and worked hard, they would give me a Master’s degree for staying an extra semester. During those months, in addition to doing some forgettable research on the creep of metals, I took half a course on quantum mechanics – taught, as it turned out, by Sidney Drell – thinking that a slightly better understanding of Nature could not hurt a practicing engineer. It did more. Before leaving Cambridge, to return to India and find work, I applied to some graduate schools for admission in Physics and asked Drell for advice. He told me about a Harvard instructor named Walter Kohn who had recently gone to Carnegie Tech, and was doing interesting work in solid state physics. During the summer of 1956, as I contemplated jobs in which the salary was inversely proportional to the challenge, telegrams arrived offering me fellowships to Harvard and Carnegie. I accepted the latter, causing consternation among my American friends’ mothers.

So it came about that at 22, I met an oh so mature, pipe-smoking, wise, research scientist – aged, I now calculate, 33! (Only later did I discover another side to him, a side that could be reduced to uncontrollable tear-making merriment over something like a poem by Christian Morgenstern.) Gravely, he advised me about courses, including the wonderful suggestion that I might profit from a second course in quantum mechanics taught by Gian Carlo Wick, even though I had not finished a first one. The second year was a disappointment; he was on leave and the courses on statistical mechanics and solid state physics, which I should have taken from him, were taught badly and bizarrely by someone else. By contrast, the last year and a half were quite wonderful. His suggestion for a research project was very much to my taste, and his supervision of it was precise and constructive. He thought hard during our meetings about how to keep the project moving along. A steady stream of seminar visitors, including the likes of John Bardeen talking impenetrably about the new theory of superconductivity, made it clear that, largely because of Walter, I was at a major research center for my new line of work. He arranged for me...
Vinay Ambegaokar

to spend the summer of 1959 at the Bell Labs, then the ultimate ivory tower and a summer paradise for solid state physics. [“...les vrais paradis sont les paradis qu’on a perdu.” – Proust.] There was a semester visit by John Ward, whose course gave me a liberal education in how to calculate deftly . . . . And, all too soon, it was over, though not before Walter helped me find a post doctoral berth in Copenhagen, and then a position at Cornell.

My wife Saga and I remember Lois and Walter Kohn very fondly from those years. There were many social occasions, thanksgiving dinners, tennis parties, and opportunities to become easy with each other.

Owing so much to him, I offer these old memories in friendship on his 80th birthday.

About the Author: Vinay Ambegaokar is Goldwin Smith Professor of Physics at Cornell University. His Ph.D. thesis was on a many body formulation of the effective mass theory of semi-conductors with few carriers, building on Walter Kohn’s pioneering work. Since then he has worked on various problems in condensed matter and low temperature physics, and written a book “Reasoning about Luck: Probability and its uses in physics” (Cambridge University Press, 1996) for serious general readers. Contact: Department of Physics, Cornell University, Ithaca, NY 14853, U.S.A.; va14@cornell.edu
Memories of Walter Kohn at Bell Labs and After

Philip W. Anderson

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Walter was one of an exciting generation of students who showed up at Harvard right after the war, having all kinds of interesting backgrounds – Los Alamos and other wartime labs, the military, refugees like Walter, mixed in with very young recent undergraduates For good reason, he took his studies very seriously, and he may have thought of me as rather light-weight – in any case, we were not much more than acquainted, and went our separate ways, he to a postdoc in Europe and I to Bell Labs, having now become serious, since I had acquired a family to support. So my very real and deep friendship with Walter began after ’53, when he and Quin Luttinger began the annual summer visits to Bell Labs which were so enormously fruitful for both parties. They represented for us an important contact with the wider world of theoretical physics as it was then evolving, with new mathematical methods such as quantum field theory, as well as a new lifestyle which Jeremy Bernstein was to later dub “the Leisure of the Theory Classes” involving personally administered research contracts which allowed lecturing at summer schools or workshops held in glamorous resorts, attendance at international meetings, and the like. But at the same time Quin and Walter were producing a series of papers of great practical as well as intellectual value, doing such things as sorting out effective mass theory in the complex case of real semiconductors, putting transport theory on a sound basis, and comparing detailed theory of shallow impurity states with experiment. Walter, in particular, wrote several of the papers I consider his best during that period, papers obviously stimulated by the Bell group and Bell’s experimentalists’ needs.

It is Walter’s great strength to find important new physics in areas which ordinary people consider to have been long since mined out. Such a paper was his discussion of the localizability properties of Wannier functions, which he worked out in one dimension but in such a way that Blount could base a comprehensive review on his methods. His remarkable paper revisiting the question of why insulators are so to all orders, and introducing the U(1) gauge symmetry, is a prime example. And his paper on the “n+1 body problem” was an important precursor of Landau theory.
Our appreciation of Walter was not diminished by the fact that we were able to use him as a stick with which to beat the Bell Labs management, which, if possible, appreciated him even more than we did. In 1955–56 we (Conyers Herring, Peter Wolff, as well as myself, and at the beginning Harold Lewis and Gregory Wannier) were encouraged to organize a new theory department, probably because of the rather rapid attrition of theorists from the Labs. We had of course lost Charlie Kittel and John Bardeen, and both Harold and Gregory were negotiating transfers to academia at the time. One of the commonest phrases we used in the negotiations with our managers was “let’s organize a department into which Walter Kohn would be happy to come”. We managed to get, with this as our strongest argument, a democratic structure with rotating chairmanship, postdocs, of course a measure of control over our own fates (until then we had been separately supervised by experimental department heads), a liberal travel and sabbatical policy, and (quietly and informally) considerably improved salaries – a lot to owe Walter for!

I am reminded also of another almost forgotten cause for our appreciation. Bell Labs had its own incident of McCarthyism, a “questionnaire” about one’s links, past and present, with “subversive” organisations, which was required of all employees. Why Walter as a temporary was asked to respond, I don’t know, but I do know that he refused, providing a measure of cover for the shockingly few permanent employees who refused to fill it out as a matter of principle (Gregory, Alan Holden, perhaps Hal Lewis, and myself, among my immediate acquaintance). There were no consequences – this was late in the McCarthy era, true to the AT&T’s characteristic ponderousness, and by the time the responses were all in, my reaction would have looked silly. But we did not know that at the time. Walter and Alan Holden, with their mutual interests in music, the arts, and literature, became good friends, an additional link from Walter to Joyce and myself.

Of course, we should be so lucky – we did not attract Walter, instead he decided to take on the responsibility for growing the physics department at the new branch of the University of California at San Diego – an assignment he carried on with remarkable success, as I’m sure is detailed elsewhere in this book. But perhaps others will not realize one of the major sources of that success, namely his intimate knowledge of Bell Labs personnel, gained over many years of summer visits. Three of the mainstays of the fledgling department were Bernd Matthias, Harry Suhl and George Feher, three of Bell’s most exciting and original finds. So, over the years, I feel that Walter and Bell came out about even – although that’s not counting his postdocs and students, like Maurice Rice and Pierre Hohenberg, who came our way.

Of course, Joyce and I remained friends with Walter and his then wife; we visited San Diego a number of times, notably in 1967 when Bernd and he brought me there for a month as Regents’ lecturer, and I gave the talk that became “More is Different”. But we seemed to drift into different branches of physics until he became head of the ITP, where I labored for three years as a member of his advisory board during his tenure; and since then I have taken
great pleasure, on visits to the ITP, in seeing him and Mara in the wonderful setting of their hillside house filled with memorabilia from her family — I always try to sneak into that wonderful library and snoop about during any visit.

About the Author: Philip Anderson is Joseph Henry professor, emeritus, in the physics department of Princeton University. His interests at present are (1) the theory of strongly correlated electron systems, particularly high-$T_c$ superconductors; (2) aspects of complexity theory, as a long-time steering committee member of the Santa Fé Institute, and one of the founders of its economics program; (3) reviewing popular books on science, mostly for the Times (London) Higher Ed Supplement, and occasionally writing on such subjects. He won 1/3 of the Nobel Prize in physics in 1977; his most recent honorary degree is from the University of Tokyo, Dec 2002. Contact: Dept. of Physics, Princeton University, Princeton, NJ 08544, U.S.A.; pwa@pupgg.princeton.edu; pupgg.princeton.edu/www/jh/pwa/
A Twinkle in the Eyes

Neil W. Ashcroft

Cornell University, Ithaca, U.S.A.

My memory is becoming increasingly porous, but I believe I first met Walter in February of 1965, in San Diego, during a short reprieve from the winter besetting the University of Chicago (and I deduce from this that Walter must have been on the right side of 40 then). At Cambridge, where I had studied for my Ph.D., I had made very close acquaintance with the metallic state of matter. But the insulating state, in a fully many-body context, was a puzzle to me then. Walter had written some beautiful papers on this topic, and in early 1965 began to set me on the right path. I thought then (and do so now) that he was one of the most congenial of men, and that he was endowed, as we all know, with the most extraordinary physical insight and perspicacity. The nearly four decades that have since passed have only served to solidify my view; in fact during this period I simply cannot count the number of times that, tracking back through the literature on a particular research topic in condensed matter theory, I have ended up at a seminal paper of Walter’s. It is abundantly clear that he has had a profound impact not only on our chosen field, but on allied fields as well. His remarkable creation of the density functional approach to correlated quantum systems has actually far transcended theoretical condensed matter physics per se.

I was made aware of this, in a rather amusing way, in October of 1998, on one of those especially luminous days when the Nobel Prizes are announced. By a strange conjunction of world lines, I happened to be attending, at the University of California, Santa Barbara, a meeting of Directors of NSF’s Materials Research Centers. There was much rejoicing, as I recall, at the morning’s news on that very day that Walter was co-winner of the Nobel Prize in Chemistry. At a mid-afternoon break, I thought it at least worth a try to see Walter and to offer my congratulations personally, though given the boisterous activity at Broida Hall I assessed my chances as slim. But by a remarkable fluctuation there he was, in his office, momentarily unencumbered.

Walter was in high spirits as might well be expected and we had an entirely vivifying chat, mainly about the long story of the development of electronic structure. But at last I had to get back to render unto Caesar, and just as I was
leaving Walter said, with a slight frown framing nevertheless a clear twinkle in the eyes, “But Neil, what will the physicists really think of all this?” with a distinct emphasis on ‘physicists.’ Well, I did not think it my place to speak for the grand-ensemble of all physicists, but having a somewhat ecumenical, and perhaps even a ‘colonial’ viewpoint on these matters I answered Walter that I thought there was actually a pretty good precedent. One Ernest Rutherford had also received the Nobel Prize in Chemistry.

As we all know, to our common delight and long-term pleasure, Walter has a truly encyclopedic memory and there is also not much in the history of physics that has escaped his attention. He quickly responded with the most potently well-justified and yet ‘politically correct’ of all possible answers: “Yes, and Marie Curie too”.

Walter’s work has meant a great deal to me, actually more so on average in the classical domain than in the quantal. For classical systems where strong correlation is a sine-qua-non (for example in the burgeoning field of inhomogeneous classical fluids) the density functional viewpoint has led to quite remarkable advances, many of them stemming from appropriate classical limits of the quantum problem and stemming from Walter’s insights. He is surely one of the great physicists of our age, and at this 80 year mark I thank him for his many, many contributions that have so enlivened our subject.

About the Author: Neil W. Ashcroft received his Ph.D. from Cambridge University in 1964, spent a year as a post-doctoral associate at the University of Chicago, and has been at Cornell University (where he is now Horace White Professor of Physics) ever since. Contact: Physics Department, Clark Hall, Cornell University, Ithaca, NY 14853-2501, U.S.A.; nwa@ccmr.cornell.edu
On September 11, 2001, I was in San Lorenzo de Escorial, Spain along with Walter Kohn, Matthias Scheffler, and perhaps others who are contributing to this book. We were attending the Ninth Annual International Conference on Density Functional Theory. As I left the auditorium after one of the talks that afternoon, I was approached by Steven Richardson who was a former postdoctoral from our group. As I stood speechless, he recounted the terrible events that had occurred in New York and Washington. I rushed to my hotel and could only get the news in Spanish. Although the pictures told most of the story, it wasn’t until my wife and I joined others at the conference to watch the BBC in English that we learned the horrible details.

The next morning Walter was scheduled to talk. He faced an audience of agitated, concerned, and depressed people. We obviously were not in the mood to hear about Density Functional Theory, but that’s what we were there for, and we felt a need to be together. Walter took the stage, and in a calm and thoughtful manner began discussing what had happened in the United States. Those of us who knew Walter were accustomed to his gift of going directly to the important, central issue of a problem in science, but I was still amazed at how he did exactly that in the midst of the emotional turmoil we were all experiencing.

Walter focused on the central issue—man’s inhumanity to man. He’d seen it before. He recognized that this was a major crime against the innocent, and he knew what to say. Those of us who knew of Walter’s past experiences were possibly even more touched by his calmness and his ability to find the words that calmed everyone else. His talk was appropriate and inspiring.

As we sat learning, understanding, and acknowledging that Walter had just said the things that should be said, Walter paused briefly and then began his technical talk. It was a signal that life goes on.

I was very impressed by Walter’s speech at Escorial. All of us who spoke after him expressed our sadness and, although there were many eloquent words spoken, none had the impact that Walter’s did. Although I’ve seen Walter since then, our Escorial encounter is the one I remember most vividly.
I do remember my first encounter with Walter Kohn. I was a graduate student in the early 1960’s and I read his review chapter in Volume 5 of *Solid State Physics*, “Shallow Impurity States in Silicon and Germanium,” written in 1957. I shouldn’t say read – I practically memorized it. I looked up everything I didn’t know or couldn’t understand in the chapter. Since I didn’t know much, I looked up a great deal of material. The chapter was so well written and clear, it directed my study of the material. I learned an enormous amount from this man I was yet to meet.

I can’t remember my first face-to-face meeting with Walter. It was probably around 1963 or 1964. I was well aware of his work through many conversations with Quin Luttinger at Bell Labs in 1963-64. I studied the Hohenberg-Kohn-Sham approach as it came out and examined every argument. Although I trusted the results, I couldn’t wait to test the method against experimental data. However, by the time I was in a position to use the DFT approaches, we had already solved the electronic structure problem for dozens of semiconductors using an empirical pseudopotential method (EPM). We were able to explain optical and photoemission in terms of interband transitions and had won every debate with experimentalists on the interpretation of their spectra.

I never had Walter’s patience. His approach was to get all the fundamentals nailed down and make sure everything grew from there. I wanted answers to the experimental puzzles, and I wanted to make predictions. So I was happy with the EPM. In addition to their use in getting the physics right, they pointed the way for the *ab initio* theory. Of course, I was still strongly motivated to produce a method which required no experimental input. Along the way to first principles, we used semi-empirical approaches ranging from Fermi-Thomas, Wigner, and Slater potentials, but I was determined to employ DFT with the pseudopotentials. This became possible once a student John Walter and I produced electron charge densities. The Walter-Cohen paper produced some name confusion and considerable controversy.

Although chemists and physicists had visualized what the covalent bond in semiconductors like silicon and germanium would look like, many didn’t like our pictures. The attacks by prominent chemists and physicists were harsh. In addition, it was at this point that I witnessed how much the chemists were suspicious and outright hostile toward DFT. It is ironic that Walter’s Nobel Prize is in chemistry. It is also a lesson on how it sometimes takes a new generation to clean out the unfounded convictions of their older colleagues.

We implemented the DFT and converted our pseudopotentials to *ab initio* forms. At first I felt Walter was skeptical. I think it was because the methods grew out of empirical approaches. I remember after a seminar I gave showing that the electron densities were consistent with experiment and that we could predict and explain “everything” about silicon, he asked what experimental input I had used. I said, “nothing.” He said, “really?” I said, “a little.” I needed to know the candidate crystal structures to test, and when predicting superconductivity in compressed silicon, I needed to input the appropriate
Coulomb repulsion parameter. Walter’s eyes twinkled, and he nodded making me very happy.

Finally the chemists joined the DFT bandwagon. In many of their calculations, they needed more precision than we did — so called “chemical accuracy.” But on the whole they began to explore and use the DFT. Linus Pauling’s reactions to our work were surprising. He was a great fan of our paper which calculated the total energy and structural properties of semiconductors. When I explained that the method was based on plane waves having no bonding prejudices, pseudopotentials, and DFT, he asked questions which showed that he understood what was going on. However, he referred to it as “your quantum mechanical approach” and said it wouldn’t work for metals if I counted valence electrons in the same way as I did for semiconductors. I disagreed. He challenged me to do calculations for tin, but when I showed him the successful results, he was upset and then made a joke about physicists.

I told Walter that Linus believed DFT for semiconductors but he needed convincing about metals. We decided that Linus had a point concerning strongly correlated systems but he’d have to yield on itinerant electron metals. Of course by that time, the chemists were on board.

For many years I have given talks where I begin with Dirac’s 1929 quote about quantum mechanics: “The underlying physical laws necessary for a large part of physics and the whole of chemistry are thus completely known, and the difficulty is only that the exact applications of these laws lead to equations much too complicated to be soluble.” I refer to this as “Dirac’s Challenge.” Walter Kohn’s contributions have helped us answer this challenge. I was therefore delighted when many of the public announcements heralding Kohn and Pople’s Nobel Prize used this quote. Dirac would have also been pleased since he did research on DFT in the 1930’s.

Let me close by relating an incident that occurred in my home when I was showing some guests a photograph of Einstein my wife had given me. I pointed out that it was a Roman Vishniac photograph and that Vishniac’s daughter was married to Walter Kohn. One of the guests asked, “And how are you related to Kohn?” I answered, “Unfortunately, only through physics.”

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I first met Walter in the summer 1953 at a Gordon conference where, after my first trip across the Atlantic, I met the cream of the people then active in the electronic structure of solids. I had used, three years earlier, his technique with Rostoker to analyze the conduction band of polyvalent metals such as Mg or Al, to understand how and how much they deviate from the free electrons model in their cohesive properties. By 1953, my interest had switched to impurities in metals; and it is only with André Blandin that, after Walter himself and John Ziman, I would come back to this question, using a simplified perturbation technique.

Walter was then for me a young but established theoretician, with broad interests from nuclei to solids. We met a number of times in Paris, Brussels and especially at the 1956 Varenna Conference, where a number of us brought our young families and formed lasting friendships. This was helped by the fact that, staying through all this summer school, we had plenty of time to see each other's and were not encumbered with too many students!

It was then natural for my first PhD student, Emile Daniel, to follow Walter as a postgraduate to La Jolla, where he was arrested by the police for walking at night along the road to San Diego! André Blandin, my second PhD student, followed Pierre Gilles de Gennes traces to Charlie Kittel in Berkeley; but as he had lost his two initial races with Walter's own PhD students (on the Landau diamagnetism of impurities and on NMR in Cu base alloys), this Californian stay was the time when Walter and André became firm friends. This started a regular fashion for young theoreticians of Orsay to come as postgraduates to Walter or Charlie.

Walter started visiting Orsay on a regular basis soon after we moved there – one of the first times with Hohenberg, just after his work that would lead him to his Nobel Prize. We found renewed common interests in the field of surfaces, where Walter Kohn and André Blandin attended a CNRS meeting in Lille, and later when Denis Jérome, after staying with Walter, started his search for low temperature excitonic insulator phases. Walter's early visits were found most stimulating by the lab in Orsay. This led André Blandin,
My Friend and Colleague Walter Kohn

then a young professor, to propose Walter for one of the first Doctorates Honoris Causa in the newly created University Paris Sud (Orsay), a move that pleased the many friends and visitors Walter had at the Atomic Energy in Saclay. In more recent years, it is Chuck Sommers who, with the help of his computer, has kept most regularly the contact with Walter; and I am thankful for the opportunity he thus offers me to state how much I have derived from knowing Walter.

Walter had an uncle who lived in Lyon since before the war. And we often had his visits, on his way to or from Lyon, in our Yonne country place at Palteau. Our children were often there at the time, and sometimes he came with André Blandin. It was during one of these visits, on return from a drive through the autumn colored vineyards of the Auxerrois, that Walter invited me to a stay in Santa Barbara, where he was in charge of the newly created Institute of Theoretical Physics. Walter and Mara were in fact wonderfully generous of their time to make this stay a complete success, including the lending of their car to allow me to explore the coastal road up to Berkeley and back. The working of the Institute was very striking by its informality as well as by the high calibre of its participants, in a range of subjects that only somebody like Walter could have initiated. This stay in Santa Barbara helped to strengthen ties which, in a way, could be thought of being prepared when, in 1940, Mary was helping German speaking and mostly Jewish refugees, pushed out of the South Coast of England, to settle uneasily in a workhouse, north of Bristol, in the uncertain hope that the younger members of their community would, like Walter, be allowed to leave the Isle of Man.

But Walter is not somebody to reminisce too much on the difficulties of his past, when in Paris, his preoccupations in recent years have been much more on how to save UNESCO, on the best way – Spanish or English? – To teach young Mexican immigrants in California, or on how to keep a purposeful contact with young American students.

With his Nobel prize, Walter has no doubt now more opportunities to air his views, but perhaps less time to think quietly at home!

About the Author: Jacques Friedel is Professor Emeritus at l’Université Paris-Sud Orsay. Born 11th February 1921. Elected member of the French Academy of Sciences the 17 of January 1977. In the last few years also interested in high temperature superconductors and in one-dimensional low temperature organic superconductors. Contact: Académie des Sciences, 23, quai de Conti, 75006 Paris, France.
Working with Walter

Michael Geller

University of Georgia, Athens, U.S.A.

I am delighted to be able to wish Walter a happy 80th birthday. Here I will briefly explain what it was like to be his graduate student.

The Morning Paper

I came to Santa Barbara in 1989 specifically to work with Walter. I had visited him a few times before then, and we always had wonderful discussions in our meetings. I would ask him what problems he was currently interested in, and he would explain them to me in great detail and with his characteristic clarity.

During my first year in Santa Barbara I lived downtown with my girlfriend Robin (who is now my wife) and took the bus to campus. In those days I held the mistaken view that every physicist should read Physical Review Letters cover-to-cover, so I would try to read PRL on the bus. One day Walter sat down next to me and said “I see you’re reading the morning paper.” That morning I was reading an article about disordered systems, so we talked about that. He confided that some loose ends in the theory of Anderson localization have always bothered him. It turned out that we often took the same bus, so we had many similar discussions.

Walter doesn’t read much physics literature himself anymore. Countless times I showed him journal articles that I thought might be relevant to our research. Hesitantly, he would look at it, but I could tell that it was more out of setting a good example for me than thinking he would learn something. After a paragraph or two his demeanor would change. “It’s not clear, it makes unnecessary assumptions, and there is nothing new in it,” was often his response.

“Take Five Minutes”

During my first year of graduate school I took the condensed matter physics course taught by Alan Heeger the first quarter and Walter the second. Walter
Working with Walter

had us fill out questionnaires detailing our prior condensed matter/solid state coursework. He wanted to be able to assume that the elementary aspects of the subject were understood so that he could cover more advanced material. A few of us, including me, had not taken an undergraduate condensed matter course and so were required, as part of Walter’s course, to take an oral exam during the quarter on the 20 “essential” chapters of Ashcroft and Mermin’s text. I studied very hard for this exam and worked through all of the homework problems from those chapters. He asked me a few questions about Brillouin zones and magnetism, and I passed. A classmate failed because he did not know the band structure of aluminum. According to Walter: You do not understand metals if you do not understand aluminum.

In addition, we wrote extensive term papers on some aspect of condensed matter theory. My paper was on spin glass theory, and I included Parisi’s solution of the Sherrington-Kirkpatrick model, and discussions of replica-symmetry-breaking, ultrametricity, and so on. Walter returned the term papers to us during the next quarter, long after the course was over and grades finalized. However, he still required many students to make specific improvements to their papers and return revised versions to him!

I began working with Walter the summer following my first year. The next year he taught graduate condensed matter again, but covered some different material, so I sat in for those lectures. One day while discussing spin glasses he tells the class that “we are fortunate to have an expert on spin glasses with us today.” We all turned around looking for that expert. Walter turned to me and asked whether he covered all of the essential ideas, and, regrettably, I pointed out that he did not fully discuss the concept of frustration. He handed me the chalk and asked me to “take five minutes.” I did my best, but I was so nervous that afterwards I could not remember anything I had said.

“You’ve told me nothing”

Walter was an extremely demanding advisor. People are often surprised to hear this, because he has such a kind and gentle personality. But when the office door closed for a meeting, he became a tiger. There was no opportunity to say “well, I got stuck” and he was rarely interested in the details of a calculation. He wanted the answer right up front, so that we could spend our time together planning the next step.

I can recall with fondness the many times I was really excited at having solved some aspect of the problem we were working on, and I marched into Walter’s office, full of energy, and gave what I thought was a clear and concise chalkboard presentation about it. He sits there listening quietly, and I am thinking that he will ask me to write it up for publication. But before the chalk dust had time to settle, he said “You’ve told me nothing”! I kid you not. His standards are extremely high, and it is rare that he thinks a new theoretical result – coming from his students or anyone else – is highly significant.
Publishing, or Not, with Walter

Finally, I come to perhaps the favorite subject of Walter’s former students and postdocs – finishing a project with him and publishing. This does not happen very often, because as I mentioned, his standards are extremely high. My thesis on compositionally graded semiconductors eventually led to three publications,¹ so in that respect I guess I did pretty well. He spent countless hours with me patiently teaching me how to write.

Once I asked Walter why it was so important to clearly explain the logic behind a calculation, as the final result is usually insensitive to this. He responded that “Clarity in writing is everything: As theoretical physicists our job is to explain why or how some phenomena happens, and the better we do this the better the work is.” Readers of Walter’s scientific work know that he follows his own advice.

Walter, I have so many wonderful memories from those days. I will always cherish the time we spent together. Thank you and Happy Birthday!

About the Author: Michael Geller is an Associate Professor in the Department of Physics and Astronomy at the University of Georgia. He is a condensed matter theorist interested in strongly correlated electron systems, mesoscopic and nanoscale mechanical systems, and vortex dynamics in superfluids and superconductors. Contact: Department of Physics and Astronomy, University of Georgia, Athens, GA 30602-2451, U.S.A.; mgeller@physast.uga.edu; www.physast.uga.edu/~mgeller/group.htm

My Favorite “Walter Story”

Alan J. Heeger

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Shortly after the announcement in October, 1998, that Walter Kohn had been awarded the Nobel Prize in Chemistry, our newly appointed Executive Vice Chancellor arrived on the UCSB campus. The Physics Department invited the EVC to visit the Department and to meet our distinguished faculty members. After introducing the EVC to the Physics faculty, the Department Chair asked that we introduce ourselves individually and one at a time in turn around the room, and that we each say few words about our research interests. As Professors will often do, the comments by many of the faculty were a little drawn out. When it came to Walter’s turn, the great physicist said simply the following (with a characteristic smile on his face): “My name is Walter Kohn and I do Chemistry”.

As a physicist, as a scientist, as a humanitarian, as a colleague, as a friend – and even as a chemist, Walter Kohn is an inspiration.

Walter, we love you. Happy 80th Birthday!

About the Author: Alan J. Heeger serves as Professor of Physics and Professor of Materials at the University of California, Santa Barbara and also heads a research group at the university’s Center for Polymers and Organic Solids. He was awarded the Nobel Prize in Chemistry (2000) for his pioneering research in and the co-founding of the field of semiconducting and metallic polymers; his research efforts continue to focus on the science and technology of semiconducting and metallic polymers. Current interests include studies of conjugated polyelectrolytes, and the use of such luminescent water-soluble semiconducting polymers (and oligomers) as components in bio-specific sensors. Contact: Center for Polymers and Organic Solids, University of California, Santa Barbara, CA 93106-5090, U.S.A.; ajh@physics.ucsb.edu; www.cpos.ucsb.edu
A Personal Tribute to Walter Kohn on His Eightieth Birthday

Pierre C. Hohenberg
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I first met Walter Kohn in 1963 when I was a fresh PhD spending a post-doc year in Paris at the Ecole Normale Superieure in the group of Philippe Nozieres. As it happened, Walter was also spending time at the Ecole Normale and conditions being what they were I was privileged to share an office with Walter during an extended period. If I remember correctly, this relatively large space was Philippe’s own office and even if it wasn’t I remember it to have been a general meeting place and thoroughfare, a little bit like trying to think deep thoughts in the middle of Times Square.

In any event, soon after I met Walter he did me the honor of inviting me to join him in a new research project he was undertaking, the examination of theoretical methods for treating the inhomogeneous electron gas. To say that this project turned out to be successful is a singular understatement, since the paper we produced in the Spring of 1964 [P. Hohenberg and W. Kohn, Phys. Rev. 136, B864 (1964)] was one of two works cited by the Nobel Committee in awarding the 1998 Chemistry Prize to Walter. At the time I remember that it felt like being led through an enchanted, and at times haunted, forest by a trusted guide. The journey was both instructive and entertaining, but I have some trouble identifying what I could possibly have contributed that would merit Walter’s generosity in placing my name first on the 1964 paper. One incident stands out in my memory. After we had proved the primary theorem on density functionals we started exploring possible applications to real systems. When Walter suggested using the theory to make improvements in the prevailing methods for determining the band structure of solids, I said to him that I feared this would require expertise in practical areas of materials science I knew nothing about. He then drew himself up to his full five feet ten inches and said to me, “Young man, I am the Kohn of Kohn and Rostoker!” [Phys. Rev. 94, 1111 (1954)].

The year 1963 was a difficult one for me personally since in November, while I was at the Ecole Normale, my father died suddenly in Paris, where my parents lived. Like Walter, my father was born and grew up in Vienna, but he had emigrated to France in the nineteen twenties and so was spared the
trauma of leaving his home town when Hitler marched in after the “Anschluss” (1938). The Viennese connection is another link I feel with Walter since he represents a kind of intermediate step for me between my father, who was an adolescent during World War I, and my own experience growing up during and after World War II.

During my year in Paris I discussed with Walter my search for a position the following year and sought his advice. Having gone through the standard list of university departments Walter asked me “Have you thought about Bell Labs?”. I answered “What’s that?” He then told me about the Bell Labs theory group where he and Quin Luttinger had spent so many fruitful summers, and concluded “I think you should definitely apply there. Spending a year or two at Bell Labs will do you a lot of good, it will professionalize you”.

As it turned out I followed Walter’s advice and went there for one more postdoc, which turned into a job lasting 30.5 years to the day. To my own surprise I found Bell Labs to be the right mix of “professionalism” and scientific freedom and I am grateful to Walter for having steered me to that great institution.

My scientific interests did not remain with electronic structure so I watched the development of Density Functional Theory as an admiring outsider, observing how Walter steered it forward with persistence and vision. An incident I remember clearly occurred when I saw Walter some time in the mid-eighties and he was reporting to me on the progress of “our baby”. He said to
A Personal Tribute to Walter Kohn on His Eightieth Birthday

me “Things are heating up. The chemists have finally caught on”. Prophetic words.

After Paris I next came into close contact with Walter in 1980 when he invited Jim Langer and me to organize a research program at the newly opened Institute for Theoretical Physics in Santa Barbara, whose director he had just become. Jim and I accepted his invitation and “Pattern Formation in Nonequilibrium Systems” became the first full-fledged research program of the new institute, attracting scientists from many centers throughout the world and from many fields inside and outside of physics. For Jim Langer, who had been a colleague of Walter’s at “Carnegie Tech” early in his career, this visit presaged a move to Santa Barbara, where he eventually became the director of the Institute. For me the year spent in Santa Barbara marked a shift in scientific interest to the field of nonequilibrium physics and pattern formation, an interest that has persisted for many years. My connection to the Institute also remains to this day. I have visited many times, participated in conferences and research programs and was a member and chair of its Scientific Advisory Board.

The wonderful news of Walter’s Nobel Prize held special significance for Lu Sham and me since we were his junior collaborators and co-authors on the two seminal papers cited by the Nobel Committee. Walter did us both the honor of inviting us to join him and his family in Stockholm for the prize ceremony, along with Bob Parr (University of North Carolina) a long-time friend and colleague. Unfortunately, health problems prevented Lu from attending and he was sorely missed. The pageantry of this greatest of all scientific rituals and the opportunity to participate publicly in honoring a mentor and friend were truly unforgettable experiences for which I will always be grateful to Walter.

Some years ago I decided on a drastic change of professional orientation, abandoning the quiet life of a research scientist for that of a university administrator. In preparing for this change I assembled a list of personal references and I asked Walter if he would be willing to write on my behalf, since he had knowledge of my organizational abilities from my work at the ITP. Walter’s immediate reaction was “Don’t do it! I’ve seen better men than you trampled underfoot and completely destroyed by university administration”. In this one case I did not heed Walter’s advice; I did not ignore it, I just decided otherwise. After nearly eight years as Deputy Provost for Science and Technology at Yale University I will not say that I regret my decision, but I will affirm that I understand more fully what Walter was talking about! As I now contemplate re-entering the world of science and facing the prospect of formal retirement I look to Walter as a shining example of intellectual vigor and general wisdom. I eagerly await his next piece of good advice.

About the Author: Pierre Hohenberg is currently the Deputy Provost for Science and Technology at Yale University, where he oversees the physical, engineering and biological sciences. He also acts as the chief research officer for the university. His
Pierre C. Hohenberg

research has ranged over electronic structure, theory of superconductivity and superfluidity, the study of critical phenomena and phase transitions (especially dynamics), and pattern formation and chaos in spatially extended nonequilibrium systems. Hohenberg spent the bulk of his research career at Bell Laboratories, from which he departed in 1995 to take up his current position. Contact: Department of Applied Physics, Yale University, New Haven, CT 06520, U.S.A.; pierre.hohenberg@yale.edu
Just 40 years ago I was applying for academic positions from Birmingham England where I was finishing two postdoctoral years in Rudi Peierls’ wonderful department. In those early post-sputnik halcyon days you then applied for assistant professorships and collected the offers that came in by return mail. (I exaggerate only slightly.) Peierls had suggested that I send such a letter to Walter Kohn, who disappointingly offered only a two-year postdoc. But I had never been to California and Peierls clearly had a high regard for the man, so this otherwise noncompetitive proposition was still very tempting. I asked the people at Cornell, who did come through with a faculty position, whether I could defer it for two years to do a postdoc in La Jolla. Two, they said, was too many but one would be OK. Walter said one was fine with him, so in August 1963 I showed up in La Jolla.

Walter was not there. He was finishing a sabbatical in Paris. But there were several other terrific postdocs (Vittorio Celli, Bob Griffiths, Lu Sham), the Physics Department was still located right on the beach, Dorothy and I found a fantastic house in Del Mar on a cliff overlooking the Pacific for $128 a month, and life was good. (I had written a letter from Birmingham to an acquaintance from college who was then a postdoc in La Jolla, asking what it was like there. All I remember from his reply was “Volley ball is standard on the beach at noon.”)

Eventually the moment of truth arrived. The boss returned. The holiday threatened to end. Walter invited me to his office to say hello. It was immediately clear that this was a very kind, charming, witty man. After we had exchanged pleasantries, he told me about a little theorem he and Pierre Hohenberg had proved back in Paris. The proof was one of those clever three-line arguments that wouldn’t have occurred to me if I had thought about it for a hundred years, but was utterly simple and transparent when Walter laid it out in front of me.

He asked me to think about how to generalize the theorem from the ground state to thermal equilibrium. I returned to my office to consider it and quickly realized that a strange variational principle for the free energy that I had for-
mulated in Birmingham for an utterly unrelated purpose, seemed to be tailor
made for generalizing the Hohenberg-Kohn theorem to non-zero temperature. It took me less than an hour to check that their proof did indeed go through
in exactly the same way if the ground state variational principle they used
was replaced with my thermal equilibrium variational principle.

So I went back to Walter’s office and knocked on the door. Here’s how you
do it, I said. He seemed somewhat taken aback by this and before I got very
far into my explanation he kindly offered to explain again what the problem
actually was that he wanted me to work on. He treated me to his beautiful
and transparent argument again. I said yes, that was what I had understood
the argument to be (he really had explained it very well the first time) and
my point was that it worked just as well when the temperature was not zero
if you used this variational principle I was trying to tell him about. He was
deeply skeptical. Slowly it began to dawn on me that this was the problem he
had hired me to spend the year working on.

It took me a day to convince him that I had indeed answered his question.
Then he was very pleased and I, needless to say, was ecstatic. Throughout
childhood I was the last to be picked when baseball teams were being formed.
I could never get my bat to make contact with the ball. But one day, by sheer
chance, I got the bat in the right place at the right time and the ball went
sailing over the heads of the outfielders. I must have been ten years old. It
was a magical moment. Now I was 28, at the beginning of my career, and it
had happened to me again. Never in the forty years of professional life that
followed did I ever again have as glorious an experience.

Since I had finished the year’s work, Walter encouraged me to think about
whatever I felt like thinking about – pelicans, whales, body surfing, physics.
We became good friends. As we said goodbye at the end of the year he said,
“By the way, when you get to Cornell, write up that theorem. Some day it
may be important.”

Frankly, I wasn’t so sure. But I did feel he ought to get something out
of having maintained me in that semi-tropical paradise for a whole year, so I
dutifully wrote a very short paper in Ithaca. On the occasion of writing this
memoir I subjected my Hohenberg-Kohn Corollary to a citation search. The
first decade after it appeared (1965-74) bears out my doubts. The number of
citations per year ranged from 0 to 2, most of them traceable back to Walter
and his collaborators. Then, ever so slowly, Walter’s “some day” started to
dawn. During the next six years (1975-1980) annual citations varied between
3 and 7. Between 1981 and 1993 there were between 17 and 23 citations per
year (except for a 12 in 1982). And from 1994 to 2002 (with the Nobel year
1998 smack in the middle) there were never less than 28 and as many as 40. It
may now be my second most cited paper. Not bad for an hour’s work! In
1998 I realized that it had also became my second publication in the field of
chemistry.

Honesty compels me to acknowledge that Walter has a different view of
this history. He maintains that it took me 24 hours, not just one, to do his
postdoctoral project. Although his ability to recall ancient events is normally phenomenal, in this case I am rather sure that doing the job took an hour; it took me the other 23 to convince him that I really had done it.

Walter was not only directly responsible for my finest hour, but he was also present at my finest half-hour, a quarter of a century later. In 1989 I gave a lecture in St. Louis on quantum nonlocality at a March APS meeting with the nonstandard title “Can You Help the Mets by Watching on TV?” To my amazement and the astonishment of several of my friends, 2000 people showed up. (It was in a hotel ballroom but there was standing room only.) Among them, to my surprise and delight, was Walter, who had watched, bemused, as my interest in foundations of quantum mechanics slowly developed during the 1980’s. Every time I met him during those years he’d want to know just what it was that bothered me about good old quantum mechanics. He was always very nice about it, saying he just didn’t get it – what exactly was the problem? I knew that deep in his heart, though he was much too kind to come out with it directly, he didn’t really think this was a fit preoccupation for one who had once been capable of doing a year’s work in a day. (No, Walter, in an hour!)

After a wild half-hour talk and an even wilder question period, Walter made his way through clusters of fiercely arguing people up to the podium to say hello. He shook my hand warmly, beamed his wonderful smile at me, and said “I still don’t get it.”

I cannot claim to have been present at any of Walter’s finest moments, but I was there for two quite fine ones. The first was his 60th birthday celebration in Santa Barbara, twenty years ago. Vinay Ambegaokar and I sang a long song, setting his CV to music in the form of a well-known Gilbert and Sullivan number. The banquet audience, provided with copies of the words, served as chorus. Well do I remember the voice of Pierre Hohenberg, who served as master of ceremonies, lustily belting it out.

Unfortunately Vinay and I have both lost the text, and when I wrote Walter a few years ago asking for a copy of the copy I gave him, he had to confess that he couldn’t find it either. [If any readers of this memoir have kept their libretti, please do send me a copy!] I have wracked my brain to recreate the words for this occasion, but my brain has fought back and all I can offer for this 80th birthday Festschrift are fragments. The recurrent refrain was “Now I am director of the ITP” so the whole business would not work, now that it has become the KITP.

I can recollect the opening verse, which celebrated Walter’s very first publication:

When I was a lad I thought a lot
About the heavy and symmetric top.
I thought so much they published me
In the ’Merican Mathematical Society….
N. David Mermin

But aside from that I remember only the verse on Kohn anomalies, the second line of which was inspired by a wonderful phrase from Walter’s paper:

Amongst the phonons I could see
The image of the surface of the Fermi sea!

[Chorus:] Amongst the phonons who but he
Could dare to see the image of the Fermi sea?

After the performance Walter remarked that he had always been quite proud of that line, as indeed he should be. I have always admired him as a prose stylist as much as I admire him as a physicist — whoops, chemist!

I was also present, along with about five thousand others, at Walter’s meeting with Pope John Paul that he spoke about so movingly in a subsequent talk at Santa Barbara¹. To appreciate the moment from my perspective you have to know the old joke about Louie:

Louie knew everybody. “There ain’t nobody I don’t know,” he boasted. “C’mon,” said Al, “you don’t know the Pope.” “Want to bet?” said Louie. So off they flew to Rome, where we find them standing amidst a huge throng in Piazza San Pietro, waiting for the papal blessing. People in the crowd who walk past them are saying “Hi Louie,” “Wie geht’s? Louie,” “Ciao, Louie,” etc. “Stay here,” says Louie to Al, and disappears into the crowd. Some time later the Swiss Guard appear on the steps, there’s a fanfare of trumpets, and out comes the Pope onto the balcony of St. Peters, arm in arm with Louie! An enormous cheer goes up from the crowd and a monk, standing near Al, turns to him and shouts over the roar, “Who’s that guy up there with Louie?”

At the end of a week of sixty simultaneous conferences in all areas of human knowledge, in celebration of the Jubilee Year 2000, there was a final super-plenary session at the Vatican. A group of us walked there from our hotel and just as we came upon an enormous array of outdoor tables alongside St. Peter’s, loaded with five thousand little plastic cups of espresso (“With the compliments of His Holiness”) we noticed that Walter had disappeared. There were rumors that he had been siphoned off for something special, but nobody knew for sure. The rest of us were conducted to seats fifteen or twenty rows back from the stage in an auditorium that made my ballroom in St. Louis look like an intimate seminar room. After a couple of hours of inspirational warm-ups, His Holiness appeared on the stage, listened to four people each give a one minute summary of 15 conferences, and then gave some remarks of his own to put it all into perspective.

¹ (see http://www.srhe.ucsb.edu/lectures/text/kohnText.html)
Memorable Moments with Walter Kohn

That being done, a couple of dozen cardinals formed a line in the central aisle and one by one walked up onto the stage and, kneeling before the Pope, kissed the ring and were blessed. They were followed by an even larger collection of Archbishops, who were followed by an endless string of Bishops. Then the laity joined the procession and suddenly we noticed the back of a familiar head slowly approaching the stage. Could it be our missing Walter? What would he do? Nervously we awaited the meeting. Yes, it was Walter! When the time arrived, standing before the Pope, he shook the hand and launched into what, in comparison to the preceding brief encounters, could only be called a little chat. At this precise moment Giovanni Bachelet, a native Roman, who had encouraged several of us, including Walter, to participate in this extraordinary week and who had made it clear to us that he was a devout member of the Church, called out to me across several aisles: “David, who’s that guy up there with Walter?”

My mind has just favored me with the final set of verses from our 60th birthday song. They were delivered in the form of an encore after everyone thought it was all over, and they make an appropriately festive note on which to end this 80th birthday offering:

Happy Birthday, Walter Kohn;
I bet you thought our little song was done,
But like your own career, you see,
It turns about most unexpectedly:

From a lad with a top you have grown to be
The Founding Director of the ITP.
[Chorus:] That boy with a top has gone on to be
The one and only leader of the ITP.

Were the party today, I would change the final line to

A certified Practitioner of Chemistry.

About the Author: N. David Mermin is a professor of physics at Cornell University, a member and former director of Cornell’s Laboratory of Atomic and Solid State Physics, and a founding member of Cornell’s Faculty of Computation and Information. He has done research in statistical physics, low temperature physics, mathematical crystallography, and foundations of quantum mechanics. He is currently interested in the theory of quantum computation and its implications for quantum foundational issues. Contact: Physics Department, Clark Hall, Cornell University, Ithaca, NY 14853-2501, U.S.A.; ndm4@cornell.edu; www.lassp.cornell.edu/lassp_data/NMermin.html
The Bonding of Quantum Physics with Quantum Chemistry

Robert G. Parr

University of North Carolina, Chapel Hill, U.S.A.

The bond that developed between quantum physics and quantum chemistry, that led to the award of a big chemistry prize to the physicist Walter Kohn in 1998, developed not without trial. Here I give an account of it. An element in this bond has been a friendship between Walter Kohn and me. My having reached 80 first, he has already kindly spoken of this\(^1\). Now it is my turn.

In the 20s and early 30s there was a flush of successes in establishing the ability of quantum mechanics to describe the simplest molecules accurately: the Born-Oppenheimer approximation, the nature of chemical bonding, and the fundamentals of molecular spectroscopy. But then the quantitative theory of molecular structure, which we call quantum chemistry, was stymied, by the difficulty of solving the Schrödinger equation for molecules. The senior chemical physicists of the 30s pronounced the problem unsolvable. But the younger theoreticians in the period coming out of WWII thought otherwise. Clearly one could make substantial progress toward the goal of complete solution, because the equation to solve was known and had a simple universal structure. The boundary conditions too were known. It would not be as easy as handling an infinite periodic solid, but a number of us set to work. The special demand of chemistry was to quantify very small molecular changes. Successes came slowly, but with the development of computers and a lot of careful, clever work, by the 90s the quantitative problem was essentially solved. The emergent hero of the chemical community was John Pople, whose systematic strategy and timely method developments were decisive. The methods of what is termed “ab initio” quantum chemistry became available and used everywhere.

Over the years the quantum chemists did a lot more than gradually improve their ability to calculate wavefunctions and energies from Schrödinger’s equation. All the while they have served molecular spectroscopy, physical inorganic chemistry, and physical organic chemistry. Relevant for the present story was the development by Per-Olov Löwdin in 1955 of the density matrix

reduction of the Schrödinger equation, especially the identification and mathematical physics of natural spin orbitals and their occupation numbers. The hope was, although hope floundered, that the Schrödinger problem could be resolved in terms of the first- and second-order density matrices. Foundering came because of the difficulty of incorporating the Pauli principle.

Beginning way back in the 20s, Thomas and Fermi had put forward a theory using just the diagonal element of the first-order density matrix, the electron density itself. This so-called statistical theory totally failed for chemistry because it could not account for the existence of molecules. Nevertheless, in 1968, after years of doing wonders with various free-electron-like descriptions of molecular electron distributions, the physicist John Platt wrote\(^2\) “We must find an equation for, or a way of computing directly, total electron density.” [This was very soon after Hohenberg and Kohn, but Platt certainly was not aware of HK; by that time he had left physics.]

From the end of the 40s, I was a happy participant in most of these things, ab initio and the rest, although from about 1972 I became pretty much an observer. We plunged into density-functional theory.

DFT soon intoxicated me. There were the magnificent Hohenberg-Kohn and Kohn-Sham 1964-65 papers. The Xalpha method of John Slater was popular in those days, but it was not sufficient for the high accuracy needed. And I was much taken by the work of Walter Kohn, whom I had known since 1951. There were many things to do: Improve upon the LDA to reach the accuracy needed for chemical applications. Shift the emphasis on fixed, very large electron number toward variable, small number, since that most concerns chemistry. Enlarge the language to include chemical as well as solid-state concepts. Introduce into DFT, as appropriate, some of the theoretical advances already made within quantum chemistry. All of these things subsequently came about. The methods and concepts of DFT became available and used throughout the chemical community.

I had been on the faculty of Carnegie Institute of Technology for a couple of years when Walter Kohn arrived in 1951. I was aware from the beginning of the strength of physics at Carnegie, especially solid state physics. Fred Seitz was the Head when I arrived, and several other solid state experts also were there. I bought Seitz’s great book for $6.38 and browsed in it, noting in particular the fine description of the Hartree-Fock method (but not finding any treatment of the invariance to unitary transformation of orbitals that is so important for understanding the equivalence of localized and non-localized descriptions of molecules). I enjoyed pleasant interactions with a number of the physicists. Soon after Kohn arrived I had two physicist postdocs, Tadashi Arai from Japan and Fausto Fumi from Italy, who became acquainted with him. On the thesis examination committee of Walter’s graduate student Sy Vosko, I learned that it was okay to use trial wavefunctions with discontinuous

\(^2\) John R. Platt, letter to RGP, dated October 23, 1968
derivatives. I was pleased to attend an evening party at the Kohns, and I was disappointed when Walter left Carnegie for elsewhere.

I do not recall when I first heard of the Hohenberg–Kohn–Sham papers, but I do know that the quantum chemistry community at first paid little attention to them. In June of 1966 Lu Sham spoke about DFT at a Gordon Conference. But in those days there was more discussion about another prescription that had been on the scene since 1951, the Slater Xa method. The Xa method was a well-defined, substantial improvement over the Thomas-Fermi method, a sensible approximation to exact Kohn-Sham. Debate over Xa went on for a number of years. Slater may never have recognized DFT as the major contribution to physics that it was. [When I asked John Connolly five or six years ago how he thought Slater had viewed DFT, he replied that he felt that Slater regarded it as “obvious”.]

Walter Kohn’s appearance at the Boulder Theoretical Chemistry Conference of 1975 was memorable. On June 24 he presented a formal talk, in which he outlined DFT to the assemblage of skeptical chemists. There were many sharp questions and a shortage of time, so the chair of the conference decided to schedule a special session for the afternoon of June 26. With quite a crowd for an informal extra session like this, Walter held forth on his proof. In his hand he held a reprint of the HK paper, from which he quietly read as he slowly proceeded: “...and now we say...”. The audience sobered down quickly. It was a triumph. The interest of quantum chemists in DFT began to grow at about this time.

Our group began contributing to DFT in the 1970s. In some of our first work, my graduate student Robert Donnelly generalized the original idea to
functionals of the first-order density matrix. In 1977 I described the central
result at Walter Kohn’s luncheon seminar in San Diego: All natural orbitals
with non-zero occupation numbers have the same chemical potential. Dis-
cussing this with Walter at the blackboard afterwards, I remember his saying
“This must be correct”. [Walter himself recently recalled this incident.] First-
order density matrix functional theory is receiving fresh attention nowadays.

As we ourselves kept plugging along, the quantum chemical community
largely was negative about DFT, even antagonistic. Their “house journal”
International Journal of Quantum Chemistry, in 1980 published a pointed
criticism of it 3: “There seems to be a misguided belief that a one-particle
density can determine the exact N-body ground state”. In 1982 Mel Levy and
John Perdew replied with a letter that was both incisive and eloquent 4: “The
belief is definitely not misguided”. Yet, in the same issue of IJQC, the editors
called for further discussion of the “controversial” subject 5. It was going to
be awhile before quantum chemists were convinced.

Over the period 1979–1982, Mel Levy supplied a major advance with in-
troduction and careful discussion of the constrained search formulation of
density functional theory 4. This greatly heightened confidence in the theory
(and it still does!). Then in 1983 came Elliott Lieb’s masterly detailed analy-
sis, which validated DFT as rigorous mathematical physics. [Once in the 70s
I asked Barry Simon, the mathematician who with Lieb had done a famous
rigorous analysis of the Thomas-Fermi theory, what his opinion was of DFT.
“It may be good physics”, he said, “but it is not good mathematics”. Lieb’s
paper signaled the end of the period of doubt about DFT. The space for fur-
ther development was now wide open and the interest of chemists began to
accelerate.

What computational chemists wanted above all else was calculational
methods for molecules, and the LDA just was not enough. The need for more
accurate exchange-correlation functionals was met in the 80s, with an accu-

(1982)
affinity, hardness, softness, and local softness. Much more too. DFT is a single language that covers atoms, molecules, clusters, surfaces, and solids.

Walter Kohn has been a great help to many scientists over many years, an expert consultant and helpmate and a fine, unobtrusive, even-handed host of good meetings in lovely places. We thank him. In recent years I have discussed with him (among other things), circulant orbitals, the monotonic density theorem, and the information theory point of view on what constitutes an atom in a molecule), the latter during a stolen few minutes in a Stockholm hotel in December of 1999. Walter may or may not “like” chemistry, and he claims not to have studied chemistry in the university. But what does one call a great teacher of chemical principles? I would say, CHEMIST, full caps.

About the Author: Robert Parr is Distinguished Professor in the Department of Chemistry, University of North Carolina. Author (with Weitao Yang) of Density-Functional Theory of Atoms and Molecules (Oxford), he has been doing research in quantum chemistry continuously since the late 1940s. Contact: Department of Chemistry, University of North Carolina, Chapel Hill, N.C. 27599-3290, U.S.A.; rgparr@email.unc.edu

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6 RGP was delighted to be Walter Kohn’s guest. See photograph on page 221) and P. Hohenberg, this volume, p. 120
7 E. Eliel, This volume, p. 79
Happy Birthday, Walter

Lu J. Sham

University of California, San Diego, U.S.A.

“Birthdays are good for you, Mr. Wilson. The more you have, the longer you live.”


To the calls for contributions to this book to celebrate Walter Kohn’s eightieth birthday, there will be an outpouring of fond good wishes, as there were for his sixtieth birthday and his seventy fifth. Walter treats his work colleagues as friends and family. Walter considers Quin Luttinger, a long time collaborator, one of his best friends. When I was his postdoc in La Jolla (1963–66), he introduced the students of Quin Luttinger to me as cousins. T.V. Ramankrishnan was one who comes to mind. Indeed, from Walter’s legion of collaborators, postdocs and students, I have made many friends, with Pierre Hohenberg, Norton Lang, Vinay Ambegaokar, Bill Butler (some of whom would no doubt have their own say in this book), even though we do not meet often.

Besides his renowned work on density functional theory, Walter Kohn has made many fundamental contributions to solid state physics. With Quin Luttinger, he provided a method for computing the properties of charge carriers in semiconductors which is used to this day. His theory of the shallow donor was recently celebrated since impurities are of course the lifeblood of semiconductor devices. He treated the extra electron in a semiconductor as the (N+1)th electron with interaction in the many-body system and showed that it could be treated as a single particle with some “renormalized” properties, so that the other N electrons could be put out of mind. This argument becomes relevant again recently when it was challenged whether the semiconductor with its gazillions of electrons could be used for quantum computing or information purposes. From the same line of research came also the “Kohn Theorem” that the electron interaction does not enter into cyclotron motion (in the sense of a rotation of the system). This also comes to prominent consideration for recent experiments in micro- and nano-structures.
Walter and Quin also attacked the basic problem of the day, to prove the existence of the Fermi surface. He told me that they failed. Actually, on closer examination of their work, they showed that at very, very low temperatures, pure electron-electron interaction could induce superconductivity which meant that the Fermi surface could not be a ground state property.

Then there is what in polite company is known as the Kohn effect in lattice vibrations of metals. Kohn gave a characteristically simple but elegant argument that the electron response to the lattice vibration would change as the excited electron momentum increase exceeds the Fermi surface diameter. At first the experiment could be made to show the Kohn effect only if one examined the atom-atom interaction as a function of the distance to interpret the Kohn effect as a Fourier transform of the Friedel oscillations. As quasi-one and two dimensional metals became reality, the Kohn effect appeared directly in neutron measurements. In Walter’s group in La Jolla in the sixties and seventies, the Kohn effect was always referred to as the Kohn anomaly accompanied by a big smile, for the thought of another Kohn anomaly was so impossible. (Really? Read on!)

With the founding chairman Keith Bruckner, Walter Kohn built the Physics Department at University of California at San Diego into a well-known one in a short time. I got a sense of this achievement from the victims of his raids of talent. In the eighties, our department recruited in succession three physicists from Bell Laboratories, Duncan Haldane, Cliff Surko, and Bob Dynes. Then I used to spend summers at Bell. One summer when I showed up, there was a clamor in the tearoom to give me a red escort badge, which meant that I had to be accompanied everywhere, including the bathroom. People were upset because Charles Tu had just left for San Diego also. I was then a dean but I had nothing to do with some of these recruitments. A friend who was at Bell since the fifties explained that people thought it was deja vu from the fifties, when after every summer visit by Walter Kohn, someone would move to La Jolla: Berndt Matthias, George Feher, Harry Suhl.

Walter has a larger effect on the San Diego campus. He was the driving force behind the start of the Judaic studies with an endowed chair. He was instrumental in setting up rules and regulations on the young campus. Right after he left in the late seventies for Santa Barbara as the founding director of the Institute for Theoretical Physics, I was chair of the Committee of Educational Policy at the San Diego campus. Every time (surprisingly frequent) the committee got embroiled in some hot campus educational affair, the supremely capable Lynn Harris would dig up a memo from the fifties which set a sensible precedent on what to do. The signature at the bottom of the memo would be the familiar flourish of Walter Kohn, chair, CEP.

Walter had a great interest in Jewish history. When we were in Krakow for a conference in June 1994, we went to visit a famous historic Jewish quarter north of the city. Walter gave my wife Georgina and me a detailed and delightful tour of the synagogue. We ended up having tea in a book store in the quarter (see photo on page 33). Upon Walter’s recommendation, we bought
a beautiful picture book, “Journeys to the Promised Land”. The book was put together in Israel, published in London, and printed and bound in Hong Kong.

Walter is interested in politics. He is one of the few people I know who actually act on his belief. He is, for example, active in presenting arguments to persuade the regents of the University of California to discontinue management of the nation’s weapons laboratories. About ten years ago, during my sabbatical leave in Santa Barbara, Georgina and I were invited by Walter to lunch at home with a few friends (which meant several tables in the garden). Mara was out of town and so Walter proudly announced that he would take care of lunch. Everything was going swimmingly until the main course. The subject of the regents approval of the next contract on the national laboratory management came up at our table. Incautiously I said that I thought the contact of lab personnel with the academic faculty was a good point of having the University managing the labs. Whereupon Walter audibly organized his thoughts and marshaled his facts and proceeded to treat us with a fact-filled and carefully analyzed account of why the University should have nothing to do with the weapons labs. By that time, all the other tables were waiting for the next course and Georgina was throwing looks at me which were not, as one comedian put it, the looks for which I married her. Walter calmly stood up and announced that he would serve a delicious (with a deprecating characteristic sound somewhere between a giggle and a laugh) dessert right after he finished this important lecture (dead earnest but the exact words are lost in the fog of history) to Lu.

Everyone who knows Walter must know the care with which he speaks. He is an interesting raconteur. But few people without personal experience would believe that he is a daredevil in sports. The first inkling came with a phone call from the hospital around March, 1965. Walter asked me if I could take his place for an invited talk (the first one on the density functional theory) at the March Meeting of the American Physical Society. He had broken his shoulder in several places in a bad fall while skiing. Since he was known as an excellent skier, I pressed for details. Well, on the wide slope in June mountain, he saw a man going straight down without traversing, flying over the moguls gracefully. That reminded him of his youth in Austria where he used to build ramps by hand for ski jumps. So Walter decided to follow suit. Oh yeah, he broke his leg the time in Austria too.

One summer in La Jolla after a storm, the wind was stiff and the surf was high. The surfers rushed to the beach. And Walter invited me to try out his new sailfish. (To this day, I do not know if the weather and the invitation were connected or not.) Both families came to Kellogg’s beach. Marilyn (his eldest daughter) joined us sailing. We swam the sailfish over the surf with a little difficulty. The stiff wind did propel the sail of the supersized surf board rather nicely until a squall wiped us out (capsized the boat). After vain attempt at righting the sail, we started pushing the sailfish towards shore. The sailfish was ripped off our hands by the heavy surf and we had to swim past the rough
Happy Birthday, Walter

stuff to reach land. We all sprawled on the sand while a lifeguard stood over us listing all the dangerous acts we committed. As I dozed off, he was saying that, for starters, the sailfish was built for two . . . .

There was the triumph of the soccer match at the first picnic of the physics department. The graduate students challenged the faculty to games. So a soccer and a baseball match were negotiated. The faculty was small and press-ganged the postdocs into service. The faculty members were very good soccer players. I was in the backfield with nothing much to do except admiring these old men (old is relative as I now find out), especially Walter Kohn and George Feher, running circles around the students. They seemed to have a simple strategy. They simply kick the soccer ball above the ground but with an arm’s reach of the students. The graduate students grew up mostly playing American football in which no players except a designated one are allowed to kick the ball. So they instinctively reached out to bat the ball down. The faculty side won with the advantage of a rash of penalty goals. Then they were too tired to play baseball, which was just as well, from the point of view of preserving the soccer victory.

With these reminiscences, I wish Walter a happy birthday and many happy returns.

About the Author: Lu J. Sham is a condensed matter theorist, currently a professor of physics at the University of California, San Diego. He was fortunate to have been taught by Harry Jones (Imperial College, London), John Ziman, Volker Heine, Phil Anderson, Jim Phillips, David Thouless, and Neville Mott (at Cambridge), and Walter Kohn (La Jolla). He is having great fun working with a bunch of collaborators on quantum computing and on spintronics. Contact: Department of Physics, University of California at San Diego, 9500 Gilman Drive, La Jolla, CA 92093-0319, U.S.A.; lsham@ucsd.edu; physics.ucsd.edu/~ljssst/ljs.html
My first personal encounter with Walter was a rather distant one, in fact one that Walter was not even directly aware of, but one that was the first step on a path that had a profound and direct effect on the course of my own career. In 1982, the Institute for Theoretical Physics at UCSB organized a major workshop on Polymer Dynamics (I still have the t-shirt). I was five years beyond my Ph.D. that year, a recently tenured associate professor at the University of Minnesota, and deeply involved in research on polymer diffusion in entangled polymer fluids. The ITP workshop included participation and remarks by Walter both on the field of the workshop and on the general nature of ITP.

I remember Walter as not only being engaged in the in the workshop but also being, then as now, regarded with exceptional respect and collegiality, among a very distinguished group of soft condensed matter physicists, peppered with the occasional engineer. I am sure that the openness, indeed proactive inclusiveness, which has characterized ITP’s approach to disciplines other than physics from the beginning, is due in no small part to Walter Kohn’s ecumenical view of science.

Not only was that workshop an exciting and stimulating boost to my career and the research directions I was pursuing at the time but it was the definite beginning of a series of interactions I had with the University of California, Santa Barbara, culminating seventeen years later and four years ago in my permanently joining the faculty of UCSB. This initial encounter with UCSB, via the ITP led by Walter, was followed three years later by an offer to join the UCSB faculty of chemical engineering. The College of Engineering in 1985 was led by Robert Mehrabian, an incredibly dynamic and persuasive leader, who is arguably the individual most responsible for leading UCSB to preeminence in materials research. Nevertheless, I declined the offer. However, the fine early impressions gained through the experience of considering this opportunity, coupled with my early ITP exposure, created a very strong base of affinity and admiration for UCSB in me.
Twice more, in 1989, and again in 1994, I returned to ITP for a month or so each time to participate in programs on polymer and biomolecular physics. In the intervening years, my own research interests turned more toward surface properties of macromolecules. One of several lines we pursued in this area, in collaboration with Arup Chakraborty (now chair of chemical engineering at U.C. Berkeley) and Ted Davis (now dean at Minnesota), was attempting to understand the chemistry, both macromolecular and electronic structural, of polymer adhesion to solid surfaces. Density functional theory proved to be a powerful tool to give insight into the nature of chemical bonding at polymer-solid interfaces. A series of publications in the late 1980's and early 1990's with this set of collaborators showed, for example, how common polar functionalities, such as carbonyl groups, play the key role in creating chemical bonds at many interfaces of practical importance. This finding is one of many examples of why and how engineers have been enabled to tackle important practical problem via tools that Walter and his work have provided.

This chain of interactions with UCSB, all routed in organizations and scientific innovations led by Walter Kohn, led me steadily and, in retrospect, rather relentlessly to acceptance in 1999 of offer of the position of dean in the College of Engineering at UCSB. The collegiality and interdisciplinarity instilled early in the ITP under Walter’s leadership remain a hallmark, in fact, defining characteristic of UCSB’s approach to research. All of us at UCSB owe Walter a great debt for his scientific leadership and also for his example as a creative, inspirational colleague.

About the Author: Matthew Tirrell received his undergraduate education in Chemical Engineering at Northwestern University and his Ph.D. in 1977 in Polymer Science from the University of Massachusetts. He is currently Dean of the College of Engineering at the University of California, Santa Barbara. From 1977 to 1999 he was on the faculty of Chemical Engineering and Materials Science at the University of Minnesota, where he served as head of the department from 1995 to 1999. His research has been in polymer surface properties including adsorption, adhesion, surface treatment, friction, lubrication and biocompatibility. He has co-authored about 250 papers and one book and has supervised about 60 Ph.D. students. Professor Tirrell has been a Sloan and a Guggenheim Fellow, a recipient of the Camille and Henry Dreyfus Teacher-Scholar Award and has received the Allan P. Colburn, Charles Stine and the Professional Progress Awards from AIChE. He was elected to the National Academy of Engineering in 1997, became a Fellow of the American Institute of Medical and Biological Engineers in 1998, was elected Fellow of the American Association for the Advancement of Science in 2000 and was named Institute Lecturer for the American Institute of Chemical Engineers in 2001. Contact: Office of the Dean, College of Engineering, University of California, Santa Barbara, CA 93106-5130, U.S.A.; tirrell@engineering.ucsb.edu; www.chemengr.ucsb.edu/people/faculty/tirrell.html
Walter Kohn, World Citizen and Professor Extraordinaire

Henry T. Yang

University of California, Santa Barbara, U.S.A.

How can I find the words to express all that Walter Kohn means to me, and to our University of California, Santa Barbara campus?

Walter has been a member of our faculty since 1979, when he became the founding director of the National Science Foundation’s Institute for Theoretical Physics at UCSB. The institute’s goal was to bring together leading scientists from all over the globe to share their ideas and work together on major topics in theoretical physics and related fields. Under Professor Kohn’s dynamic and visionary leadership, it quickly developed into a premier re-
search center that has been looked to as a pioneering model by institutions and countries all over the world. Today, more than 1,000 prominent physicists and scientists from diverse backgrounds participate annually in the institute's programs and conferences.

The institute’s unique mode of operation stimulates collaborative interactions that lead to new insights and discoveries at the frontiers of science. Physics Professor Brian Green of Columbia University recently stated, “Theoretical physics would not be where it is today without this vital institute” (now known as the Kavli Institute for Theoretical Physics). World-renowned physicist Stephen Hawking has said, “Some of my best work has been done there.” The KITP is a reflection of Professor Kohn’s scientific acumen and creative leadership, and our campus is proud that the building that houses the institute is named in his honor.

Nobel Laureate Walter Kohn (© D. Folks)

In 1998, Professor Kohn was awarded the Nobel Prize in Chemistry for his leading role in the development of the density functional theory, now widely used in quantum chemistry. This theory simplifies the calculations to obtain knowledge of the inner structure of molecules, the building blocks of matter. Professor Kohn’s discovery meant that scientists could take complex molecules such as proteins and use computer-generated models to predict their chemical behavior. This theory has enabled the development of new drugs, led to a new understanding of the make-up of interstellar matter, and provided insight into
Walter Kohn, World Citizen and Professor Extraordinaire

how chemical reactions affect our ozone layer, to name just a few examples. I've been told that about half of all publications in quantum chemistry now make reference to his theory.

I remember the headlines of our campus newspaper after Walter Kohn received his Nobel Prize: “Walter Kohn: A Nobelist with Heart.” Walter is such a special person, and so beloved on our campus, it made us all tremendously happy to see him recognized with this prize. I said at the time that Walter’s inspiration and impact on our proud UCSB campus was beyond what I could describe. That is still true today.

Professor Kohn has received a number of other prestigious awards for his scientific research, including the Oliver Buckley Prize in Solid State Physics, the Davisson-Germer Prize in Surface Physics, the National Medal of Science, the Feenberg Medal in Many-Body Physics, and the Niels Bohr/UNESCO Gold Medal. Clearly, he is one of the world’s top scientists. At the same time, he is deeply engaged in matters spiritual and societal.

In 2001, Professor Kohn was the inaugural speaker for the Templeton Research Lectures on Science, Religion, and the Human Experience, sponsored by the Templeton Foundation and hosted by UC Santa Barbara. He spoke about the interaction between science and religion, and about how science and technology pose both great promises and great threats to mankind in this global age.

Rollerblading at West Beach, Santa Barbara (© Vic Cox)
Henry T. Yang

I was particularly struck by his statement that “science by itself is not an adequate basis for conducting one’s life.” This has certainly been a guiding principle in his own life. Throughout his career, Professor Kohn has been a mentor and role model for colleagues and students alike. Many have been inspired by his incredible life story and his work to promote tolerance and world peace.

Dr. Kohn is a senior statesman, scientist, and advocate for peace whose life and work have had a profound impact on our world. At the same time, he is a down-to-earth person who is great fun to be around. He is most well known to all of us as a patient, kind, and wise man who gives his time generously to help others. His wide-ranging interest in the arts and humanities matches his enthusiasm for the social causes he holds dear. He is an aficionado of classical music and a great scholar whose passion for literature spans centuries and cultures. I also hear he makes a delicious ratatouille. And watch out when Walter gets on his rollerblades!

Walter is deeply devoted to the university and our community. For example, he recently gave the keynote address at the County of Santa Barbara’s installation of elected officials and convening of the 2003 Board of Supervisors. To prepare for it, he researched deeply and broadly on the topic, and then he educated us all on the importance of the interdependent and mutually beneficial partnership between UCSB and the community. As another example, Walter has been active in Santa Barbara’s Building Bridges Community Coalition, a group dedicated to promoting tolerance and celebrating diversity through collaborative community projects.
Walter Kohn, World Citizen and Professor Extraordinaire

My wife, Dilling, and I have gotten to know Walter and Mara very well since we came to UCSB in 1994. Their friendship is a gift. The hours we’ve spent talking and laughing together are ones I treasure dearly. Walter always has such incredible stories to share, full of wisdom, humor, and heart. I value tremendously the guidance and sage advice he has given me over the years.

Two years ago, we presented Walter with the Santa Barbara medal, our campus’s highest honor. The inscription read:

“Walter Kohn
Maestro of the molecular,
activist intellectual,
Nobel Laureate, colleague –
UCSB proudly salutes you.”

Our campus once again proudly salutes our colleague, friend, teacher, and mentor – Professor Walter Kohn. On behalf of the UCSB community, it is my great honor and delight to congratulate Walter on the very special occasion of his 80th birthday.

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Appendix I – Autobiography

Walter Kohn

University of California, Santa Barbara, U.S.A.

I suppose I am not the first Nobelist who, on the occasion of receiving this Prize, wonders how on earth, by what strange alchemy of family background, teachers, friends, talents and especially accidents of history and of personal life he or she arrived at this point. I have browsed in previous volumes of “Les Prix Nobel” and I know that there are others whose eventual destinies were foreshadowed early in their lives – mathematical precocity, champion bird watching, insatiable reading, mechanical genius. Not in my case, at least
not before my late teens. On the contrary: An early photo of my older sister and myself, taken at a children’s costume party in Vienna – I look about 7 years old – shows me dressed up in a dark suit and a black top hat, toy glasses pushed down my nose, and carrying a large sign under my arm with the inscription “Professor Know-Nothing”.

Here then is my attempt to convey to the reader how, at age 75, I see my life which brought me to the present point: a long-retired professor of theoretical physics at the University of California, still loving and doing physics, including chemical physics, mostly together with young people less than half my age; moderately involved in the life of my community of Santa Barbara and in broader political and social issues; with unremarkable hobbies such as listening to classical music, reading (including French literature), walking with my wife Mara or alone, a little cooking (unjustifiably proud of my ratatouille); and a weekly half hour of relaxed roller blading along the shore, a throwback to the ice-skating of my Viennese childhood. My three daughters and three grandchildren all live in California and so we get to see each other reasonably often.

I was naturalized as an American citizen in 1957 and this has been my primary self-identity ever since. But, like many other scientists, I also have a strong sense of global citizenship, including especially Canada, Denmark, England, France and Israel, where I have worked and lived with a family for considerable periods, and where I have some of my closest friends.

My feelings towards Austria, my native land, are – and will remain – very painful. They are dominated by my vivid recollections of 1 1/2 years as a Jewish boy under the Austrian Nazi regime, and by the subsequent murder of my parents, Salomon and Gittel Kohn, of other relatives and several teachers, during the holocaust. At the same time I have in recent years been glad to work with Austrians, one or two generations younger than I: Physicists, some teachers at my former High School and young people (Gedenkdiener) who face the dark years of Austria’s past honestly and constructively.

On another level, I want to mention that I have a strong Jewish identity and – over the years – have been involved in several Jewish projects, such as the establishment of a strong program of Judaic Studies at the University of California in San Diego.

My father, who had lost a brother, fighting on the Austrian side in World War I, was a committed pacifist. However, while the Nazi barbarians and their collaborators threatened the entire world, I could not accept his philosophy and, after several earlier attempts, was finally accepted into the Canadian Infantry Corps during the last year of World War II. Many decades later I became active in attempts to bring an end to the US-Soviet nuclear arms race and became a leader of unsuccessful faculty initiatives to terminate the role of the University of California as manager of the nuclear weapons laboratories at Los Alamos and Livermore. I offered early support to Jeffrey Leifler, the founder of the student Pugwash movement which concerns itself with global issues having a strong scientific component and in which scientists can play a
useful role. Twenty years after its founding this organization continues strong and vibrant. My commitment to a humane and peaceful world continues to this day. I have just joined the Board of the Population Institute because I am convinced that early stabilization of the world’s population is important for the attainment of this objective.

Walter Kohn with his sister and parents, about 1932

After these introductory general reflections from my present vantage point I would now like to give an idea of my childhood and adolescence. I was born in 1923 into a middle class Jewish family in Vienna, a few years after the end of World War I, which was disastrous from the Austrian point of view. Both my parents were born in parts of the former Austro-Hungarian Empire, my father in Hodonin, Moravia, my mother in Brody, then in Galicia, Poland, now in the Ukraine. Later they both moved to the capital of Vienna along with their parents. I have no recollection of my father’s parents, who died relatively young. My maternal grandparents Rappaport were orthodox Jews who lived a simple life of retirement and, in the case of my grandfather, of prayer and the study of religious texts in a small nearby synagogue, a Schul as it was called. My father carried on a business, Postkartenverlag Brüder Kohn Wien I, whose main product was high quality art postcards, mostly based on paintings by contemporary artists which were commissioned by his firm. The business had flourished in the first two decades of the century but then, in part due to the death of his brother Adolf in World War I, to the dismantlement of
the Austrian monarchy and to a worldwide economic depression, it gradually fell on hard times in the 1920s and 1930s. My father struggled from crisis to crisis to keep the business going and to support the family. Left over from the prosperous times was a wonderful summer property in Heringsdorf at the Baltic Sea, not far from Berlin, where my mother, sister and I spent our summer vacations until Hitler came to power in Germany in 1933. My father came for occasional visits (The firm had a branch in Berlin). My mother was a highly educated woman with a good knowledge of German, Latin, Polish and French and some acquaintance with Greek, Hebrew and English. I believe that she had completed an academically oriented High School in Galicia. Through her parents we maintained contact with traditional Judaism. At the same time my parents, especially my father, also were a part of the secular artistic and intellectual life of Vienna.

After I had completed a public elementary school, my mother enrolled me in the Akademische Gymnasium, a fine public high school in Vienna's inner city. There, for almost five years, I received an excellent education, strongly oriented toward Latin and Greek, until March 1938, when Hitler Germany annexed Austria. (This so-called Anschluss was, after a few weeks, supported by the great majority of the Austrian population). Until that time my favorite subject had been Latin, whose architecture and succinctness I loved. By contrast, I had no interest in, nor apparent talent for, mathematics.
which was routinely taught and gave me the only C in high school. During this time it was my tacit understanding that I would eventually be asked to take over the family business, a prospect which I faced with resignation and without the least enthusiasm.

The Anschluss changed everything: The family business was confiscated but my father was required to continue its management without any compensation; my sister managed to emigrate rather promptly to England; and I was expelled from my school.

In the following fall I was able to enter a Jewish school, the Chajes Gymnasium, where I had two extraordinary teachers: In physics, Dr. Emil Nohel, and in mathematics Dr. Victor Sabbath. While outside the school walls arbitrary acts of persecution and brutality took place, on the inside these two inspired teachers conveyed to us their own deep understanding and love of their subjects. I take this occasion to record my profound gratitude for their inspiration to which I owe my initial interest in science. (Alas, they both became victims of Nazi barbarism).

I note with deep gratitude that twice, during the Second World War, after having been separated from my parents who were unable to leave Austria, I was taken into the homes of two wonderful families who had never seen me before: Charles and Eva Hauff in Sussex, England, who also welcomed my older sister, Minna. Charles, like my father, was in art publishing and they had a business relationship. A few years later, Dr. Bruno Mendel and his wife Hertha of Toronto, Canada, took me and my friend Joseph Eisinger into their family. (They also supported three other young Nazi refugees). Both of these families strongly encouraged me in my studies, the Hauffs at the East Grinstead County School in Sussex and the Mendels at the University of Toronto. I cannot imagine how I might have become a scientist without their help.

My first wife, Lois Kohn, gave me invaluable support during the early phases of my scientific career; my present wife of over 20 years, Mara, has supported me in the latter phases of my scientific life. She also created a wonderful home for us, and gave me an entire new family, including her father Vishniac, a biologist as well as a noted photographer of pre-war Jewish communities in Eastern Europe, and her mother Luta. (They both died rather recently, well into their nineties).

After these rather personal reminiscences I now turn to a brief description of my life as a scientist.

When I arrived in England in August 1939, three weeks before the outbreak of World War II, I had my mind set on becoming a farmer (I had seen too many unemployed intellectuals during the 1930s), and I started out on a training arm in Kent. However, I became seriously ill and physically weak with meningitis, and so in January 1940 my “acting parents”, the Hauffs, arranged for me to attend the above-mentioned county school, where – after a period of uncertainty – I concentrated on mathematics, physics and chemistry.
However, in May 1940, shortly after I had turned 17, and while the German army swept through Western Europe and Britain girded for a possible German air-assault, Churchill ordered most male “enemy aliens” (i.e., holders of enemy passports, like myself) to be interned (“Collar the lot” was his crisp order). I spent about two months in various British camps, including the Isle of Man, where my school sent me the books I needed to study. There I also audited, with little comprehension, some lectures on mathematics and physics, offered by mature interned scientists.

In July 1940, I was shipped on, as part of a British convoy moving through U-boat-infested waters, to Quebec City in Canada; and from there, by train, to a camp in Trois Rivieres, which housed both German civilian internees and refugees like myself. Again various internee-taught courses were offered. The one which interested me most was a course on set-theory given by the mathematician Dr. Fritz Rothberger and attended by two students. Dr. Rothberger, from Vienna, a most kind and unassuming man, had been an advanced private scholar in Cambridge, England, when the internment order was issued. His love for the intrinsic depth and beauty of mathematics was gradually absorbed by his students.

Later I was moved around among various other camps in Quebec and New Brunswick. Another fellow internee, Dr. A. Heckscher, an art historian, organized a fine camp school for young people like myself, whose education had been interrupted and who prepared to take official Canadian High School exams. In this way I passed the McGill University junior Matriculation exam and exams in mathematics, physics and chemistry on the senior matriculation level. At this point, at age 18, I was pretty firmly looking forward to a career in physics, with a strong secondary interest in mathematics.

I mention with gratitude that camp educational programs received support from the Canadian Red Cross and Jewish Canadian philanthropic sources. I also mention that in most camps we had the opportunity to work as lumberjacks and earn 20 cents per day. With this princely sum, carefully saved up, I was able to buy Hardy’s Pure Mathematics and Slater’s Chemical Physics, books which are still on my shelves. In January 1942, having been cleared by Scotland Yard of being a potential spy, I was released from internment and welcomed by the family of Professor Bruno Mendel in Toronto. At this point I planned to take up engineering rather than physics, in order to be able to support my parents after the war. The Mendels introduced me to Professor Leopold Infeld who had come to Toronto after several years with Einstein. Infeld, after talking with me (in a kind of drawing room oral exam), concluded that my real love was physics and advised me to major in an excellent, very stiff program, then called mathematics and physics, at the University of Toronto. He argued that this program would enable me to earn a decent living at least as well as an engineering program.

However, because of my now German nationality, I was not allowed into the chemistry building, where war work was in progress, and hence I could not enroll in any chemistry courses. (In fact, the last time I attended a chemistry
class was in my English school at the age of 17.) Since chemistry was required, this seemed to sink any hope of enrolling. Here I express my deep appreciation to Dean and head of mathematics, Samuel Beatty, who helped me, and several others, nevertheless to enter mathematics and physics as special students, whose status was regularized one or two years later.

I was fortunate to find an extraordinary mathematics and applied mathematics program in Toronto. Luminous members whom I recall with special vividness were the algebraist Richard Brauer, the non-Euclidean geometer, H.S.M. Coxeter, the aforementioned Leopold Infeld, and the classical applied mathematicians John Lighton Synge and Alexander Weinstein. This group had been largely assembled by Dean Beatty. In those years the University of Toronto team of mathematics students, competing with teams from the leading North-American Institutions, consistently won the annual Putman competition. (For the record I remark that I never participated). Physics too had many distinguished faculty members, largely recruited by John C. McLennan, one of the earliest low temperature physicists, who had died before I arrived. They included the Raman specialist H.L. Welsh, M.F. Crawford in optics and the low-temperature physicists H.G. Smith and A.D. Misener. Among my fellow students was Arthur Schawlow, who later was to share the Nobel Prize for the development of the laser.

Walter Kohn as a Canadian soldier, 1944/45
During one or two summers, as well as part-time during the school year, I worked for a small Canadian company which developed electrical instruments for military planes. A little later I spent two summers, working for a geophysicist, looking for (and finding!) gold deposits in northern Ontario and Quebec.

After my junior year I joined the Canadian Army. An excellent upper division course in mechanics by A. Weinstein had introduced me to the dynamics of tops and gyroscopes. While in the army I used my spare time to develop new strict bounds on the precession of heavy, symmetrical tops. This paper, “Contour Integration in the Theory of the Spherical Pendulum and the Heavy Symmetrical Top” was published in the Transactions of American Mathematical Society. At the end of one year’s army service, having completed only 2 1/2 out of the 4-year undergraduate program, I received a war-time bachelor’s degree “on-active-service” in applied mathematics.

In the year 1945–6, after my discharge from the army, I took an excellent crash master’s program, including some of the senior courses which I had missed, graduate courses, a master’s thesis consisting of my paper on tops and a paper on scaling of atomic wave-functions.

My teachers wisely insisted that I do not stay on in Toronto for a Ph.D, but financial support for further study was very hard to come by. Eventually I was thrilled to receive a fine Lehman fellowship at Harvard. Leopold Infeld recommended that I should try to be accepted by Julian Schwinger, whom he knew and who, still in his 20s, was already one of the most exciting theoretical physicists in the world.

Arriving from the relatively isolated University of Toronto and finding myself at the illustrious Harvard, where many faculty and graduate students had just come back from doing brilliant war-related work at Los Alamos, the MIT Radiation Laboratory, etc., I felt very insecure and set as my goal survival for at least one year. The Department Chair, J.H. Van Vleck, was very kind and referred to me as the Toronto-Kohn to distinguish me from another person who, I gathered, had caused some trouble. Once Van Vleck told me of an idea in the band-theory of solids, later known as the quantum defect method, and asked me if I would like to work on it. I asked for time to consider. When I returned a few days later, without in the least grasping his idea, I thanked him for the opportunity but explained that, while I did not yet know in what subfield of physics I wanted to do my thesis, I was sure it would not be in solid state physics. This problem then became the thesis of Thomas Kuhn, (later a renowned philosopher of science), and was further developed by myself and others. In spite of my original disconnect with Van Vleck, solid state physics soon became the center of my professional life and Van Vleck and I became lifelong friends.

After my encounter with Van Vleck I presented myself to Julian Schwinger requesting to be accepted as one of his thesis students. His evident brilliance as a researcher and as a lecturer in advanced graduate courses (such as waveguides and nuclear physics) attracted large numbers of students, including
many who had returned to their studies after spending “time out” on various war-related projects.

I told Schwinger briefly of my very modest efforts using variational principles. He himself had developed brilliant new Green’s function variational principles during the war for wave-guides, optics and nuclear physics (Soon afterwards Green’s functions played an important role in his Nobel-Prize-winning work on quantum electrodynamics). He accepted me within minutes as one of his approximately 10 thesis students. He suggested that I should try to develop a Green’s function variational method for three-body scattering problems, like low-energy neutron-deuteron scattering, while warning me ominously, that he himself had tried and failed. Some six months later, when I had obtained some partial, very unsatisfactory results, I looked for alternative approaches and soon found a rather elementary formulation, later known as Kohn’s variational principle for scattering, and useful for nuclear, atomic and molecular problems. Since I had circumvented Schwinger’s beloved Green’s functions, I felt that he was very disappointed. Nevertheless he accepted this work as my thesis in 1948. (Much later L. Fadeev offered his celebrated solution of the three-body scattering problem).

My Harvard friends, close and not so close, included P.W. Anderson, N. Bloembergen, H. Broida (a little later), K. Case, F. De Hoffman, J. Eisenstein, R. Glauber, T. Kuhn, R. Landauer, B. Mottelson, G. Pake, F. Rohrlich, and C. Slichter. Schwinger’s brilliant lectures on nuclear physics also attracted many students and Postdocs from MIT, including J. Blatt, M. Goldberger, and J.M. Luttinger. Quite a number of this remarkable group would become lifelong friends, and one — J.M. “Quin” Luttinger — also my closest collaborators for 13 years, 1954–66. Almost all went on to outstanding careers of one sort or another.

I was totally surprised and thrilled when in the spring of 1948 Schwinger offered to keep me at Harvard for up to three years. I had the choice of being a regular post-doctoral fellow or dividing my time equally between research and teaching. Wisely — as it turned out — I chose the latter. For the next two years I shared an office with Sidney Borowitz, later Chancellor of New York University, who had a similar appointment. We were to assist Schwinger in his work on quantum electrodynamics and the emerging field theory of strong interactions between nucleons and mesons. In view of Schwinger’s deep physical insights and celebrated mathematical power, I soon felt almost completely useless. Borowitz and I did make some very minor contributions, while the greats, especially Schwinger and Feynman, seemed to be on their way to unplumbed, perhaps ultimate depths.

For the summer of 1949, I got a job in the Polaroid laboratory in Cambridge, Mass., just before the Polaroid camera made its public appearance. My task was to bring some understanding to the mechanism by which charged particles falling on a photographic plate lead to a photographic image. (This technique had just been introduced to study cosmic rays). I therefore needed
Walter Kohn
to learn something about solid state physics and occasionally, when I encoun-
tered things I didn’t understand, I consulted Van Vleck.

It seems that these meetings gave him the erroneous impression that I
knew something about the subject. For one day he explained to me that he
was about to take a leave of absence and, “since you are familiar with solid
state physics”, he asked me if I could teach a course on this subject, which
he had planned to offer. This time, frustrated with my work on quantum
field theory, I agreed. I had a family, jobs were scarce, and I thought that
broadening my competence into a new, more practical, area might give me
more opportunities.

So, relying largely on the excellent, relatively recent monograph by F.
Seitz, “Modern Theory Of Solids”, I taught one of the first broad courses on
Solid State Physics in the United States. My “students” included several of
my friends, N. Bloembergen, C. Slichter and G. Pake who conducted experi-
ments (later considered as classics) in the brand-new area of nuclear magnetic
resonance which had just been opened up by E. Purcell at Harvard and F.
Bloch at Stanford. Some of my students often understood much more than I,
they were charitable towards their teacher.

At about the same time I did some calculations suggested by Bloember-
gen, on the recently discovered, so-called Knight shift of nuclear magnetic
resonance, and, in this connection, returning to my old love of variational
methods, developed a new variational approach to the study of wavefunctions
in periodic crystals.

Although my appointment was good for another year and a half, I began
actively looking for a more long-term position. I was a naturalized Canadian
citizen, with the warmest feelings towards Canada, and explored every Cana-
dian university known to me. No opportunities presented themselves. Neither
did the very meager US market for young theorists yield an academic offer.
At this point a promising possibility appeared for a position in a new West-
inghouse nuclear reactor laboratory outside of Pittsburgh. But during a visit
it turned out that US citizenship was required and so this possibility too
vanished. At that moment I was unbelievably lucky. While in Pittsburgh, I
stayed with my Canadian friend Alfred Schild, who taught in the mathemat-
ics department at the Carnegie Institute of Technology (now Carnegie Mellon
University). He remarked that F. Seitz and several of his colleaguers had just
left the physics department and moved to Illinois, so that – he thought – there
might be an opening for me there. It turned out that the Department Chair,
Ed Creutz was looking rather desperately for somebody who could teach a
course in solid state physics and also keep an eye on the graduate students
who had lost their “doctor-fathers”. Within 48 hours I had a telegram offering
me a job!

A few weeks later a happy complication arose. I had earlier applied for a
National Research Council fellowship for 1950–51 and now it came through. A
request for a short postponement was firmly denied. Fortunately, Ed Creutz
agreed to give me a one-year leave of absence, provided I first taught a com-
pressed course in solid state physics. So on December 31, 1950 (to satisfy the terms of my fellowship) I arrived in Copenhagen.

Originally I had planned to revert to nuclear physics there, in particular the structure of the deuteron. But in the meantime I had become a solid state physicist. Unfortunately no one in Copenhagen, including Niels Bohr, had even heard the expression “Solid State Physics”. For a while I worked on old projects. Then, with an Indian visitor named Vachaspati (no initial), I published a criticism of Froehlich’s pre-BCS theory of superconductivity, and also did some work on scattering theory.

In the spring of 1951, I was told that an expected visitor for the coming year had dropped out and that the Bohr Institute could provide me with an Oersted fellowship to remain there until the fall of 1952. Very exciting work was going on in Copenhagen, which eventually led to the great “Collective Model of the Nucleus” of A. Bohr and B. Mottelson, both of whom had become close friends. Furthermore my family and I had fallen in love with Denmark and the Danish people. A letter from Niels Bohr to my department chair at Carnegie quickly resulted in the extension of my leave of absence till the fall of 1952.

In the summer of 1951, I became a substitute teacher, replacing an ill lecturer at the first summer school at Les Houches, near Chamonix in France, conceived and organized by a dynamic young French woman, Cécile Morette De Witt. As an “expert” in solid state physics, I offered a few lectures on that subject. Wolfgang Pauli, who visited, when he learned of my meager knowledge of solids, mostly metallic sodium, asked me, true to form, if I was a professor of physics or of sodium. He was equally acerbic about himself. Some 50 years old at the time, he described himself as “a child-wonder in menopause” (“ein Wunderkind in den Wechseljahren”). But my most important encounter was with Res Jost, an assistant of Pauli at the ETH in Zurich, with whom I shared an interest in the so-called inverse scattering problem: given asymptotic information, (such as phase-shifts as function of energy), of a particle scattered by a potential $V(r)$, what quantitative information can be inferred about this potential? Later that year, we both found ourselves in Copenhagen and addressed this problem in earnest. Jost, at the time a senior fellow at the Institute for Advanced Study in Princeton, had to return there before we had finished our work. A few months later, in the spring of 1952, I received an invitation from Robert Oppenheimer, to come to Princeton for a few weeks to finish our project. In an intensive and most enjoyable collaboration, we succeeded in obtaining a complete solution for S-wave scattering by a spherical potential. At about the same time I.M. Gel’fand in the Soviet Union published his celebrated work on the inverse problem. Jost and I remained close lifelong friends until his death in 1989.

After my return to Carnegie Tech in 1952, I began a major collaboration with N. Rostoker, then an assistant of an experimentalist, later a distinguished plasma theorist. We developed a theory for the energy band structure of electrons for periodic potentials, harking back to my earlier experience with
scattering, Green’s functions and variational methods. We showed how to determine the bandstructure from a knowledge of purely geometric structure constants and a small number (∼ 3) of scattering phase-shifts of the potential in a single sphericalized cell. By a different approach this theory was also obtained by J. Korringa. It continues to be used under the acronym KKR. Other work during my Carnegie years, 1950–59, includes the image of the metallic Fermi Surface in the phonon spectrum (Kohn anomaly); exponential localization of Wannier functions; and the nature of the insulating state.

My most distinguished colleague and good friend at Carnegie was G.C. Wick, and my first PhD’s were D. Schechter and V. Ambegaokar. I also greatly benefitted from my interaction with T. Holstein at Westinghouse.

In 1953, with support from Van Vleck, I obtained a summerjob at Bell Labs as assistant of W. Shockley, the co-inventor of the transistor. My project was radiation damage of Si and Ge by energetic electrons, critical for the use of the recently developed semiconductor devices for applications in outer space. In particular, I established a reasonably accurate energy threshold for permanent displacement of a nucleus from its regular lattice position, substantially smaller than had been previously presumed. Bell Labs at that time was without question the world’s outstanding center for research in solid state physics and for the first time, gave me a perspective over this fascinating, rich field. Bardeen, Brattain and Shockley, after their invention of the transistor, were the great heroes. Other world class theorists were C. Herring, G. Wannier and my brilliant friend from Harvard, P.W. Anderson. With a few interruptions I was to return to Bell Labs every year until 1966. I owe this institution my growing up from amateur to professional.

In the summer of 1954 both Quin Luttinger and I were at Bell Labs and began our 13-year long collaborations, along with other work outside our professional “marriage”. (Our close friendship lasted till his death in 1997). The all-important impurity states in the transistor materials Si and Ge, which govern their electrical and many of their optical properties, were under intense experimental study, which we complemented by theoretical work using so-called effective mass theory. In 1957, I wrote a comprehensive review on this subject. We (mostly Luttinger) also developed an effective Hamiltonian in the presence of magnetic fields, for the complex holes in these elements. A little later we obtained the first non-heuristic derivation of the Boltzman transport equation for quantum mechanical particles. There followed several years of studies of many-body theories, including Luttinger’s famous one-dimensional “Luttinger liquid” and the “Luttinger’s theorem” about the conservation of the volume enclosed by a metallic Fermi surface, in the presence of electron electron interaction. Finally, in 1966, we showed that superconductivity occurs even with purely repulsive interactions – contrary to conventional wisdom and possibly relevant to the much later discovery of high-\(T_c\) superconductors.

In 1960, when I moved to the University of California San Diego, California, my scientific interactions with Luttinger, then at Columbia University, and with Bell Labs gradually diminished. I did some consulting at the nearby
General Atomic Laboratory, interacting primarily with J. Appel. My university colleagues included G. Feher, B. Maple, B. Matthias, S. Schultz, H. Suhl and J. Wheatley, — a wonderful environment. During my 19-year stay there I typically worked with two postdocs and four graduate students. A high water mark period were the late 1960s, early 1970s, including N. Lang, D. Mermin, M. Rice, L.J. Sham, D. Sherrington, and J. Smith.

I now come to the development of density functional theory (DFT). In the fall of 1963, I spent a sabbatical semester at the École Normale Supérieure in Paris, as guest and in the spacious office of my friend Philippe Nozières. Since my Carnegie days I had been interested in the electronic structure of alloys, a subject of intense experimental interest in both the physics and metallurgy departments. In Paris I read some of the metallurgical literature, in which the concept of the effective charge $e^*$ of an atom in an alloy was prominent, which characterized in a rough way the transfer of charge between atomic cells. It was a local point of view in coordinate space, in contrast to the emphasis on delocalized waves in momentum space, such as Bloch-waves in an average periodic crystal, used for the rough description of substitutional alloys. At this point the question occurred to me whether, in general, an alloy is completely or only partially characterized by its electronic density distribution $n(r)$: In the back of my mind I knew that this was the case in the Thomas-Fermi approximation of interacting electron systems; also, from the “rigid band model” of substitutional alloys of neighboring elements, I knew that there was a 1-to-1 correspondence between a weak perturbing potential $\delta v(r)$ and the corresponding small change $\delta n(r)$ of the density distribution. Finally it occurred to me that for a single particle there is an explicit elementary relation between the potential $v(r)$ and the density, $n(r)$, of the groundstate. Taken together, these provided strong support for the conjecture that the density $n(r)$ completely determines the external potential $v(r)$. This would imply that $n(r)$ which integrates to $N$, the total number of electrons, also determines the total Hamilton $H$ and hence all properties derivable from $H$ and $N$, e.g. the wavefunction of the 17th excited state, $\Psi^{17}(r_1, \ldots, r_N)$! Could this be true? And how could it be decided? Could two different potentials, $v_1(r)$ and $v_2(r)$, with associated different groundstates $\Psi_1(r_1, \ldots, r_N)$ and $\Psi_2(r_1, \ldots, r_N)$ give rise to the same density distribution? It turned out that a simple 3-line argument, using my beloved Rayleigh Ritz variational principle, confirmed the conjecture. It seemed such a remarkable result that I did not trust myself.

By this time I had become friends with another inhabitant of Nozière’s office, Pierre Hohenberg, a lively young American, recently arrived in Paris after a one-year fellowship in the Soviet Union. Having completed some work there he seemed to be “between” problems and I asked if he would be interested in joining me. He was. The first task was a literature search to see if this simple result was already known; apparently not. In short order we had recast the Rayleigh-Ritz variational theorem for the groundstate energy in terms of the density $n(r)$ instead of the many electron wave function $\Psi$, leading to what
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is now called the Hohenberg Kohn (HK) variational principle. We fleshed out this work with various approximations and published it.

Shortly afterwards I returned to San Diego where my new postdoctoral fellow, Lu J. Sham had already arrived. Together we derived from the HK variational principle what are now known as the Kohn-Sham (KS) equations, which have found extensive use by physicists and chemists, including members of my group.

Since the 1970s I have also been working on the theory of surfaces, mostly electronic structure. The work with Lang in the early 1970s, using DFT, picked up and carried forward where J. Bardeen’s thesis had left off in the 1930s.

In 1979, I moved to the University of California, Santa Barbara to become the initial director of the National Science Foundation’s Institute for Theoretical Physics (1979–84). I have continued to work with postdoctoral fellows and students on DFT and other problems that I had put aside in previous years. Since the middle 1980s, I have also had increasing, fruitful interactions with theoretical chemists. I mention especially Robert Parr, the first major theoretical chemist to believe in the potential promise of DFT for chemistry who, together with his young co-workers, has made major contributions, both conceptual and computational.

Since beginning this autobiographical sketch I have turned 76. I enormously enjoy the continuing progress by my younger DFT colleagues and my own collaboration with some of them. Looking back I feel very fortunate to have had a small part in the great drama of scientific progress, and most thankful to all those, including family, kindly “acting parents”, teachers, colleagues, students, and collaborators of all ages, who made it possible.

This is not a science book, nor even a book about science, although most of the contributors are scientists. It is a book of personal stories about Walter Kohn, a theoretical physicist and winner of half of the 1998 Nobel Prize in Chemistry. Walter Kohn originated and/or refined a number of very important theoretical approaches and concepts in solid-state physics. He is known in particular for Density-Functional Theory. This book represents a kind of "oral history" about him, gathered – in anticipation of his 80th birthday – from former students, collaborators, fellow-scientists, and friends.