Klaus Ensslin
Luis Viña *Editors*

Manuel Cardona

Memories and Reminiscences

Springer

Manuel Cardona

Klaus Ensslin · Luis Viña
Editors

Manuel Cardona

Memories and Reminiscences

Springer

Editors
Klaus Ensslin
Department of Physics
ETH Zurich
Zurich
Switzerland

Luis Viña
Departamento de Física de Materiales
Universidad Autónoma de Madrid
Madrid
Spain

ISBN 978-3-319-20342-3 ISBN 978-3-319-20343-0 (eBook)
DOI 10.1007/978-3-319-20343-0

Library of Congress Control Number: 2015943366

Springer Cham Heidelberg New York Dordrecht London

Printed on acid-free paper

Springer International Publishing AG Switzerland is part of Springer Science+Business Media (www.springer.com)

Manuel
Cardona
time
many
one
first
work
group
years
also
physics
students
Manuel's
Raman
research
Inge
different
Stuttgart
University
semiconductors
scattering
scientific
optical
always
experimental
properties
well
later
much
remember
people
Spain
papers
results
spectroscopy
Physics
Phys
Institute
scientists
new
energy
made
like
several
Rev
book
two
received
became
28Si
science
interesting
published
theory
Spanish
knowledge
light
Brown
state
family
office
year
young
exciton
visit
experience
together
semiconductor
temperature
synchrotron
way
back
long
stay
field
visited
course
already
small
studies
MPI
electronic
life
spectra
came
just
high
met
took
went
home
used
using
great
Germany
band
days
experiments
discussions
effects
Barcelona
months
found
working
started
talk
since
German
known
never
three
One
PhD
State
important
good
wife
study
scientist
personal
told
worked
see
world
still
early
even
radiation
decided
now
know
asked
times
Solid
paper
materials
collaboration
graduate
states
position
student
isotope
knew
problem
got
end
Physical
MPI-FKF

Preface

Manuel Cardona was an important figure in science, in general, and in condensed matter physics, in particular. In addition he was an exceptional human being as evidenced by his support of human rights and ethics in science and his deep knowledge of culture, history and languages. This book is meant to document Manuel's achievements and his incredible network of scientists around the world and also to show the many facets of his fascinating character.

In practically all of the following contributions, the reader will find descriptions and accounts of both his personal and scientific lives. A set of words converted to images meant to summarize these facets has been gathered on the preceeding page of this book.

The first word is "Inge", and indeed Manuel's life (and the life of many of us) would have not been the same without his wife Inge. She was inseparable from him and all of us have enjoyed her hospitality and also somehow profited from her support for Manuel. Every time one of the visitors, postdocs, students would need something at the personal level, there was Inge providing all the required help. None of us will ever forget her courtesy during the parties at the Knappenweg. On many occasions, Inge and Manuel together have been as close to us as members of our own families.

Other accounts appearing throughout this book relate to aspects of Cardona's professional life such as: science, knowledge, physics, research, university, PhDs, students, materials, semiconductors, experimental, theory, Raman scattering, spectroscopy, light, synchrotron radiation, optical properties, excitons, bands, electronic states, energy, and temperature.

Work was also something inseparable from, and intrinsic to, Cardona, but a special kind of work, a work that one enjoys and that spreads a contagious passion for physics. Cardona never mentioned to his collaborators that they should work harder; it just came by itself when one worked with him.

These activities are linked to locations all over the world, but special places appear recurrently in the book: Spain, the US, Germany, Latin America, Barcelona, Brown, Stuttgart, Max-Planck-Institute. In these places, he contributed to the education of a cohort of students and scientists, whose common leitmotif has been to follow his attitude towards science. Those of us who had the enormous luck to interact, learn and work with Manuel will ever keep in our memories how working in his group was like being in a nirvana for doing science. Everybody helped everybody in a friendly atmosphere, where no negative competition was present. Being there aroused the feeling of belonging to a privileged group of people.

Other words appear conspicuously on the preceeding page. “First one”: indeed Cardona, was a pioneer and therefore the first one to start many new research activities in Solid State Physics. “Many” is linked to these facets of his life: the many languages he spoke, the many subjects he could be involved into, his incredible memory and ability to relate facts that enabled him deal with many ideas simultaneously (he mentioned at some time in his later life that a signal of aging was losing the ability of multi-tasking, especially participating in conversations), the many friends, colleagues and disciples that never will forget him.

We thank Claudia Vinzens for organizing the book with so many coauthors and Claus Ascheron for publishing this book with Springer Verlag.

Zurich, Switzerland Klaus Ensslin
Madrid, Spain Luis Viña

Contents

Contributors

Maria Isabel Alonso Materials Science Institute of Barcelona, Barcelona, Spain

Massimo Altarelli European XFEL GmbH, Hamburg, Germany

Claus Ascheron Springer-Verlag Heidelberg, Berlin, Germany

David Aspnes North Carolina State University, Raleigh, USA

Walter Braun BESSY, Berlin, Germany

José M. Calleja Universidad Autónoma de Madrid, Madrid, Spain

Andrés Cantarero University of Valencia, Valencia, Spain

Kikuo Cho Institute of Laser Engineering, Osaka University, Suita, Japan

Marvin L. Cohen Department of Physics, University of California, Berkeley, USA

Francisco de la Cruz Centro Atómico Bariloche, San Carlos de Bariloche, Argentina

Alex Fainstein Centro Atómico Bariloche and Instituto Balseiro, San Carlos de Bariloche, Argentina

Alejandro Goñi Institut de Ciència de Materials de Barcelona, Barcelona, Spain

Walter A. Harrison Department of Applied Physics, Stanford University, Stanford, CA, USA

Isaac Hernández-Calderón Physics Department, Mexico City, DF, Mexico

Carlos P. Herrero Instituto de Ciencia de Materiales de Madrid, Madrid, Spain

Jesús Sánchez Izquierdo Madrid, Spain

Reinhard K. Kremer Max-Planck-Institut für Festkörperforschung, Stuttgart, Germany

Rudolf Lauck Max Planck Institute for Solid State Research, Stuttgart, Germany

Lothar Ley University of Erlangen, Erlangen, Germany

Chengtian Lin Stuttgart, Germany

Elías López-Cruz Benemérita Universidad Autónoma de Puebla, Puebla, Mexico

Beverly Martin Palo Alto, CA, USA

Richard Martin Palo Alto, CA, USA

Emilio E. Mendez Brookhaven National Laboratory, Stony Brook University, New York, USA

José Menéndez Arizona State University, Tempe, USA

Roberto Merlin University of Michigan, Ann Arbor, USA

Francisco J. Meseguer Universidad Politécnica de Valencia, Valencia, Spain

Narcís Mestres Institut de Ciencia de Materials de Barcelona, Barcelona, Spain

Alfonso Muñoz Universidad de La Laguna, Tenerife, Spain

Andreas Otto University of Düsseldorf, Düsseldorf, Germany

Ramon Pascual ALBA Synchrotron Light Source, Barcelona, Spain

Richard Phillips University of Cambridge, Cambridge, UK

Aron Pinczuk Columbia University, New York, USA

Klaus H. Ploog Paul Drude Institute for Solid State Electronics, Berlin, Germany

Rafael Ramírez Instituto de Ciencia de Materiales de Madrid, Madrid, Spain

Aldo H. Romero West Virginia University, Morgantown, West Virginia, USA

John E. (Jack) Rowe North Carolina State University, Raleigh, USA

Ulrich Rössler University of Regensburg, Regensburg, Germany

Paulo V. Santos Paul-Drude-Institut für Festkörperelektronik, Berlin, Germany

Jorge Serrano Yachay City of Knowledge, YachayTech, Urcuquí, Ecuador

Shigemasa Suga Osaka University, Osaka, Japan

Katuhisa Suzuki Osaka University, Osaka, Japan

Mike L.W. Thewalt Simon Fraser University, Burnaby, Canada

Bernard Weinstein State University at Buffalo, New York, USA

Félix Yndurain Universidad Autonoma de Madrid, Madrid, Spain

Peter Y. Yu University of California, Berkeley, USA

Jörg Zegenhagen Diamond Light Source Limited, Oxfordshire, UK

Roland Zeyher Max-Planck Institut für Festkörperforschung, Stuttgart, Germany

Stefan Zollner New Mexico State University, Las Cruces, USA

Manuel Cardona: Extraordinary Scientist, Teacher and Human Being—A View from Argentina

Francisco de la Cruz

Manuel Cardona has been one of the most successful contemporary physicists. He was an extraordinary scientist enjoying the creation and dissemination of knowledge through collaborative work with colleagues and students.

Manuel published more than thirteen hundred scientific articles and wrote several books that are used by students and professors at different levels of education and research. Many of his former students are today outstanding scientists serving universities as well as the industry in the United States, Germany, Spain, Italy, France, England, Russia, Israel, China, Japan, Cuba, Brazil, Chile, Mexico, Argentina, and other countries. He not only taught physics and trained dozens of scientists: he educated his students and colleagues in questions that touch upon ethics, particularly when applying a society's limited resources to science and education.

Manuel could have been a theoretical as well as an experimental physicist. He chose to lead research in laboratories where many experimental physicists became trained. His broad and deep understanding of physics was shined when interpreting experimental results as well as when he planned future actions. His daily presence discussing results, suggesting experiments and, quite frequently, bringing information on social and political problems, made Manuel's lab not only productive in science but a stimulating meeting point. His interest in the social and political agenda problems became part of the formation of citizens.

Cardona had a privileged intelligence. He was fast suggesting ideas and procedures to understand new phenomena. He enjoyed sharing initiatives with colleagues and particularly with the many students he advised over the course of his life. He was able to mentor students that were simultaneously doing research in a wide range of scientific subjects. Manuel himself wrote papers on an equally wide range of topics. He was a gifted writer and seldomly had to revisit his initial drafts. On the other hand, it was interesting to see how many hand written "suggestions" and "annotations" he made on manuscripts "carefully" elaborated by his students. Usually, Manuel's observations were correct, but it was a pleasure and a challenge to discover, from time to time, that he was not always right.

Manuel Cardona was born on September 7, 1934 in Barcelona, Spain, two years before the beginning of the Spanish Civil War. As many other Spaniards he and his

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_1

family suffered the consequence of war, and he retained the horror of the war in his memory. Manuel attended high school in a prestigious school in Barcelona. In 1955 he graduated with excellent marks as “licenciado” in Physics at the University of Barcelona. During those years in the university he became aware that the scientific level of physics in Spain was low. He decided to pursue a doctoral degree abroad. He received several offers and elected Harvard.

The period 1956–1961 is almost certainly the most important in his life. On one side he obtained his Ph.D. degree at Harvard under the supervision of William Paul but even more important he found Inge, his wife and friend forever. Inge is an exceptional woman that made possible the full time dedication and success of Manuel in science. Inge not only took in her hands the organization their family’s daily activity but she has been the dedicated hostess that made the visits of many students and colleagues that often went through Cardona’s home so especially enjoyable.

Cardona devoted most of his life to the experimental and theoretical study of semiconductors and high temperature superconductors through the analysis of optical spectroscopic data. During his years at RCA and Brown University (1962–1971) Cardona became also active in the study of thermodynamic properties of low temperature superconductors. After his Ph.D. graduation at Harvard, Cardona accepted a research position at RCA laboratories, in Zurich (1959–1961) first to be continued in Princeton New Jersey (1961–1964). During this period Cardona concluded that the research of optical properties of semiconductors was near saturation. At the same time, he considered understanding the behavior of superconducting materials an intellectual challenge.

As later demonstrated while studying superconductivity, unexpected experimental results induced the discovery of a different type of superconductors. In contrast with the full flux expulsion recognized in Type I superconductors, the compatibility of the superconducting condition in some materials with partial magnetic field penetration was observed. Those materials were named Type II superconductors. The theoretical explanation of the new phenomenon was given by A.A. Abrikosov in 1957 introducing the concept of vortex lines.

During his stay at RCA, Cardona and collaborators analyzed the nature of the electronic states in the core of the vortices. Their microwave absorption experiments provided evidence that the energy dissipation associated with the vortex core was intrinsic to the new superconducting state and should not be linked to thermally induce electronic excitations. As a result, Cardona and collaborators were among the first to suggest that the specific heat of Type II superconductors should evidence a linear temperature dependent contribution associated with electron states in the vortex core, which was subsequently confirmed experimentally. This was quite a relevant contribution to the understanding of what we now consider “traditional” superconductivity. Cardona and collaborators were also able to detect the predicted nucleation of surface superconductivity in Type II superconductors when the magnetic field is applied parallel to the surface of the material.

Once at Brown (1964) Cardona decided to go back to studying the optical properties of semiconductors but now through optical modulation techniques, increasing notoriously the sensitivity for detection of the optical spectra of semiconductors. Cardona and his team researched superconductivity studying metastable supercooling and superheating states of Type I superconductors in the presence of magnetic fields on parallel tracks. These experiments were continued until the beginning of the seventies providing valuable data on the superconducting parameters characterizing Type I superconductors.

In 1965 Cardona accepted an invitation from the University of Buenos Aires as a scientific advisor for a program supported by the Ford Foundation dedicated to modernizing the experimental facilities of the Physics Department. Cardona and Inge spent 3 months in the winter of Argentina. This was his first of many deep contacts with Latin American scientists, students and friends. Manuel and Inge felt quite pleased with the atmosphere and friendship found in Buenos Aires. The connections created by Manuel and Inge in Argentina and extended later to all Latin America, have been very important then and in the years that followed. At that time, Argentina was under a democratic regime. Cardona used to say that it was his first experience living in a democracy “in Spanish”.

While in Buenos Aires, Manuel made a short visit to the Instituto de Física de Bariloche (today Instituto Balseiro) establishing contact with me and María Elena, my wife. We were both working on our Ph.D. thesis in the Low Temperature Laboratory, where a few years earlier (1961–1962) John Wheatley had established a research program in solid state physics. Manuel’s presence in Bariloche was to our family the beginning of a lasting friendship. I explained to Manuel that I was researching transport properties of metals at low temperatures for my Ph.D. thesis. His interest, questions, recommendations impressed me very much. We started talking in the morning and late in the afternoon María Elena told us to go home for dinner. The three of us had a long after supper talk. We were much impressed by the general knowledge of this young physicist. No doubt the fact I was born in Barcelona during the Spanish civil war, staying in Barcelona until I was 13, was a topic for long remembrance. Cardona offered me help when looking for a postdoctoral position.

Several of Cardona’s students at Brown at the end of the sixties were Argentineans. This was the result of his short and fruitful visit to Argentina and quite ironically “stimulated” by the military takeover of the Argentinean government at the beginning of 1966, a few months after Manuel ended his “sejour” in Buenos Aires. Cardona followed with concern the perils encountered by researchers and students in Buenos Aires. He was of great help offering openings for graduate students in his lab at Brown University as well as looking for open positions in USA and abroad for a good number of students and researchers that suffered the persecution of the military regime. Cardona’s involvement when political turmoil affected life of ordinary citizens was a constant in his life. He and Inge frequently opened their house and resources to mitigate the anguish of suffering people.

During the sixties at Brown, Cardona recognized the great opportunity triggered by the application of modulation spectroscopic techniques studying the dielectric function of semiconductors. As a consequence, while continuing research in superconductivity, he decided to invest most of his efforts and resources in the study of semiconductors. A good number of today's well respected Argentinean physicists in the area of semiconductors have been trained or collaborated with Cardona as a result of the strong scientific and human relation the Cardonas maintained with Argentine physicists since their first visit to Argentina.

After finishing my Ph.D. in Bariloche in 1968, I received an offer from Cardona for a post-doctoral position in his laboratory. Manuel gave me the opportunity to choose the area in which I would be working. Thinking in going back to Bariloche after finishing the post-doctoral training I decided to work in superconductivity since most of the necessary infrastructure was available there.

While in Providence the Cardona and de la Cruz families became very good friends. Since then our families have remained very close, in spite of time and distance. Our children, now grown up, keep memory of the good time they spent together. I was a few years younger than Manuel but many years behind in knowledge and experience. I learned from Manuel scientific integrity and commitment to students with diligence and dedication.

Cardona and family left Providence in 1970 and the de la Cruz family returned to Bariloche in 1971. Since then we have kept frequent contact either personally in Germany or Argentina or by phone calls. Of course, we miss Manuel but his presence remains among us.

Back in Bariloche, Maria Elena and I joined the Low Temperatures Laboratory. As a result of the experience gained with Cardona at Brown we initiated research in Type I and Type II superconductors first and in high temperature superconductors, later. Our interaction and collaboration with students from the Physics Department of the University of Buenos Aires stimulated a program of research in the same laboratory where Manuel had spent time in the 1960s. In this way his contributions, some years earlier, were in some way recovered and superconductivity and low temperature physics restarted in the Buenos Aires group. Today Bariloche and Buenos Aires have well established low temperature laboratories. In one way or another Cardona made a relevant contribution for their respective successes.

The use of spectroscopic techniques to understand the physics of materials was delayed many years in Bariloche as a consequence of lack of economical resources. It was only in the eighties when two excellent young physicists that received training from Manuel at the MPI in Stuttgart installed a facility that is recognized today as an excellent laboratory at the international level.

Finishing our rather incomplete description of Manuel extraordinary dedication we quote The "John C. Wheatley Prize" of the APS citation when awarded to Cardona in 1997:

> For being a dedicated mentor and guide to a whole generation of Latin American physicists and playing a decisive role in the development of physics in Latin America. By example, enthusiasm and very exacting standards he has inspired a respect for excellence and collegiality which now motivates many groups throughout Latin America.

Acknowledgments I thank María Elena for her collaboration in reviewing and commenting this short memory. It has been useful to have at hand the "Manuel Cardona I Castro" biography by Pere Bonnin. Fundació Catalana per la Recerca (1998).

Playing with Symmetry and Other Memories

Maria Isabel Alonso

One of my earliest memories of Manuel is not related to science. I don't remember all the details but I have a vivid image in my mind of a Saturday afternoon that was very surprising for me by that time. I'm certain it was in early 1985 although I don't remember the date. It must have been almost spring time, when it was still rather cold but most of the snow of that icy winter was gone. Manuel invited the three Catalan students in the group: Narcís Mestres, Miquel Garriga (my husband) and me, to join him and meet some people at "Casa Nostra" (which means "Our Home" in Catalan). This place in Böblingen still exists and is a cultural meeting point especially for young people. Nowadays they are very organized and, as a non-profit-organization, they offer on their webpage many activities, like fitness, dancing, concerts, meetings, parties, courses, etc. Probably the building is not the same as back then, and I am not sure whether some link to Catalonia remains, but they still keep the name "Casa Nostra—Unser Haus". This association actually started in Switzerland when a group of Catalan emigrants founded "Casa Nostra" in Zürich in 1963 and held meetings with other emigrants in south Germany. Soon afterwards the delegation in Stuttgart was created and remained linked to the Swiss one until it became an independent association in 1970.

When we went there in 1985 the common language was German, although a few people were Catalan-speaking. Manuel had contact to this group of people and from time to time he met them. On that occasion, he was going to give a talk about Japan, where he had been on a work trip. He brought many slides with pictures of all the places he had visited together with Inge. He was talking nearly 1 hour about Japanese culture and he spent a substantial amount of time explaining details of the tea ceremony, which he dissected with precision. I could not follow some the explanations because then my German was not very good. What I recall is that the gathered audience, about 20 people, were very interested in everything: they commented and asked a lot of questions about geishas. It was amazing to see how the audience esteemed Manuel, as much as we all valued him in a scientific seminar. It was a most surprising experience to see a Max-Planck-Institute Director in this context. This and other similar memories make me remember Manuel not only as an extraordinary scientist. He was a friendly and cordial person with an overwhelming humanistic culture and exceptional communication skills. He enjoyed teaching any subject.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_2

Needless to say, he had the same exceptional ability for science. He was a leader with a privileged intelligence and the predisposition to share his knowledge and motivate others. As a Ph.D. student at the MPI in Stuttgart, I started to work at the Epitaxy Group led by Dr. Elizabeth Bauser and my subject was Silicon-Germanium (Si-Ge) alloys. Manuel was my thesis adviser and therefore I reported my results regularly to him. I had relative freedom to do research and when some result or subject were interesting for him he guided me to pursue that line of work. He was always very positive and allowed the people to develop their best qualities and potentials. When he was involved in a subject he could become very impatient and demanded a lot of effort to get results as fast as possible. One example was the study of the Raman spectra of Si/Ge superlattices grown on Si (001). As we were discussing the results, he realized that for some combinations of layers the symmetry of the superlattice was orthorombic, actually whenever the number of atomic layers of both Si and Ge was even. On his table there was always a zincblend model made of wooden black and yellow balls (atoms) joined by metallic dowels (bonds). He took the model and started to name the atoms for the case n = 2, and enumerating the symmetry operations that were preserved and those that were not. He was so fast that I could not follow instantly. He knew by heart all symmetry operations of the diamond structure and could visualize them effortless. I went out of the office with the task of classifying those superlattices in their different space groups and relating the symmetries to our lattice dynamics calculations. Concerning the symmetries, I needed to think and understand the space group tables. I had just started to figure out something when after a while he came to my office with the ball-dowel model and asked whether I had finished. Well, not yet, I'm working on it. "Look: silicon-silicon-germanium-germanium, see?" For some time I became able also to imagine all those structures and their symmetries. For me it was quite difficult. For him, a kid's game.

Long time after that, during a sabbatical stay that Fernando Cerdeira spent at the Institute for Material Science in Barcelona, we worked together on Raman scattering from organic crystals. Manuel visited us in the frame of a collaboration sponsored by ICREA (Catalan Institution for Research and Advanced Studies) and he aided us (Fernando, Miquel and myself) in the interpretation of the data. We could enjoy again with symmetry analysis, this time in molecules with many atoms and their crystals. During this stay, like always, I remember the pleasure from the contact with Inge, a wonderful and extraordinary woman.

Manuel had a deep insight and knowledge for a large variety of problems in solid state physics, which was truly unique. His mind was never at rest, if he was not doing science, he would be reading, thinking or speaking about history, cultures, traditions, art, politics, economy … he could make any subject fascinating. He had not only the knowledge and information about them but he also had the rare ability to assimilate those to his own vital experience, enrich them, and share them with others. Like on so many other people, he had a very positive influence on me and I will never forget him.

Manuel Cardona: Scientific Mentor, Man of Culture and Friend

Massimo Altarelli

My first encounter with the name of Manuel Cardona was through the scientific literature. My thesis advisor in Rome, Franco Bassani, gave me a substantial bibliography to read on the optical properties of solids, and of course the name of Manuel Cardona was all over! The then new tool of synchrotron radiation was being exploited to access a very broad wavelength range, and Manuel, in a sabbatical in Hamburg was flooding the world with the red DESY preprints… a primary source of information for my first confrontations with frontier physics research. Thanks to Bassani I was given the privilege to attend the Varenna School on Atomic Structure and Properties of Solids in 1971, on the beautiful shores of Lake Como in northern Italy, and that is where my first encounter with Manuel actually took place. He joined the school with his wife Inge and their children. Manuel would lecture with his characteristic energy and enthusiasm, running through tons of slides. As the lecture was over and a break followed (the students, me included, were totally exhausted and overwhelmed), he shortly disappeared to show up in his swimming suit and lead the children into jumping competitions in and out of the water…definitely, no shortage of energy!

At the lunch break, he sat in a big table with many colleagues, leading several conversations at a time, switching instantly from English, to German, to French, Spanish, Italian… depending on the nationality of the person he was addressing. I had never met such a force of nature!

In 1972, I saw him again in Warsaw, at the International Conference on the Physics of Semiconductors. Western participants had to cope with the peculiarities and the many inefficiencies of communist-ruled Poland. One morning, after waiting half-an-hour for a never appearing bus, he addressed an Italian colleague, well known for his extreme left-wing political opinions: "so, at the next election, will you finally vote for the Democrazia Cristiana?"

We started to develop a friendly and intense relationship. I was just a complete beginner in physics, he was an international star, yet he always made me feel at ease.

After a few years in the US, I had a wonderful opportunity to come back to Europe when Ole Andersen offered me a position in his newly formed theory group at the Max-Planck-Institute in Stuttgart. My task was to provide theoretical support to the semiconductor physics activities in the experimental departments, and

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_3

especially in Cardona's group. This was an incredibly exciting time. Not only the physics was very interesting, as the emerging field of low dimensional systems provided new phenomena and new paradigms; but the continuous stream of Spanish and Latin-American students and post-docs was generating a permanent "fiesta" atmosphere on the 7th floor of the Institute. I met some life-long friends there (too many to mention individually…), learnt some rudimentary Spanish and enjoyed, after days of hard work, the many social opportunities. Best of all, the parties in the home of Inge and Manuel, where everybody was welcome like a special guest, everyone was really feeling at home, and the differences in seniority, rank, national culture would play no role. The excellent food and wine was complemented by Manuel's inexhaustible collection of anecdotes, stories, about people he met all over the world, or historical characters, books, … A wonderful entertainment! His immense culture, not only in science but also in the arts and in history, made his conversation an endless source of inspiration.

A lot of guests and visitors from all over the world stopped by in Stuttgart to give seminars (9:00 am on Friday, Seminar Abteilung Cardona, was the official denomination).

His typical work style consisted in having a huge desk completely covered with papers, at least two students discussing their results, his secretary Kerstin (later Mrs. Christenesen) handing urgent papers to sign for Solid State Communications, while he was talking on the phone with someone else. But nobody involved in this incredible multi-tasking felt that he/she was not receiving enough attention: he was on top of everything, for at least 10 hours a day.

Many years later I received a phone call from an eminent physicist from the University of Rome. He was member of a University committee in charge of screening nominees for honorary doctorates. Manuel was one such nominee, and my former professor told me something like "he has over 700 publications: that's impossible, he must have a huge institute and sign all the papers produced by his colleagues, without even knowing what they are about…" I told him that I was ready to bet any sums that, although this may seem physically impossible, Manuel could explain to anybody the why, the where, and the how of each experimental result reported in his papers, and was thoroughly familiar with every sentence in each of those hundreds of papers. (Apparently I was convincing: Manuel was awarded the honorary degree!)

After I left Stuttgart for Grenoble, I continued to maintain a professional relation and a good friendship with Manuel. Although he had been basically spending his whole science career abroad, he always felt very deeply bound to Spain. Around 1988 the CSIC (Consejo Superior de Investigaciones Científicas) decided to create an advisory board (or evaluation committee) for its four institutes of Materials Science (in Madrid, Barcelona, Sevilla and Zaragoza), and asked Manuel Cardona to chair this board. Manuel accepted with enthusiasm this task; and I was asked to be a member of the committee, that, at each meeting, visited in turn one of the four institutes in depth, for several days, and then delivered a report to the President of CSIC. Later Emilio Mendez, from Stony Brook, took the Chair, and both Manuel and myself stayed on as members. The visits of the committee were paid a lot of attention from the scientific staff of the institutes, and we got many detailed talks and visits of laboratories. At the end, report writing was a collective enterprise orchestrated by the Chair.

I remember Manuel insisting with the institute's Directors to cut down the time devoted to dinners, receptions, sightseeing excursions, and to concentrate on talks and laboratory visits. He was really committed to contributing to the improvement of the science output of the four institutes and, in general, of the country. These meetings were quite strenuous (given also the Spanish habit of not even thinking about going to dinner before 9:30 or 10 pm, nor about going to bed before 1:00 am!) but also very instructive and scientifically stimulating.

Another example of his keen interest in Spanish science, and the people behind it, is the readiness of Manuel to open chances and opportunities for young scientists that he regarded as talented and promising. When I moved to the European Synchrotron Radiation Facility (ESRF), at the time still under construction in Grenoble, my task was to build up, together with a life-sciences colleague, the scientific division. Spain was one of the early generous contributors to the new international institute, and I was keen to have a good representation of the funding countries in the scientific staff, of course without compromises on quality. When I mentioned my low rate of success in recruiting brilliant scientists with some synchrotron radiation experience in Spain, Manuel Cardona suggested to me Salvador Ferrer, then at the Autonomous University in Madrid. He told me that this young man had built a very impressive surface science lab in Madrid, and that if he could be interested to come to Grenoble, he had all the talent to build equally impressive synchrotron instrumentation, though he never did it before. Manuel's words were enough: few weeks later, I took a plane to Madrid just to go to meet Salvador, and I soon thereafter offered him a job in Grenoble. He accepted and, working in the Surface Science group with Fabio Comin, he built a beautiful surface X-ray diffraction instrument; but most importantly, he became a magnet attracting scores of Spanish visitors, students and post-docs to Grenoble, year after year, producing a generation of excellent X-ray scientists working in a state-of-the-art facility. When later the Alba synchrotron was built near Barcelona (Salvador returned to Spain to take the scientific direction of the new lab) the young people trained in Grenoble provided many of the staff scientists and the users of the Spanish light source, besides continuing to staff many positions at the ESRF.

In conclusion, even though I was very privileged in having met so many extraordinary physicists, who influenced the development of our discipline in a substantial way, I feel that Manuel Cardona stands out in my memory and in my gratitude for complementing scientific excellence with an exceedingly warm and human personality, for his all-round culture and for the generosity and spontaneity with which he shared all his talents and wisdom with others.

Memories of Manuel Cardona by His Publisher Springer

Claus Ascheron

I am honored to have the opportunity to say a few words about Manuel Cardona from the perspective of a publisher. His publishing activities were impressive. Not only was Manuel the author or co-author of about 1300 scientific publications in international journals, which made him one of the most cited physicists worldwide with a Hirsch index of 113, he was also very productive as book author and editor and wrote and edited 10 monographs on solid state physics. I have enjoyed working with him on several editions of the renowned textbook "Fundamentals of Semiconductors" which he co-authored with Peter Yu. It appeared in four editions, was translated into different languages and became the leading textbook in this field for many years.

Manuel's publishing activities were also intended to support the scientific progress by providing books needed by scientists and graduate students for their research and learning. In his role as co-editor of the book series "Solid State Sciences" at Springer Verlag, he selected and recommended expert scientists as book authors or editors. Together with the other scientific advisors and co-editors Peter Fulde, Hans Queisser, Klaus von Klitzing, Horst Störmer, Roberto Merlin and Helmut Lotsch, Manuel contributed to making this a prestigious book series in the field of solid state sciences.

As an editor at Springer I was privileged to have close contact with Manuel over many years, and we became friends through our frequent collaboration. I greatly admired his intensive working style which continued right up to his last hour in his office at the Max Planck Institute. Most of all I will miss his personal qualities as a warm-hearted human being with a deep understanding of other people.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_4

Manuel Cardona, The Person

David Aspnes

Because others will treat Manuel's scientific accomplishments in detail, I am writing this from a more personal perspective. In a field filled with outstandingly intelligent people, Manuel was arguably the brightest individual I ever met. His language skills were legendary, and his scientific skills no less. Much of what I have accomplished in my career, and certainly much of the recognition that has come my way, I owe to Manuel. Probably most graduate students and postdoctoral research associates who have had the opportunity to work with Manuel will say the same thing, which gives some idea of his impact.

I first saw Manuel as a graduate student at the University of Illinois Urbana/Champaign, when he arrived to give a solid-state-physics seminar on modulation spectroscopy. Not only did he speak maybe 50 % faster than anyone else I had ever seen lecture, in 5 min he had filled the blackboard, which stretched across the lecture room, from one end to the other. Yet the talk was quite understandable, highlighting the physics and what we had learned from the work, rather than concentrating on formalism and experimental techniques. I don't recall meeting Manuel personally on that visit, but my Ph.D. advisor, who was a physical chemist, recognized that one of the best things that could happen to me at this stage of my career would be to land a postdoctoral research associate position in Manuel's group.

Somehow it happened. That fall, my wife, small son, and I rolled into Providence with everything that we owned in $5' \times 8'$ U-haul trailer. In their typical generous fashion Manuel and Inge insisted that we stay in their house while we found accommodations, since they were out of town. This made finding a place to stay enormously easier.

At this time modulation spectroscopy was reaching its peak, and Manuel was making an impact in the field from both experimental and theoretical perspectives. At the time Manuel's laboratory and many of his methods could be described more or less accurately as "early casual": students were doing electroreflectance by applying fields to semiconductors using electrolytes, light shielding on spectrometers was done courtesy of cardboard boxes taped together, and band-structure calculations were done using the $k \cdot p$ method. Nevertheless, the overall goal was results, and the physics being done there was world-class. This period set the stage

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_5

for the rest of my career. Jack Rowe and Fred Pollak were only two members of the group who become lifetime collaborators and friends.

My next significant professional overlap with Manuel came in the mid-1970s, when he made arrangements for a 6-month stay in his department at the Max-Planck-Institute für Festkörperforschung. Getting a 6-month sabbatical from Bell Laboratories was unusual, but my management there decided that it would be good to make some connections with the world-class effort going on in Stuttgart. This was the time of when spectroscopic ellipsometry was beginning to make an impact, and synchrotron radiation spectroscopy was also starting to be a factor. Manuel's laboratory was now much more polished, suggestive (correctly) of much better financial support. The group was also much more international, which was not surprising as by that time Manuel's reputation was worldwide. However, Manuel's office was the same as it was at Brown: papers stacked everywhere, particularly on his desk. Nevertheless, despite the apparent disorganization, Manuel always seemed to find what he needed when he needed it.

In later years we had fewer interactions, but in the late 90s, Manuel and Wolfgang Richter (another Manuel product) arranged a short stay for me at the Max-Planck-Institute, although this was more a visit to renew old acquaintances and review successes than to make further scientific progress.

Of the many aspects of Manuel that I found exemplary, one of the best is that none of the well-deserved acclaim that he earned ever went to his head. He was invariably considerate, approachable, and (as usual) knew more than you did about any topic that you cared to discuss.

To Manuel, friend, scientist, and person extraordinaire: It was a privilege to know you, let alone to be able to work with you. We miss your presence, but your contributions, influence, and impact remain alive and well.

Manuel Cardona and His Relation to Synchrotron Radiation

Walter Braun

At the beginning of 1973, I was finishing my diploma thesis at the institute of solid state physics of Karlsruhe University. The advisor was Professor Wolfgang Ruppel, an old friend of Manuel Cardona as I learnt from Manuel when I visited him first. In the frame of an institute excursion, I visited the Max-Planck-Institute for Solid State Research (MPIFKF) for the first time in 1972. Among other directors, Manuel gave a talk during this visit. He presented results of his research in Raman scattering. We were very impressed by his presentation. This event was a publicity campaign to recruit graduate students, as I learnt from Professor Queisser more than a quarter of a century later. At the beginning of 1973, I visited both, Manuel and H.-J. Queisser on a Saturday morning and both offered to join their department as a graduate student. Finally I decided to accept Manuel's offer to perform an experimental thesis in his group using the experimental technique of photoelectron spectroscopy. Far-sighted as he was, he suggested to investigate the electronic structure of chalcopyrites including their valence bands. These materials were thought already by that time to play an important role in future solar cell technology. Indeed, thin films of these materials are the most efficient solar cells presently in practical application. Unfortunately, the basic materials for their production are seldom and rather expensive.

In August 1973, I started my thesis work as Manuel's second German graduate student in the building of the former "Porsche Rennstall" in Heilbronner Straße, which was the first home of the MPIFKF. I started with the investigation of the valence bands of thin films of some chalcopyrite semiconductors using He- and Ne-resonance lines. We observed a very strong dependence of the line shape of the valence band spectra on photon energy. This lead Manuel to the suggestion to use, in addition, synchrotron radiation in further experiments at the DESY synchrotron. There, I performed absorption and photoemission experiments at the, by that time worldwide unique, plane grating monochromator for the XUV-range in the group of Ch. Kunz. Among other results, we observed the Cooper-Minimum in the absorption of 4d-electrons of $AgInTe_2$. Manuel was very pleased with these observations. This probably strengthened his view of synchrotron radiation being a powerful tool for solid state physics research.

I finished my thesis work in autumn 1975. At the end of October, we travelled together to Karlsruhe University for the presentation of the thesis followed by the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_6

exam. A year ago the so-called “Nelken Revolution” had taken place in Portugal. In Spain, the Franco regime was still in power. During the trip, Manuel explained to me that the changes expected would be completely different to the revolution in Portugal, because Spain did not have any more colonies. And he was right. The changes in Spain did not start via the military but by civil resistance. In the evening of the successful exam we had a party at my home. Among other guests my mother was there, who operated a typical small “Schwäbisch” farm close to Stuttgart with some cattle, pigs and hens. Manuel and my mother talked nearly all evening about the problems how to run such a farm properly. Manuel seemed to be an expert in running such a farm, she told me later. My mother was very pleased with the discussion with Manuel and that she could communicate in her dialect so well with my professor. After my Ph.D., I worked as assistant professor at a German university and continued to use synchrotron radiation for my research work at DESY in Hamburg.

Manuel was the head of a commission to study the necessity of dedicated synchrotron radiation sources in Germany and provided this report in January 1977. The commission recommended the construction of two such light sources, one for the XUV and one for the X-ray range. This study and further considerations about the location lead to the decision first to build an XUV source in Berlin. The first scientific director of the BESSY GmbH was Professor Baumgärtel. For building the facility BESSY he was looking for people who had some experience with experiments using synchrotron radiation and the necessary instrumentation in this rather new research field. I decided to join the team to build BESSY. Manuel was appointed as head of the first scientific advisory committee, the so-called “baubegleitender Ausschuss”. Besides my work to design and build modern beam lines, I had to take care of this committee. During the meetings, I also took care of Manuel. He used to stay overnight in my home. During evening dinners, we had the chance to discuss the points which seemed important to him for the facility under construction.

After the construction phase, Cardona’s group started to perform experiments at BESSY. In particular, the first ellipsometer to be operated in the photon energy range up to 35 eV had been built by scientists of the Max-Planck Society under the leadership of Manuel. This instrument was the only one worldwide and is still used by various groups in Germany. This is one of the many enduring projects initiated by Manuel.

On the occasion of the 20th anniversary of BESSY, he was invited to present a talk. This was entitled: “Semiconductor Research Using Synchrotron Radiation, a Personal Retrospective”. In this talk, he emphasized the importance of this research tool in modern science. He praised the work of the BESSY staff and the users of the facility which resulted in high quality publications. Many of them were on a high level in the citation index. Checking this index and the “Hirsch-Factor” was a hobby of his. As in former years, he stayed with us over night before the celebration. During dinner he suddenly asked, whether I could manage to get the book “Mein Jahrhundert” by Günter Grass before noon the next day. He had to fly to Spain immediately after his talk the next day, because he had to moderate the bestowal of the “Prince of Asturias Price” to Günter Grass. He wanted to read the book on his way to Spain. We managed to get two of the books in time. Therefore, I now have an autograph of a later Nobel Prize winner of literature. Whenever he visited us, he brought an interesting book as a present which he thought important to be read. In general, he was right. He really had an enormous overview in the literature. In this context I also want to mention that Manuel performed ancestry research of thesis advisors and put together his genealogical table reaching back to the 17th century. One of these people was Reginald Victor Jones. He was an English physicist and military intelligence expert who played an important role in the defense of Britain in the Second World War. After the war he had become a professor in Aberdeen. Manuel strongly recommended to read his book entitled “Most Secret War” in which Jones described his work in the Royal Air Force. Manuel was right. This book is one of the best books describing the work of a secret service which really helped saving the country.

My wife and I really always enjoyed visiting Inge and Manuel at their home, the last time at the beginning of 2014. I lost a mentor and good friend.

How to Get Absolute Values from Relative Measurements

José M. Calleja

The combined use of Resonant Raman and Brillouin scattering together with a "simple" description of the energy bands allows to determine the absolute values of scattering cross sections and of phonon deformation potentials (including sign) in semiconductors and insulators. Besides, a lesson on how to treat green researchers (and people in general) is given.

Introduction

During my several visits to the MPI between 1973 and 1982, Manuel Cardona proposed me to work on several research topics, most of which had a common denominator: To determine the absolute Raman scattering cross sections, and the phonon-deformation potentials in a variety of materials: Diamond [1], ZnO [2], ZnSe, ZnTe, GaP, and some fluorides [3]. For a 25 years old researcher, unexperienced in the field, this was quite a task. Raman scattering gives generally relative values, as the light excitation and collection geometrical factors are difficult to estimate. Therefore, absolute values were generally obtained (apart from theory) either by comparison with well-known standards or by measuring simultaneously Raman and Brillouin scattering. This method, used first by M.H. Grimsditch and A. Ramdas in 1975 [4], eliminates the need of knowing the details of the scattering geometry.

Working Method

Apart from the experimental tricks, I was surprised by the way Manuel extracted physical information from the data: With a simple description of the energy dependence of the dielectric function, he managed to end up with absolute values of

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_7

the deformation potentials. Even their sign was determined by the relative intensity of the LO and TO phonons in the Raman spectra. The working method used is well known for all of Manuel's collaborators: After discussing the latest measured results with him (or most frequently after listening to him) for a while, I went back to my office with a couple of quickly handwritten paper sheets, to be deciphered in the next hours or days. In the deciphering process Manuel was always ready to help with more paper sheets, until in the end the physics behind the measurements and "what to do next" were clear.

Theory

The values of the deformation potentials were also calculated using pseudopotential theory, with reasonable agreement with the experiments in many cases. Actually, being mostly experimentalist, Manuel was also very strong in using theory to support the conclusions. In his view, intuition and common sense were crucial to address any scientific problem. This part of the work required a crash-course of Manuel on pseudopotentials and several hours punching cards for the IBM computer.

Experiment

The experimental work always conveys some risks, in particular for the samples. The first ZnO sample we studied, a 3 × 3 × 2 mm hexagonal prism, cut from a beautiful 2 cm long single crystal, was accidentally dropped in the floor. It never appeared again. I spent some time looking for words to finally dare to tell Manuel what happened. At this point Manuel demonstrated to be very different from what I expected to be a German Director. He just said: "Cut another slice and be more careful next time". This was done at the outstanding "Probenvorbereitungslabor" (sample preparation lab) run by Dr. Kisela with the help of Gudrun Fröhlich and Dagmar Skuras. So I got a new ZnO sample and the work could be done [2].

Conclusions

From the above results and considerations one can conclude that the careful measurement and analysis of the Raman resonances in semiconductors, as well as the simultaneous measurement of Brillouin and Raman spectra can lead to the absolute values of deformation potentials and scattering cross sections. This is just an example of how outstanding Manuel was in extracting physical information from experimental data. He was an excellent scientist, full of interesting ideas and eager to share them with his collaborators. There are not known bounds for his work capacity.

Manuel was also an excellent teacher, always ready to explain complicated matters with simple arguments, and always finding time (and new samples) for freshmen needing it. He was able to bring out the best of his collaborators.

Manuel was an excellent human being, full of curiosity (not only in physics) and ready to help those who needed it.

He was also decisive in boosting research in semiconductor and superconductor physics in other countries. In particular, a significant number of young and senior scientists in Spain owe to Manuel Cardona both the formation and the support needed to develop their careers. As a pale sign of gratitude, Manuel Cardona was named Doctor Honoris causa by four Spanish Universities.

We miss him.

Note Added in Proof

Manuel was lately quite interested in bibliometric issues. There is an interesting case for those who believe blindly in citations, h numbers and alike. It is an example of "Sleeping Beauty": [2] received a total of 35 citations in the first 30 years after publication. Then ZnO became a fancy material due to its emerging interest for optoelectronic applications. As a result, the number of citations climbed to 508 (21-10-2014). The paper is, of course, the same.

References

1. J.M. Calleja, J. Kuhl, M. Cardona, Resonant Raman scattering in diamond. Phys. Rev. B **17**, 376 (1978)
2. J.M. Calleja, M. Cardona, Resonant Raman scattering in ZnO. Phys. Rev. B **16**, 3753 (1977)
3. J.M. Calleja, H. Vogt, M. Cardona, Absolute Raman scattering efficiencies of some zincblende and fluorite-type materials. Phil Mag. A **45**, 239 (1982)
4. M.H. Grimsditch, A.K. Ramdas, Brillouin scattering in diamond. Phys. Rev. B **11**, 3139 (1975)

From My Postdoc at the "Abteilung Cardona"

Andrés Cantarero

Before finishing my Ph.D. in September 1986, I had already a non permanent position at the University of Valencia, it was called at this time assistant professor (profesor ayudante), with a lot of teaching. I sent a letter to Manuel at the beginning of 1987 and, after receiving the letter, he called immediately by phone my thesis advisor, Prof. Alfredo Segura. Manuel told him: I have a lot of people at the moment, but if he is very good, I will accept him. Finally, he sent me an acceptation letter and I got a fellowship from the local government and the Ministry of Education. I did my postdoc from December 87 to December 89. I met Manuel a few months later in 87, in a School in Toulouse (France), organized by Prof. Ferdinand Pradal, where we had time to discuss the possible topics to be developed during my postdoctoral stay. From the options he offered me, I accepted to work on Raman under high magnetic fields. He told me that they had found huge resonances in the Raman scattering by LO phonons in GaAs [1] as a function of magnetic field at low temperatures, but they were not able to relate the resonances with the band structure. During the School, there was an excursion to Carcassonne, a nice medieval village between Toulouse and Narbonne. I told him that I would probably stay in my room preparing some proofs for a permanent position. Finally, I decided to go. When he saw me in the bus, he told me "the flesh is weak". We visited a wine cellar (in a castle) to test some wine and I recommended Manuel to buy some bottles of white wine. He liked it and reminded me at many occasions the nice wine we bought there. In all occasions I spent some period in Stuttgart, when we had the opportunity to meet, he always asked me to test several wines to tell him which was the best.

From the personal point of view I had from the very beginning a very good relationship with Manuel and his wife, Inge, which remained strong along the years. They were very friendly, I was invited many times at home for dinner or for breakfast during my postdoctoral stay or the many times I visited the MPI. They brought me to visit many places around or to go shopping with them. I remember an excursion to a very nice region called Weissenhof Estate (Weißenhofsiedlung). It consists of a set of very peculiar, of modern style, houses built by many architects belonging to several countries around for a German exhibition in 1927 (Deutscher Werkbund). He also invited me for dinner many times when somebody was leaving the Institute. I remember one of the times, when Prof. Grigoriĭ E. Pikus visited him, it was in the 90s. We had dinner in a typical "Schwäbisch" restaurant, called

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_8

“Gasthaus Löwen”. Manuel could speak in Russian with him. As far as I know, Manuel could speak perfectly German, English, Spanish, Catalán, French and Italian. He also had some level of Greek, Russian and a good level of Portuguese. In this last case, although he probably had a very neutral accent, he corrected me a couple of times my Portuguese (I had been living in Brazil for 8 years when I was a child). At the end of 2006, Manuel and Inge invited me for a couple of days to their new house in Stuttgart-West. Manuel had a very funny story with the carpets, moving the furniture during the night. He explained me the physical reason why the carpet was moving the furniture.

When I arrived to the Max Planck Institute (MPI) at Stuttgart, Tobias Ruf was doing the Ph.D. studying resonant Raman under high magnetic fields. I helped him to develop a program to manipulate the magnet in an automatic way, and at the same time I was trying to find the origin of the resonances. We have found several transitions corresponding to the resonances, but it was the theory developed with the help of Ulrich Rössler what helped us to understand deeply the physics behind the resonant behavior. During 1988, Richard Phillips joined us with an extraordinarily good InP thin film, grown by liquid phase epitaxy, and we could see all the Landau levels down to the split-off band and beyond. We could finally give a clear theoretical explanation to the work started by G. Ambracevicius, Roberto Merlin and Manuel in 1987 [1]. We sent the work to the 11th ICPS Conference in Warsaw and later on we published a paper in Phys. Rev. B [2]. As in the remaining topics I studied with Manuel, he was never happy until we did not understand deeply the physics insights.

Also during 1988, Prof. Carlos Trallero came to the MPI, invited by Manuel, in order to shed some light into the absolute measurements of resonant Raman scattering. Manuel asked me to help him in the theoretical analysis. First, because I had already some theoretical background, and second, Carlos Trallero was a theoretician of “pencil and paper” and did not understand completely the experiments. We published several works on the topic, related to the Raman scattering due to deformation potential interaction, Fröhlich interaction and the interference between them [3–6]. The role of the impurity-induced Raman scattering was clarified in a further stay at the MPI [7]. We also analyzed the double and triple resonant behavior when uniaxial stress was applied. We interpreted the experimental data of Antigoni Alexandrou, a very efficient Ph.D. student of Manuel at the time [8]. I remember a couple of anecdotes at this time, with Manuel and Carlos. We were in the office of Manuel many times for discussions and Carlos was always referring to the “Maue” equations. Finally, Manuel asked him: which are the “Maue” equations? Trallero said in his Cuban language: the 4 “Maue” equations, then we realized that he was talking about the Maxwell equations. A second anecdote is related to the way Manuel made some drawings on a sheet of paper during the discussions. He was trying to write something in a paper and the ink of his ball pen finished. Then, he took from my hand a pen with a very thin end, which I used to write the bar to the h (Planck constant) in the manuscripts, and press very strongly to write or to draw, and destroyed it! Then, he said: this pen also does not work.

During these years, there was a coffee time at the 7th floor, after lunch, where people of the whole Institute met for discussions. It was an opportunity to meet people, to discuss many topics, give ideas, solve problems, start collaborations, and so on. In this way I started a fruitful collaboration with Alejandro Goñi and Karl Syassen. Alejandro was doing the Ph.D. with Manuel working with high pressure. We published together a nice paper on the excitonic behavior of GaAs with hydrostatic pressure [9], where I solved by complex variable analysis the excitonic continuum integral, giving a very simple expression for the absorption, with only two-three sums instead of the integral.

At the end of my postdoctoral stay, Manuel asked me to measure silicon under uniaxial stress using an old system built a few years before by E. Anastassakis. The idea was to measure the stress on silicon with a YAG laser, instead of using visible light, in order to diminish the influence of the surface (Si was transparent at 1.064 μm). I had already done some measurements when Evangelos Anastassakis came to Stuttgart and Manuel asked him to help me to finish the experiments. I did a theoretical model based on an old program called REDUCE (the embryo of Mapple or Mathematica) which fitted very well the experiment. We also predicted from the model the anharmonic elastic constants of diamond. When I started the measurement, Manuel told me that David Vanderbilt was doing some calculations on the topic and we were comparing our results with those of Vanderbilt. I introduced in my model several three-body force constants, but the model of Vanderbilt was a little bit more complete. The disagreement between our results and those of Vanderbilt was obvious from the beginning. Manuel and I were discussing in his office, for hours, until we found the problem in Vanderbilt’s calculations. We told him, but he was convinced that their results were fine and that we had some error. Thus, we published our results in a PRB paper [10], and at the same time Vanderbilt and Taole published their results [11]. A few month later they found a mistake in the program code and they published an erratum [12], where they had to replace all the tables of the manuscript. Their new results were in agreement with ours.

During 1989 the first triple spectrometer (DILOR GmbH) came to Stuttgart and Manuel asked me to test the system. I did some modifications in the original program with the help of the son of Richard Martin (he was very good in informatics), who spent a few months at the MPI. The modifications were later introduced in the commercial code. Then, Anastassakis and I measured the second order Raman scattering of silicon under uniaxial stress. It was a hard work which took a couple of months (we measured at low temperature silicon bars oriented along the [100], [110] and [111] and some of them broke just when we started). We measured in all possible scattering configurations, not only backscattering but also 90 degrees scattering. Once we finished the last work during my postdoc stay, we passed our results to Christoph Grein and Stefan Zollner, who published a paper with the experiment and the theoretical developments [13].

I returned to Stuttgart in the next years either financed by the Spanish Ministry of Education, my University or the MPI, at least two periods of 3 months and a period of 6 months. During these years I started a nice collaboration with Paulo V. Santos (see, for instance [14]), with a contract in Manuel's group at this time, which is still active. Also, the collaboration with Karl Syassen was enriching. I had two bi-lateral projects (DAAD and Ministry of Education of Spain) with Karl Syassen and we did a lot of work, mainly theoretical, with pressure, some of them with Daniel Olguin, who also did a postdoctoral stay with Manuel. These stays, although shorter, allowed me to continue my scientific and personal relationship with Manuel. I also have a very friendly relationship with Karl Syassen, till nowadays. Another fruitful collaboration was with Vladimir I. Belitsky, a postdoc of Manuel. After the work in Stuttgart, he spent 3 years in Valencia with a NATO and Marie-Curie fellowships, then he came back to the MPI. From my postdoctoral stay, I have collaborated with many of the scientists I met in Manuel's group: Vladimir Gavrilenko, Alejandro Fainstein, Martin Kuball, Daniel Olguin, Nobert Esser, Andreas Göbel, Wolfgang Limmer, Clemens Ulrich, Ulrich Schwarz, Zoran Popovic, Jorge Serrano, Bernard Weinstein, Frank Widulle, etc. I also remember many scientific and personal discussions (during our squash matches) with Martin Stutzmann and Christian Thomsen.

I published with Manuel 45 papers from 1987 to 2005. From the collaboration some exchanges arose, I sent some Ph.D. students to Stuttgart and some people from Stuttgart came to Valencia, supported by a couple of INTAS projects I had with Manuel or to the joint actions between the Ministry of Education of Spain and the DAAD. Manuel became doctor honoris causa in the University of Valencia in 2005. There is still a web page [15] with the link available.

During all these years I had the opportunity to meet in Stuttgart outstanding scientists as Elias Burstein, Aron Pinzuk, Michael Tinkham, Emilio Mendez, Felix Yndurain and many other excellent physicists. Also, during my stay as a postdoc I remember that Herbert Fröhlich was visiting the MPI for a few months, either in 1988 or 1989 (he died in 1991).

I remember another anecdotes in Manuel's office. Luis Antonio García Navarro was the conductor of the Opera in Stuttgart during many years, then he moved to Spain to conduct the orchestra of the Spanish Radio and Television. He was a good friend of Manuel and visited him many times. In one of these visits at the MPI, Manuel called me to his office (García Navarro was also from Valencia) and introduced us. He told him: look, this is Andres Cantarero and he came to Stuttgart to study the band of Valencia (he always liked to make jokes with the words). Valencia is very famous for the symphonic bands, there is one in every town. Chiva, the birth town of García Navarro, has one of the most famous symphonic bands in Spain. As a final anecdote, Manuel invited me to go with them to the Spanish Consulate in Stuttgart on December 12th (around 2000), the Spanish day. At this time, the Spanish Consul in Stuttgart was a peculiar person, living with two sisters and some dogs. It was a coincidence that he had a house in Valencia and he was Consul in Tanger in the past. Then, Manuel told him that I was born in Tanger and the Consul said: "otro cabrón de Tánger". Inge was astonished. They reminded me many times of this anecdote with the Consul. Actually, this word in Spanish was very strong in the past, but nowadays it is commonly used in the sense of a person who does not behave properly or even a cleaver person. Its real meaning is the male of the goat.

I kept a very strong relationship with Manuel until the end of his life. I used to call him by phone very often. We spoke about everything: science, the political situation, personal problems, etc. The last time I was in Stuttgart I spent a day with Inge; Manuel was at the March Meeting, it was in March 2013. After that, the only contact I had with him was by phone.

References

1. G. Ambracevicius, M. Cardona, R. Merlin, Phys. Rev. Lett. **59**, 700 (1987)
2. T. Ruf, R.T. Phillips, A. Cantarero, G. Ambrazevicius, M. Cardona, J. Schmitz, U. Rossler, Phys. Rev. B **39**, 13378 (1989)
3. A. Cantarero, C. Trallero-Giner, and M. Cardona, Sol. State Commun. 69, 1183 (1989)
4. A. Cantarero, C. Trallero-Giner, M. Cardona, Phys. Rev. B **39**, 8388 (1989)
5. C. Trallero-Giner, A. Cantarero, M. Cardona, Phys. Rev. B **40**, 4030 (1989)
6. A. Cantarero, C. Trallero-Giner, M. Cardona, Phys. Rev. B **40**, 12290 (1989)
7. A. Alexandrou, C. Trallero-Giner, A. Cantarero, M. Cardona, Phys. Rev. B **40**, 1603 (1989)
8. C. Trallero-Giner, A. Cantarero, M. Cardona, M. Mora, Phys. Rev. B **45**, 6601 (1992)
9. A.R. Goñi, A. Cantarero, K. Syassen, M. Cardona, Phys. Rev. B **41**, 10111 (1990)
10. E. Anastassakis, A. Cantarero, M. Cardona, Phys. Rev. B **41**, 7529 (1990)
11. D. Vanderbilt, S.H. Taole, S. Narasimhan, Phys. Rev. B **40**, 5657 (1989)
12. D. Vanderbilt, S.H. Taole, S. Narasimhan, Erratum. Phys. Rev. B **42**, 11373 (1990)
13. C.H. Grein, S. Zollner, M. Cardona, Phys. Rev. B **43**, 6633 (1991)
14. P.V. Santos, A. Cantarero, M. Cardona, R. Notzel, K. Ploog, Phys. Rev. B **52**, 1970 (1995)
15. http://www.uv.es/uvweb/office-principal/en/awards-distinctions/honoris-causa-doctors/honoris-causa-doctors-21st-century/date-investiture/ceremony-naming-manuel-cardona-castro-doctor-honoris-causa-1285872205625/Honoris.html?id=1285878566102

Unforgettable Days at MPIF in Stuttgart

Kikuo Cho

After my Ph.D. study in Tokyo University, I spent 5 years and a half in Europe, during which I worked as a visiting scientist at Max-Planck Institut für Festkörperforschung in Stuttgart from September 1973 to December 1976. This was the starting period of my international scientific career, which provided me with many stimulating experiences about science, colleagues and/or friends, as well as the European way of life. This special period of my life has given me a very fertile source of my later life.

My stay at MPIF began by my writing a job-hunting letter to Manuel Cardona, which lead to a position of Gastwissenschaftler in the Institute. Being a theorist, I was put in the theory gruop of Heinz Bilz and assigned to work with the colleagues of experimental groups of Cardona and H.-J. Queisser. This mixed group, called "exciton study group" later, studied the optical properties of excitons and impurities of semiconductors under magnetic and electric field, stress, etc.

From the very first day, a meeting was held among the experimental colleagues and me, talking about how to write a paper out of the magnetooptics data of excitons in GaAs measured by them. This gave me a strong impression about the ambition of scientists gathering in the newly starting MPIF in Stuttgart, which turned out to become a crossing street of international scientific community, where we met famous scientists as well as young ambitious ones.

Our collaboration was well supported by the three bosses in organizing workshops, smaller discussion meetings at various places including the high magnetic field laboratory in Grenoble. The result of the collaboration turned out to be a paper selected as one of the invited talks in the semiconductor conference at Stuttgart in 1974, where I gave the talk on the stage of Beethoven Saal of Liederhalle, the main concert hall of the city.

My role in the collaboration was to analyze the fan chart of the exciton levels of GaAs by taking into account of the exchange interaction as well as the detailed band structure of the zincblende type semiconductors. The energy levels of exciton was assigned to the reflection minima of measured spectra, and compared with the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_9

calculated levels in magnetic fields. Though I gave the talk smoothly and the bosses expressed their content to me, there remained a dissatisfaction in my mind about the naive way of our making fan chart. It grew, much later, into a new theory directly calculating the reflectivity spectrum in terms of multi-level exciton polariton scheme.

It is not only from this collaboration, but also from nearly all the discussions with experimentalists, that I got the seeds of my future works. The typical example was my last long paper at MPIF, where I dealt with the symmetry breaking effects of the excitons in zincblende and wurtzite type semiconductors, summarizing all my experiences with my colleagues of MPIF about the perturbing effects of electric and magnetic fields, strain field, and finite wave vector [1]. Because of its rather abstract character, this paper can be used beyond the expectation of the author, which actually happened several times.

The largest seed I brought back from MPIF was the problem of additional boundary conditions (ABC), which turned out to be a very fundamental problem of optical response of bounded media. Through the study of the first-principles derivation of ABC, I walked into the new area of microscopic nonlocal response theory describing the “size, shape and internal structure”-dependent optical response of nanostructures [2]. Furthermore, this theory was found to be useful to derive macroscopic response (Maxwell equations) via an unprecedented logically clear way. In fact, by applying the long wavelength approximation to the fundamental equations of microscopic nonlocal response, we could derive the macroscopic constitutive equations describing all the electric, magnetic and chiral polarizations in terms of a 3×3 susceptibility tensor. The new feature of this theory is the single susceptibility character and the explicit microscopic expression of the susceptibility tensor [3].

Due to the presence of many coworkers, the paper production rate became higher, which seemed important as a raison d’être in the competing life at MPIF. In one occasion, one of colleagues wanted to put Cardona’s name as a coauthor, but he denied it, saying that he did not contribute to the work. I thought that he showed his pride and principle very clearly in this event.

Because of the presence of many foreign guests, there were many parties, picnics, wine probe excursions, etc. as entertaining programs. Among them the parties at director’s houses were very frequently held in large and small scale, where we developed international friendship. It was a good training chance of social relationship for young people from Asian countries like me. The topics were quite wide spread, and I was much impressed by the rich stock of topics of our hosts. Especially, Cardona had a peculiar talent about languages. He could, not only pronounce Japanese (and other foreign) languages correctly, but also showed a deep knowledge about them. When we were on a conference tour, he sang me a melody of Leporello’s aria from Don Giovanni by Mozart, shrugging his shoulders at the phrase “Ma in Ispagna son già mille e tre!”.

The friendship obtained during the stay at MPIF extended further afterward, which gave me many chances of new collaborations and visits to interesting places.

After my official retirement from Osaka university, I am still enjoying the research of the problems, the remote source of which can be found in my days at MPIF. For all this I am deeply grateful to the three bosses, especially Manuel Cardona who opened the way to MPIF for me.

References

1. K. Cho, Phys. Rev. B **14**, 4463 (1976)
2. K. Cho, *Optical Response of Nanostructures: Microscopic Nonlocal Theory* (Springer, New York, 2003)
3. K. Cho, *Reconstruction of Macroscopic Maxwell Equations: A Single Susceptibility Theory* (Springer, New York, 2010)

Manuel Cardona: Personal Recollections and Comments

Marvin L. Cohen

My first introduction to Manuel Cardona was through his publications. In the early 1960s, I was a graduate student at the University of Chicago, supervised by Jim Phillips, who asked me to look up all of Manuel's papers. I was working on semiconductors, and Jim was not only educating me, he was building a case to try to bring Manuel to Chicago. I still remember how impressed I was by what Manuel had done and was doing. I then became depressed when I realized that, "this guy is only 6 months older than I am". My high regard for Manuel continued and increased as time went on. As it turned out, I was fortunate enough to work in the same area as Manuel and to profit directly from his great productivity and deep insights throughout my career.

When in graduate school, I shared an apartment with a fellow student, Fred Pollak, who later worked with Manuel and gave me even closer connections to him and the results of his experimental studies. Unlike the atomic spectra of gases, which exhibited sharp lines that revealed the energies and symmetries of the electronic transitions causing them, solids had broad spectra that were difficult to interpret. Manuel Cardona was the pioneer in both measuring optical properties of many semiconductors, recognizing the connections between them, and eventually he contributed in a major way to interpreting optical spectra in terms of electronic band structures of semiconductors. I was fortunate to be a part of the extensive international experimental-theoretical collaboration that essentially solved the basic problem associated with deciphering semiconductor optical properties in the visible and ultraviolet.

This area in which Manuel laid the cornerstones turned out to be much more important than we, or at least I, expected at the time. Understanding the responses of materials to probes like photons in term of response functions became a central goal of condensed matter physics, and explaining response functions in terms of elementary excitations is the basis of our current theories. The methods and techniques developed for solving the puzzles associated with deciphering semiconductor electronic structures is a prototype for condensed matter physics.

The work Manuel did during this period stands as a monument to him. He saw order that others missed, and classifications where others saw complex spectra having no special features. He carried this special ability to other areas of

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_10

condensed matter physics, such as his classic work on Raman scattering, and became one of the most highly cited physicists in the world.

Manuel and I became close friends over the years, and as I said above, he was a part of my professional life since I started, so it has been difficult for me to adjust to his passing. I'm sure others feel the same way. We saw each other often at conferences, and he would spend time in Berkeley. Whenever he visited, people came out of the woodwork to talk to him. He fostered interactions and discussions, and we often found out through him what our Berkeley colleagues were doing. He had the ability to raise the activity level of any group he encountered.

In my opinion Manuel knew more about the basic nature of semiconductors than anyone in the world. His path-breaking work in this field that I discussed above spilled over into other areas of condensed matter physics and into other fields of science. He was extremely productive, creative, and influential. His experimental work was guided by his deep understanding of the underlying theory, consequently his research brought insight into the fundamental science of materials. Technology has also benefited enormously from this insight. Band gap engineering, the design of optical materials, understanding electronic devices in terms of electronic properties, catalysis, nanotechnology, etc. are some examples of the latter.

Manuel was one of the most "alive and curious" men I've ever met. He had a broad and profound knowledge of history of many different cultures, and of politics. And he was a truly international man who thought deeply about world events. He would send me books, and we had long discussions about many non-technical subjects. I hasten to add that we had even longer discussions on technical subjects.

My wife Suzy and I have had a close friendship with Inge and Manuel for many years, and we have valued and enjoyed their friendship tremendously. Suzy's standard comment about Manuel was, "he knows everything!", and her standard comment about Inge was, "she is one of the nicest and warmest people I know". I, of course would then echo her comments.

I would like to add a brief story about how Manuel saved my life. We were in a meeting in Spain and celebrating outside on a warm evening in the countryside. I was standing near the edge of a country road when a car that I didn't see came speeding around a curve directly at me. The driver obviously didn't see me either, but fortunately Manuel saw and understood what was about to happen. At the last moment before the car would have hit me, Manuel pulled me out of the path of the car. The driver stopped, and several of us held a screaming Manuel back, so that he wouldn't physically attack the man. I don't speak Spanish, but I could still understand what Manuel was saying.

I will miss Manuel as a colleague and as an intellectual companion, and mostly Suzy and I will miss his verve, his warmth, and his friendship. We feel fortunate for the time we had with him.

Strong Coupling at Cardona's Abteilung

Alex Fainstein

The Open Door

I first met Manuel when he chaired a Conference in Bariloche, it was 1992. The saying was that he had personally revised, and corrected, all submitted papers. More than a hundred if I recall well. I was finishing my Ph.D. Thesis, and surely more thanks to a recommendation from Paco de la Cruz than the weight of my CV, I was all lucky to have postdocs opportunities, one at Bell Labs and another one with Manuel at the MPI-FKF. Attracted by the fame of Manuel's group, what Pablo Etchegoin enthusiastically described of it as a recently arrived Ph.D. student, by the subject of optical spectroscopies, and by the prospects of an experience in Europe, I decided for the MPI. Manuel proposed that I apply for a Humboldt fellowship, and sent me by fax immediately after arriving back to Stuttgart a recently published paper on which I had to base my plan: "Observation of spin precession in GaAs inversion layers using antilocalization" [1]. That was all. And it left me in the dark. I had no idea about semiconductors or optics (my Thesis had been on macroscopic magnetic properties of High Tc superconductors, using magnetic resonance techniques), I had no idea about band structures of III-V materials either. How could I write an original proposal related to subtle electronic effects due to the lack of inversion symmetry, spatial dispersion, terms cubic and linear in **k**? Well, I did it, I wonder how. Manuel gave his agreement, and the application was submitted and approved by the Humboldt Foundation, including a project entitled "Inversion asymmetry spin-splitting in the conduction band of semiconductor heterostructures, and its effects in spin relaxation through transport and optical measurements under stress" (this was more than 10 years before these subjects became hot with spintronics!). The door was open to Germany, to the MPI, and to an experience that transformed my life.

And so I married (otherwise Mariana would have not followed me to the antipodes…, I also have to thank Manuel for *that*), spent two wonderful months at Schwäbisch Hall swimming and learning German, and finally one morning in April

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_11

1993 I arrived to Cardona's Abteilung. It was a huge jump from the group in Bariloche where I had worked for my thesis. It was in those years a lively and motivating environment, with around 30 permanent people (researchers, postdocs, graduate and undergraduate students) and many more visiting all the time. It felt like going from the countryside to the world capital; in many different senses. As an initiation rite into this new world, I shared my office with an introverted postdoc from India, who would rarely communicate, and only when something called his attention. One of the subjects that did call his attention was religion. I remember that discussions became somewhat weird, and would finish with him extracting a presentation card from his wallet, certifying his belonging to a kind of "intelligent people society". Did we have one of these cards? If the answer was no, as it always was the case, he would just state that the discussion was useless, and turn around leaving us wordless. Luckily I also shared the office with Emilio Artacho, who was later to be one of the inventors of the DFT code "Siesta". That ground cable helped me find the way through that maze.

At Cardona's Abteilung lights were on during nights and weekends, discussions populated the offices and labs, and at coffee time. There one could meet many people, and see groups remaining afterwards sitting in tables and scribbling on white papers ideas that could explain running experiments, or lead to new ones. I remember A. Cantarero, C. Trallero Giner, E. Ivchenko, all generously sharing experiences and explanations with me. Manuel used to recall his first scientific discussion with Carlos Trallero Giner, in which Carlos would refer repeatedly to "Maue's" equations, Manuel embarrassed to ask who this "Maue" could be, not to evidence that apparently somebody so famous that did not need further introduction was ignored by him. Only to realize long after that with his strong Cuban accent Carlos was referring to Maxwell… At those coffee times which would make everybody leave their offices to share a moment, Manuel would randomly walk between groups zapping subjects and languages, going through Physics but also his other many almost obsessing subjects: politics, history, racism and discrimination, fraud in science, German language and culture, literature….

So the doors were indeed open. And one of those was Manuel's office. Most of the time one did not need to go through his secretary; there it was that door wide open calling you to get in. Well, that was easy to say, it was (at least for me) not that easy to do. The protective force field in that open space was way more indomitable than a material door. I felt that to enter one had to have what one was convinced were relevant questions that one could not solve alone, things read, thought, and thought again. Or data collected and analyzed so that something interesting could come out. So there we were, approaching the open room with papers, figures and written equations under one arm, asking timidly if we could come in. Manuel would make place on his table, always full of papers and books, listen to what one had to say, grab a block of paper, and start drawing curves and equations. Many times without realizing that with his passion holes were scratched on the paper, or the pencil was broken. Incomprehensible matrix elements connecting excited states in $\mathbf{k}\cdot\mathbf{p}$ theory, tendencies induced in the bands, generalized behaviors that should appear in the experiments… The end of the story would be with one living the room with a block of paper doubled in size, thinking that some light shined (surely in *his* head), but mostly confusion prevailed.

So I started with optical experiments under magnetic fields, learnt my first lessons from Tobias Ruf and the great Victor Sapega, who guided me in my first experiences with lasers, photoluminescence, and Raman scattering. And as planned for my Humboldt fellowship, I made my trials on the two dimensional electron gases. Most probably because of my lack of ability, although I preferred to believe that the highly doped samples grown at that time at the MPI were not good enough, the fact is that I could not find what I was looking for. Not even reproduce the beautiful experiments Bernard Jusserand at the CNET laboratories had published on the same subject before I could even arrive to Stuttgart [2]. Luckily I didn't even have time to become depressed considering my poor situation. Manuel's wave simply rushed me away from that state, attracting me to something that was passionately calling his attention.

Occam's Razor

A group at Southampton had published a series of papers based on the observation of optical anisotropies in reflectivity experiments on GaAs, claiming through them the observation of time-nonreversible optical effects in zinc-blende semiconductors [3]. This meant that bulk GaAs in equilibrium, a cubic semiconductor, could display natural birefringence due to symmetric terms linear in wavevector in its dielectric function. These terms linear in wave vector **k** would arise when spin-orbit interaction is included, and would lead to natural birefringence. To put it simple, the lack of inversion symmetry in zinc-blende semiconductors leads to the spin-splitting of states of finite **k**, something that can be interpreted as a k-dependent effective magnetic field. The argument then went that this would manifest in macroscopic effects similar to those of a magnetic field, and so to a lack of time-reversal symmetry even without the application of an external magnetic field. Manuel had a long experience with spatial dispersion effects on the dielectric constants of semiconductors, starting with the historical paper with Peter Yu describing effects quadratic in **k** [4], and more recently with experiments performed by Pablo Etchegoin, describing terms linear in **k** (and optical activity) induced in GaAs by uniaxial stress [5]. Manuel argued, I would say passionately, that this was not possible. Based on spatial symmetry arguments, and quite general considerations using Onsager relations for the kinetic coefficients, he argued that the referred symmetric terms were forbidden. So he put Paulo Santos (group researcher) and Lew Yan Voon (postdoc) to make calculations, and Pablo Etchegoin (Ph.D. student) and me to perform optical transmission experiments in bulk GaAs.

Paulo and Lew demonstrated theoretically that the claimed effects supposedly related to spin-split states at finite **k** in the band structure cancelled out after the Brillouin zone integration required in calculating optical constants was performed [6]. As expected from the more macroscopic arguments commented above. Our experiments showed that anisotropic contributions as that claimed to be due to time-nonreversibility in cubic bulk materials were indeed present, though of magnitude much smaller than seen in the previous reports (10^{-3}). And we also observed that these polarization changes did not scale with sample thickness, but rather varied erratically from sample to sample [7]. The conclusions were obvious: defects or surface reconstructions always present in surfaces (which obviously lack cubic symmetry) were the cause of the observed tiny anisotropies.

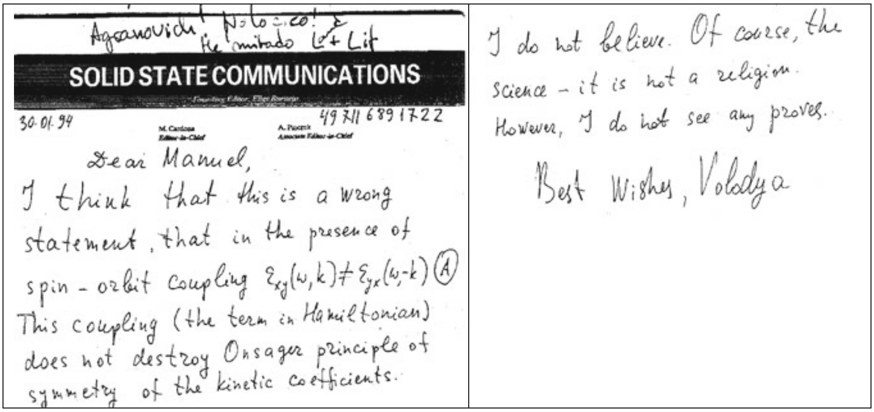
This was the first time I heard Manuel citing "Occam's razor". Occam's razor states basically that among competing hypotheses that predict something equally well, the one with the fewest assumptions should be selected. That is, in the absence of differences in predictive ability, the fewer assumptions that are made, the better. I have to say that I have resorted many times to the help of Occam's razor. Sometimes with students. Many more with referees that like to ask why complex explanations are not better than the simpler ones one proposes. I cannot avoid at those times to remember Manuel with a smile.

Faith in Science

I believe that Manuel had a deep faith in science, in the rational explanation of physical phenomena. And hence also on the passionate search of subtle effects that according to prediction should appear, irrespectively of how tough the experiment could be. That is how we found ourselves embarking many times in difficult experiments. As concerns the ones described above, I have to say that not only the experiments were rather subtle, also the subject was hard to grasp. Though we all spent hours and hours discussing rather in the dark (bright people as Paulo, Lew and Pablo), I am not sure to reflect what everybody felt. So I will only claim my ignorance: I did not understand deeply what we were discussing, and in the end only felt at ease when the experiments talked by themselves.

I do not feel that bad, though, because this story led to another crucial learning. In fact, Manuel did not only discuss these things with us. He did it also with the world expert in spatial dispersion effects in crystal optics, Vladimir Agranovich. I keep from Manuel a copy of one of those exchanged faxes (see fragments of it in Fig. 1). Beginning by one of typical Manuel's written comments stating "Agranovich! No lo *cree*!" (Agranovich!, does not *believe* it!), we can read Agranovich himself stating that "I *think* that this is a wrong statement", rounding his arguments with a "I do not *believe*", just to finish with a cyclic self-negating argument like Catch-22: "Of course, science is not religion".

I find this a wonderful letter. If such two enormous people were arguing in these terms, I could at least not feel so bad. So with this story I learnt from Manuel several things. I realized that while there are very bright people in science, there are no Gods. That curious belief some of us may have had when young, that somebody


Agranovich

SOLID STATE COMMUNICATIONS

30-01-94

M. Cardona, Editor-in-Chief — A. Pinczuk, Associate Editor-in-Chief

49 711 689 1722

Dear Manuel,

I think that this is a wrong statement, that in the presence of spin-orbit coupling $\varepsilon_{xy}(\omega,k) \neq \varepsilon_{yx}(\omega,-k)$ (A)

This coupling (the term in Hamiltonian) does not destroy Onsager principle of symmetry of the kinetic coefficients.

I do not believe. Of course, the science – it is not a religion. However, I do not see any proves.

Best Wishes, Volodya

Fig. 1 Fragments of a fax from V.M. Agranovich sent to Manuel, January 30, 1994

must have the answer, an answer that is for us to be found, disappeared. And I also realized that while somehow faith in science is our precept, it is also true that many times arguments in science are not necessarily void of faith.

One Experiment More

The optical transmission experiments in bulk GaAs, and the discussions involved, lead Manuel to propose us to extend these studies to the optical anisotropy of multiple quantum wells (MQWs), with light propagating along the layers of the structure. Quantum confinement, the argument goes, should change the cubic in **k** dependence of spin splitting of electronic bands, so that linear in **k** dependencies are expected to appear with the corresponding consequences in the macroscopic optical properties. Paulo Santos was involved with calculations, and together with Bert Koopmans they performed reflectance difference spectroscopy studies from the side of the structures [8]. Because the lateral size of the MQWs were of the order of a micron, a confocal microscope was developed for that purpose. For the transmission experiments we had to rely on wave guiding concepts. Experiments were tough, but finally with Pablo Etchegoin we were able to measure and report the in-plane birefringence of GaAs/AlAs MQWs [9], also with applied uniaxial pressure along the growth axes [10]. These latter experiments were really painstaking. Sample preparation would take 2–3 weeks, and the sample would break as soon as we started to apply pressure. Finally after some 3 months of hard work and repeated failures we arrived to extract valuable data from *one* sample…., before it broke in thousands of pieces. I was exultant, kind of madly shouting success. Pablo, calmly and without a hint of a smile, stood up to grab his coke (he had always one at hand), and just commented that anyway, all experiments had to be repeated again. To the best of my knowledge, that was the single and only ever obtained optical anisotropy data for in-plane propagation in MQWs with uniaxial pressure along the growth axis. We escaped to our next scientific scales before Manuel could ask us to repeat anything like that.

Pablo in Cambridge, myself as a postdoc with Bernard Jusserand in Paris (another great door opened by being a "former Cardona student"), we continued analyzing with Manuel the birefringence data, particularly a strange observed dependence with MQW period of the zero-frequency limit of the birefringence. Manuel proposed that we were observing a long sought local field effect in the dielectric constants, something quite difficult to evaluate theoretically [11, 12]. Pablo made some simplified numerical calculations that agreed with what we observed, and so we wrote a four pages paper to be submitted to PRL. Tenths of faxes went one way and the other between Stuttgart, Cambridge and Paris. We were polishing the text, minor details before submission, when we realized that something might be wrong. The experiments were OK, but we probably had made a mistake when extracting the birefringence from the data. The subject was important, the experiments complex, calculations seemed to confirm what we claimed, the paper was ready for submission…, but the doubt was planted. Since one measures *one* value for a specific laser energy (the transmitted intensity through crossed polarizers), but has *two* unknowns (the birefringence and the value of the integer number of π phase shifts acquired by the beam in traversing the sample), one needs to assume that, well below the gap, the birefringence is essentially constant for two successive energy points. It is a standard approximation. It is reasonable in many cases. Here because of strong dispersion it turned out to be dramatically wrong.

Figure 2 is a cite from Locke that Manuel gave Pablo and me at that time. Capital letters are Manuel's. The PRL manuscript was sent to hibernate, and Andrei Sirenko who arrived as postdoc some time before we left Stuttgart, performed the conclusive experiments [13]. In his beautiful experiments, using as additional degree of freedom the sample length in a tapered sample, Andrei demonstrated that indeed our quantitative estimation of the birefringence in [9] was badly wrong. And so was everything we had concluded afterwards concerning local field effects.

So we learnt that one should always perform an additional experiment if it is readily available and can define an open question. But the main lesson I recall is not

Fig. 2 When to stop experiment. Copy of cite given by Manuel to Pablo Etchegoin and me

Error is not a fault of our knowledge, but a mistake of our judgement giving assent to that which is not true …

… It is in man's power to content himself with the proofs he has, if they favor the opinion that suits with his inclinations or interests and so STOP FROM FURTHER RESEARCH

John Locke
17th century

necessarily that one. In fact, I did not see in this shared long experience, either in Manuel, or Pablo, any bad feeling because of the published wrong paper, or the time lost with discussions, calculations, and writing of the useless follow-up paper. They were simply happy that we had understood something. And ready to continue with the same unperturbed passion, with the following call.

Manuel and Argentina

I understand Manuel first came to Argentina with Inge in the 60s, as a visiting scientist for a sabbatical stay at the Physics Department of Buenos Aires University. He has many stories from those times. One he always liked to refer to was about a paper he received from a group in Buenos Aires some time later as Editor, concerning band structure calculations in semiconductors. He referred the paper himself. Apparently the paper was very good, but he also felt it was incomplete. He wrote back to the authors asking them to perform some obvious extensions that would enrich the work. The answer was short and convincing enough so that Manuel accepted the paper without further comment: they agreed, and had already thought about that. But a military coup, a very dark in Argentina's history had taken power (others had afterwards the opportunity to improve that ability to ever darker levels), and one of the consequences had been the closing of the calculation facilities of the involved Physics groups. So they had had to stop the project in the middle, and leave the country. In fact many scientists left Argentina in that and subsequent military coups, some welcomed and protected by Manuel at the MPI.

The list of Argentinians that were Manuel students or close collaborators is large, including A. Gavini, F. Cerdeira, R. Merlin, D. Olego, J. Menéndez, A. Goñi, P. Etchegoin, M. Grimsditch, F. de la Cruz, and A. Pinczuk. Manuel was recently honored with the Leloir Prize of the Ministry of Science and Technology of Argentina. The prize is named after one of Argentina's science Nobel prizes, the physician and biochemist Federico Leloir, and distinguishes foreign scientists that through collaboration had a particularly strong impact in Argentina's science and technology. As a Spanish born working in Germany, and before in the US, I believe Manuel saw the international migration of people as something to be welcomed, as part of the development of international science and collaboration. His group was a good example of that. However two cases called his attention as exaggerate, as clear examples of "brain drain". One was Argentina. Another one, particularly when we were in Stuttgart, was Russia. He would joke that he had strongly profited from the bad policies of those governing these countries, with so many excellent scientists that had passed some time at his group. The fact is also that he was always giving back, generous as a host, helping those that tried to return.

In 1980 Manuel visited the Balseiro Institute in Bariloche for a meeting celebrating its 25 years of (short) history. Together with other scientists, including Nobel Prize winner John Schrieffer, he was asked to provide an evaluation of the Institute, and recommendations for future developments. The conclusions suggested that the Institute should open to what was new and promising at the time, the installation of clean rooms for the development of semiconductor materials and devices, and to optical spectroscopies, a vacancy at an Institute that otherwise had strong experimental traditions. Three of us, Alejandro Goñi, Pablo Etchegoin, and myself, former students from the Balseiro Institute in Bariloche, and trained as Ph. D. or postdoc researchers at the MPI, presented in 1996 a proposal for the installation of a Raman lab. The proposal was accepted and funded, and Pablo and me we returned to start this new enterprise. The laboratory now has several optical spectroscopies (including Raman) and ultrafast laser facilities, is actively productive and collaborates with many groups worldwide. Some twenty excellent students obtained their degrees working in these techniques and subjects at the laboratory. Manuel can be viewed at the origin of all this, these students as his scientific "grandchildren". Not only because he first proposed the existence of the laboratory, and we entered these subjects by his hand. He also accompanied us in these developments through active support, and recommendations when asked.

The People Around Manuel

The stories above are just a very personal recollection of some events in my relation with Manuel. My experience in Stuttgart, and particularly at the MPI, was also marked by the people around Manuel. Besides the many named above, two wonderful women left their mark on all of us. Inge, always there warmly making ourselves and our families feel at home. And Sabine Birtel, the soul of the group, always so well disposed, efficient, and close.

Manuel must have had much more than an enormous brain to gather so great people around him. Those of us that were lucky and had the opportunity to spend some time in Cardona's Abteilung, were strongly influenced by his knowledge and great personality. To put it in some informal physics terms: we experienced "strong coupling". We did not just cross, we rather anti-crossed. Our wavefunctions got irremediably mixed from then on. I wonder how Manuel did it; I guess his norm must have been larger than one, to spread so markedly in so many people… Now that his wavefunction has somehow "collapsed" (has it?), we are left on our own, his traces warmly and indelibly entangled on us, for us to discover, learn and remember. At least that is what I believe.

References

1. P.D. Dresselhaus et al., Phys. Rev. Lett. **68**, 106 (1992)
2. B. Jusserand et al., Phys. Rev. Lett. **69**, 848 (1992)
3. N.I. Zheludev, et al., Phys. Rev. B **50**, 11508 (1994)
4. P.Y. Yu, M. Cardona, Solid State Commun. **9**, 1421 (1971)

5. P. Etchegoin, M. Cardona, Solid State Commun. **82**, 655 (1992)
6. L.C. Lew Yan Voon et al., Phys. Rev. B **52**, 2201 (1995)
7. P. Etchegoin et al., Solid State Commun. **92**, 505 (1994)
8. B. Koopmans et al., Appl. Phys. Lett. **69**, 782 (1996)
9. A. Fainstein et al., Phys. Rev. B **50**, 11850 (1994)
10. P. Etchegoin et al., Phys. Rev. B **53**, 13662 (1996)
11. N. Wiser, Phys. Rev. **129**, 62 (1963)
12. R. Resta, A. Baldereschi, Phys. Rev. B **23**, 6615 (1981)
13. A. Sirenko et al., Phys. Rev. B **60**, 8253 (1999)

A Personal View to the Figure of Manuel Cardona from a Few Disperse, Little Stories

Alejandro Goñi

I first met Manuel in April 1986 at my arrival at the Max-Planck Institute FKF in Stuttgart, appointed as his Ph.D. student for the next 3 years. Prior to that I have had just two mails from him (no emails those days). In the first one he asked about my research experience and my interest in pursuing a career in Germany. With the second one he already told me that he was willing to give me this chance by offering me a grant. Later I learned that such a "miracle" (at least for me) happened after Manuel called Paco de la Cruz in Bariloche, Argentina, and Paco recommended me for this job. Needless to say how grateful I am to both for that opportunity. My life changed completely in a few months: I finished my degree, set myself to move to another country with different culture and language and, one month before traveling, I got married! Small detail, one would say, but I still remember the suspicious face of Manuel when at our first meeting, talking about all the things I was supposed to do, I just slipped the issue that I did not come alone, that my wife was also a physicist and was looking for a Ph.D. grant, etc. He was not precisely "happy", but I did not know why and he did not tell. Thus, it does not take much to imagine my state of mind when only 3 months later we went to the doctor because my wife was feeling bad very often (more than expected from my driving style in hilly Stuttgart) to be informed that she was pregnant! The truth is that I felt ill in bed for 3 days not having the guts to tell Manuel about the third member of the family. Three days and nights having only nightmares in which I saw both of us returning to Argentina without title, without career, without money but a big belly… Of course what happened was exactly opposite to my fears. When the baby was born, Inge and Manuel were as excited and happy as we were (even though I called past midnight to communicate the good news).

As a scientist Manuel was certainly unique. A grazing look at his vitae provides sufficient evidence of his incredible professional career, his many scientific achievements, his managerial skills and his astonishing productivity. Many more than thousand publications including books, several of them which were real pioneering studies that became, as time went by, the holy grail of subjects like modulation spectroscopy, Raman scattering in solids or the effects of strain, isotopic substitution or temperature on the vibrational, electronic and optical properties of semiconductors and their nanostructures. Even though he did not write all of them, you can be sure that he read every single page, always providing added value to the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_12

publication with his suggestions or corrections. There is, though, no better impression of his greatness than the one you obtained after one of these memorable brainstorming sessions in his office, sitting around a small table full of papers distributed in apparently disordered piles that you would have to shift aside to find a place where he would write down his thoughts. I still keep some of the sheets of paper with his drawings and back-of-the-envelope calculations, which he used to materialize with a fountain pen with an extra wide nib. Only his pen was capable to sustain the extremely high pressure applied by Manuel. Pressure that was proportional to the time you needed to understand his point, with the usual consequence that the paper did not stand the multiple passes of the wet nib, the latter producing long scratches on the sheet and whatever you left underneath (other papers, books, acquired spectra, a postcard or photograph, etc.). In such tête-a-tête occasions you would be able to admire his mind, sharper than Japanese steal and agile like a panther, his multidimensional knowledge and above all, his unbelievable memory. It happened that all of the sudden he would scream that this or that was already studied by somebody else. Manuel just stood up and went directly to climb his bookcases full of issues of the Physical Review and Solid State Communications volumes, using a feeble staircase that would let you foresee only the worst. During the searching process he would provide you with all kinds of information about one or more of the coauthors like marital status and corresponding affaires, number of nervous breakdowns or jobs he/she did not get, including probably several other citations to related work. He then would pick up the right issue out of the thousands on the wall and by jumping back at his place would open the issue almost at the correct page and with an ample smile would put his finger at the previously mentioned graph or equation. These sessions, I resume, were memorable but scarce. As a student I had probably three or four times a year the benefit of such encounters. They lasted typically for hours and when your brain was about to explode from the uninterrupted effort of trying to assimilate the terabites of information thrown at you and to follow Manuel's line of reasoning, he would let you recover a little by talking about whatever came to his mind in this moment: Politics, art or history, you just name it. For all the profit you might have taken home from such a brainstorming, the feeling thereafter was of devastation. You just felt that you needed an additional brain and a second life to cope with the "easy" to-do list of things you were supposed to check, measure, learn, estimate, calculate or process. This feeling was nevertheless efficiently overcompensated by the profound impression caused by seeing him in action and the contagious enjoyment at performing science for the very pleasure of enhancing your understanding about whatever problem. These were the moments when you grasped an idea of what is all about in science.

Despite his scientific stature and brain power, there were things at which Manuel was not god at. Yes, Sir, he was also human! One of these things was car driving. He is without doubts the lousiest driver I ever met. Him driving the 200 km from Stuttgart to Munich, for example, was a real torture. The car in second gear all the time, hauling so loud that nobody in the car could hear Manuel's explanation about the Raman active modes of a novel high-Tc superconductor. Let us face it, driving a car and a life representation of phonon eigenvectors using hands, shoulders, head and legs are incompatible activities. Another handicap was his fine motility in the lab. He would irrupt into your lab in the middle of an experiment, the data on the screen would catch his attention, and that was it. He would then start touching all the screws of the optical setup it took you hours to align, to finally conclude that the observed phenomenon though interesting, was not reproducible. By the time he left, you kept yourself asking about what phenomenon, while desperation increasingly crawls over you at watching the leftovers of your well-aligned setup like a smoking battlefield.

Another story which might not be well known concerns what he told me once, when I was looking for a postdoc position in the US and got already my first rejections. He provided some relief by pointing out that such a thing happened even to him. I could not believe it, for you think that people like Manuel never fail, since they were born knowing how nature works (I imagine him reproducing all symmetry operations of his set of wooden blocks in kindergarten, for instance). He confessed me with an incipient smile on his face that once he applied for a position at General Electric Research Laboratory. The person in charge of the interview was nobody else than Evan O. Kane, who is considered the father of the $\mathbf{k}{\cdot}\mathbf{p}$ method, an empirical perturbation approach for the calculation of the electronic band structure of crystals that needs a small number of input parameters such as the values of the fundamental band gap and a few atomic parameters like the spin-orbit coupling or the momentum matrix element between atomic s and p-type orbitals. This method gained rapidly widespread acceptance due to its simplicity and ease of use, allowing you to obtain a fast and sometimes sufficiently accurate estimate of important parameters like the carrier effective masses. Furthermore, it allows for the straightforward incorporation of the effect of stress and/or quantum confinement on the electronic band structure. The envelope-function approximation, which is widely used to solve the Schrödinger equation in the presence of quantum confinement, for example, is one of the byproducts of the $\mathbf{k}{\cdot}\mathbf{p}$ method. In the mid-sixties, however, the method was not very well known yet and Manuel, not aware of recent publications on that matter, did not get the job because Kane was not satisfied with an explanation in other terms rather than using $\mathbf{k}{\cdot}\mathbf{p}$. Somehow ironic is the fact that precisely Manuel has been one of the principal contributors to the development of such empirical calculation method, applying it for the interpretation of the changes in optical spectra of all kinds of semiconductors as a function of temperature, uniaxial stress or hydrostatic pressure.

Finally, I would just like to say thanks Manuel. Thanks for sharing with me your knowledge and excitement for science (and many other things). Thanks for being a teacher, sometimes a father, a colleague and, despite the age difference, also a friend. I sometimes fancy about my personal tribute to Manuel consisting in trying to emulate him in the way of transmitting to students and/or colleagues whatever I might have learned, something new I had figured out, or whatever I might have discovered. This is the simplest yet most effective way I can imagine to make further perdurable his impact upon us and upon science. Herr Böhringer, a great and beloved technician at the Max-Planck Institute FKF, used to say that nobody is irreplaceable. Manuel is probably the exception that confirms the rule…

Manuel to Walt

Walter A. Harrison

Manuel to Walt:

"Germans tend to take simple problems and make them difficult. Fortunately, Germans are very good at solving difficult problems!"

With many fond memories,
Walt Harrison

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_13

Manuel Cardona, a Distinguished Promoter of Solid State Physics in Latin America

Isaac Hernández-Calderón

It was a privilege to be part of the research group of Manuel Cardona, one of the most influential physicists of the last decades. It was also a privilege to share his friendship for many years. Manuel had a strong influence in the development of Solid State Physics in Latin America. He organized and attended regional meetings and received a significant number of Ph.D. students, postdocs and senior researchers in Brown University and after 1971 at the MPI in Stuttgart, many of them became distinguished scientists.

After finishing my M.Sc. Studies at the Physics Department in Cinvestav (Mexico City) I went to the Institute of Physics of the State University of Campinas (Unicamp) in Brazil, to work on my Ph.D. thesis. Often I heard my professors, among them Ray Tsu (my advisor), Fernando Cerdeira, Sergio Porto, Rogerio Leite and others, talking about optical properties of semiconductors and referring to the "papers of Cardona". When I started experiments on Raman spectroscopy of several types of coals and graphites I was looking for some publications to understand the technique. I found many papers where one of the authors was M. Cardona, then I had the satisfactory feeling that very soon I was going to be able to contribute to the conversations of my professors. One day in 1978, while I was walking in the main corridor of the Institute of Physics I heard Ray Tsu calling me, he was at the other side of that corridor talking with great enthusiasm with a visitor. As I reached them he introduced me with Prof. Manuel Cardona, I was very happy to know him. Ray told Cardona that I was a good candidate for a postdoctoral position in his group. Manuel told me that when I was nearly finishing with my thesis I should contact him. It was a short but friendly encounter; when he knew that I was Mexican he talked to me in Spanish and told me that he enjoyed very much Mexican food. Due to my Ph.D. on carbonaceous materials, at the end of my doctoral work I received an offer from the Atlantic Ritchfield Company (ARCO) to spend a postdoctoral stay in its laboratories in USA. This was very exciting, at that time oil prices were very high and salaries for a postdoc in this area were very good. In December of 1980 I prepared my CV to send it to ARCO and went to the copier machine to make some duplicates. Going back to my laboratory, I unexpectedly

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_14

meet with Manuel Cardona who was again visiting Unicamp. To my great surprise, he remembered me, he saw that I had copies of my CV in my hand and told me "¡ya veo que vienes preparado!" (I see that you are ready!). I gave him a copy of my CV. Few weeks later, in January 1981, I received a letter, in his own handwriting, indicating that I had been accepted for a postdoctoral position in his group at the Max-Planck-Institut für Festkörperforschung in Stuttgart. Then, I had to take an important decision, work in the private industry in ARCO or take the postdoctoral position in one of the most prestigious institutes in solid state physics. I was convinced that going to the Max-Planck-Institut (MPI) was the most attractive choice. In October 1981 I was already in Stuttgart supported by a scholarship of the Alexander von Humboldt Foundation.

Initially, I was going to work in the growth and characterization of $Ge_{1-x}C_x$ alloys. However, when I arrived to the MPI, after a general description of the research activities at the institute and in his group, he told me that was much more interesting to work in the epitaxial growth and characterization of α-Sn. This explanation took place in his office, over his desk, which was an apparently disordered multilayer of papers, books, pictures, notes, drafts, mailing envelopes, cards with annotations and one of his most indispensable desk tools: his Gerber variable scale. α-Sn is a zero gap semiconductor with diamond structure and is unstable at room temperature; the α to β-Sn (tetragonal, metallic) phase transition takes place around 13.2 °C. The goal was to produce metastable α-Sn thin films by molecular beam epitaxy (MBE) on top of InSb, a nearly lattice matched substrate. The previous studies of the electronic and optical properties were done in small crystals highly contaminated with Hg. This work was done in collaboration with Hartmut Höchst, who was an expert in photoemission spectroscopy and very skillful for mounting experimental systems. Sometimes the downside of his designs was that they were suitable for a person of his height (over 2 m), but unwieldy for a normal person. Employing a very compact MBE system built at home, we produced thin films of α-Sn that were characterized by angle resolved ultraviolet photoemission spectroscopy (ARUPS) and reflection high energy electron diffraction (RHEED). These studies produced a significant number of publications describing the structural and electronic properties of α-Sn. The films had the advantage that due to substrate stabilization the transition temperature from the α to β phase was larger than 100 C. These films were also used for optical characterization by ellipsometry (Luis Viña), Raman spectroscopy (José Menéndez) and other studies. Near the end of my second year of postdoc, Manuel asked me to stay another year to continue the work on α-Sn and also to complete work recently initiated related to the implementation of system of high resolution electron energy loss spectroscopy (HREELS). With this system, I studied the surface phonons of InSb and α-Sn and made studies of the vibrational properties of hydrogen adsorbed on the different surfaces of these materials.

His notable capacity for intensive work and singular intelligence and memory allowed Manuel to benefit of the great facilities available at the MPI to develop with success his scientific activities. On the other hand, there is no doubt that Manuel established his best partnership with his wife Inge, she is probably the one that most contributed to his scientific success. Inge was always near Manuel with unconditional and loving support that allowed him to focus on his scientific activities. Her care and kindness inspired us great confidence and helped to give a familiar look to the group of Manuel. They formed a delightful and almost inseparable couple. Warmth and friendly, they made us feel part of the family. They were happy to assist anyone in many ways. Manuel and Inge often organized dinners at home to welcome some important visitors or to celebrate a major achievement. They were excellent hosts, we enjoyed rich snacks and food and very good German wines. It was a great opportunity to establish friendships with young researchers and renowned scientists. The wives of many of us found in Inge a great friend and confidant helping them overcome the initial stages of adaptation to the German lifestyle.

During my 3 years stay at the MPI I had the opportunity to talk many times with Manuel of very different matters. He was an educated man, besides his impressive comprehension of physics he had a broad cultural knowledge, it was always a learning experience. For me, and all the members of his *Abteilung*, it was very motivating to find opportunities to talk with Manuel of the recent news. He was always aware of the most relevant political issues at the international level, he described facts with names, amounts and details and he had a strong democratic conviction. We also enjoyed very much to talk about etymology, in particular of the very different words with the same meaning and the very different meanings of the same word depending on the particular language and country. During his last visit to Mexico in 2010 he was very interested in the word “tlapalería”, which describes a store selling paintings, tools, building materials, electrical equipment and many other domestic and industrial materials. It comes from the Nahuatl word “tlapalli” and means “color”, a term with great significance to optical spectroscopists.

His first trip in 1965 to the University of Buenos Aires (Argentina) was the beginning of a fruitful and long academic and friendly Latin American relation, triggered in 1966 by the persecution of graduate students by the Argentinian military dictatorship. His intervention saved the life and contributed to the formation of many Argentinian physicists. He received many Ph.D. students and postdocs from Mexico, Argentina, Brazil, Colombia and Cuba. Of course, he also received numerous students and visitors from Spain. Manuel participated in the organization of several meetings in Latin America. He carefully reviewed dozens of papers and edited the proceedings of many conferences. His renowned reputation contributed to attract many top scientists who later hosted graduate students and established collaborations with young regional researchers.

Every day after lunch, the “Abteilung Cardona” members and many others met for coffee on the 7th floor of the MPI building. We spent very pleasant and amusing time talking with Manuel and many other internationally famous scientists who constantly visited Manuel for short and long stays. At the beginning of the 80s Manuel Cardona had already built a strong international reputation that attracted the most important semiconductor physicists. Very often, the coffee time extended much more than the German rules dictated. We realized that we were an important crowd speaking Spanish, something that we tried to avoid because we had an implicit agreement of not staying together too long; we wanted to be fully integrated with the MPI community. I remember Luis Viña, Narcis Mestres, Francisco Messeguer, José Menéndez, Elías López, Gerardo Contreras, Rafael Miramontes, Fernando Cerdeira, Manuel Vázquez, Pepe Sanjurjo and me around Manuel, who had always many interesting anecdotes about physics, physicists, politics, history and many other topics.

Manuel and Inge visited Mexico many times; they had friends and former students and postdocs along the whole country and were very attracted by the Mexican folklore, art and indigenous cultures. Manuel had a profound knowledge of

Picture 1 Manuel Cardona and Isaac Hernández-Calderón at the Physics Department of Cinvestav in 1999

Picture 2 Manuel Cardona, entering the Mexican Academy of Sciences as corresponding member, 2002

Mexican history and recognized with great gratitude that Mexico had received tens of thousands of Spanish immigrants who arrived between 1939 and 1942 as refugees from the Spanish civil war. He participated in numerous scientific events and gave talks in Cinvestav (Picture 1), the National University (UNAM), the National Polytechnic Institute (IPN), University of San Luis Potosí and many other institutions. In 2002 he became a corresponding member of the Mexican Academy of Sciences (Picture 2). He was very comfortable visiting my place of work, Cinvestav in Mexico City, a center for research and advanced studies that he considered one of the top scientific institutions in Latin America. I organized several workshops where old friends meet and Manuel enjoyed talking with young students. In a workshop of optical spectroscopy in 2002, during one of the coffee breaks, a long line of students spontaneously formed and patiently waited for his autograph in his famous book Fundamentals of Semiconductors. Smiling he signed more than

Picture 3 Inge and Manuel Cardona in Cinvestav, Mexico City, 2012

40 books and the happy students talked with him and took pictures as if he were a soccer star. In August of 2010 he received a doctorate honoris causa from Cinvestav (Picture 3). Very grateful and touched mentioned that it was the first of its kind received from an institution of an American country. He made the remark that his collaborations with researchers of this continent had been the most productive of his life. Inge and Manuel enjoyed visiting museums and small Mexican towns and appreciated very much the broad variety of foods, regional traditions and cultural activities. Two of his Ensslin grandsons lived in Mexico for around 1 year, Jonathan in Merida in 2012 and Caroline in Cuernavaca in 2013, Manuel was very proud that they learned Spanish and could speak as Mexicans. He knew many Mexican songs and enjoyed to sing at the sound of the Mariachi bands (Picture 4).

In August of 2014 I organized in Mexico City the Sixteenth International Conference on High Pressure in Semiconductor Physics (HPSP-16), a satellite of the International Conference on the Physics of Semiconductors. As a part of the conference, we were planning a special session to celebrate Manuel's 80th birthday. Unfortunately, with deep sadness and dismay we received the news that on July 2, just weeks before the event, Manuel passed away while in the MPI. In his honor we

Picture 4 Manuel Cardona singing Mariachi songs. Querétaro, August 2012 (Picture courtesy of Peter Yu)

organized an emotive memorial session. Among others, Peter Yu, Karl Syassen, Bernie Weinstein, Andres Cantarero, and me recalled his great scientific achievements, but especially his great humanity, generosity and friendship.

There is a large community of scientists, that inspired by Manuel, integrate an international family that remembers him with great affection and admiration.

Manuel Cardona, Isotopic Effects, and Path Integrals

Carlos P. Herrero and Rafael Ramírez

We are grateful to Manuel Cardona in many respects, beginning from the moment when we arrived at the Max-Plank Institute (MPI) in Stuttgart in the 1980s. In fact, his positive influence on our lives started before going to Germany. His help was essential for one of us (RR) to have the opportunity to start a Ph.D. in a chemistry department at the MPI. Such a generous help has been determinant not only for a subsequent professional career but for a whole life trajectory.

From the moment of our arrival to our departure from Stuttgart, Manuel always helped us in whatever questions or problems we could have. In spite of his multiple activities, including the direction of a large and prolific research group, he was always ready to make that we could feel ourselves there in Stuttgart as if we were at home.

During our stays in Germany we did not collaborate directly with Manuel, but benefited from many illuminating discussions, comments, and suggestions of him. It is not easy to summarize in a few words the deep insights that we obtained from discussions and conversations with Manuel about problems on which we were working, and that were apparently not among his immediate interests. The sense of treating with an extraordinary human being has been always present in our interaction with him.

It was several years after leaving Stuttgart when Manuel contacted us in relation to our papers on path-integral simulations of solids. It has been generally acknowledged his great insight and capacity of connecting facts and data obtained from different techniques and for different problems, apparently unrelated. Thus, he saw that our simulations confirmed some questions that he had already predicted long time before, such as the renormalization of the electronic band gap of semiconductors due to nuclear motion, which is usually known as due to phonon-electron coupling.

As Richard Martin indicated in his paper "A personal tribute to Manuel Cardona, theorist" [1], if Manuel were not a renowned experimentalist, he would be known as a distinguished theorist. In fact, among his theoretical contributions one finds original developments, simple penetrating arguments, crucial tests of basic ideas, as well as important applications.

Our direct scientific collaboration with Manuel begun then in connection with path-integral simulations of solids, in order to study problems where quantum

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_15

nuclear motion has some relevance for the physical properties of various types of materials. These questions have been traditionally studied by using some kind of quasi-harmonic approximation for the solid vibrations, but this can be improved using path-integral simulations in cases where the anharmonicity is large. Among these applications, we had studied earlier the renormalization of electronic gaps due to quantum nuclear motion, as well as isotopic effects on structural and thermodynamic properties of diamond and related materials. These works called the attention of Manuel, so that he suggested us to apply our methods to study nuclear quantum effects in silicon carbide, including isotope effects for both elements C and Si.

At that time, Manuel was no longer a Director at the Max-Plank Institute in Stuttgart, but he continued there as emeritus scientist. We were, however, greatly surprised by his vast activity and enthusiastic support, answering immediately our e-mails commenting new results, sometimes from remote countries where he was spending some days of work or holidays.

We carried out path-integral molecular dynamics (PIMD) simulations of cubic silicon carbide (3C-SiC) as a function of pressure and temperature. These simulations treated both electrons and atomic nuclei by quantum mechanical methods. While the electronic structure of the solid was described by an efficient tight-binding Hamiltonian, the nuclei dynamics was treated by the Feynman path integral formulation of statistical mechanics. To assess the relevance of nuclear quantum effects, the results of PIMD simulations were compared to those of other simulations where either Si nuclei, C nuclei or both were treated as classical particles.

The experimental thermal expansion of 3C-SiC was realistically reproduced by these simulations. The calculated bulk modulus of 3C-SiC and its pressure derivative at room temperature showed also good agreement with the available experimental data. The effect of the electron-phonon interaction on the direct electronic gap of 3C-SiC was calculated as a function of temperature and related to results obtained for bulk diamond and Si. Comparison to experimental data obtained in Manuel's group twenty years earlier showed satisfactory agreement. The effect of treating the atomic nuclei as classical particles on the direct gap of 3C-SiC was assessed [2].

As is well known, Manuel had an intense activity in the field of isotopic effects on the optical and electronic properties of semiconductors, especially after 1990, when it became possible to obtain appreciable quantities of highly-enriched materials, such as ^{70}Ge [3, 4]. In this context, we used path-integral simulations in a later collaboration with Manuel, to study the dependence of the lattice parameter of 3C-SiC upon isotope mass. This computational method allows a quantitative and nonperturbative study of such anharmonic effect. At 300 K the difference between lattice parameters of 3C-SiC crystals with ^{12}C and ^{13}C amounted to 2.1×10^{-4} Å. The effect due to Si isotopes was found to be smaller, and amounted to 3.5×10^{-5} Å when replacing ^{29}Si for ^{28}Si. These differences between lattice parameters, although they may seem small, can be obtained without difficulty from X-ray diffraction experiments. Results of the PIMD simulations were interpreted in terms of a quasi-harmonic approximation for the lattice vibrations [5]. In this respect, Manuel was really talented to explain the results from simple conceptual aspects of general physics.

Apart from these scientific collaborations, we benefited from many conversations with Manuel, not only on scientific questions, but also about different cultural, social, and every-day aspects of our current life. His deep imprint will remain with us forever.

References

1. R.M. Martin, Phil. Mag. **70**, 521 (1994)
2. R. Ramírez, C.P. Herrero, E.R. Hernández, M. Cardona, Phys. Rev. B **77**, 045210 (2008)
3. M. Cardona, Phys. Status Solidi B **220**, 5 (2000)
4. M. Cardona, M.L.W. Thewalt, Rev. Mod. Phys. **77**, 1173 (2005)
5. C.P. Herrero, R. Ramírez, M. Cardona, Phys. Rev. B **79**, 012301 (2009)

Firma y Rúbrica

Reinhard K. Kremer

At some point or other, we all have been admiring the tremendous number of books in the bookshelves of Manuel Cardona's office. Needless to say, we could not help but being deeply impressed. But more so, we admired the broad knowledge that Manuel was able to display through his constant and voluminous reading. Manuel's bookshelves covered the wall which had not been fully finished in the early days of the Institute on the Heisenbergstrasse. There used to be a big hole which allowed for a glance into Heinz Bilz's office, Manuel's neighbor on the seventh floor in those days. Manuel's bookshelves could easily be taken as the second Institute's library, as he had filled them with his comprehensive collection. His books ranged from books written or edited by himself, or simply books on scientific and cultural knowledge that he fancied. Gradually, gravity reared its head by causing a more or less pronounced bending of the wooden boards. At some point there had certainly been a more systematic arrangement.[1] However by and by, as it is familiar to most of us, more and more books had accumulated. A passerby might make a mental note of books lying horizontal and effectively filling the empty gaps above those standing upright. Filling these gaps certainly assisted to support the boards above, thus stabilizing the whole structure. The snapshot taken of Manuel in his later years in front of his office bookshelves tells his tale of "Vor der Buecherwand"[2] (Fig. 1). In his shelves one can easily spot the terracotta-brownish color of the Springer Series volumes in Solid-State Sciences which Manuel together with his colleagues in Stuttgart edited for so many years. What also stands out are the golden letters on

[1]Only a novice may have assumed that there was no order in the book shelves. Within seconds Manuel was able to pick out a book, open it at a particular page and discuss an equation or a figure or even spot a reference. Very often he had met and even recalled a little anecdote of the referenced authors.

[2]I am aware that Germany's first post-war Federal President, Theodor Heuss, uses this phrase as a title for his book Skizzen zu Dichtern und Dichtung (transl. Sketches on Poets and Poetry (Tübingen 1961), still nowadays an enjoyable reading. Interestingly, one of the early essays in Heuss' book deals with the importance of the Spanish poet Miguel de Cervantes for the European literature. I am sure Manuel would have liked that.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_16

the spines of scientific textbooks of earlier days. Here and there we also recognize reprints of special volumes of scientific journals. Surely, Manuel had either contributed to them or they contained a collection of articles in his honor.

Among the many textbooks and print products of daily usage one discovers a number of precious bibliophile valuables, such as early physics textbooks published shortly after WWII or in the early fifties. For example, one finds the seminal textbooks on group theory and its application in physics by Wigner, Heine and Tinkham reflecting Manuel's interest in group theory in which he was an inimitable master. Still with great admiration this author remembers Manuel's lectures on group theory in the seminar room on the seventh floor. It was not an unusual occurrence that Manuel would lecture without using any notes easily for an hour or more.[3] Another interesting book in Manuel's collection is the Jahnke-Emde pre-war compilation of *Tables of Functions with Formulae and Curves.*[4] This book is particularly curios since it is a bilingual edition printed by G.E. Stechert and Company in New York[5] with the German text in the left and the English translation in the right column on the same page. Also in Manuel's collection there is seemingly a 1st edition of R.E. Peierls book on *Quantum Theory of Solids* from 1955 or an early post-war reprint of W. Pauli's 1933 Handbuch article on *Die Allgemeinen Prinzipien der Wellenmechanik* from 1946, published by J.W. Edwards in Ann Arbor.

Although Manuel had an Ex Libris he rarely used to mark his books with it (Fig. 2). Rather, he had the habit of signing his books, attaching his signature usually at the top of the very first page. I found the earliest such handwritten ownership marker in a French physics textbook, actually a paperback, entitled *Cours de Physique Générale Mécanique* by G. Bruhat, a Professor at the Université de Paris and director of the L'École Normale Supérieur. Manuel signed this book in the usual way but also added what he rarely did later on, the date, namely *Dec 1951*. It could easily be that this volume represents the earliest textbook Manuel was using at the University of Barcelona in his physics courses,[6] In any case, this and the next signature are especially fascinating since they show Manuel's almost calligraphic 'firma y rúbrica'[7] type signature of his young years (see Fig. 3). The next example of Manuel's impressive handwritten ex libris can be found in Peierls textbook on

[3]Manuel once showed me his notes taken in the lectures of Julian Schwinger at Harvard. He recalled that Schwinger's lectures were like a theatre performance. Following his inner voice Schwinger lectured elegantly and continuously without any break or interruption, which he also did not allow from the students. In the latter respect, Manuel was certainly the opposite.

[4]F. Emde the second author, like Manuel, has been a Professor at the TH Stuttgart from where he retired in 1938.

[5]In Germany published by B.G. Teubner in Leipzig and Berlin.

[6]It would certainly be worth investigating to what extent university physics textbooks were available at all at that time in Spain.

[7]I.e. 'signing and initialing'. I am sure Manuel would have known everything about how much Arabian calligraphic heritage influenced the Spanish handwritten '*firma*' and the origin and highlights of '*la rúbrica'.*

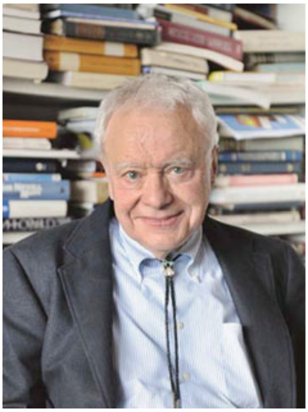


Fig. 1 Manuel Cardona in front of the bookshelves in his office on the seventh floor of the institute in the Heisenbergstrasse in Stuttgart Büsnau (Copyright: Friedemann Bayer)

Fig. 2 Manuel's late Ex Libris which—to the best of my knowledge—he rarely used to mark his books

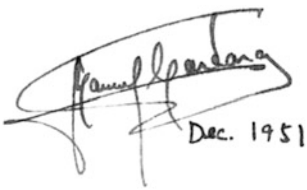


Fig. 3 Manuel Cardona's signature written at the age of seventeen in the physics textbook entitled *Cours de Physique Générale Mécanique* by G. Bruhat published by Masson and Cie, Paris in 1948. This is the only book known to me which is containing the Ex Libris sticker displayed in Fig. 2

the *Quantum Theory of Solids* (see Fig. 4). It is dated September 1956, about the time Manuel started graduate work under Bill Paul at Harvard.

In the following years, Manuel's signature became less stylized, may we say, more modern as can be seen from his inscription in V. Heine's book on *Group Theory in Quantum Mechanics,* a re-print from 1964. Nevertheless, with some additional curls, e.g. in the first letter of his family name, *Manuels* Iberian heritage still shines through.

The following two examples are taken from books that must have come into Manuel's possession after 1968 (see Figs. 5 and 6). Obviously before Manuel owned the reprint of W. Pauli's Handbuch article *Die Allgemeinen Prinzipien der Wellenmechanik* and the Jahnke–Emde compilation of mathematical *Tables of Functions* (already mentioned above), they have been the property of Rohn Truell. Rohn Truell who passed away in 1968 at the age of 55, was a professor of applied mathematics and a colleague of Manuel at Brown University. Truell was a pioneer in the investigation of solids by means of ultrasonics and founder of the Metals

Fig. 4 Signature in *Quantum Theory of Solids* by R.E. Peierls published in Oxford by Clarendon Press in 1955

Fig. 5 Signature in V. Heine's *Group Theory in Quantum Mechanics* published by Pergamon Press in Oxford, London, New Paris in 1964 (Reprint of the 1960 first edition)

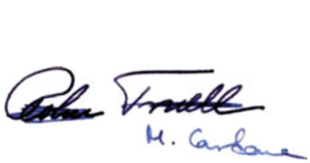


Fig. 6 Manuel Cardona's signature in W. Pauli's *Die Allgemeinen Prinzipien der Wellenmechanik*, Reprinted from Handbuch der Physik, 2. Aufl., Band 24., 1. Teil; J. Edwards Publisher, Ann Arbor Michigan, 1946. Rohn Truell's signing has been crossed out apparently by the new owner

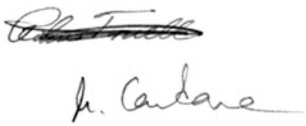


Fig. 7 E. Jahnke's and F. Emde's bilingual compilation of *Tables of Functions with Fomulae and Curves*, 3rd edition published by B.G. Teubner in Leipzig and Berlin in 1938 and by G.E. Stechert and Co in New York previously owned by Rohn Truell

Fig. 8 Manuel's signing of a personal dedication in the 4th edition of his and P. Yu's well known textbook on the *Fundamentals of Semiconductors, Physics and Materials Properties*. Springer, Berlin, Heidelberg, New York, 2010

Research Lab at Brown.[8] Underneath the scratched out signature of Rohn we discover a later and more familiar version of Manuel's signature (Figs. 6, 7).

At the end of our little graphological[9] tour d'horizon through some of Manuel's textbooks we are taking a closer look at one of Manuel's later signature. It is found in a dedication written in the fourth edition of Peter Yu's and Manuel's very successful and widespread textbook on the *Fundamentals of Semiconductors* (Fig. 8). We can't help but feel that this signature is a bit reminiscent of an earlier more stylized signature. Is it a wrong impression—what would Manuel say?

Acknowledgement I gratefully acknowledge valuable discussions with and suggestion by Reiner Ramlau and Éléonore Reinéry. I am especially thankful for Éléonore's careful reading of the manuscript. Thanks go also to Daniela Kabinova who brought Manuel's early Physics textbook to my attention and Carmen Müller for the excellent computer scans.

[8]See Rohn Truell's CV published by Brown University at http://www.brown.edu/Administration/News_Bureau/Databases/Encyclopedia/search.php?serial=T0150. Since 1968 Brown University awards members of the graduation class who have achieved special distinction with the Rohn Truell Premium Prize. Cf. http://www.dam.brown.edu/graduation/graduation2013.htm.

[9]Graphology is commonly considered a pseudoscience, a judgment I fully share.

Isotopic Crystal Affairs: When I Was Cardona's Last Crystal Grower

Rudolf Lauck

From 1981 to 1983, I was a post-doc coworker in Dr. Erich Schönherr's crystal growth group of the Max Planck Institute for Solid State Research in Stuttgart, Germany. But my crystal growth for Prof. Manuel Cardona needed a detour via the small company of Dr. Lamprecht in Neuhausen (Enzkreis), a former member of this institute. I was an employee in this company and Cardona was a customer of II-VI compound crystals, even though our products were too expensive in his opinion.

But that was my chance to become his freelancer when that company was sold. It was a new challenge for me to grow isotopically modified crystals from small amounts of materials.

At the end of the Soviet Union, large amounts of highly enriched stable isotopes had become available in Russia at moderate prices [1]: 1,000–5,000 $ per gram. Only the offer for the sulfur isotope 34 was not acceptable: 50,000 $ per gram! Cardona persuaded the Russian colleagues to reduce the price to 10,000 $! Therefore he bought 2 g for my work.

In the case of the cadmium isotope 114 at the beginning of our cooperation, only the oxide ^{114}CdO was available. I spent a lot of time to reduce the oxide, to purify ^{114}Cd and to grow a cadmium sulfide crystal of 1 cm^3 in size for the first neutron scattering experiments on isotopically pure CdS [2]. He understood my problems and defended my efforts while discussing with his students and assistants and he said: Please wait because crystal growth is pretty difficult!

For more than 20 years, Manuel Cardona had been a great mentor and cordial pedagogue to me. He had been supporting me in many respects. He was very patient to explain to me the physics of phonons and photons. But he also told me a lot of his eventful life. I'm very grateful to him as he helped me in a health crisis. Besides we talked about wine and everything under the sun.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_17

References

1. M. Cardona, in *Festkörperprobleme: Advances in Solid State Physics*, ed. by R. Helbig (Vieweg, Braunschweig, 1994), Vol. 34, p. 35
2. A. Debernardi et al., Solid State Commun. **103**, 297 (1997)

Thirteen Years with Manuel at the Max Planck Institute

Lothar Ley

The first time I met Manuel was in 1974 while I was a post-doc in Berkeley in the group of Dave Shirley. We, that is three graduate students and myself, had been rather successful for the previous couple of years doing photoemission using the first commercial photoelectron spectrometer with a monochromatized X-ray source. Among other things we had done a systematic study of the density of states of a series of III–V semiconductors and had, with the help of Marvin Cohen and his students derived critical point energies from the spectra. It must be said that we were treading on thin ice because neither Dave and his students, chemists by training, nor I, an "ex-nuke", had any prior dealings with solid state physics. So when Manuel visited the lab we had no idea that he was one of the gurus of semiconductor physics and promptly put his visit in the back of our minds despite his inquisitive questions in particular about our semiconductor work. However, a few weeks later I got a call from Manuel. He praised the fantastic facilities of the newly founded Max Planck Institute in Stuttgart and offered me a temporary job in his group to do photoemission. I was at that time in negotiations with the IBM San Jose Laboratories about a permanent staff position. After a few days of deliberation, however, I took the job with Manuel which, on the outside was inferior to the IBM offer. There were a number of considerations that went into that decision. The decisive one was the clear impression that remained from our short conversation that he was a man with more depth, both scientifically and culturally, than any of the people I had met at IBM. It would therefore be more interesting to work with him.

My arrival at the MPI early in 1975 was a shock. I knew that a new building was going up on the outskirts of Stuttgart but what awaited me in the temporary quarters at Heilbronner Strasse was an old, rather dilapidated building next to the railway station that was moreover completely overcrowded. When Manuel showed me my office and introduced the people I was going to share it with, it turned out that there were three desks for four people. Manuel nonchalantly brushed my concerns aside with the remark that we were spending most of our time in the lab anyway—which turned out to be true.

What the temporary quarters lacked in space and facilities was made up for by a sense of being pioneers in the extraordinary endeavour of setting up one of the world leading physics laboratories and the ensuing spirit of camaraderie that

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_18

embraced everyone from the directors down to the youngest workshop apprentice. In my memory this time was hard work during the day and wild parties at night right on the premises. There was a constant influx of people from all over the world and what better way to get to know them than over a couple of "Viertele". I remember a particularly remarkable visitor, Ray Tsu, an eminent scientist from the IBM Watson research centre who had worked with the Nobel price winner Leo Esaki. He introduced himself on one of these parties by jumping up on the table and "playing the nose", i.e. humming over a foil-covered comb and tweaking his nose to the beat of the tune. This spirit lingered on for much of my time at the MPI and made it among other things such an extraordinary place to work. We needn't go to see famous people—they came to us; and not least due to Manuel's personal connections throughout the world.

Visitors, in particular when they came from abroad, were known as guests and were treated as such. My wife and I, having lived in the US for 3 years, felt a bit like one of them and were treated accordingly. We stayed for a couple of months at one the guest apartments, placed together with a swimming pool (hardly ever used) at the bottom of the six houses, stacked one on top of the other up a slope in one of the more desirable parts of Stuttgart, that the Max Planck Society had built for the directors. This brought us in close contact with Inge who took us and in particular my wife under her wings. The Cardonas were the most helpful and hospitable people we ever met. Their parties were frequent and lively with guests from all over the world. There would be Inge's famous Chili con Carne and fine Lemberger or Trollinger wines; we would sing Tom Lehrer's "The Elements" and occasionally Ray Tsu, who was a ballet aficionado as well as a "nose player", would be doing the grand "jete" in Inge's living room. Manuel was a collector and there were always collector's items on the shelves such as minerals, pottery, the occasional fossil he had brought from one of his travels, a rare book or a newly acquired old map to add to his growing collection. All this gave opportunity for Manuel to carry on about these things, put them in context, reveal their history, or point out fine aspects that we might have missed. He would pour us a drink of Spanish brandy, for example, and its name, Carlos Primero, would take him on an extempore on European history by pointing out the identity of Carlos Primero and Karl V. There was not a topic on which Manuel wouldn't have something profound, witty or interesting to say; and all this in an easy, light hearted manner that didn't subdue his guests but rather enlivened the conversation and contributed to the high spirit of the Cardonas' parties.

Coming back to the story of our work, in September of 1975 the institute moved to its new quarters, a splendid building, close but not too close to the University of Stuttgart, a situation both spatial and metaphorical. Manuel and the Raman group resided on the top, the seventh floor, and I, now in charge of the photoemission group got lab and office space on the sixth floor. Manuel bought in short succession the X-ray photoemission spectrometer that I had worked with in Berkeley and a spectrometer that allowed angle resolved photoelectron spectroscopy for band mapping. Since Manuel was familiar with the use of synchrotron radiation for spectroscopy stemming from a sabbatical he had spent at DESY, Hamburg in

1969/70, he suggested that I set up a photoemission experiment at HASYLAB, the recently established synchrotron radiation facility at the Doris storage ring at DESY. I did that together with Robert Johnson who eventually took over this operation. Another synchrotron activity initiated by Manuel and that I maintained until my retirement in 2008 was a novel kind of angle resolved spectrometer that I set up and used together with John Riley and Robert Leckey at the newly built dedicated synchrotron radiation source BESSY in Berlin. John Riley was one of the earliest visitors to my lab in 1976/77 from La Trobe University in Melbourne, Australia. He and his colleague Leckey were among the photoemission pioneers and had taken by choice as much as by necessity to building their own spectrometers. This led to the really novel design of a toroidal analyzer which promised to speed up the time consuming task of taking angle resolved spectra from days to minutes. They built a version that was shipped to Berlin in 1983, a year after the first light at BESSY. This spectrometer was the working horse for our band mapping activity for the years to come and it led to many visits of John Riley and numerous students from Australia to BESSY and the MPI. After I moved as a professor to Erlangen in 1989 the collaboration and friendship continued and is still now maintained—this time by me visiting La Trobe.

The spatial separation (sixth vs. seventh floor) went along with a considerable freedom that Manuel allowed me in the running of the photoemission group. He hardly ever interfered with my activities but, of course, he was the source of new ideas and inspiration. That regularly took the form of first page copies of interesting papers that he sent me through the internal mail system with remarks like "Interessant!", "darüber müssen wir sprechen" or "das sollten wir machen" jotted across them. Indeed, many of my activities were initiated by Manuel, among them the work on amorphous semiconductors which turned out to be the most fruitful. It eventually went far beyond photoemission embracing Raman and infrared spectroscopy as well as the fabrication of superlattices and the investigation of their peculiar transport properties. It was also Manuel who provided me with graduate students and post docs, and it was he who gently prodded me to apply for a professorship by placing ads for university positions on my desk. Conversely, I would regularly walk up to his office to seek his advice which he gave freely and copiously. These private talks as well as the numerous seminars in which Manuel's remarks and comments were often more enlightening than the talk itself taught me virtually all I know about semiconductor physics and spectroscopy. The lively and informal exchange of ideas involving people from all over the world was one of the hallmarks of Manuel's way to do science. Science for him was not merely an intellectual activity—it was as much a vehicle of social interaction; he enjoyed and cared deeply for the people who were joined with him in the never ending endeavour of learning about nature.

I have often mused why he did not participate more directly in photoelectron spectroscopy—there are comparatively few common papers on the topic. Part of it may have been that he realized that the post-docs running the photoemission group were in need of making a name of their own to be able to secure permanent positions in academia later on. Part of the reason and likely the more important one may have been that most photoemission results didn't lend themselves to quantitative interpretations by straightforward calculations as Manuel liked to do them. A more trivial explanation for his abstinence was given by Manuel himself. On one of my excursions up to Manuel's office to discuss a problem I had been fretting over unsuccessfully for days and he solved in a matter of half an hour, I exclaimed in exasperation "Manuel, what am I doing here? You can do everything so much better and faster." To which he answered in a matter-of-fact tone "That's true; but I don't have the time."

With Manuel's untimely death I have lost my mentor, a man who was passionate about science and people, and a true friend.

My Memories of Manuel Cardona

Chengtian Lin

It is hard for me to lift the pen delivering this eulogy for you, dear Manuel Cardona. The preparation of these memories has been difficult, but also very rewarding. It allowed me to reflect on the time I had the privilege to spend together with Manuel Cardona and to remember what a wonderful and intelligent person he was. I am glad that I have the opportunity to share some of these memories with you and some of the attributes that made Manuel Cardona so special.

Manuel Cardona was first of all a great scientist in physics, chemistry and mathematics but he also had an exceptionally large knowledge in sociology, history, archeology, art and literature. Remembering the time I joined his workshop in Ringberg Castle and my talk focused on single crystal growth and structure. During the workshop, it was surprising to me that Manuel Cardona was not only familiar to the field of crystal growth on the compounds growth techniques and growth mechanisms, but also on the various crystal structures, symmetries and the related physical and chemical properties. One of the important topics at that time was the twinned and untwined crystal structure of high Tc superconductor YBCO. He encouraged me to make an untwined YBCO crystal, to help to reveal the nature of superconductivity and till now, this is still a very hot research subject investigated in the worldwide.

Once, on lunch time with him, I was curious to ask if any natural pearls are produced in Spain. Manuel Cardona immediately came up with incredibly accurate information and told me that the famous Majorica pearls are man-made imitation pearls that were produced on the island of Majorca, Spain. They were made in a special way using spherical pieces of opaque white glass of a similar weight to real pearls and with a special mixture of pearly essence called “Essence d’Orient”. This was made of substances extracted from ground up fish scales and shells and mixed with a resin like substance. The pearls were then dried and polished to remove lumps and irregularities. This manual process was repeated 8–34 times, according to the quality that was desired and the pearls were then coated with a layer of lacquer. The final product was the beautiful costume jewelry and had a very different look compared to genuine pearls.

Manuel Cardona was not only familiar with the European history but had also a vast knowledge of the oriental culture, history and natural sciences. A deep impression on me is a visit to the Forbidden City Museum in Beijing (Materials and

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_19

Mechanisms of Superconductivity Conference tour 1997), where exhibits many valuable antiques and artifacts, such as porcelains, bronzes, ironware and Chinese ink paintings, etc. In the exhibition room I thought I, as a Chinese, should be responsible to give a shallow interpretation on these artifacts, although I knew only a little about these. It turned out that Manuel Cardona stood at the demonstration window and pointed each exhibit to pour forth a steady stream of information in its history, origination, producing time, dynasty and belonging to which empire. The other colleagues and I were quietly pleased to listen to him. Visiting the great gardens of the Golden Palaces, Manuel Cardona pointed to the giant copper vats placed on the scattered and ordered positions and asked us for what purpose these vats were used. We were silent and no answer was given. Manuel Cardona gave us then the answer, they were “fire extinguishers” and were parts of the fire-fighting equipment in the palace for the ancient time, they were used to store extinguishing water in case of fire. From October to February every year, the vats were covered with quilts to prevent the water freezing and on very cold days, they were heated by charcoal firs. After listening, we all admired and nodded. Manuel Cardona was a man of genius in natural and social sciences. He has left us, but he will never be forgotten. Allow me to quote for him with a Tang Dynasty’s poetry: The sun is set down in widely grey clouds; The goose is flying with north wind and snowing one after another. Don’t worry no friends on the road ahead; No one in the world doesn’t know you.

千里黄云白日曛,
北风吹雁雪纷纷。
莫愁前路无知己,
天下谁人不识君?

Manuel Cardona: Some of My Recollections

Elías López-Cruz

It was November or December 1980 when I was trying to get a post doc position with someone somewhere in the world. The plans of development for our Departamento de Física de la Universidad Autónoma de Puebla, took into consideration the possibility of spending a 2 year period of postdoc. Taking that into account, I wrote some letters to different persons in different countries and institutions of research. Those application letters arrived to places like BOEING at Seattle and to EXXON in Washington—I think—and last but not least, one of those letters was sent to Max Planck Institut für Festkörperforshung (MPI-FKF) in Stuttgart, Federal Republic of Germany.

I received almost no response to my letters. That was understandable because I picked up an issue of Physical Review B, and selected some names and addresses and wrote to them asking for a position. None of them knew me.

At that time I had an invitation to spend a month in Brazil with the group of Dr. Guilherme Fontes Leal Ferreira starting on the beginning of January 1981. I went to Brazil and we started to do some research concerning the transit time of electrons in some platelets of orthorhombic sulfur. It was some 2, or 3 weeks after my arrival, at Instituto de Fisica e Quimica de Sao Carlos in Sao Paulo, Brazil, that a letter from my University reached my hands. An envelope containing another envelope with a letter written by Professor Manuel Cardona informed me that I was accepted to spend the 2 year period in his group.

One particular thing drew my attention: he apologized because it was Christmas time when my letter arrived to Stuttgart, and he had no secretary, so he wrote a manuscript letter, with his usual blue ink, using his fountain pen. I still have that letter. In the letter he sketched what would be the work plan for the time I would spend at his group—“Abteilung” as later I learnt was the name for group in German. In few words he proposed me to do some research using the diamond anvil cell and Raman scattering. I had not the faintest idea about that kind of technique but I accepted.

Some weeks afterwards I was informed, by means of a professor from the Hebrew University of Jerusalem—during a visit to México city—that my application at EXXON was accepted too. When I told him about my decision of going to Stuttgart he told me that I preferred the academic work instead of the money.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_20

September 1981 was the date to arrive at Stuttgart, and after the paperwork at the German Embassy in México, I finally found myself at that city, completely new for me. A driver from the MPI-FKF was sent to pick me up from the hotel where I spent the first night in Germany. I met professor Cardona at his office and introduced myself. From the very beginning I felt a certain atmosphere of confidence, and good companionship that gave me a lot of peace of mind to start to work as soon as possible. Two or three weeks later we were invited to go to the Oktoberfest, and Professor Cardona was teaching us everything we needed to know in order to enjoy that fest.

The 1st of May 1982, after my first winter in those lands with a very strong cold weather, we were invited to go to a wine tasting at a place whose name I learned many years later, Hohen Hasslach. That experience was repeated in 1983. It was in 1999, during a visit of Dr. Cardona to México at the CINVESTAV, that he told me the name of that place. As an extra bonus, I received an engraving (of course a copy) from 1500 that Dr. Cardona sent me by mail (the old fashioned mail).

He took some time during one of his trips—when invited to the March Meeting of 1984—to pay a visit to my University and spent a couple of days doing some touristic trips to different historic places near Puebla. In Cholula he told me that the pyramid over which a catholic church was erected was the biggest in the world. His invitation to the March Meeting was to receive the Isakson Prize for his contributions to science.

Well, in other occasions and in other places Dr. Cardona told us a lot of anecdotes concerning his experiences with the scientists from Latin America: Argentina, Chile and Brazil, countries where the political situation were not so good for the development of science. He played a very important role inviting a lot of scientists from those countries, and later also from México and Cuba. I had the opportunity to talk to Dr. Cardona and his wife Inge during the different encounters we had. The last one was in San Luis Potosí, México, where Dr. Cardona was awarded a Honoris Causa doctorate from the University in 2001.

In this short note I would like to thank posthumously Dr. Manuel Cardona.

Acknowledgments Cardona for his personal encouragement during the time I spent collaborating with his group, which gave me the chance to feel the taste of doing research at a center, that was considered as the Mecca of Solid State Physics, as a good number of scientists from different countries used to say.

How Manuel and Inge Changed Our Lives

Richard and Beverly Martin

We will always appreciate and never forget Manuel and Inge. Manuel was one of the most active, energetic scientists we have ever known, and yet he was a compassionate, friendly, warm human being. This combination made him truly unique. Manuel had an incredible memory: Richard could barely remember what was in his old papers, but Manuel could quote the page numbers of his many more papers! Manuel could speak at least five languages more fluently than we could speak one. These talents were used not only in his work but just as much in personal relations with his younger colleagues and their families. Inge was no less caring and together they made a profound difference in the lives of many, many people.

Richard first got to know Manuel while he was a post-doc at Bell Labs and became interested in optical properties and Raman scattering in semiconductors. Interactions with Manuel and Dave Aspnes led to a joint paper [1] given at the Semiconductor Conference in August, 1970.[1] The connections included Eli Burstein, who worked with Manuel and Gerry Lucovsky, which certainly helped in getting a position in Lucovsky's group at Xerox Palo Alto Research Center (PARC). We can't give all the credit to Manuel, but certainly he was a key in the course of our lives from 1970 on!

The early 1970's was the period when Richard worked in areas related to Manuel and developed connections that cemented our scientific relationship, which then led to our long friendship. We had the pleasure of attending an Enrico Fermi School at Varenna, Italy, in 1971. Richard gave a talk on Raman scattering and learned a lot from the series of lectures given by Manuel on optical properties of semiconductors. Richard's work on resonance Raman scattering in semiconductors led to a review article with Leo Falicov, published in a book edited by Manuel and Eli Burstein, and many further interactions with Manuel.

In 1974 we spent 6 months at the Max Planck Institute in Stuttgart (the old location on Heilbronner Strasse near the railway station) taking advantage of the Xerox policy of leave of absence for research and study. Officially our host was Heinz Bilz (the connection was work on phonons which was Bilz's interest), but in fact the interactions were in large part with Manuel. During the time in Stuttgart,

[1] Richard did not actually attend the conference because our daughter Barbara was born only a few days after the conference!

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_21

Inge and Manuel took us under their wings. We arrived in early February, 1974, with our 3 and 5 year old children. A couple of weeks later, Inge and Manuel invited us to their home for dinner. Spaghetti never tasted so good! All the children played together, building great Lego creations and playing games.

Through the years, we met in many places in the world, all physics-related, of course. Inge and Beverly shopped, went to museums, explored cities, and just enjoyed being together. Manuel's memory was amazing. He could remember friends and family of ours, and would ask about them years later. He loved puns and jokes. He and Inge laughed, loved, and lived to the fullest. One occasion many years later was the time we were together with Manuel and Inge in the Canary Islands. Manuel pointed out the street named Martin and recounted many Martins in Spain. Once we stopped at a small bar for a cool drink. We heard guitar music and soon a couple (in their 30–40s) started dancing to the music—very sexy and suggestive dancing. We talked and laughed about this for years after. Manuel would say, with a twinkle in his eye, "Remember those dancers in the Canaries?" Manuel and Inge were full of interesting information about interesting things to do. One was to have tarte flambée and apple cider in little places with outside tables along the roads in Alsace—still a favorite for us.

In the early 1980s Manuel was a visitor at Xerox PARC. We found places for them to live which included the home of our friends who were away for a period. After our friends returned, they bought a catered paella dinner at a charity auction as a special treat for the Cardonas—yet another time when friendship with Manuel and Inge lead to further friendships and enjoyable times. The physics work at PARC actually came back to issues discussed in Stuttgart in 1974, including the concept of an absolute deformation potential. This was another case where there were many discussions with Manuel and Conyers Herring, and it led to work with a student Chris Van de Walle, who became known in part for that work. Another area has a personal story. There is a quantity called the internal strain parameters in certain crystals (silicon in this case) that is very difficult to measure and there was a controversy. At the time Karel Kunc was working at PARC developing methods using density functional theory that is now widely used, but at the time it was not so clear how accurate was the density functional method. The theory predicted a value different from the most accepted experimental value at the time—and later experiments verified that we were correct. Even though Manuel is not a theorist, he is first author on a joint paper [2] because he was so interested in the experimental issues and he grasped the key idea aspects of the theoretical analysis. Several years later when Richard was considered for a position at the University of Illinois, the only experimentalist he asked to be a reference was Manuel. Of course, we do not know exactly what happened, but it worked!

The Cardonas played a major role in our family in yet another way. In 1989, during the summer after his junior year in college our son Michael worked with

Chris Thomsen in Manuel's group. He even rented a room in the Cardona's home! Michael's project involved Raman scattering experiments in which light was collected using a CCD array. His job was to write a computer code to throw out data points that were far off-scale, probably due to cosmic rays. (He swears that it was not used to throw out data that Manuel did not like!) Michael is now a scientist at the Lawrence Berkeley National Lab, where he runs the infrared beam line at the Advanced Light Source. He gives talks to young budding scientists and he tells them that his experience at the MPI was directly responsible for his choice of career in experimental physics working in the area of spectroscopy.

In 1994–1995 we were visitors in the MPI in Stuttgart again (this time in the new buildings in Busnau). Officially, Richard was in the group of Ole Krough Andersen, but Manuel and Ole both were sponsors for Richard to receive the Alexander von Humboldt award that made our stay possible. We lived in an apartment on Klopstock Strasse overlooking Stuttgart. Richard would walk to the S-Bahn every day using stairs that had 144 steps; when Manuel heard about that, he said "That's gross!" One our favorite pictures of Manuel and Inge is taken on the balcony of the apartment. One event we remember because it captures so well two aspects of the Cardonas. When Richard's brother Bill and his wife Betti visited us, the Cardonas had all of us to their home for dinner—an example of their warmth and caring. Almost immediately after we left, Betti said, "That's the smartest man I ever met!"

During the year we were in Stuttgart was the fest for Manuel's 60th birthday. For the occasion Richard wrote a tribute to Manuel as a theorist [3] based on many

experiences in which Manuel knew and applied the appropriate level of theory very well. A picture shows him with Richard and Morrel Cohen at the birthday celebration. Morrel was Richard's thesis adviser at the University of Chicago and he knew Manuel well, having tried to lure Manuel to Chicago in the 1960s. This is a characteristic pose with Manuel thinking—you can see the wheels turning.

We have always been impressed with Manuel's concern for scientists in difficult positions. One stands out to us. Manuel took special interest in helping a young professor who had previously worked with Richard and then took a position where he was isolated in a hostile environment. Now he is a thriving scientist and doing very well in large part to Manuel's inspiration and efforts behind the scene.

It is a pleasure to recollect the many ways that we have benefitted from our long relationship with Manuel and Inge, professionally and personally. We have observed the way they have taken young families under their wings and realize that this is just what they did for us. They have been an inspiration to us and we appreciate this chance to put into writing our appreciation.

References

1. J.A. Van Vechten, M. Cardona, D.A. Aspnes, R.M. Martin, Theory of the third-order susceptibility of semiconductors, in 10th Intenational Semiconductor Conference, vol. 82 (1970)
2. M. Cardona, K. Kunc, R.M. Martin, Internal strain parameter of silicon and GaAs and planar force constants. Solid State Commun. **44**, 1205 (1982)
3. R.M. Martin, A personal tribute to Manuel Cardona, theorist. Philos. Mag. B **70**, 521 (1994)

The Teacher and Mentor I Almost Had

Emilio E. Mendez

This book is full of reminiscences from the myriad students, disciples, and collaborators Manuel Cardona had through his long career. Unfortunately, I cannot count myself among them. I never did any scientific work for or with him—although, I came close to it, twice. Both times I chose someone else over him, and yet he later paid me back by recommending to me two of his best students, by promoting my scientific work, and, most important, with his lifelong friendship. This is how Manuel Cardona was.

In early 1978, as I was close to completing my Ph.D. and started looking at the next professional step, I wrote Cardona ("Manuel," for me, would come much later) to explore whether it would be possible to work in his group as a postdoc. I knew the name since my earlier days as a Research Assistant at Universidad Autonoma de Madrid (UAM), back in 1972–74, where he was regarded as the Spanish luminary in Solid State Physics who was still abroad. The other star, Nicolas Cabrera, had returned from America a few years earlier to lead the Physics Division in the newly created UAM. Cardona's response to my letter was cautiously positive, suggesting that we meet at the forthcoming March Meeting of the American Physical Society in Washington D.C.

It was there that I first saw him "in action," fully engaged in the Meeting's sessions, asking the deepest questions in all kinds of topics. I was most impressed by him, and I said to myself, I want to learn from him, but will he want to teach me? Our meeting went well, and he invited me to give a seminar in Stuttgart in the summer, taking advantage of a trip I had planned to participate in the International Conference of Semiconductors in Edinburgh. I started taking German classes, and by October I got Cardona's formal offer.

On my return from Europe, I had received a letter from the IBM Research Division in Yorktown Heights, inviting me to interview for a postdoctoral position. By November I was facing the dilemma of choosing between Manuel Cardona in Stuttgart and Leo Esaki in New York, not exactly an easy decision. At the end, I chose IBM's postdoc's offer, and Cardona, who knew the place well, understood and did not take it personally.

The story repeated itself 2 years later, as I now was looking for a job with longer stability. Again I approached Cardona; again he offered me a position, this time for 3–5 years; and once more I declined at the last minute in favor of IBM, perhaps

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_22

because I preferred the comfort of the known—an institution, a country, a language—to the excitement, but also the uncertainty, of the unknown. To my surprise, Cardona, by then one of the most prolific and best-known solid-state-physicists in the world, did not take my second no as a personal affront. On the contrary, he encouraged me to pursue a career at IBM Research.

My first assignment at Yorktown Heights, back in 1979, was to study the optical properties of semiconductor superlattices, a materials concept pioneered by Esaki, Raphael Tsu, and Leroy Chang. My first paper on the subject was on an *Electroreflectance Study of Semiconductor Superlattices*. Ironically, Cardona was one of the leading experts in that technique, and I learned it by reading his papers on the subject from more than a decade earlier, and collaborating with Fred Pollack, one of Cardona's junior associates at Brown.

I later exploited electric fields in other ways to control the electronic properties of semiconductor quantum wells and superlattices, which must have got Cardona's attention, as not long after he suggested my name to one of his brightest students, Luis Viña, who was looking for a postdoctoral stay in the US. I learned of Viña's interest and immediately accepted him; since I had lost my chance to work with the master, I did not want to pass the opportunity to work with his disciple. It was through Luis that Cardona and I co-authored an article on *Resonant Raman Scattering in GaAs-GaAlAs Quantum Wells in an Electric Field*, which combined the experimental techniques he had developed with the materials I was deeply involved with. A few years later, another very smart student of Cardona's, Antigoni Alexandrou, showed interest in my group at IBM. For the second time, I quickly said yes, and had another opportunity of witnessing Cardona's indelible imprint on those he had trained.

By then, Cardona was already Manuel. We were serving in an advisory committee for the Materials Science Institutes of Spain's National Research Council that every year traveled to one of the five Institutes across Spain for a friendly review of the centers. Manuel was the president of the committee and I one of its junior members. Like years before in Washington DC, I again saw him in command, asking penetrating questions, offering plenty of advice, and even reorganizing the meeting's agenda as to spend less time at meals and more on technical discussions.

We served together in that committee for a decade, eventually he offering me to take his place as its president. His generosity toward me did not end with that magnanimous gesture. He continued to promote my name whenever the occasion arose, and his words and actions were crucial in my receiving scientific recognition.

We became and remained friends, the geographical distance, the age difference and his intellectual towering stature notwithstanding. We went out to dinner several times, and once to the theater, to a New York revival of *Death of a Salesman*, with Brian Dennehy in the leading role. Dennehy, Manuel noted, reminded him of John Quinn, his former colleague at Brown. In those occasions, we spoke about many things: Cardona's culture was as vast as his interests; his opinions, strong but always reasoned; his conversation, fast and illuminating. But, somewhat surprisingly, we seldom discussed physics, perhaps because I never made it into his inner scientific circle. I can only wonder how much I would have learned from him had I been part of it.

Loose Fingers Sink Ships—15 Years of E-mails from Manuel Cardona

José Menéndez

Introduction

The correspondence of prominent personalities with friends and colleagues has always been a primary source of insight for their biographers. The demise of snail mail and the rise of electronic communications, however, has created a nightmare for future biographers, who will have a hard time recovering files buried in old, unreadable media, or incompatible with the latest software. Hopefully, cloud storage and format standardization will mitigate those problems from now on, but chances are that at least 20–30 years of electronic correspondence may eventually be declared lost. This overlaps with Manuel's years at the keyboard, starting with his early and enthusiastic embrace of Bitnet in the mid 80s, which initiated a continuous correspondence with me and many others. Of this correspondence I am unable to retrieve anything prior to the year 2000, as might be feared from the above considerations. Nevertheless, an exhaustive presentation of Manuel's correspondence is clearly not the purpose of this volume. Instead, I would like to share a few excerpts from Manuel's e-mails since 2000. I believe they reproduce (much better than a printed letter, which tends to be a lot more formal) those conversations that we all miss, in which Manuel jumped from digression to digression while displaying his erudition on any imaginable topic.

The excerpts below were written in Spanish and English. I translated the Spanish ones and I corrected the most obvious typos in the English e-mails. Other than that, what you read is the real Manuel. I chose to concentrate on Manuel's statements and skip the other side of the conversation, except for the last e-mail. This requires that some context be provided, which is done in italics.

I must confess that I instigated some of these comments by feeding Manuel with the appropriate triggers. I learned to do this by competing with my friend Luis Viña—while we were both Manuel's graduate students—on getting Manuel to tell unsuspecting Max-Planck visitors the story of King Favila of Asturias (Spain), who in the eighth century was mauled by a bear. Well read on Spanish history, Manuel believed

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_23

that the King had died in a less romantic way, but he viewed the entire affair as an amusing early example of political spinning that everybody might (should?) be interested in knowing about.

Manuel's Words

(*November 14, 2000, translated from the Spanish*)

Of course we voted! And of course we all voted for ……! Both Democratic and Republican organizations here in Germany were very active this year. Maybe they smelled trouble beforehand.

(*May 19, 2001. After former Miss Universe Cecilia Bolocco—at the time married to Carlos Menem, former president of Argentina—apologized for wearing a dress with the Argentine flag colors. Translated from the Spanish.*)

In the US this would be protected symbolic speech by some Amendment I don't remember. But don't try it in Arizona. You could be lynched there, with or without Amendment.

By the way, did you read (*Mario Vargas Llosa's*) "The Feast of the Goat"? It's fantastic. Besides, I remember very well some of the events he describes, and they are told accurately.

(*August 7, 2003. Writing as honorary Chairman of the 27th International Conference on the Physics of Semiconductors, held in Flagstaff, USA in 2004*)

Thanks for the picture of ICPS. It certainly should be projected at the opening session and maybe left in the large auditorium the first day. Most of the participants are dead (they could be easily identified). I think that those days the average age of semiconductor physicists was higher than now.

Two comments.

1. Concerning the proceedings. The cd publication of Edinburgh has not been received very well. I do not even know how to cite a paper in the cd, no index in print. I hope you plan hardcopy publication.
2. Outreach. I do not find in the draft of the proposal any mention of the large Native American population of the area, the number of reservations and the large Indian population at NAU (*Northern Arizona University*). It should be mentioned in a not too patronizing way somewhere. This led me to think that maybe one should have a few fellowships for Native American students interested in attending.

(*September 17, 2004. Translated from the Spanish*)

The only thing missing from my CV is that I was just named honorary member of the Pakistan Society of Semiconductors. (I may be subject to CIA spying from now on!).

By the way, I just read that the Pope is about to beatify Karl I, the last emperor of Austria. He was emperor during World War I. He didn't cause much fight but he allowed the use of gas.

(*June 22, 2006. After A.N. Chantis et al. clarified the historical record regarding the determination of the spin splitting parameter in InSb in Phys. Rev. Lett. 97, 39903, 2006*)

This sets the record straight. I had already had an argument with my good friend E. Rashba about it. Theorists often do not realize the intricacies involved in a sign determination.

(*January 20, 2007. After being asked to join the grassroots campaign "Draft Obama". Translated from the Spanish*)

What do you think about this dinner? He may oppose Hillary in the primaries. Can he win? Is "America" ready for a young black president? They are looking for people to help them in Arizona.

(*March 5, 2008*)

I have read the three first papers of Raman and I must say that I agree with Mandelstam that the first one (Nature) is irreproducible, possibly incorrect. The second one is somewhat more believable. The third one, in the proceedings of the Indian Academy is superb. If you have a copy of Light Scattering in Solids 9, please look at my introductory article. Mandelstam (or Landsberg) claim that they delayed publication of their work (they published later DATED plates with a date preceding Raman's first plates by a few days) because a close relative had been sentenced to death or the like and they had to help him. He was not executed. The Nobel committee accused them of lying and Ginzburg published much later a note accusing the Committee of slander.

(*May 5, 2008. On disappeared people in Uruguay*)

I got more information than I needed but the case is very similar to that in the book "A veinte años luz" (*by Elsa Osorio. English translation: My name is Light, Bloomsbury USA*). I just found a report of a Uruguayan juridical investigation (over 1000 pages). If interested switch Google to articles in Spanish (I just learned how to do it) and search Juan Gelman Elias Bluth.

The thing became more interesting because Inge, who follows the Spanish literature more than I do, recalled that recently Juan Gelman (Argentine of Jewish-Ukrainian origin) received the VERY prestigious Cervantes prize as the foremost living Spanish language poet. During the Argentine-Uruguayan terror ($\sim$1975) Gelman's son (20) and his pregnant daughter in law disappeared. Years later the boy was found dead (shot in the head) in a barrel filled with concrete. The daughter-in-law had been taken to Uruguay and handed over to the military. She delivered a girl and the mother was killed after delivery. The baby was put in a basket and left on the doorsteps of a high-ranking police officer (he and his wife were not able to have children). Across the street there was the Church of the

Virgen de la Macarena, of which the policeman's wife was very devout. She asked the Virgin to perform a miracle and that was it: the baby appeared, was adopted, and called Macarena.

In the mid 1980s the terror was over and Elias Bluth (remember Elias Bluth?) as secretary of state for the presidency negotiated on behalf of President Sanguinetti with the military the transition to democracy. The military wanted some kind of general amnesty, which they got.

In the meantime, Juan Gelman had heard rumors that his daughter-in-law had been killed in Uruguay and the granddaughter possibly adopted by military personnel. He contacted Sanguinetti and Bluth, who claimed to be totally shocked: such things did not happen in Uruguay, if anywhere they happened in Argentina. Many Uruguayans had been killed in Argentina and any queries had thus far remained unanswered. They promised to do everything in their power to help solve the case. Gelman never heard again from them. Sanguinetti was voted out of office and I presume Elias lost the job but got one in Sanguinetti's lawyer's office). The next president, Batlle, was somewhat more cooperative. Be it as it may, Gelman got a tip about the whereabouts of his granddaughter and met her in early 2000. In due course DNA did its job and it was proven that Macarena was his granddaughter. She held no grudge against her putative parents, even gives them the benefit of the doubt concerning the basket. The putative father, Taurino, died recently, so she cannot even ask him. Three years ago she changed her name from Taurino to Gelman. She is now 31 and spends most of her time trying to find out where her mother's bones may be (digging and DNA).

You will find lots of articles about Elias. His guilt, if any, is only peripheral. How is that for a story?

(*March 27, 2010. Commenting on his doctoral genealogical tree, which included the names of Lindemann and Nernst as academic ancestors. Translated from the Spanish.*)

Regarding Lindemann, he was a strange but interesting guy. There is a recent biography called "Prof". I recommend it. He took many German physicists to Oxford, saving their lives, and at the same time creating Clarendon Lab. He had a lot of money (from his father, who was German but emigrated to England). He became Churchill's right hand during the war. He is often accused of promoting the Dresden bombing, but this is not very accurate: he discussed the pros and cons with Churchill for a long time. Lindeman's most important associate was R.V. Jones, scientific warfare chief. He has a book entitled "Most secret war" (the English and American editions have different titles). Also very interesting. I can't believe the kind of ancestors we have.

Lindemann (and Nernst) proposed to use two Einstein oscillators to explain the specific heat of diamond. If you look at the phonon dispersion curves of diamond, you'll see why, but they didn't have the foggiest idea about dispersion relations. When you explain specific heat in class you can mention it…

I don't know if you have been following the pedophilia reports in German schools: catholic (Jesuit), secular, and even Protestant. It's horrifying.

(*February 12, 2012. Translated from the Spanish. Background: Argentines have a reputation for being puffed up.*)

A Spaniard meets an Argentine and starts some small talk:

- What a beautiful day! It's really splendid!
- Thank you very much. I do what I can.

(*February 19, 2014. Background: Argentine universities were autonomous entities since 1918, and this autonomy enabled remarkable achievements. This includes two Nobel prizes—Physiology and Chemistry—for work done at the University of Buenos Aires, UBA. In spite of several military coups, university autonomy was more or less respected until 1966. In the "The Night of the Long Batons", federal police finally "invaded" the University and the golden age was over. Manuel's research was interrupted by this event. Translated from the Spanish.*)

I was at UBA during our summer (their winter) in 1965. Over there we started, together with Norberto Majlis (now emeritus professor at McGill University, Montreal), calculations on band structures with complex wave vectors. I returned to Brown University when I finished my stay at UBA. By snail mail (there was no e-mail those days) we completed a paper that was submitted to Solid State Communications. (*The reference is SSC **4**, 631, 1966*) In it we included calculations along the (100) and (111) directions, but we did not have time to finish the (110) direction due to the Night of the Long Batons: my collaborators Majlis and Mauricio Chaves had to leave the country. It is not clear who destroyed the computer, which I believe was called Ferranti Mercury-Meteor.

Perhaps you would be interested to learn that during my time in Buenos Aires I collaborated with Ricardo Sussman. When the "Night" happened, he was in the People's Republic of China with a scholarship from the Chinese government. Clearly, he could not return to Argentina. His wife Silvia, then personal secretary of Dean Rolando García, was beaten by the police batons. She is the one who phoned or telegraphed Ricardo asking him not to return. With my help and that of Greenaway, Ricardo found a temporal position with RCA in Switzerland. He was later hired by DeBeers in England to work on CVD of diamond. He became the director of the pilot plant to make diamonds. I have had contact with him ever since and I have a fabulous diamond wafer with which I run demos at conferences (enormous thermal conductivity). After retiring, Sussman worked at universities in Paris and Florence. He gave me the wafer because it was a low-quality "second", but this didn't matter for the thermal conductivity or the specific heat. Recently I received the Leloir Prize in Buenos Aires and met several of those who were beaten by the police and had worked with me at UBA, including the famous "Pipo" Westerkamp.

The next fascist felony happened in Chile, when in 1969 Majlis, his wife and other Argentines were given 24 h to leave the country. I think you can find about it on Google.

(*June 14, 2014. This is the last e-mail the author received from Manuel, in response to an inquiry about spherically averaged hole effective masses in semiconductors.*)

I am sorry not to be with you so as to check the sphericalized expressions. I am sure that you can solve the problem. My Parkinson mind should not be overloaded at this point. Sorry Manuel.

(*June 14, 2014. This is the author's response to Manuel's last e-mail*)

Manuel:

If I were offered to have your "Parkinson mind" in lieu of mine, I'd take it, without hesitation. But you are right, I think this can be resolved easily because I suspect that we are dealing with a combination of those pesky, unavoidable typos and, as Peter (*Yu*) writes and I show in part in my write-up, with different ways of defining spherical averages.

See you soon,

Pepe

Remembering Manuel Cardona

Roberto Merlin

I remember the first time I heard of Manuel Cardona. It was my undergraduate advisor, Aron Pinczuk, who told me about him and the contacts he had with Argentina, as he was busily writing a chapter with Elias Burstein for the original book of the series on Light Scattering in Solids. Soon after, it was a letter from Aron what persuaded Cardona to offer me in 1974 a fellowship to pursue graduate studies in Germany.

The first time I met Cardona was in 1975 at a conference in Campinas. I remember him surrounded by a crowd of physicists who wanted to chat with him, pausing briefly to say a few words to me and shake my hand. I also remember, a few months later, when I first saw him in Stuttgart in his much cluttered office at the Max-Planck-Institut FKF, with his desk and the floor fully covered with journals and papers.

As a student, I was once asked to play the role of Cardona at a Fasching's play mocking the institute's directors. He took it well. I remember him talking to me in Spanish and, simultaneously, in German to his secretary, while switching to French (or was it Italian?) to greet a guest who had just stopped by his office. I still do not know how many languages Cardona spoke. I also remember that he liked to sing and often joined others in singing during the parties at the house he shared with Inge and his children, in Knappenweg.

At MPI, the research I was involved with, on correlated electrons, was spearheaded by Gernot Güntherodt who was then a member of the permanent staff of Cardona's Raman scattering group. Although he was my thesis advisor, Cardona refused to co-author several of my papers because he did not think he had contributed much to the work. I remember the first manuscript I wrote completely on my own, the draft of which he so patiently read and kindly told me that it needed more work (it surely did!). I also recall his dislike of dissertations that were too short and the fact that he could write the draft of a long paper for Physical Review in one afternoon. At that time, he was still for me Professor Cardona. He will later become my colleague and then, simply, *mi amigo* Manuel.

As the years went by, and I moved from Urbana-Champaign, where I worked in the group of Miles Klein, to the University of Michigan, the rate at which Manuel and I spoke on the phone kept increasing. We would usually start with a discussion of some physics question I had posed (I always had questions for him), but then the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_24

conversation would shift towards world politics, movies, literature or whatever it was Manuel wanted to talk about.

I remember remarks Manuel made about the Popess Johanna at the restaurant Gandy Dancer during one of his few visits to Ann Arbor. For some reason, this episode remained stuck in my memory and the historical character who very few of the people at the table (including me) knew about became forever tied to Manuel.

I remember Manuel in Calcutta talking about the relationship between Hindus and Muslims in India… in front of a large group of Indians! And I recall his comments about „l'art roman" (in English, romanesque art or norman art? he would have probably known the answer!) in a small chapel in Corsica. I have also memories of him telling the audience about a surgery he had just gone through during a speech he gave honoring the awardee of a prize at an APS March Meeting! And I remember him talking about the civil war in Spain, in front of a plate of spaghettis. Most of all, I will always remember the breakfasts with him when he came to a conference in Ann Arbor in 2012 and stayed in my house. Manuel's brain was already at full speed at 7 a.m. and didn't stop until he went to sleep.

Manuel was a great scientist and mentor, at times a generous colleague, a good friend to many and, among countless other things, a gentle man who could listen and also loved to be listened to (it was a real pleasure to listen to him). He probably had a clear sense of his own value, but his ways were never arrogant. I will remember Manuel as somebody whose curiosity about the world didn't seem to know any limits. He was blessed with an incredible memory. His recollection of improbable facts was nothing short of extraordinary and far from being just encyclopedic.

Manuel Cardona loved Physics and also knew how to enjoy a good talk with friends and a good glass of wine. Maybe he was one of the last Renaissance men.

Manuel Cardona: Remembrances About a Great Master and Mentor

Francisco J. Meseguer

> *If you can't explain it simply, you don't understand it well enough.*
>
> Albert Einstein

Last July 2nd 2014, we knew the absence of our beloved master and mentor Manuel Cardona (Barcelona 1934). Now at the solitude of my desk, it is emerging from the depth of my memory some moments from old days between 1981 and 1983 I had the privilege of enjoying the work and also the life near Manuel and his family, specially his wife Inge. Cardona is one of the top scientists in Solid State Physics. Firstly he studied Physics at the University of Barcelona, Spain (1956), and after 3 years at the Universidad Complutense of Madrid, he decided to seek better fortune, and left Spain, like doing now many of our Spanish students, then because of the mind blindness of the government, and now because of the collapse of the Spanish R & D system… Unfortunately, history repeats itself again and again…, and Cardona went off Spain… and he did not return back. However he was always very close to his home country.

Manuel made a brilliant career, supported by his beloved Inge, who dedicated all her life to him. Manuel had two great passions, his family and Physics. His career started in USA where he got the Ph.D. degree (Harvard, 1960). Since then, he was fully involved on semiconductors research. He worked on electronics for the RCA Company at Zurich and Princeton and later he was appointed Professor at Brown University. In 1971 he was appointed as founding director of the newly created Max Planck Institut for Solid State Physics (MPI-FKF) in Stuttgart, Germany. His professional achievements are staggering, with over 1,300 published works. The enormous impact and influence of his work deserved him 60 awards and distinctions as relevant as the Principe of Asturias (1988) award, as well as being appointed as distinguished Fellow of the American Physical Society, and the National Academy of Sciences of USA among many other distinctions.

However, I would like to highlight other personal qualities typical of a great master. He had interest for other areas like Art or History. However Physics was his work field…and also the playing field…,and please trust me, he was a very good

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_25

player. As a great master, he had the ability of solving complex problems with simple models. He always tried simplifying complex problems. One example was the estimation of the deformation potentials of semiconductors he liked very much calculating. He obtained quite reasonable results by reducing the problem to a simple 2 × 2 matrix eigenvalue calculation (*…2 × 2 matrix calculation can grasp the essence of the problem.*. he told to me). He also had an outstanding capability to find connections and links between different scientific areas. I remember a seminar in 1982, at the MPI-FKF when I was there for a Postdoc stage. In this seminar the speaker, Dionisio Bermejo, a postdoc fom Cardona´s group, was reporting on the polarizability calculation made in collaboration with Cardona. This work (Sol. Stat. Comm. **42**, 153 (1982)) shows how the polarizability value of gaseous hydrocarbons like ethane, could give reliable information about the polarizability of an absolutely different system, a solid and very hard material as diamond. This seminar shocked me so much, since it showed how a very open mind, as the Cardona way of thinking, could find some sort of universality in the information, and it can be transferred to different fields of science. He also had the personal quality found in both, the children and the great geniuses: the capacity of surprise and engagement he had when learning new aspects of science and nature. Then, he was immediately engaged in the joy of knowledge where space and time disappears. His other great personal quality was the ability found in outstanding communicators. He was able to transmit us his passion for science, which dragged all of us around him. He was like a whirlwind of new ideas and projects, which trapped us and inspired us. And it made us work much harder and much better. The work became like art and like playing games… and time did not exist. And it was really so. There was no Saturdays or Sundays…. it was pure passion for science. Professor Cardona had the great ability of sowing the seeds of love for science into hundreds of those of us who were his students and colleagues. Then, many of us were spread worldwide with an impressive multiplier effect. He has collaborated with scientists from around 20 different countries. Specifically, he has trained around 20 Spanish scientists, which constitutes an enormous support to the scientific background research in Spain. This is the reason why I stated above that although Cardona left Spain, he always was very close to us, looking after us.

His deep sense of well-done work, dedication and effort has remained until literally his last breath. He died working… playing the exciting game of knowledge. He had a good death.

Manuel, you will always be in our minds and hearts!.

Valencia, Spain, January 20th 2015

Because You Do not Send Me Bad Stuff to Consider…

Narcís Mestres

I joined the Abteilung Cardona in 1982 as a graduate student encouraged by Jordi Pascual, at the time my professor at the Universitat Autònoma de Barcelona (UAB), and who in turn got inspired by the early works of Manuel in modulation spectroscopy during his Ph.D. [1]. This Abteilung was a model of cultural and ethnic diversity. Both Manuel and Inge always cared for the wellbeing of those who were away from home. They would help you with any matter you had, either professional or personal. They would always find an interesting topic to talk about, which was related to the history, the culture or the language of the homeland of each of us. Just to mention one example: the warm Ph.D. Graduation parties organized at their home in Vaihingen.

As an example of his prestige amongst scientist colleagues as well as journal editors, I would like to share here the following anecdote:

Back in 1985, we were following an interesting work started the year before during a stay at Max-Planck Institut of Professor Fernando Cerdeira on Raman scattering from electronic excitations in heavily doped germanium [2]. Fernando Cerdeira was a former Ph.D. student of Manuel at Brown University from Argentina, and at the time a Professor at Campinas University (Brazil). Since germanium is a group IV non polar semiconductor, this work was the first direct observation of light scattering by pure plasmon modes in a doped semiconductor taking advantage of the resonant enhancement in the regions of the E_1 and $E_1 + \Delta_1$ optical gaps. After the verification of the collective electronic excitations, Manuel

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_26

told me to look for the single particle excitations. In resonance conditions, I was able to detect the low-frequency Raman scattering by spin-density fluctuations in heavily doped n-type Ge. The Raman selection rules were fulfilled and the shape of the resonance curve in the proximity of the E_1 gap was also measured. Manuel got very excited with the new results. However, this was not enough for him. It was characteristic of Manuel to bring an experiment and the theory closer together, so I had to calculate the absolute values of the scattering efficiencies. On those days, Manuel and José (Pepe) Menéndez were investigating interference effects between allowed and forbidden LO-phonon Raman scattering in GaAs, to separate quantitatively the contributions of intrinsic and impurity-induced forbidden scattering. They estimated the absolute values of the Raman efficiencies and compared them with the experiment with excellent agreement [3, 4]. I remember having a hard time and discussing with Pepe the search of missing factors in the calculation of the absolute Raman cross section. Finally, the magnitude and the shape of the resonance Raman profile by spin-density fluctuations could be fitted thanks to the band gap parameters extracted by Luis Viña and Stergios Logothetidis from ellipsometry measurements performed on the same samples [5], and Manuel decided to send the paper to Physical Review Letters. As a young student, I was very excited to send a paper to be considered for publication in such a reputed and high-level journal.

At that time (it was not the Internet era yet), the communication with the editorial office was made by regular mail and things were going much slower than today. After 2 months of eager expectation from my part and to my big surprise, we received the following acceptation letter from the editor, whose name I prefer to keep in secret.

The paper was received by the editorial office on 24th of June 1985 and published in the 2nd of September issue 1985 [6].

The total number of published Physical Review Letters papers by Manuel Cardona progressively increased over the years, reaching the number of 60 by the year 2007 [7].

I had an enriching experience carrying out my Ph.D. work in Stuttgart focused on resonant Raman scattering studies in heavily doped semiconductors. The things I learnt from Manuel were decisive in my professional work for sure, but also in my attitude towards life. I was always impressed by Manuel's energy, enthusiasm, and curiosity, not only for science, but also for any aspect of history, culture, language and politics. His great mentorship of a myriad of students, postdoctoral fellows, and visiting scientists lives on as his greatest legacy, and I feel very fortunate to belong to this community.

Thank you, Manuel.

8 August 1985

Dr. Manuel Cardona
Max-Planck-Inst. F. Fest.
Heisssenbergstrasse 1
D-7000 Stuttgart 80
West Germany

Re: Manuscript No. LF3233

Dear Manuel:

Enclosed is the only referee report received on LF3233 by Mestres and you. The letter is accepted and will appear soon.

I decided to approve publication because I know you, and because you do not send me bad stuff to consider. (My computer says only 11 Letters sent in versus 75 PR articles; of 11, 7 are published as Letters and 1 as a Rapid Comm.)

My concern - and reason for this note - is simple.
Suppose Mestres was sole author. Then I have no other evidence (or emotion) to sway my judgment for or against publication. But is it fair to consider past performance and personal acquaintance? What if I'm not aware of an author's success in other journals if I was never lucky enough to meet him?

Clearly, there is no definite answer. I'll let these remarks simply lead to an exchange of views over a beer one day.

Regards,

Editor
Physical Review Letters

References

1. J. Pascual, J. Camassel, H. Mathieu, Fine structure in the intrinsic absorption edge of TiO_2. Phys. Rev. B **18**, 5606 (1978). doi: 10.1103/PhysRevB.18.5606
2. F. Cerdeira, N. Mestres, M. Cardona, Light scattering by plasmons in germanium. Phys. Rev. B **29**, 3737 (1984) (Rapid communication). doi: 10.1103/PhysRevB.29.37372
3. J. Menéndez, M. Cardona, Interference between allowed and forbidden Raman scattering by longitudinal-optical phonons in GaAs. Phys. Rev. Lett. **51**, 1297 (1983). doi: 10.1103/PhysRevLett.51.1297
4. J. Menéndez, M. Cardona, Interference effects: a key to understanding forbidden Raman scattering by LO phonons in GaAs. Phys. Rev. B **31**, 3696 (1985). doi: 10.1103/PhysRevB.31.3696
5. L. Viña, S. Logothetidis, M. Cardona, Temperature dependence of the dielectric function of germanium. Phys. Rev. B **30**, 1979 (1984). doi: 10.1103/PhysRevB.30.1979
6. N. Mestres, M. Cardona, Resonant Raman Scattering by Spin-Density Fluctuations in n-type Germanium. Phys. Rev. Lett. **55**, 1132 (1985). doi: 10.1103/PhysRevLett.55.1132
7. A.H. Romero, R.K. Kremer, W. Marx, The scientific road of Manuel Cardona: a bibliometric analysis. Ann. Phys. (Berlin) 523, **1**–**2**, 179 (2011). doi: 10.1002/andp.201000090

My Recollections of Manuel Cardona, a Great Scientist

Andreas Otto

The first time for me to take notice of Manuel was when he started to cooperate with Rupprecht Hänsel, in 1970. Rupprecht, later Professor at Kiel University that time, had been employed as a "security person in radiation protection" at the Synchrotron DESY in Hamburg. There was just a small room where radiation was monitored. That relatively small "bunker" was soon filled with X-ray and Vacuum UV spectrometers. Manuel, arriving from Brown-University, was working there and cooperated closely with Rupprecht. Later I heard about the plans of a new Max Planck Institute building for solid state research, with Manuel as one of the directors.

I cannot recall whether I met Manuel personally before 1974. In this year he was participating at the Spring meeting of the German Physical Society in Freudenstadt. There I was giving a plenary talk on the excitation of surface plasmon polaritons by attenuated total reflection. Shortly after this first personal meeting I received a telephone call from Manuel, whether I would know a good candidate for a permanent and well paid position in his group. Lacking an overview of the academic staff outside Munich I had to deny. Then Manuel asked me to come, and with great pleasure, with a feeling of being honoured by him, I agreed immediately to move from Munich to Stuttgart.

The initial MPI laboratory in a small business building was overcrowded with equipment and scientists. Only in the new specially designed building in Stuttgart-Büssnau was enough space. The research in this institute was far beyond the level of the research I did know from the Maximilian-University in Munich. I just had experience in leading a small group of diploma and PhD-students working on electron energy loss spectroscopy and optical spectroscopy.

My first task at the MPI in Stuttgart was to take Raman spectra of metallic suboxides of the alkali metals. Two technicians were tending lasers and spectrometers. This project did not deliver meaningful spectra. Soon thereafter I was joining Manuel to the meeting with the external scientific board of referees. I was presenting a general talk on the literature of Raman spectroscopy of metals. Manuel and Inge often invited his research group and scientific guests to their home. I very much liked these private meetings. Manuel and Inge have been wonderful hosts.

I was joining Manuel and other MPI-members to the conference on semiconductor physics in Rome in summer 1977. The group of Raman spectroscopists were

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_27

also participating in 1977 under his leadership at the 3rd International Conference on Light Scattering in Solids, in memory of the famous Raman Spectroscopist Porto. My impression was, that Manuel was permanently writing research papers. His Spanish speaking secretary had even troubles to type and keep up with his literature list.

After the military coup in Argentine, he immediately gave shelter to scientists which had to escape. He pushed the Max-Plank society to consider its entanglement with the Nazi regime. We had long discussions about the reunification of Germany.

Once he told me about the many awards and prizes which had received. In summary Manuel gave me the chance to widen my knowledge on solid state physics and group theory, which was important for me teaching at the University in Düsseldorf. I am proud to have been with him. At the “Physikalisches Kolloquium” in Düsseldorf in January 2002 he spoke about mono-isotopic diamond in a way that nobody was lost in understanding, slowly developing the topic. What a skilled wonderful teacher he was!

At the 20th International Conference on Raman spectroscopy in August 2006 in Yokohama, Japan, my wife and myself had lively and very friendly conversations with Manuel. He had deep knowledge about Japanese culture and introduced us to some modern Japanese literature.

After I had left the MPI I regularly phoned him in Stuttgart all the years until last year some days before Christmas. Then we have had always long conversations about family, health, friends and general matters.

When I was passing Stuttgart for going further south, I visited Manuel and Inge. We were walking with him in the nearby streets at his home, where he showed us the footsteps built in bronce in memory of killed Jewish people during the Nazi regime. Manuel and Inge have been also with us in our home at the Mosel river. Together we visited the Black Madonna in the church of the Kapuziner cloister in Beilstein, opposite to our home. This Madonna was given to the local people by Spanish soldiers in the 17th centuries. But Manuel saw immediately the statue of the Saint Therese of Avila and told “her story” to us. Neither me nor my wife have been aware of Theresa before.

I feel very thankful that I had this close connections with Manuel and his wonderful wife Inge.

Manuel Cardona and the ALBA Synchrotron Light Source

Ramon Pascual

Manuel Cardona left the University of Barcelona before I arrived there to study Physics, so I had never met him before he came to the University of Madrid, at the end of the 60s, while I was working as a professor in it. He came to give a seminar which I attended just to learn something, since solid state physics was not my speciality. I do not remember the exact subject of the seminar but I remember quite well that he told about an experiment done at DESY (which was well known to me) using "synchrotron radiation".

After that seminar I met him a few times when he visited his colleagues in Barcelona. One special occasion was when we discussed with the Higher Council of Scientific Research (*Consejo Superior de Investigaciones Científicas*) the idea to create a new Institute of Materials Science at the campus of the Autonomous University of Barcelona. I was the Rector of the University at that time and one of my conditions to take part in it was that the Institute had a kind of Scientific Advisory Committee chaired by Manuel. Of course I followed the different stages of creation of the current Institute of Materials Science of Barcelona, *ICMAB*, to which Manuel contributed very much, but it was not my field of interest.

My closest cooperation with Manuel came a few years later. As a theoretical high energy physicist, I followed the different ideas of having a particle accelerator in Spain, where several groups of experimentalists existed and a new one started at the UAB campus, the High Energy Physics Institute, *IFAE*, when Spain rejoined the CERN at the beginning of the 80s. Ten years later there was the idea to establish in Spain a Tau-Charm Factory, a collider of electrons and positrons with sufficient energy to create tau leptons and hadrons with charmed quarks. My proposal to establish such a facility in Barcelona, with the collaboration of the CERN and some other countries, was accepted by the Catalan Administration, the *Generalitat de Catalunya*, and I was put in charge of preparing a formal proposal for the project. The main idea was to have a centre of scientific excellence in the field of high energy physics and accelerator science and technology, to have a large infrastructure in Catalonia, to spread the knowledge in accelerator science and technology and to develop related technologies in the industries.

After a detailed analysis I arrived to the conclusion that, if the Tau-Charm Factory were really built, it would surpass the characteristics of the first CERN design study and exceed the limits of local capacities. So, what could be done? I

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_28

remembered Manuel's seminar in Madrid and the general ideas that I had about the development of synchrotron radiation after the first parasitic experiments in high energy physics laboratories and the development of new dedicated synchrotron facilities. Apparently, the planned objectives of constructing a Tau-Charm Factory could be accomplished at an acceptable cost and a benefit for a much larger community of scientists. Was that a good idea?

One of the first things that came to my mind was to ask Manuel. I wrote a letter to him (in 1992 the email was not so extensively used) and we decided that a visit to Stuttgart would be convenient. I was very well received by Manuel and Inge at the laboratory, at home and even at a concert. It seemed that the idea of constructing a synchrotron light source in Spain was not so bad and we decided that it needed to be explored. After the visit to Stuttgart and after gathering some other opinions, I exposed the idea to the highest Catalan authorities and to some other colleagues and was authorized to assemble a small group of scientists, some of them from the synchrotron science field, to prepare a Feasibility Study. In view of the positive result of the study, the Catalan Government made a public announcement of its decision to build a synchrotron light source. The announcement was followed by the creation of two committees, one of an administrative or political nature and a Scientific Advisory Committee, which was chaired, of course, by Manuel. At the same time a special call for fellowships was opened and a small group of people was selected to learn the essentials of synchrotron light. Some of them are now part of ALBA's staff.

Manuel was able to call a very high level group of specialists and laboratory directors which met several times in Barcelona and made the right decisions for the future success of the project. This was at the beginning of the nineties in coincidence with the severe post-Olympic financial crisis which obliged to keep the project under hibernation. But the project was alive. The first Director, Prof. Joan Bordas, was selected and the first detailed study was prepared under his direction.

This is not the place to describe the difficult years until March 2002 when the Spanish and Catalan administrations signed a protocol creating a Consortium for the Construction, Equipment and Exploitation of a Synchrotron Light Source near Barcelona. The rest until today is the history of the ALBA synchrotron Light Source: www.cells.es.

The last time I phone him was 1 year ago. As President of the Royal Academy of Sciences and Arts of Barcelona, of which he was a corresponding member since 1982, I invited him to come to the events of the commemoration of the 250th anniversary of the Academy. Unfortunately he was not able to come.

Memories of Manuel Cardona

Richard Phillips

Manuel's influence on solid-state physics was already deep and extensive when I first encountered him in 1977. By that point, of course, the founding Directors of the Max-Planck-Institut für Festkörperforschung in Stuttgart had already established the laboratory at the forefront of research, in just a handful of years from its inception. That would probably have seemed remarkable to a more experienced physicist; as a novice, I just accepted it as a normal state of affairs. Manuel's group at the MPI was already a prime destination for students just completing their Ph.D., and it vied with "Bell Labs" as the best place to go. The group to which I belonged was also home to the retired Nevill Mott, who had encouraged the growth of interest in amorphous semiconductors in Cambridge, and that set my initial scientific background. A breakthrough had occurred in 1976, when methods for doping in the disordered state were brought to the fore by Walter Spear and Peter LeComber. It turned out that for a while there was some dispute about what really made up the material which was capable of being doped (in the highly disordered state of amorphous semiconductors doping was not easily achieved because of the high density of states in the "gap"). Now, superficially this does not look like Manuel's type of problem. These disordered systems lack most ingredients for being rewarding candidates for detailed study of the physics—every sample is subtly different, small changes in deposition can produce large changes in properties, most model descriptions resort to an unconvincing amount of arm-waving—and worst of all—there isn't an interesting group representation in sight! However, there was one redeeming feature of this problem: the controversy centred on the nature and rôle of hydrogen incorporated in the films grown by glow-discharge deposition, and this was tailor-made for resolution by Raman scattering. This consideration made it natural that Manuel was a star attraction at the annual Amorphous Semiconductors conference in London in December 1977. Manuel spoke about his work with Marc Brodsky on Raman scattering by amorphous silicon deposited by glow discharge in various gases. The importance of hydrogen had been conclusively shown by Bill Paul a year before, but the manner of its incorporation was an important unresolved problem. Manuel's paper—based on

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_29

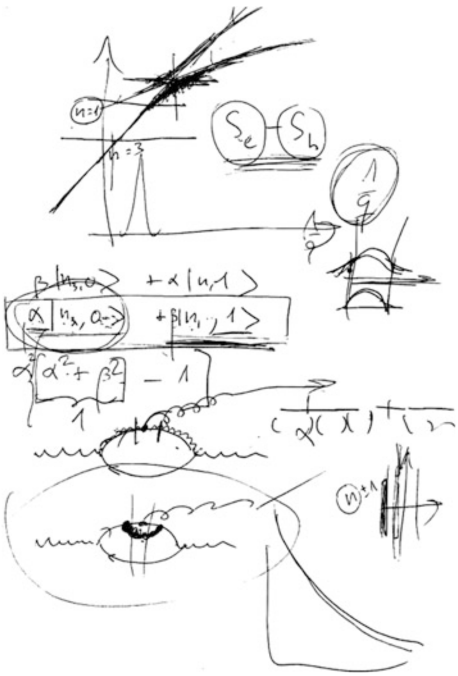
one published 2 months earlier in Physical Review—detailed the observation of the vibrational signatures of numerous species, and the study encompassed many of the standard Raman tricks, including deuteration to verify frequency shifts. After conclusive demonstration of the different vibrations of hydrogen, the reader of the Physical Review paper finds the sentence: “For convenience we call this type of material true hydrogenated a-Si and use the nomenclature a-Si:H”. With around a thousand citations, this work exemplifies Manuel’s qualities as a pioneering and influential scientist.

There is a special reason to remember Manuel’s talk, because it was easily the most animated at the conference. There was still perhaps some lingering doubt about the Raman results among some members of the audience, and Manuel was ever keen to present a convincing argument—so he ran through the various bonding configurations which were unambiguously detected in the Raman spectra. The key defect contributing deep states in the gap was the “dangling bond” on a threefold-coordinated silicon atom, and Manuel showed graphically by pumping his arm in an out, that a hydrogen atom bonded at such a silicon site would contribute a mode at some specific frequency. This was a different sort of arm-waving, and the audience were clearly being won over. Two hydrogen atoms sometimes were bound to the same silicon, which meant that Manuel could explain that for the SiH_2 centre there was a symmetric vibration—both arms pumping in and out in unison—and an antisymmetric one—arms pumping in antiphase. It was clear that he was enjoying this performance, and so was the audience—all the physics was clear. Someone—a well-known figure in the front row—shouted out “Manuel, show us SiH_4!”….

That was my introduction to Manuel’s work, and it made a big enough impact that after I completed my Ph.D., his group was the most attractive place for a postdoctoral position. Manuel kindly agreed to support an application for a Fellowship, and equally kindly forgave me when I was faced with the conundrum of accepting it, or the offer of a permanent academic position in the UK. Factors not connected with physics tipped the balance, and so my closer contact with Manuel was put off until I had the opportunity to take leave in the summer of 1988, and join his group for 4 months.

The atmosphere in Manuel’s group was extraordinary, and the range of talent among the research students, staff and visitors produced a constant stream of interesting physics. It took a couple of weeks to get going, but once settled in there were some months of experiments ahead working with Tobias Ruf on Raman scattering in GaAs and InP in magnetic fields. Tobias was in the midst of his Ph.D., and probably could have done without having to put up with a visitor joining in, but we quickly found that we worked well together, and it proved possible with two people to keep the data taking going at an intensity which would have been a challenge for someone working alone. The 12.7 T split-coil superconducting magnet dated from the early days of the Institute, and was getting to the point where reaching top field was always done rather carefully. I recall one occasion, sitting in the dark after changing polarisation conditions in the experiment at 12.7 T, half a Watt of cw infra-red laser beam passing just behind me, and about to begin the downward ramp for data acquisition, when the magnet quenched. I don't think I've ever jumped so far out of my seat.

There was a good spirit of camaraderie in the group. I recall once breaking a piece of glassware which was an essential part of the apparatus for filling the magnet cryostat with liquid helium. I dropped it during a 3 a.m. refill; as far as I knew it was the only one, and its demise meant the end of data taking—as well as a poor reflection of my own competence. Making the best out of a bad job, I picked up the pieces and arranged them back into the shape of the original, and left it with a humorous but apologetic note on Herr Hirt's desk, and went to bed. When I got in, late, that morning, there was a replacement waiting for me! This seemed typical of how the group helped maximise everyone's output: there could really not have been a better working atmosphere. The resources of the Max Planck Society ensured that minor problems did not obstruct progress—there often seemed to be a spare tucked in a cupboard, even for quite expensive items of equipment. That support extended to covering the almost insatiable appetite of the magnet system for liquid helium—4300 l in August and 3100 l in September. Following all that accumulation of data, the long sessions of interpretation which periodically we had with Manuel were always both stimulating and demanding. The magneto-Raman and piezoreflectivity experiments revealed a wealth of phenomena, with ingredients in the physics which included resonances whose position depended on the details of the magnetic band structure, couplings which related to electron-phonon interactions and excitonic effects. I recall long discussions about the different potential interpretations of the data, as understanding gradually converged on a detailed description of what was going on. Having been mainly interested in disordered systems in my previous research the process of refining the physical picture in this new environment was for me a revelation—it felt like the first time I had encountered real physicists in action, and was a world away from the hand-waving approach taken in less rigorous work. The magnetic band structure was modelled by $\mathbf{k}\cdot\mathbf{p}$, of course, which had been impressively mastered by Tobias Ruf. Other visitors contributed their special expertise, with Andres Cantarero joining in some of the experiments, and Carlos Trallero-Giner apparently able to find a confluent hypergeometric function to model any excitonic effect which might be important. Manuel had an encyclopaedic knowledge of the symmetry aspects of the $\mathbf{k}\cdot\mathbf{p}$ couplings and scattering mechanisms, and could immediately recall all the relevant representations, and any direct product decomposition you might care to consider. This meant that he could identify quickly all the possible channels for a scattering process which might contribute in a given circumstance, and his decades of experience constantly shone through as he drew upon multiple sources to check ideas—apparently in any language you might imagine. During these discussions he constantly illustrated the evolution of his argument with the sketches familiar to all his co-workers, in which $\mathbf{k}\cdot\mathbf{p}$ interactions and Feynman diagrams for Raman processes always prominently featured. The accompanying figure dates from 1993, and Manuel's co-workers will instantly recognise the style.



One further remarkable aspect of the nature of Manuel's group was the effect on it exercised by Inge. She and Manuel would generously welcome those in the Institute into the Cardona family home where the visitor would quickly appreciate the other dimensions in Manuel's life. Discussion of the collection of ceramics and other interesting objects would lead to entertaining stories about the way the various items had ended up in Stuttgart, frequently via a flea market, whether in the Portobello Road in London, somewhere in São Paulo or anywhere else on the planet. The welcome extended by Inge and Manuel was such that in many respects, rather than just creating a "school" or style in doing physics—as Manuel undoubtedly did—the Cardonas together built an extended family with branches all around the world. The full range of Manuel's influence, as a physicist, mentor, friend and colleague is hard to convey, but in my own case it is no exaggeration to say that working in Manuel's group changed my direction completely, for which I have been grateful ever since.

Manuel Cardona: Mentor, Colleague, Friend

Aron Pinczuk

Mentor

During more than 45 years Manuel Cardona has been a trusted and much appreciated friend. He was a mentor when I was a junior scientist and later he was a highly valued colleague and collaborator. This concise text is a small tribute to our long friendship. As a graduate student under Eli Burstein I was very much impressed by Manuel's pioneering research on optical properties of group IV elemental semiconductors and III–V compound semiconductors [1, 2]. In 1968 with Eli we published a short paper that identified resonant Raman scattering by optical phonons of InSb surface layers as mediated by the, so-called, E_1 interband energy gap [3]. The E_1 gap results from excitonic optical transitions that are associated with large valence band to conduction band joint density of states. The E_1 gap was first identified in [1] and studied in greater detail in [2].

After joining the Max Planck Institute for Solid State Research (Festkörperforschung) in Stuttgart as one of its founder-directors Manuel started a vigorous program of research on resonance Raman scattering in semiconductors and insulators. In 1975, at the time I was back in Argentina, Manuel invited me to join him at the Max Planck Institute as a short-term visiting scientist to explore joint research in the field of Raman scattering in semiconductors.

I arrived in Stuttgart in December 1975. During much of 1976 with Manuel we studied resonant inelastic light scattering by collective excitations of the electron gas in the n-type doped semiconductor GaAs. This research followed up on work done a few years earlier [4]. Gerhard Abstreiter, then a research scientist in the "Abteilung" Cardona, played a major role in resonant Raman scattering work at the Max Planck Institute. The many conversations I had with Gerhard served as an introduction to the physics of 2D electron systems in semiconductors, a topic that

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_30

was new at the time and that, as we now know, was to become one of the major fields of research in the condensed matter sciences community.

After the military coup in March 1976 I could not return to Argentina. At that time Manuel provided the crucial assistance and support that facilitated my return to the United States to continue a career as a research scientist first at IBM and later at Bell Labs.

Collaborator

The collaboration with Abstreiter and Cardona led to publication of several papers that elucidated different aspects of resonant inelastic light scattering by collective excitations of the electron gas in doped n-type GaAs. Much of this work is reviewed in the book-chapter that we published in 1984 [5].

Colleague and Friend

In 1992 Manuel Cardona was appointed Editor-in-Chief of '*Solid State Communications*'. Soon afterwards he invited me to join him as his Associate Editor-in-Chief. As an editorial team we worked together for about 12 years. This was an exciting period in which I learned about managing a journal and encouraging the growth and maturity of several major areas in the condensed matter sciences.

The friendship with Manuel Cardona lasted for 45 years. This is an extended period of time in which I had been able to see Manuel in action as a prominent scientist of extraordinary productivity in many areas of condensed matter. I knew him as trusted editor and as mentor and supporter of the careers of many colleagues. Manuel was a kind and supportive friend who will be greatly missed.

References

1. M. Cardona, G. Harbeke, Excitons at the L absorption edge in zinc blende-type semiconductors. Phys. Rev. Lett. **8**, 90 (1962)
2. M. Cardona, K.L. Shaklee, F.H. Pollak, Electroreflectance at a semiconductor-electrolite interface. Phys. Rev. **154**, 696 (1967)
3. A. Pinczuk, E. Burstein, Raman scattering from InSb surfaces at photon energies near E_1 energy gap. Phys. Rev. Lett. **21**, 1073 (1968)
4. A. Pinczuk, L.L. Brillson, E. Burstein, E. Anastassakis, Resonant light scattering by single-particle electronic excitations in GaAs. Phys. Rev. Lett. **27**, 317 (1971)
5. G. Abstreiter, M. Cardona, A. Pinczuk, *Light scattering by free carrier excitations in semiconductors*, in Light Scattering in Solids IV, ed. by M. Cardona, G. Güntherodt, Topics in Applied Physics, vol 54 (Springer, Berlin, 1984)

From Semiconductor Quantum Wells and Superlattices to Paul Drude—Personal Recollections of Manuel Cardona from 1974 to 2014

Klaus H. Ploog

Before joining the Max Planck Insitute in early 1974, I was browsing through the central library of the Nuclear Research Center (KFA) Jülich in December 1973 to get information about the founding directors of the new institute on solid state research in Stuttgart. Some of my collegues at that time were pointing in particular to Manuel Cardona, who was supposedly famous for having published a lot of papers. And the colleagues were warning me about the pressure expected to be imposed on me to get results and to publish as fast as possible.

After joining the Rabenau group of the Max Planck Institute in Stuttgart in January 1974 to start the MBE (molecular beam epitaxy) project, I first had rather little interaction with Manuel himself, but much more with many of his group members, including Walter Braun, Russell Schmidt, Nigel Shevchik, Pepe Calleja, Jeff Lannin, Bruce McCombe, and many others. I do remember, however, that in the temporary building of the institute near the central railway station (Heilbronnerstrasse 69) Manuel was sharing his tiny office with his secretary Miss. Güttig, that he possessed a tremendous curiosity about almost everything that he came across, and that in lively discussions with his group members he was frequently switching from one language to another with an enormous speed (I don't know if Catalan was among them, but it probably was).

Then, after moving into the new building in Büsnau in 1975, the interaction got closer because the Cardona group and the Rabenau group had their laboratories and offices on the same seventh floor. Now experimental collaborations got started with Gerhard Abstreiter, Ray Tsu, R. Trommer, Diego Olego, Jochen Stolz, and others. After-work parties on the seventh floor after finishing successful experiments in the labs became famous in the entire building. Ray showed his progress in ballett dancing, Manuel prepared "Chili con Carne" and served Cava, and numerous graduate students celebrated their successful oral Ph.D. exams. At that time, till about 1981, Manuel himself was not very interested in the first GaAs quantum wells and superlattices that we had grown by MBE in our laboratory 7M7.

After his former Ph.D. student Roberto Merlin had joined Miles Klein at the University of Illinois to study light scattering from phonons in GaAs/AlAs

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_31

superlattice samples fabricated at Bell Laboratories (our competitors), I discussed several times with Manuel about the chances to use also our samples for Raman scattering studies in his labs. First, he was not really enthusiastic, but then his interest changed first gradually and in 1984 dramatically after A.K. Sood from India had joined him as a very active postdoctoral fellow. In a short time A.K. Sood got many interesting new results from our GaAs/AlAs superlattice samples culminating in two papers on different subjects in the same issue of Physical Review Letters in 1985. From then on, Manuel was continously following our progress to fabricate new quantum well sample configurations by MBE, and we had numerous exciting discussions in the coffee corner or in his office on the seventh floor.

In September 1991, I left Stuttgart to persue my career first at TH Darmstadt and then from 1992 on in the center of Berlin to establish a new institute emerging from the evaluation of the East German Academy of Sciences in 1991. In setting-up the organization of the Paul Drude Institute, including a Scientific Advisory Board, we thought immediately about Manuel and he fortunately agreed to join this Board from the end of 1992 on for an overall period of six years. Based on his extensive international experience and reputation, his likeable personality, and his distinct scholarship, the members of the institute, in particular those originating from East Germany, soon felt confident with him as a bright, independent and modest advisor, and they were always looking forward to discuss with him. He was always thinking thoroughly, often with scholarly diving into the remote scientific past and into literature others might think irrelevant. Manuel's never ending active service as member of the Advisory Board and beyond has been imperative for the international scientific reputation which the PDI had gained at the end of his term.

Around 2000 Manuel got more and more concerned about the parameters used for evaluating the performance of individual researchers. One day in fall 2001, he called me in my office to draw my attention to a list of "Highly Cited Researchers 2001" in the field of physics, where my name was supposedly listed even ahead of himself. I had no idea about all this, and Manuel sent me an email with a long attachment which Werner Marx from the MPI library had prepared. For that database, articles published in Web of Science-indexed journals were considered, and the selection was based on total citations of the researcher's papers published from 1981 through 1999. In many discussions with Manuel, often in his MPI office, it became clear to me that the original 2001 list gave preference to well-established scientists who had produced many publications. The more papers were generated, the more citations were received, especially if the papers had many years to accumulate citations. This method of selection hence favors senior researchers with extensive publication records. To get a more objective, transparent, and fair rating of the performance of individual researchers for evaluation, Manuel tried several different approaches and parameters in close cooperation with Werner Marx, including total cites, h-index, impact factor, cited half-life, and many more. Manuel's enthusiasm for bibliometrics and citation analysis was boosted after the discovery of scientific misconduct in the work of Jan Hendrik Schön at Bell Labs in 2002. Schön, who almost became a director colleague of Manuel in Stuttgart, had published more than 90 papers in three years in high-impact factor journals with him as lead author, which were then all withdrawn after uncovering the scandal. Having always been suspicious about bibliometric numbers, Manuel continued to focus on the article, which he was always reading carefully, and not on the journal.

In July 2006, just a few months before my own retirement, we organized a special memorial Symposium on behalf of Paul Drude, who had passed away a century before (1906), in the Magnus House of the German Physical Society in Berlin. Manuel gave a very exciting and enthusiastic keynote lecture on Drude's legacy on solid state physics based on his own bibliometric investigations of Drude's publications and many other subjects. All participants enjoyed this wonderful Drude Symposium and the exciting discussions with Manuel.

Whenever I had the chance to visit the Max Planck Institute in Stuttgart-Büsnau after my retirement in Berlin, I tried to pass-by Manuel's office in the seventh floor and stopped to discuss with him about his latest scientific studies and to enjoy talking about his many interests and pursuits. Unfortunately, in spring 2014, when Manuel had been asked to move his office to the sixth floor, I missed him at that location and I could only call him at home. He sounded a bit disappointed, but fortunately not as much as I had expected. Instead, he sounded enthusiastic and optimistic with respect to his wife Inge for whom he cared with great dedication and love.

With all those who have known Manuel, I will remember his deep and passionate dedication to science, his lucid mind, his eagerness to communicate his ideas, and his profound humanity.

Memories from Manuel Cardona from Spanish Speaking Countries

Aldo H. Romero, Jorge Serrano and Alfonso Muñoz

Along our life path sometimes we find people we admire because of something they do. Fewer times we get inspired by someone who displays an attitude, a way of being that makes us wanting to resemble that person. Manuel was one of these rare cases, a person excelling both as a scientist and as a human being. Always holding an enthusiastic and encouraging attitude, Manuel served as a leading trainer and scientific advisor of more than 80 graduate students, influencing with his artful questions and inquiries and innate curiosity to many more scientists, some of which currently hold professor positions worldwide and influence as well many students with their way of lecturing and being.

Enumerating all of Manuel's virtues that inspired us is a challenging adventure. From the large list of attributes that can be used to describe Manuel, we would like to emphasize those that we consider key for an outstanding scientist and that he has demonstrated throughout his life: his **intellectual honesty**, his **patience and perseverance** and, of course, his **openness** to different ideas and approaches to physics.

His education and scientific strengths came from experimental physics. However, he was one of the first experimental scientists to recognize the implications of using theoretical and computational techniques to shed light into the understanding of materials properties, a fact that is clearly displayed as we walk through his book *Fundamentals of Semiconductors*, co-authored by Peter Yu. This book, an icon in solid-state physics, develops the different subjects from both theoretical and experimental side.

One of the most interesting features from Manuel was his amazing memory. For the works we did on many different compounds between 2003 and 2013, he always recalled easily publications titles, summaries, and scientist names of the references involved in the characterization and synthesis, even after 20–30 years after publication. When asked about how he managed to keep such an outstanding record, he reported to always use a systematic way to connect people with publications in his memory, through his personal experiences with each of them or something close that he was refreshing all the time. Even though he did published more than 1400 papers and collaborated with scientists of more than 45 countries, he never stopped thinking about new problems and projects. His desire to explore as a scientist was

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_32

always open for new endeavors. Even lately, after his official retirement from the Max Planck Institute (MPI), he was using his efforts to understand materials from the theoretical side through collaborations that grew with many different groups around the world and keeping his strong links with experimental resources.

In 1999, one of us (JS) had just joined MPI Stuttgart to start his Ph.D. studies on Raman spectroscopy under Manuel's supervision. Being this the first time living abroad and away from his parents place, he certainly had a strong attachment to his family. He remembers asking Manuel for permission to visit his parents in Spain in the occasion of every public holiday: Easter, Christmas, summer break… Manuel was always supportive and positive about such inquiries. Family was a key value we associate with Manuel, who, together with his wife, Inge, managed to recreate a family-like environment for most of the scientists and staff that worked in *Abteilung Cardona* as we called the department run under his lead at MPI. JS remembers once in his early days at MPI asking him for holidays *to have some rest*. Manuel's answer was *Holidays! What do you want to rest from?* raising both arms with hands full open in a voice that JS interpreted with a mixture of anger and surprise. At that time he felt awkward and disappointed. However, with time he learnt to discover how passionate Manuel was about science and research. A passion that led him to devote hours and hours every day until well beyond his seventies, and to publish such an outstanding collection of articles and conference papers. To Manuel, it wasn't working what he was doing, he was enjoying as a child who is playing. This passion let him inspire and engage all of us in fruitful conversations, both about science and life, and paved the way for us to enroll in our own journey: discovering our passion and sharing it with our own students.

In 1998, there was a celebration in Cartagena, Colombia, during the Latin American symposium on solid state, SLAFES, a conference where many different scientists came over to deliver talks and have some time just to talk to him. He always felt confortable in each Latin American country he visited, in many occasions, with his closest friend and life partner, Inge. The conference papers were edited in Physica Status Solidi B, where he had the detail of getting a map drawn by pirates while they were attacking the city in the sixteen hundreds, during the time of the Spaniards. Such map was specially found by him in one of his visits to Paris and kept it in particular for that occasion. Similarly, in 2007, we organized a conference on vibrational spectroscopy in Queretaro, Mexico, where he came and deliver a beautiful description on how Raman spectroscopy has contributed to the development of materials science. He was talking about one of his dearest materials, diamond, and its particular thermal properties. This was also a place where he also demonstrated one of his passions, developing a comprehensive method to measure the quality of a scientist, the famous *h*-index. As he went around all jewelry stores in Queretaro, he was always discussing with the owners about minerals and quality of the stones. His erudition on those topics really surprised even the most knowledged people.

In 2010, we organized another conference in Queretaro, Mexico, this time to honor his 75th birthday. At that point we desired to contact many of his collaborators, many of those we did not know or at best know from references or conferences. It was enough for them to know that it was going to be a celebration for

Manuel, and many came without hesitation, from USA, Europe and Latin America. It was always easy to gather people around his name. He took that time very personally and he demonstrated the love he had for Mexico and his music, he was always demonstrating his knowledge about the Mexican culture, knowing details that even a national would not know. At the same time, Mexico returned his admiration by awarding him Doctor Honoris Causa degree from CINVESTAV in Mexico City. Interestingly enough, the number of students participating in such conference triplicated the number of senior participants. A picture remembering those times is shown in Fig. 1.

His erudition about Latin American history, literature and poetry was astonishing. His storytelling epic between his friends, where he was very well known for remembering details of places where he was or people he met, even in different environment than the scientific.

On the other hand, Manuel made many visits to the University of La Laguna located in Tenerife, Canary Islands. The first one was in 1991. Manuel visited our laboratory and gave some seminars in a condensed matter course. The students were impressed by his talks. His comments, recommendations and suggestions left a deep imprint on the audience, especially because of his humility, his intellectual honesty, patience and ability to arouse curiosity and interest of the audience. Several of the students were stimulated and became interested in the field and then worked on it in different universities. As one of us (AM) was returning to

Fig. 1 Celebration of Manuel Cardona's 75th birthday in Queretaro, Mexico, 2010

University of La Laguna in 1989 after a research stay with Prof. Richard Martin, we sat down and discuss some of the research on semiconductor heterojunctions, surfaces, and high pressure physics from ab initio methods. We remember his patience, his interest, his suggestions and how deep was his knowledge in all those fields. We also had lunchtime at home, where we discovered the encyclopedic knowledge of Manuel in many fields. He knew many historical and cultural facts about the Canary Islands, not only the importance of the islands on the Columbus's first trip to the discovery of America, or the defeat of Admiral Nelson in Tenerife in 1797. We had discussions on non-technical subjects, mainly talking on history and books. He revealed to us some writes from Argentine or Mexico and for long time we did exchange of literature books.

Manuel and Inge visited many times the University of La Laguna and the Canary Islands, and every time he did demonstrate his profound love for his mother land, Spain. In 2006 Manuel received a Honoris Causa doctorate promoted by the School of Physics of the University of La Laguna (Fig. 2). During his talk Manuel displayed the development of the experimental sciences in Spain during the XX century. He stressed the importance in Spanish physics, before the Spanish civil war, of the Canary scientist Blas Cabrera, and the important role of Cabrera's family in Physics. His son Nicolas Cabrera was director of the Physics Department of Virginia University, and his grandson Blas Cabrera is presently Full Professor at Stanford University. Manuel was also involved in many seminars. He connected very well with the students and AM remember specially a talk stimulating the participation of the students at the end, making a simple experiment using an

Fig. 2 Manuel Cardona's Honoris Causa in University of La Laguna 2006

artificial diamond disc to slice an ice bar to show the high thermal conductivity of diamond.

Science was always present in any talk with him; even when having a coffee at the hotel during his visits, he started to talk about the importance of the spin orbit interaction, the electron-phonon interaction, etc. in some problems. His profound knowledge of semiconductor physics allowed him to make many interesting suggestions to us. He also knew very deeply the ab initio theory and its limitations, but he transferred to us the interest for the problem while sharing his intuition on how to go deeply inside and make an attempt to understand it.

In the previous paragraphs we shared several moments narrating experiences we had with Manuel, with the hope that they let you glimpse the light of his wonderful human being. To us, Manuel's life with Inge managed to make real Pierre Curie's quote

> *"Il faut faire de la vie un rêve et faire d'un rêve une réalite".*

Manuel was and will be forever an inspiring example for us.

VUV Radiation Physics and Gaudeamus Igitur, Tokyo 1971

Ulrich Rössler

In almost 5 decades, since my postdoc time at Brown University, I have compiled a rich treasure of memories connected with Manuel. I have decided to report here on an early one, which in some sense is unique: it is the only event, in which I joined Manuel Cardona in some kind of Karaoke.

In 1969/70 with a Guggenheim Fellowship, Manuel spent a sabbatical at the synchrotron radiation fascility of DESY in Hamburg. The use of Synchrotron radiation as a light source for optical investigations was in its infancy with the small laboratory at the first 6 GeV accelerator in Hamburg being one of the first places worldwide. Over the decades the parasitic use of this light source evolved into a powerfull experimental technique with dedicated sources like the HASYLAB (Hamburg Synchrotron Radiation Laboratory), BESSY (Berliner Elektronen-Speicherring Gesellschaft für Synchrotronstrahlung) and the ESRF (European Synchrotron Radiation Fascility in Grenoble). Manuel Cardona was well aware of this upcoming spectroscopic tool in the VUV and X-ray range for optical and photoemission studies. His investigations were on excitations from core states of semiconductors and insulators. Towards the end of this sabbatical in Hamburg he received the offer to become one of the first directors of the newly founded Max-Planck-Institut für Festkörperforschung in Stuttgart. Over the years he had continuous contacts and collaborations with the Hamburg group. As a member of the Scientific Council of DESY Hamburg from 1975 to 1978 he contributed a lot to the expanding activities of the synchrotron radiation fascilities.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_33

The worldwide community of users of synchrotron radiation started to organize topical international conferences. The 3rd International Conference on Vacuum Ultraviolet Radiation Physics took place from August 30 to September 2, 1971 in Tokyo. Naturally, Manuel was one of the invited speakers and presented results of his experimental work with excitations from core states in semiconductors. Due to the almost dispersionless bands formed from these states, the excitation into the conduction bands was used to map the density of the final states. The picture shows him in his typical gesture acting in front of his attentive audience. At this time, laser pointers were not yet available.

The late summer weather in Tokyo was very warm and humid. At the end of the conference day the participants felt the desire to recover from the scientific program with some refreshing drinks. A group of people, mainly from Germany and in particular from the Hamburg DESY group (including Manuel) stayed together and were looking out for a suitable place. Close to the conference hotel, we found the inviting sign of a pub, which we decided to enter. The interior was decorated with the intention to simulate the atmosphere of a Bavarian Bierkeller. A small band provided the corresponding live music. Shortly after we had settled and ordered the first beers, the japanese visitors identified us as (mainly) Germans and encouraged us politely to sing some typical German songs. After a short discussion and consideration of a possible program, we entered the stage (without any rehearsal) and presented some popular songs. Our performance, enthusiastically supported by Manuel, culminated in the traditional “Gaudeamus igitur” of the academic youth, which we intoned as good as we could. As it turned out, our presentation sounded authentic and was well accepted. Under the applause of the Japanese audience we returned to our beers.

Manuel Cardona: Mentor, Friend, Teacher

John E. (Jack) Rowe

I first met Manuel in September, 1965, at a faculty-student social event hosted for Brown Physics entering graduate students. I had just traveled from South Georgia with my new wife, Susie; we had a few disappointments with our new apartment in Providence. The party was at some faculty member's home perhaps Manuel and Inge's. I had heard about Manuel from Ferd Williams whom I met at Oak Ridge National Labs the previous month and had decided after that meeting to try to join Manuel's group as an entering graduate student at Brown University. My entering class of 46 students was by far the largest in the history of the department and we were the first to occupy the newly opened Barus-Holley building. This was at the time the tallest building on campus and contained engineering as well as physics making for easy collaborative research projects. Getting back to the party Cardona asked me what type of drink I wanted. I answered with "rum and coke"; then Cardona quickly replied "Cuba-Libre", a more international name. We discussed a number of things including segregation in the American south and the popularity of College football. I was surprised at how much Cardona knew of so many diverse topics.

In those days, Cardona had a group about 14 of both experimental and theoretical students that included the small group of Assistant Professor Fred Pollak. Cardona had just hired a theory student, Jim Barbour, and he suggested that I join Fred Pollak's group which focused on experimental problems of uniaxial stress and modifications of their optical properties. I did this and soon was starting to take data on CdS crystals with stress applied along the c-axis. This changed the energy bands into those of cubic CdS and I presented my first results at the International Conference on II-VI Semiconductors held during the summer at Brown in 1966. Also during that summer Cardona announced that a brilliant theorist was joining our extended research group, David Aspnes from the University of Illinois at Urbana-Champaign. Aspnes had worked out a closed form solution to the electroabsorption coefficient that impacted the studies of our group using electrolyte-electric-field modulation of the near-normal incidence reflectance. Much to my surprise this turned out to be the young man in jeans with motorcycle helmet under his arm. He was only 2 years older than I was. Cardona called this theory the Franz-Keldysh-Aspnes theory although the named did not become commonly used.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_34

It was clear from the labeling of optical critical point structures in modulated reflectance spectra that energy bands and group theory were intimately related and in the fall of 1968 Cardona taught a course in group theory. He used only notes and no specific textbook. The course was scheduled for two 75 min lectures per week on Tuesday and Thursdays at 4 p.m. I clearly remember the first lecture; it was a classic Cardona performance. He used as an example the E_0 optical transition of compound semiconductors like Gallium Arsenide. This is a p-like to s-like transition and Cardona drew a picture of a p_z orbital on the blackboard. In his passionate way he explained that after application of pressure along the z-axis the shape of the wave function and its energy would be modified. Then he went on the explain the Fermi Golden rule that the three *p*-to-*s* optical transitions would no longer be degenerate but split into two transitions with intensity ratio of one to two. This took some time nearly the whole 75 min and then Cardona asked if there were any questions. A shy student in the back of the room raised his hand and said, "Professor Cardona, I understand all that you have said for the past 70 min. But what will I learn from this course." Quickly Cardona said, "If you already knew group theory I could have explained it to you in less than 5 min instead of 70 min". A related story concerns the collaboration that Manuel had with Eli Burstein on Raman scattering. Manuel was very excited about this and he explained to Fred Pollak and a few of us students that it was just like the space group of energy bands along the delta axis of the bulk Brillouin zone. I never bothered to learn the details of this explanation, but Cardona went on to make light scattering a major part of his research career. I clearly remember still my last visit to Stuttgart and the MPI. I had the pleasure of dinner with Manuel and Inge. Inge's voice was the same melodic quality that she had always had; it was almost operatic in quality. This was about 2003, and I have not traveled there since due to physical limitations. However, I will always treasure the many memories I have of this trip and others to see Cardona. He was a great teacher, mentor and friend.

Manuel Cardona: My Memories

Jesús Sánchez Izquierdo

Many details of my recollections fade away as time passes, but in the essential, the remaining traces of anecdotes, of experiences, of small and great stories of a whole life, remain vividly alive.

I shared with Manuel not only aspects of a professional life (without implying similarity to his intellectual capability), but also aspects of the more private, family life. So I could tell you some unknown anecdotes that even while being insignificant, may serve to illustrate his character and attitude to life.

My story begins a long time ago, some 65 years back (tempus fugit). In September 1950, at the University of Barcelona. It was the first lecture of the course, a lecture in mathematics, which was attended by students of different disciplines: architects, physicists, mathematicians… I counted the number of students: it was only 21! Groups were spontaneously formed by academic disciplines, and so I met Manuel Cardona. It was a group of seven people who had chosen “the academic degree of the future”: Physics Manuel Cardona, José M. Canosa, Francisco Guiu, Francisco Huertas, Enrique Salesa and myself. Among them, Cardona, Canosa, Guiu and I formed the group that completed their physics degree in 1955. We then kept a close professional and friendly relationship, for the rest of our lives.

Very soon, in the lectures, Manuel’s special capabilities became aparent; his questions often put the teachers in serious trouble. In particular, I remember a professor of Mathematical analysis, young and without much teaching experience, who impelled himself to say in an aside: “Mr. Cardona, I will appreciate if, when you have doubts or questions, you come to discuss them at my office and thus the continuity of the class will not be disturbed”. However, Manuel was not always able to keep silent, and continued asking troublesome questions in the lectures.

At due time he went to the compulsory University Militias, I think to the signals brigade. He did not follow the expected golden rule of the militia: not to ask anything and remain as unnoticed as possible. Because of his wide knowledge, he was commissioned to unpack and start-up some equipment stored in drawers, which no one had dared to touch before. And of course he set it all up, but the problem came when he ended his period in the militia and wanted, of course, to leave the army. The army officers tried unsuccessfully to retain Manuel, as he had made himself indispensable, but he managed to escape the militia. The equipment was left

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_35

to itself to gather rust and rot away. But surely, one day or other they could be set into operation by some professionals in the army.

His relationship with the RCA laboratories was interesting. Manuel was hired by the laboratory of semiconductors that RCA had created in the USA in Princeton. In order to be able to apply for US citizenship it was necessary for him to leave the country. This affected the research program at RCA; thus, to avoid losing Manuel, he was hired at the RCA lab in Zurich. At that time I came to Zurich to work on Univac. I was able, with my family, to enjoy Cardona's generosity and hospitality. This must have happened in 1961.

We met again in USA in 1966, when Manuel was a Professor at the Brown University in Rhode Island. I worked at the Brookhaven National Laboratory (BNL), in Long Island. We met very often, without any excuse. Once Manuel accompanied me to see Goudsmit, who was also at the BNL. I did not meet Goudsmit to discuss deep issues associated with the spin of the electron. The reason was somewhat more prosaic: negotiating the purchase of a slide projector that he sold, and which I still have. Manuel helped me in the negotiation to lower the price to $1, which was not easy.

I could continue telling similar anecdotes, but the most recent ones concerning his activity as Director of the Max Planck Institute in Stuttgart are better known by many of his collaborators. However, I cannot fail to mention that Manuel failed in the entrance examination at the School of Industrial Engineering.! (Me too). As it is a well-known recognition, I do not extend on his Prince of Asturias Award. But I had the honor of being invited by Manuel at the reception ceremony.

I finish these lines with a very endearing anecdote. It is a familial, emotive and singular anecdote. Manolo and Inge had scheduled a visit to Barcelona in spring 1968. They traveled in mid-April. After landing, a flight attendant helped them in the arrival arrangements, as Inge in addition to their own 3 kids had traveled with a baby of about 3 months and was carrying him in her arms. Their family was waiting for them and the reception included, apart from the usual hugs and kisses, expressions of large astonishment and joy. *This is the surprise that you had prepared us! Why did you not say anything! What a lovely baby, it's entirely like Manuel!* Meanwhile a lady in her fifties timidly approached the group and broke the joy of everyone by saying: *"I think that you are Cardona. I am Montse's mother and I come to pick up my grandson, who is in your wife's arms. Thank you very much from bringing him"*. The rest was an explanatory conversation to undo the confusion created by a three-month-old baby, (my son Juan José) who had traveled to Spain in Inge's arms as a great favor to us. I explain it in two lines: I had worked during 3 years in the BNL in Long Island in USA, and after many doubts we returned to Spain a month later. We would return by ship, with five children, and the entire accumulated household. The situation was too much for us, and the Cardonas helped us, with the transport of the smallest of our children to Spain.

Acknowledgement Thank you, Manuel and Inge for giving us your friendship and support. Rest in peace, Manolo, you deserve it.

Reminiscences of Stuttgart

Paulo V. Santos

On May 4, 1983 I received a letter from Manuel Cardona supporting my scholarship for a research stay in his institute in Stuttgart—the sequence of events triggered by this letter has probably shaped my life more than any other document. I was at that time a Ph.D. student and wanted to do my thesis work abroad. I wrote a letter to him with a recommendation from my advisor. Manuel's reply came a couple of weeks later: it was a short (one page long) letter where he supported my application and briefly sketched a research plan, which turned, with slight modifications, into my Ph.D. thesis a few years later.

The suggestions from Manuel were implemented and led to interesting results a few months after I arrived in Stuttgart, in part due to the continuous motivation and support from his group (in particular from M. Hundhausen and L. Ley). As a young Ph.D. student, I was impressed with the openness and vitality with which research was carried on. There were many visitors from different parts of the world, reflecting the Manuel's wide network of collaborations. The same applied to his Ph.D. students: I estimate that more than a third were from abroad.[1] The fraction of foreign postdocs and visiting scholars was much higher. Manuel's hands were continuously propelling this machinery: he would frequently send to us copies of recent reports and developments, often with suggestions for new studies. Also, all of us remember the discussions in his office, which could go for hours despite his busy schedule. In most of the cases, Manuel would first look at the experimental results (e.g., a spectrum) and see things we had overlooked. He would then sketch a simple physical picture and indicate how to extract quantitative information from a more elaborate model. At the end, we would leave the office with several hand-written notes with new ideas for subsequent studies. He was very patient during these discussion and would repeat his arguments whenever he felt that we were not following them.

The scientific activities were complemented by many "social events": the daily coffee break after lunch, parties, and excursions. Many involved families—here, Inge was always behind the organization. These events were exceptional occasions to generate and strengthen personal contacts: I remember, for instance, a discussion

[1]Manuel advised approximately 80 Ph.D. students, 9 while at Brown University and 72 in Stuttgart (I thank S. Birtel from the Max-Planck-Institut, Stuttgart for providing this information).

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_36

between Elias Burstein and my wife about the different types of apples in Germany, during one of his stays in Stuttgart! I guess that the other very many guests who passed through Stuttgart have shared similar experiences.

In the following, I would like to share some of the (more technical) experiences from my time as a post doc a few years later (in 1992). A topic of interest was then spatial dispersion effects in the optical properties of semiconductors, which was then part of the Ph.D. work of Pablo Etchegoin. The dielectric function of a cubic semiconductor is normally described by a diagonal tensor $\boldsymbol{\varepsilon}(\hbar\omega)$ with a single, energy dependent number, the dielectric permittivity. This is a very good approximation since the wavelength of visible light (λ_L) is much larger than the characteristic atomic dimensions (i.e., size of the crystal unit cell)—under this conditions, $\boldsymbol{\varepsilon}(\hbar\omega)$ is essentially a local property. In general, however, the dielectric function has a small contribution associated with the variation of the light electric field over dimensions compared to the unit cell. These non-local contributions (the spatial dispersion) can be taken into account by expressing the dielectric function as a tensor expansion in a power series of the light wave vector $\mathbf{q}$: $\hbar\varepsilon_{ij}(\hbar\omega, q) = \varepsilon_{ij}(\hbar\omega) + \varepsilon_{ijk}(\hbar\omega)q_k + \varepsilon_{ijkl}(\hbar\omega)q_k q_l$. (I remember having seen Manuel write this expression many times!). Spatial dispersion effects are normally very weak (changes on the other of 10^{-6}). They become important if they reduce the crystal's symmetry, leading to novel properties and providing, at the same time, a deep insight into the material's band structure. Probably for this reason, they had been attracting Manuel's attention since very long. One example is the linear birefringence induced by the second order term in q, which was detected in GaAs by Yu and Cardona back in 1971 [1]. The linear term in q is responsible for optical activity (or circular birefringence), e.g., difference between the refractive indices for right and left circular polarization. Optical activity is forbidden in III-V semiconductors: it can, however, be induced by uniaxial pressure, as demonstrated in GaAs by the nice work reported by Pablo in [2]. Pablo used to tell an interesting anecdote from the times he was performing experiments: Manuel was excited about the results and would come to the lab from time to time. Pablo used to tell that once, while he was aligning the setup, Manuel, who was learning over the setup, saw an intermittent signal, which he thought to be the signal they were looking for.[2] Pablo, however, soon realized that this was simply Manuel's bolo tie bouncing across the light beam!

Motivated by Manuel, we extended the studies on optical anisotropy to low-dimensional semiconductor structures like quantum wells and surfaces. One of the motivations was to probe the impact of quantum size effects on the optical properties. As in the case of strain, the reduction in symmetry induced by size effects mixes conduction and, specially, valence band states and also leads to

---

[2]Pablo, if still among us, would tell this story with much more vivid details. Unfortunately, he passed away 2 years ago after a long disease. Manuel (together with Eric le Ru) wrote a moving memoir about him (which can be found in http://www.royalsociety.org.nz/organisation/academy/fellowship/obituaries/pablo-gabriel-etchegoin/).

optical birefringence. A second one was to probe the optical properties of surfaces modified by reconstruction and adsorbates. Most of the activities were carried out between 1994 and 1998 with the participation of post docs and students (among them N. Esser, P. Etchegoin, A. Fainstein, J. Groenen, M. Kelly, B. Koopmans, M. Kuball, L. Lastras-Martínez, B. Richards, D. Rönnow, A.A. Sirenko from the experimental side and the theoretical support from A. Cantarero, L.C. Lew Yan Voon, and M. Willartzen) and resulted in more than 20 publications involving Manuel. Different interesting results were obtained from these studies. One of them was optical anisotropy studies for light propagation along the plane of GaAs/AlAs superlattices, which were carried out using microscopic reflection from the cleaved edge of a superlattice structure (as in [3]) or by optical transmission in a waveguide geometry [4]. The transmission experiments were also performed with the sample under uniaxial stress [5]. In those days, the now commonly used high resolution positioning techniques (using, for instance, piezoelectric elements) were just finding their ways to optical spectroscopy labs. The measurements were, therefore, not only very time consuming (since the sample could easily break) but also required refined experimental skills. They clearly revealed the different symmetries of the electron-heavy hole and electron-light hole transitions and yielded detailed data for the energy and pressured dependence for the refractive index anisotropy. We also addressed the optical anisotropy of III-V quantum wells and superlattices for light propagation along the growth axis. (see, e.g., [6] for an overview).

The optical anisotropy reported above for superlattices is associated with linear birefringence (i.e., with the terms quadratic in q). Quantum confinement can also induce optical activity: the experimental conditions required to discriminate circular from linear birefringence, which were crucial for the detection of optical activity in strained samples, could not be realized experimentally. During one of the numerous discussions with Manuel, he pointed out that the spin-orbit interaction (which is not essential to account for the effects mentioned in the previous paragraph) adds new electronic bands with odd symmetry with respect to $\boldsymbol{q}$. In bulk materials, the conduction band spin splitting $\Delta E_s(\mathbf{q})$ is cubic in q with contributions from the three components q_x, q_y, and q_z. In an well-known report, Manuel (with N. Christensen and G. Fasol, [7]) introduced a $\mathbf{k{\cdot}p}$ model illustrating the physical origin of $\Delta E_s(\mathbf{q})$ together with microscopic calculations. One of the open questions at the beginning of the 90s, the early years of what turned to be semiconductor spintronics, was how to extend the bulk results for the spin splittings to quantum wells, where the translation symmetry is lost along one of the q-directions (which we will assume to be the z direction). Manuel suggested to use some of the band structure calculations tools developed by us to address this issue. We soon wrote a report confirming that the splitting in QWs can be well approximated by replacing $q_z^2 \rightarrow \left(\frac{\pi}{d}\right)^2$ in the bulk expression, where d is the width of the quantum well. [8] The resulting expression is now widely used to determine the Dresselhaus (or intrinsic) contribution to the spin-orbit interaction in quantum well structures. I am particularly grateful to Manuel for this insight which became an important part of my work on spintronics several years later.

When I think of Manuel, the first image that comes to my mind shows him sitting at his desk surrounded by piles of papers and pencil-marked Physical Review volumes. Manuel was devoted to Physics: he was active on boards, organization of symposia and meetings, and a frequent guest in different kinds of events. Also, he always kept an open eye and was prepared to react whenever he detected wrong developments in the field. Manuel's range of interests, however, was much broader. He always had an interesting story to tell, and had always given suggestions about books and authors (I remember some of our discussions about Vargas Llosa) and, when going somewhere, about museums and places to visit. On these conversations, I was always surprised to experience how much Manuel (and also Inge) cared about and how much they knew about his collaborators. It remains the hope that we, within our limited capabilities, can transmit to the future generation some of the wonderful things we learned from him.

References

1. P.Y. Yu, M. Cardona, Solid State Commun. **9**, 1421 (1971)
2. P. Etchegoin, M. Cardona, Solid State Commun. **82**, 655 (1992)
3. B. Koopmans, B. Richards, P. Santos, K. Eberl, M. Cardona, Appl. Phys. Lett. **69**, 782 (1996)
4. A. Fainstein et al., Phys. Rev. B **50**, 11850 (1994)
5. P. Etchegoin et al., Phys. Rev. B **53**, 13662 (1996)
6. B. Koopmans, B. Koopmans, P. Santos, P. Santos, M. Cardona, physica status solidi (a) **170**, 307 (1998)
7. M. Cardona, N.E. Christensen, G. Fasol, Phys. Rev. B **38**, 1806 (1988)
8. P.V. Santos, M. Cardona, Phys. Rev. Lett. **72**, 432 (1994)

An Unbiased Mentor Who Offered Freedom and Equality to Young Scientists: Prof. Manuel Cardona

Shigemasa Suga

Dear Manuel, are you watching us from the heaven now?

We are fine and enjoying either scientific life and/or hobby life on the surface of our small planet called the earth.

After 38 years of my stay in MPI-FKF, Stuttgart, I visited the institute and the central part of the city and clearly remembered the time in early 1970s. But we have lost a great scientist, Manuel Cardona.

I remember the time when we met each other for the first time on the third floor of an old building of MPI-FKF in Heilbronner Strasse of Stuttgart one day in August of 1973. Since then I have been strongly impressed by your life with high scientific activity till the end of your life. You are now sleeping quietly in a beautiful cemetery in Vaihingen, Stuttgart but your spirit is followed by many scientists in the next generation.

Before finishing my Ph.D. work at The University of Tokyo (abbreviated hereafter UT), I was offered a permanent job in UT. But I had already decided to go abroad before taking a permanent job in Japan, because it is nice to see foreign cultures when one is still in 20s. This choice may have been promoted by my mentor, Prof. Takao Koda in UT, who came back from USA after a sabbatical year just after (not before) I joined his laboratory and also by my uncle who was a cosmic ray professor-physicist at UT, who stayed in MIT, USA for few years and in Bolivia also for few years for experiments. I thought that global view must be required to contribute to the physics on our planet.

My Master and Ph.D. works were on excitons, excitonic molecules, excition-polaritons, and magneto-optics of semiconductors. So I sent application letters to USA and Germany before getting the Dr. degree. I got two responses: one from a German Univ. and the other from MPI-FKF, Stuttgart. The latter was not from Prof. Quiesser to whom I sent an application letter but from Prof. Manuel Cardona. I was surprised, why? Since I was engaged in Faraday rotation and magnetic circular dichroism studies of excitons in my Master work, I knew already a lot on modulation spectroscopy, which was initiated by Manuel a few years ago in USA. I asked Profs. Yutaka Toyozawa and Takao Koda for recommendation letters to Prof. Cardona.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_37

I decided to accept the offer from Prof. Cardona for 2 years. I knew that I am the second Japanese physicist to join MPI-FKF. The application was made in late autumn in 1972 and the final acceptance from MPI was informed to me in early spring of 1973. My start in MPI-FKF was scheduled from August or September 1973.

I got a Ph.D. degree in March 1973, after the presentation and defense of my Dr. thesis for one hour in front of Profs. Takao Koda, Eiichi Hanamura, and Shoji Tanaka. Thereafter I had enough time to brush up my skills in English conversation and some time to learn German. A few years before, stimulated by the artificial satellite launched by the Soviet Union, I had taken a Russian language as the second foreign language at the university. During my Dr. studies period, I earned my living money (I was married and had already a child) by translation of Russian technical documents to Japanese. Later during my stay in Germany, I recognized that I should have learnt German in addition to Russian to have a more effective physicist life in Germany.

Since I was so curious to see foreign cultures, I decided to see Soviet Union this time and took a ship from Yokohama harbor to Vladivostok harbor. This cruise took 2 days. Then I took a train from there to Khabarovsk. Usually people took a flight from there to Europe. But I already decided to take a Trans-Siberian Railway through Soviet this time. Among the many foreign passengers, I was the only one who could speak a little Russian and thus a Swedish family asked me several times for help to translate some expressions into Russian. Anyway, after 6 days of travel via Novosibirsk passing along the Lake Baikal, I arrived at Moscow. Then I could visit the Tretyakov Gallery by myself without any communication difficulties. Finally the whole trip from Tokyo to Stuttgart took 10 days. This was possible since I had enough free time before joining MPI-FKF Cardona's group.

Manuel had already mentioned to me by airmail post that the photoemission group was overcrowded and that I should join either the Raman spectroscopy or the magneto-optics group. I knew both approaches since my graduate course. Without hesitation I selected the magneto-optics as my first subject. I was impressed already at that time since Manuel gave me the first freedom to select the subject even before joining his group. He also arranged my pickup from the Stuttgart Hauptbahnhof in the evening of my arrival. (Since the train from Hannover was delayed by 2 h, as it often happens nowadays in the case of the DB in Germany, I could not find any person to guide me to the Institute. However, when I called MPI-FKF by telephone, I could contact one Dr. student (Manfred Bettini), who immediately rushed back again to the Bahnhof, picked me up and brought me to the Institute.) Such arrangements were always nearly perfect in the Prof. Cardona's group. The hospitality of Manuel and Inge has also encouraged many visitors and laboratory members to the international friendship to large extent over decades.

On the next day of my arrival, I was suddenly asked in the laboratory whether I would like to join an overnight measurement scheduled for the next day. The experiment was going to be made under a strong magnetic field, up to 13T, with a superconducting magnet cooled down by liquid He. This kind of strong magnetic field was not easily available in Japan around that time except for some special experiment. So I said "Yes" immediately. In fact, I had so far experienced overnight experiments quite often at the Institute for Solid State Physics, UT by using a N_2 laser to study excitonic molecules in CuCl, CuBr and so on, after the staff went back home at night. I was glad to feel the personal trust of Manuel and the members of the experiment team in the potential of a new comer, like me, just by judging my personal character and the achievements made in my Ph.D. work so far. This was the time when I reasserted that mutual trust is very important in a fruitful collaboration work.

The experiments in MPI-FKF were on magneto-luminescence of CdTe and other semiconductors. Dr. Dreybrodt was a sub-leader and Mr. Manfred Betini, and I joined from the Cardona's group and Mr. Fritjof Willmann and Mr. Peter Hiesinger and later Dr. Venghaus joined from the Queisser's group. From time to time, foreign visitors joined also the experiments and theoretical analyses. By any means, all members collaborated happily and could write nice papers over few years of my stay between 1973–1976. However, you may wonder why Manuel's name is not seen on those papers. Since this period was the starting period of the institute, all directors must have been very busy with administrative task and might have had not enough time to spend for individual scientific discussions. Such attitude of providing complete freedom to younger scientists must have stimulated many young scientists in the institute in this period.

Around the time when I published several papers on magneto-optics, Manuel asked me to go to DESY and to measure photoemission spectra of a certain semiconductor together with Mr. Franz Himpsel. Up to that time I had no experience on synchrotron radiation spectroscopy and photoelectron spectroscopy. Again he trusted in my potential. Looking back to this time, I feel that this was a turning point of my scientific life, since now I am spending more than 80 % of my scientific time in photoelectron spectroscopy. I understand how important it is to offer new chances to younger scientists. Manuel demonstrated me the secret how to activate younger scientists to higher level.

One more impact was given to me by Manuel almost 1 year later when he came to my office in Heilbronner strasse on Saturday (as usual) and asked me to review a paper submitted to Physical Review. Up to that time I submitted several papers to PR, but I had never experienced a referee's job. He, of course, was responsible as a referee but he must have thought that one can become a much better science-papers writer if one has experience as a referee. I made my best for 3 days for making all possible comments to that paper. I believe that Manuel must have later improved my English comments in the referee's report. After this experience I have always enjoyed to referee papers and provide comments to positively improve their content. On the other hand, I have also benefited learning a lot on the related physics.

In this way, my stay in Manuel's lab in my 20s for 2.5 years was very fruitful in my scientific life owing to the Manuel's hospitality. After this period, I stayed in DESY for 3 months at my own expenses and joined a group with photoemission activity. I collaborated with Dr. Zimmerer and Mr. Brodtman in this stay. I also became acquainted with Mr. Volker Saile during this Samurai Training in 1976 (as I learned in my teens from the novel of Musashi Miyamoto, a famous Samurai in Edo Era in Japan, the Samurai Training is the best way to learn anything new).

I am glad that I could write a Springer book on Photoelectron Spectroscopy: Bulk and Surface Electronic Structures in 2014, which may contribute to the community of photoelectron spectroscopy as the famous Manuel's book in Springer together with Prof. L. Ley published a few decades ago.

Accordingly to the fruitful experience of my stay in Germany, I later sent several active students and assistant professors to Germany from UT and Osaka Univ. to MPI-Halle, Augsburg, Köln and Würzburg Universities, BESSY, etc. from my laboratory. In addition I brought a few young scientists to BESSY II to do measurements of soft X-ray absorption magnetic circular dichroism and hard X-ray photoemission. So far, I also brought young students and postdoctorals to the Taiwanese and European synchrotron radiation facilities, as well as to the Swiss Light Source. Joint collaborating researches overnight, with communication in English between Japanese and German team members during experiments, promotes mutual understanding, which may contribute to some extent to international peace in the future.

From the precious experience of my stay at MPI-FKF, I also hosted many German students and postdoctorals, so far at Osaka Univ. and at the Japanese synchrotron radiation facility, SPring-8. I could also organize German-Japanese collaboration experiments at SPring-8, at the synchrotron light source UVSOR and Osaka Univ. Of course, I have also invited so far many physicists from USA, Korea, Switzerland, Italy, Russia, China and Taiwan. I hope I could have contributed to the global peace to some extent as Manuel did to a large extent through his life. My philosophy is to open all knowhow to all human beings to promote science most effectively with saving redundant expenses and time-losses. Such a philosophy might have been matured by my integrated experience to work in various synchrotron facilities all over the world on our planet.

Finally I would like to add two photographs to keep our memories on Manuel. One taken in 1983 in Kamakura in front of a great statue of Buddha when Manuel visited Japan to attend an international conference held in Tokyo. The other in 1984 when I and my wife Sachiha invited Manuel and Inge to my house in Koma-Musashidai, located 50 km west of Tokyo. We were very happy to see their continuous smiles and curiosity to experience different cultures.

Life is eternal according to the concept of Samsara in Buddhism. Just we are walking around on the palm of Buddha.

Recollections of the Cardonas in Japan and in Stuttgart

Katuhisa Suzuki

My first encounter with the name of Manuel Cardona was with his paper [1] dealing with the band structure of semiconductors with diamond and zincblende structures. The theory was simple but was applicable to many of the semiconductors known at that time and could explain many of their electronic properties.

My first personal acquaintance came in 1966 when the Eighth Conference on the Physics of Semiconductors was held in Kyoto, Japan. I vividly remember Manuel sang a Spanish song in the large hall of the Conference banquet. On this occasion I readily got an impression that Manuel is an exceptionally talented scientist but also a person of warmth, frankness, cheerfulness, and sincerity. That impression remained true until the last days of his life.

I spent a year in 1980–1981 in Stuttgart MPI-FKF as a Gastwissenschaftler in Abteilung Cardona. This was an extremely memorable experience for me and my family. Every weekday after lunch people gathered for coffee at the *carrefour* of the laboratory and exchanged conversations on whatever topics in whatever languages. Manuel was often at the center, speaking fast and abundantly. He was a linguistic genius, switching from German to Spanish, to French, to Russian, to English in a second, depending on who the partner is. I enjoyed conversations with such eminent scientists as Hans Queisser, Heinz Bilz, Peter Fulde, Alexei Maradudin, and Robert White as well as with young Mitarbeiter (colleagues) who had come from all over the world, including the Spaniard Luis Viña, one of the editors of this book.

On weekends Manuel and his wife Inge often invited my family to their home for dinner. They also took us to interesting places around Stuttgart, such as a visit to a nearby vineyard and Rottweiler Fasching Umzug (carnival parade). Inge's role was immensely important at the Institute. She took care of many visitors and participated in most nonprofessional activities such as Weihnachtsfest and Abschiedsparties. The atmosphere and life of the Cardona group and the Institute as a whole were like those of a single family, reminiscent of the groups around Niels Bohr [2], Enrico Fermi [3], Rudolf Peierls [4] and Walter Kohn [5].

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_38

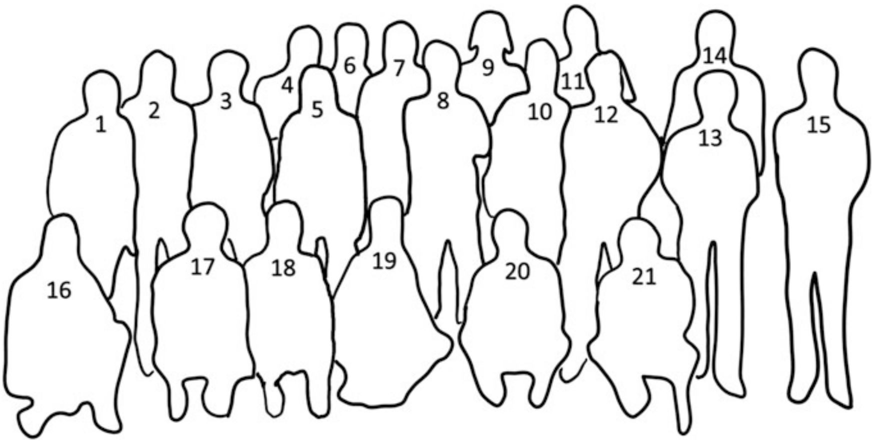
In 1983 I invited Manuel for a short trip to Japan with a fund from Yamada Science Foundation. The trip was in conjunction with his attendance at the International Conference on Amorphous and Liquid Semiconductors held in Tokyo. For 1 week after the Conference he visited the Institute for Solid State Physics and Synchrotron Laboratory in Tokyo, Tohoku University in Sendai and finally came to Osaka University, where I was based then. In all of the destinations he was warmly accepted by his old Japanese friends and delivered lectures. On the eve of his departure from Osaka, on September 1, 1983, a frightening incident arose. A Korean Airlines 747 from New York to Seoul was attacked by a Soviet Air Force missile and shot down into the ocean near the Japan-Soviet border, killing all of the 269 passengers and crew on board. Manuel had booked a seat for September 2 on that airline. In those days flights to Europe were via Anchorage, namely the same route as that to New York. Manuel was watching the news on television in my home until late at night but refused to change the flight on the next day. We were heartily relieved to be informed of his safe arrival in Stuttgart.

In 1984 I invited Manuel and Inge for an extended trip (about one month) to Japan with a fund from Japan Society for the Promotion of Science. This time the Cardonas traveled extensively from Sapporo in the north to Fukuoka in the south. Manuel delivered 16 formal lectures of various types, on physics at highly technical level to tutorial, on German national and industrial research systems and so on. He made intensive technical discussions with Japanese colleagues; Kikuo Cho, Chihiro Hamaguchi and myself in Osaka; Shigemasa Suga, Noboru Miura and Yutaka Toyozawa in Tokyo; Toshihiro Arai and Seinosuke Onari in Tsukuba; Mareo Ishigame and Yasuo Oka in Sendai; Masataka Hirose in Hiroshima; Kenji Kumasaki in Sapporo; Tatsuo Shimizu in Kanazawa; Masatoshi Nakayama in Fukuoka, etc. Many of these local hosts had an experience of working at MPI-FKF. In addition to academic institutions, he was keenly interested in Japanese industrial laboratories in comparison to their counterparts in U.S.A. and Germany. He visited Hitachi and NEC Laboratories and delivered lectures. The Cardonas had a deep interest in Japanese history, tradition and culture. They visited historic sites in Kyoto, Nara, and Hiraizumi, some of which are 1300 years old. They were particularly impressed at the exhibitions of old wooden farm houses in Kanazawa and in Toyonaka. What delighted them most was the invitation of their local hosts to their homes for dinner, which provided them with rare opportunity of seeing private life of Japanese common citizens.

On the occasion of the 1994 International Conference on Semiconductors in Vancouver numerous worldwide friends of Manuel gathered in a Chinese restaurant to celebrate his 60th birthday. It is awfully saddening for all of them that his 80th birthday can no longer be celebrated. Manuel Cardona was undoubtedly one of the greatest scientists of our time. Besides he was a strong advocate of civil rights and social justice. I heard that a young Catalonian Manuel left Spain for U.S.A. in protest to the despotic rule of Francisco Franco. In later years I repeatedly heard Manuel's word of praise for Mikhail Gorbachev, whose Perestroika eventually led to disintegration of the Soviet Union.

We immensely miss the great man.

Members of Abteilung Cardona in October 1980. Sitting, Cardona, far right, Suzuki, second from left. Standing in front row, Viña, third from right



21 Cardona, *20* Nebenführer, *19* Weissenrider, *18* Calleja, *17* Suzuki, *16* Krutina, *15* Stolz, *14* Gruntz, *13* Olego, *12* Viña, *11* Ley, *10* Richter, *9* Johnson, *8* Ves, *7* Vogt, *6* Feng, *5* Mattausch, *4* Reichard, *3* Hirtz, *2* Simmons, *1* Griep

The Cardonas in Horyuji Temple, Nara, in October 1984

References

1. M. Cardona, J. Phys. Chem. Solids **24**, 1543 (1963)
2. R.E. Moor, *Niels Bohr, the Man, His Science and the World They Changed* (Knopf, New York, 1966)
3. L. Fermi, *Atoms in the Family* (The University of Chicago Press, Chicago, 1954)
4. R. Peierls, *Bird of Passage* (Princeton University Press, Princeton, 1985)
5. M. Scheffler, P. Weinberger (eds.), *Walter Kohn, on the Occasion of his 80th Birthday* (Springer, Berlin, 2003)

Manuel Cardona and the Story of Highly Enriched ^{28}Si

Mike L.W. Thewalt

Among his many contributions to science, Manuel Cardona was the leading figure in elucidating the wide variety of effects which the stable isotope composition could have on the properties of semiconductors and insulators—the “host isotope effect”. Some of the most surprising and potentially useful of these effects are related to the unique properties of highly isotopically enriched ^{28}Si discovered during these investigations. I will here review my almost 15 years long collaboration with Manuel, with emphasis on his many essential contributions and insights.

Unlike many of the contributors to this volume, I was never a student, protégé or colleague of Manuel’s. While I was of course very well aware of his many deep contributions to our understanding of the optical properties of semiconductors, and condensed matter physics in general, we did not know each other personally until our long and fruitful collaboration on the properties of highly enriched ^{28}Si began in the year 2000. In this article I will review the history of this collaboration, with emphasis on the many seminal contributions made by Manuel, and give some indication of the richness of this field and its future directions. In doing so I will cover many rather technical subjects and results, but at a descriptive level which I hope will be of interest to the non-expert reader.

Among his many contributions to physics, Manuel Cardona can rightly be named the ‘father’ of the field of host isotope effects in semiconductors and insulators—the study of how varying the distribution of stable isotopes in a material can affect its optical, thermal, mechanical and electrical properties [1–3]. It is the optical properties we are primarily concerned with here, and these are related to the dependence of the band gap energy on isotopic composition, an effect which Manuel showed to be intimately connected with the dependence of the band gap energy on temperature, a subject which maintained his interest over many years, from at least 1985 [4] to 2014 [5]. It is interesting to note that the study of isotope effects on the optical spectra of Si lagged such studies in other semiconductors [1, 2], even though Si is ubiquitous in modern electronics, and the purity and perfection of crystals of Si having the natural isotopic composition significantly exceeds those of other semiconductors. There is one exception to this ‘late start’ which has almost been forgotten—in the 1950s, two of the earliest papers on donor electron spin resonance in semiconductors made reference to a Si sample enriched to 99.88 % of ^{28}Si, and reported improved resonance linewidths and coherence

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_39

times compared to natural Si (^{nat}Si) [6, 7]. I can find no later reference to this sample, which produced results that presage by four decades the recent resurgence of interest in spins in ^{28}Si, driven by the promise of quantum information [8]. The study of host isotope effects received a major boost from the end of the cold war, at which time large stockpiles of enriched stable isotopes became commercially available [9]. Specifically, much of the recent progress in producing highly enriched and highly chemically pure ^{28}Si resulted from the Avogadro project, which featured isotope enrichment and chemical purification in Russia followed by further purification and single crystal growth in Germany [10].

Our collaboration on ^{28}Si began in 2000, when I read an article [11] by Tobias Ruf, Manuel and others on the thermal conductivity of enriched ^{28}Si (Manuel had also been studying phonon effects in isotopic Si using Raman scattering [12]). I was not particularly interested in thermal conductivity per se, but rather in the excellent properties of the ^{28}Si used in the study, together with the fact that no one had yet studied enriched Si using photoluminescence, the technique which my lab had been applying to natural Si and other semiconductors for many years. I therefore sent Manuel an email proposing such studies, and collaborative publication should anything interesting result. Shortly thereafter I received our first samples of enriched ^{28}Si, and specifically the high purity floating-zone grown sample Si283, enriched to 99.896 % ^{28}Si.

I should begin by explaining that the semiconductor spectroscopy carried out in my lab was in some sense the opposite of the types of studies which Manuel had perfected. We studied emission and absorption transitions of localized excitations known as bound excitons, which are inherently very spectrally narrow since no kinetic energy is involved in the initial and final states. Manuel studied transitions involving free carriers, which are inherently spectrally broad, and developed many sophisticated modulation techniques to extract the maximum possible amount of information about the sample band structure from these inherently broad transitions.

Our very first study [13] of the photoluminescence of the phosphorus donor bound exciton and the boron acceptor bound exciton (all Si contains observable amounts of these two impurities) resulted in a major surprise, which has driven developments in ^{28}Si spectroscopy ever since. As shown in Fig. 1, we could very accurately determine the difference in band gap energy between ^{nat}Si and ^{28}Si from the shifts in the bound exciton energies, since these closely follow the band gap energy. As Manuel had already shown for other materials, this isotope shift can be predicted from the temperature dependence of the band gap energy. What was surprising was that all of the luminescence lines were much sharper in ^{28}Si than in ^{nat}Si (it must be emphasized that the ^{nat}Si spectra are reproduced in all 'good' samples—they are never narrower than what is shown in Fig. 1). It was therefore immediately apparent that what must be limiting the linewidths in ^{nat}Si is inhomogeneous isotope broadening, resulting from the isotope shift of the band gap energy and the statistical fluctuations of isotopic composition within the radii of individual bound excitons.

In hindsight this may seem to be an obvious potential broadening mechanism, but it has not been observed in other materials, and before this demonstration, was

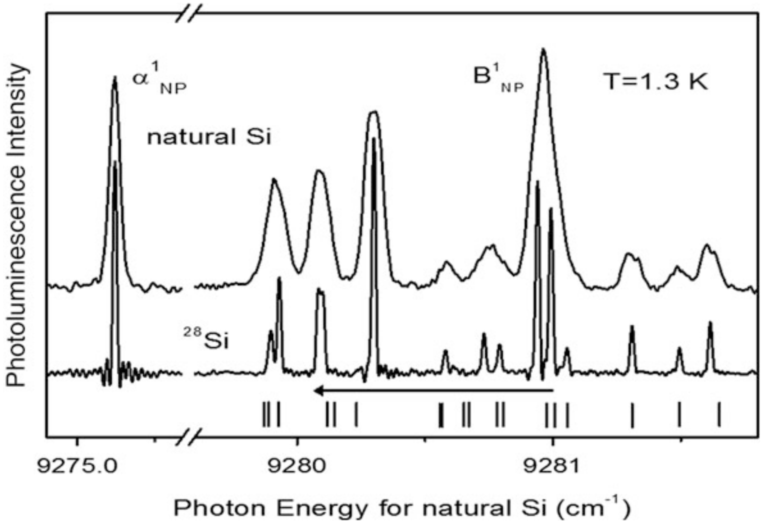


Fig. 1 A comparison of the photoluminescence spectra of the no-phonon transitions of the phosphorus donor bound exciton ($\propto^1_{NP}$) and the boron acceptor bound exciton (B^1_{NP}) in high quality natural Si (*top*) and enriched ^{28}Si (*bottom*). The ^{28}Si spectra have been shifted up in energy by 0.92 cm^{-1} (the length of the *horizontal arrow*) to account for the difference in band gap energies. It is immediately apparent that these transitions in natural Si are broadened by an effect which is absent in ^{28}Si, namely inhomogeneous isotope broadening. The ringing seen around the $\propto^1_{NP}$ line in ^{28}Si results from it being narrower than the ultimate instrumental resolution of the Fourier transform spectrometer. The *vertical lines* at *bottom* are the predicted locations of $\mathbf{B}^1_{NP}$ fine structure components, which cannot be resolved in ^{nat}Si, based on piezospectroscopic studies in ^{nat}Si. From [13]

dismissed by many as being too small to be observable. In fact, it is remarkable that so little attention was paid to the possible origins of the bound exciton linewidths in high quality ^{nat}Si, other than to mechanisms which made them broader, such as strains from dislocations or from electrically inactive impurities such as carbon and oxygen [14]. There is in fact one speculation in the literature that host isotope effects might account for the linewidth of luminescence transitions, but this was limited to a discussion of one particular radiation damage center [14]. The results of this first collaboration with Manuel [13] also set us a spectroscopic challenge, in that the linewidths in ^{28}Si were too narrow to resolve with the best available luminescence spectrometer. They also, as we will see, led to many new discoveries, proving once again that when spectral resolution is improved by orders of magnitude thanks to improvements in either in the sample or the instruments, many things which were previously hidden will be revealed.

The next surprise from ^{28}Si came from the photoluminescence of bound excitons associated with the acceptors Al, Ga and In. There was a decades-old problem in understanding acceptors in Si known as the ‘intrinsic’ acceptor ground splitting. It was known that the ground state of all these acceptors had a small splitting, which was the same for a given acceptor in all 'good' samples. This splitting had been studied and confirmed by a wide variety of techniques, but never explained. My group had earlier shown that we could observe the same splitting in the luminescence of bound excitons associated with these acceptors [15], so when ^{28}Si became available we thought that we could use it to obtain a cleaner look at the ground state splitting [16]. Instead, the splitting vanished in ^{28}Si, as shown in Fig. 2! The inescapable conclusion was that the 'intrinsic' acceptor ground state splitting in Si arose from isotopic inhomogeneity.

Again, in hindsight this may appear to be an obvious answer, since random strain (or other) fields can lift the acceptor ground state degeneracy, but there was no prior suggestion in the literature that isotopic inhomogeneity could be the cause—it was assumed to be too small an effect. There is however a related, much larger, effect which had been investigated earlier [17], the lifting of the acceptor ground state degeneracy in semiconductor alloys due to chemical randomness. Manuel was very gratified that we were able to explain the size of this effect in ^{nat}Si with a rather simple calculation, with no adjustable parameters, that gave a very good estimate of the size of the acceptor ground state broadening and splitting due to isotopic inhomogeneity [18].

Another set of relatively sharp transitions in semiconductors are the mid-infrared absorption transitions from the ground states of shallow donor and acceptor

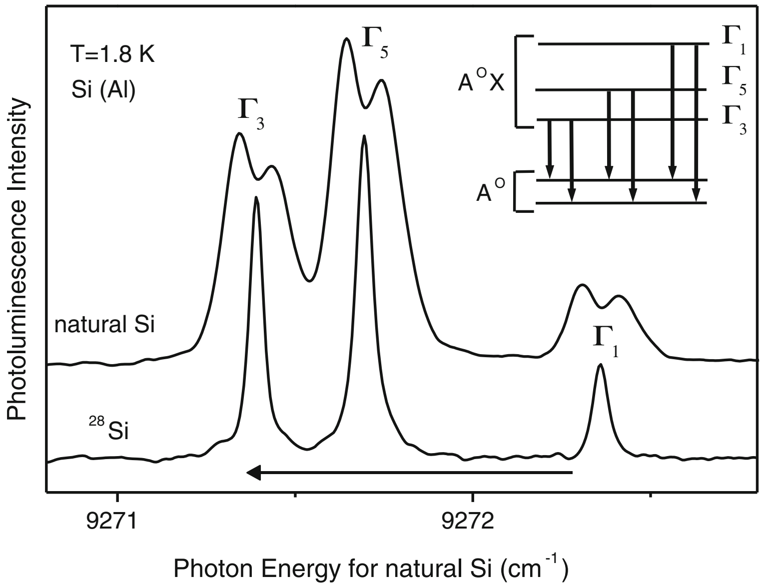


Fig. 2 A comparison of the no-phonon photoluminescence of the aluminum acceptor bound exciton (A^0X) transition in natural Si (*top*) and ^{28}Si (*bottom*). The *inset* shows the origin of the observed structure: a triplet splitting of A^0X due to electron valley-orbit states, and a doublet splitting of the A^0 ground state seen only for natural Si. Thus this 'intrinsic' splitting of the acceptor ground state must arise from the isotopic inhomogeneity present in natural Si. From [15]

impurities to their electronic excited states. These had been widely studied in ^{nat}Si for many decades, and their very reproducible linewidths were widely assumed to be lifetime-limited. Once again, our first look at these transitions in ^{28}Si produced some major surprises [19]. In this and later [20] studies we found that many (but not all) of these donor and acceptor transitions were far narrower in ^{28}Si than ever observed in ^{nat}Si, showing once again that inhomogeneous isotope broadening dominates the linewidth in ^{nat}Si. An example of these results for the acceptor boron is shown in Fig. 3, which further shows how the inhomogeneous isotope broadening of the boron ground state calculated during the acceptor bound exciton work [18] could accurately account for the inhomogeneous broadening of these quite different transitions. This collaboration was later extended to include the electronic absorption transitions of much deeper levels, again with surprising, and potentially useful, results [21].

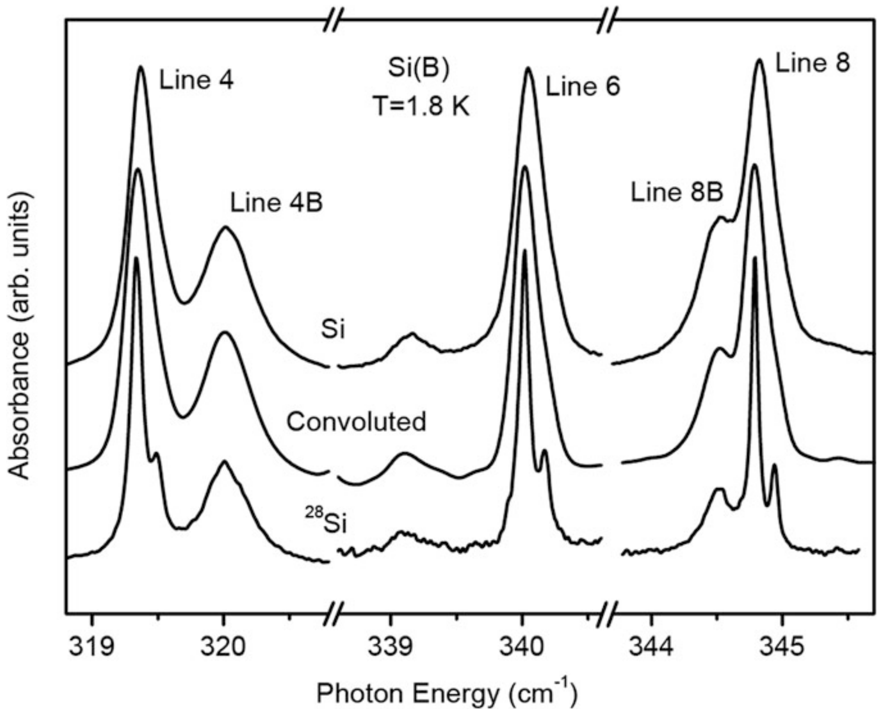


Fig. 3 Some typical mid-infrared absorption lines resulting from electronic transitions between the ground state and odd-parity excited states of the acceptor boron are compared in ^{nat}Si (*top*) and ^{28}Si (*bottom*). While some lines, such as 4B, have identical width in the two samples and must be dominated by lifetime broadening, others such as lines 4, 6 and 8 become much narrower in the enriched ^{28}Si, revealing a doublet splitting which results in the different ionization energies of ^{10}B and ^{11}B. The central spectrum shows the observed ^{28}Si spectrum convolved with the theoretical isotope broadening of the boron ground state in natural Si as determined form the acceptor bound exciton studies, which accurately matches the observed spectra for ^{nat}Si. From [30]

Related to these studies of the linewidths of these mid-infrared transitions was an investigation of the host isotope dependence of the donor and acceptor binding energies in Si [22]. Here Manuel's encyclopedic knowledge and recall of condensed matter physics was essential in providing a simple explanation of the very different sizes of this effect for donors and acceptors [22].

Another unrelated aspect of ^{28}Si spectroscopy, which again caused great surprise for the impacted research community, but which can only be covered here superficially, was the discovery that the photoluminescence of many 'deep' bound exciton transitions associated with transition metal impurities became much sharper in ^{28}Si than in ^{nat}Si [23]. This allowed the typically unresolved isotope shifts due to changes in the isotopic composition of the binding center to be resolved into 'isotopic fingerprints', producing a different line for every combination of the binding center isotopes, and revealing not only which isotopes were involved but also how many of each isotope the binding center contained. As a result, essentially all of these much-studied centers were revealed to have a different composition than had been assumed in the literature, in some cases with no overlap between the assumed and actual chemical constituents [24].

The final chapter of our collaboration with Manuel on the spectroscopy of ^{28}Si began after we solved the spectroscopic challenge of measuring the actual bound exciton linewidths in ^{28}Si by absorption, using tunable single frequency lasers, rather than in emission using spectrometers. This allowed essentially infinite instrumental resolution, so the true experimental lineshapes could be observed. The earliest and for Manuel most satisfying result came about from my casual remark to him that the bound exciton lines were now so sharp that we could see them shifting in energy with varying temperature below 4.2 K. Manuel immediately became very excited, and with very little hesitation said "I predict that in this low temperature limit the lines should shift as T^4". This was based on an argument very similar to the Debye T^3 heat capacity law, with the addition of the fact that at low energies the leading term in the strength of the electron-phonon interaction must be linear in energy. We set about to measure this as accurately as possible, and after considerable effort verified the T^4 behavior [25], as shown in Fig. 4. I believe that this was for Manuel one of the most gratifying results of the ^{28}Si story, since he had for many years been concerned with the temperature dependence of the band gap energies, and found the various phenomenological fits unsatisfying. Here was a fundamental new result, which should be true of all electronic transition energies in the limit of low temperature.

The second new direction which resulted from solving the spectral resolution problem had to do with resolving the hyperfine splitting between the donor electron spin and the donor nuclear spin in the spectrum of the phosphorus donor bound exciton transition. We had been working on this for a while, and come to the conclusion that we were seeing this small splitting in our spectra—a splitting which had been thought impossible to see in a semiconductor due its small size compared to linewdiths in solid state materials. While we were sorting this out Manuel sent an email saying "You should look at Phys. Rev. B **69**, 125306 (2004)" [26]. This paper proposed, based on our earlier results, that both the electron and nuclear spins

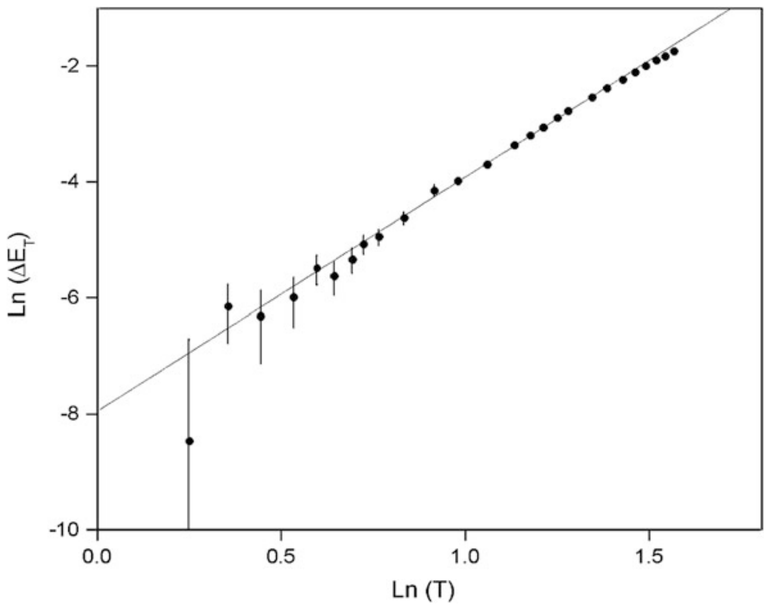


Fig. 4 Shows the natural log of the shift of the observed band gap from the extrapolated energy at $T = 0$ versus the natural log of the sample temperature. The *solid line* has slope 4, showing that below 4.2 K the band gap energy decreases with increasing temperature as T^4. From [25]

of a single phosphorus donor might be read out using the donor bound exciton transition, making us aware of the possible applications of our results to the new field of semiconductor-based quantum information. This resulted in our first paper demonstrating how the unique optical properties of ^{28}Si allowed for the optical readout of these spin systems, providing a new and unexpected 'handle' on these systems which is now being widely applied. For our own group, the most notable results have been the demonstration of what are by far record coherence times for the nuclear spins of neutral [28] and ionized [29] donors in ^{28}Si. These unique optical properties of ^{28}Si, and their application to silicon-based quantum information research, guarantee that the story of ^{28}Si has yet more chapters to be written.

Finally, I am deeply indebted to Manuel for convincing me to coauthor with him a review of host isotope effects, which has become the central reference for this field [30]. I was at first hesitant, since I typically have difficulty in finishing large writing projects, but I need not have worried. Manuel organized everything, and wrote by far the lion's share of this already highly cited (136 on WoK) review. This is in addition to having gotten us started on research into ^{28}Si, which has occupied my group for almost 15 years, and for his too numerous to count contributions and insights along the way. We will all miss him.

References

1. M. Cardona, P. Etchegoin, H.D. Fuchs, P. Molinàs-Mata, Effects of isotopic disorder and mass on the electronic and vibronic properties of three-, two- and one-dimensional solids. J. Phys.: Condens. Matter **5**, A61 (1993)
2. M. Cardona, Isotopic effects in the phonon and electron dispersion relations of crystals. Phys. Stat. Sol. (B) **220**, 5 (2000)
3. M. Cardona, T. Ruf, Phonon self-energies in semiconductors: anharmonic and isotopic contributions. Solid State Commun. **117**, 201 (2001)
4. P. Lautenschlager, P.B. Allen, M. Cardona, Temperature dependence of band-gaps in Si and Ge. Phys. Rev. B **31**, 2163 (1985)
5. M. Cardona, R.K. Kremer, Temperature dependence of the electronic gaps of semiconductors. Thin Solid Films **571**, 680 (2014)
6. G. Feher, J.P. Gordon, E. Buehler, E.A. Gere, C.D. Thurmond, Spontaneous emission of radiation from an electron spin system. Phys. Rev. **109**, 221 (1958)
7. J.P. Gordon, K.D. Bowers, Microwave spin echoes from donor electrons in silicon. Phys. Rev. Lett. **1**, 368 (1958)
8. F.A. Zwanenburg, A.S. Dzurak, A. Morello, M.Y. Simmons, L.C.L. Hollenberg, G. Klimeck, S. Rogge, S.N. Coppersmith, M.A. Eriksson, Silicon quantum electronics. Rev. Mod. Phys. **85**, 961 (2013)
9. E.E. Haller, Isotopically engineered semiconductors. J. Appl. Phys. **77**, 2857 (1995)
10. B. Andreas, Y. Azuma, G. Bartl, P. Becker, H. Bettin, M. Borys, I. Busch, M. Gray, P. Fuchs, K. Fujii, H. Fujimoto, E. Kessler, M. Krumrey, U. Kuetgens, N. Kuramoto, G. Mana, P. Manson, E. Massa, S. Mizushima, A. Nicolaus, A. Picard, A. Pramann, O. Rienitz, D. Schiel, S. Valkiers, A. Waseda, Determination of the Avogadro constant by counting the atoms in a ^{28}Si crystal. Phys. Rev. Lett. **106**, 030801 (2011)
11. T. Ruf, R.W. Henn, M. Asen-Palmer, E. Gmelin, M. Cardona, H.-J. Pohl, G.G. Devyatych, P.G. Sennikov, Thermal Conductivity of Isotopically enriched silicon. Solid State Commun. **115**, 243 (2000) [erratum: **127**, 257 (2003)]
12. F. Widullea, T. Ruf, M. Konumaa, I. Siliera, M. Cardonaa, W. Kriegseisb, V.I. Ozhogin, Isotope effects in elemental semiconductors: a Raman study of silicon. Solid State Commun. **118**, 1 (2001)
13. D. Karaiskaj, M.L.W. Thewalt, T. Ruf, M. Cardona, H.-J. Pohl, G.G. Devyatych, P.G. Sennikov, H. Riemann, Photoluminescence of isotopically purified silicon: how sharp are bound exciton transitions? Phys. Rev. Lett. **86**, 6010 (2001)
14. G. Davies, The optical properties of luminescence centers in silicon. Phys. Rep. **176**, 83 (1989)
15. V.A. Karasyuk, M.L.W. Thewalt, S. An, E.C. Lightowlers, Intrinsic splitting of the acceptor ground state in silicon. Phys. Rev. Lett. **73**, 2340 (1994)
16. D. Karaiskaj, M.L.W. Thewalt, T. Ruf, M. Cardona, M. Konuma, "Intrinsic" acceptor ground state splitting in Silicon: an isotopic effect. Phys. Rev. Lett. **89**, 016401 (2002)
17. C. Benoit à la Guillaume, Splitting of the acceptor ground state in zincblende semiconductor alloys. Solid State Commun. **48**, 513 (1983)
18. D. Karaiskaj, G. Kirczenow, M.L.W. Thewalt, R. Buczko, M. Cardona, Origin of the residual acceptor ground-state splitting in silicon. Phys. Rev. Lett. **90**, 016404 (2003)
19. D. Karaiskaj, J.A.H. Stotz, T. Meyer, M.L.W. Thewalt, M. Cardona, Impurity absorption spectroscopy in ^{28}Si: The importance of inhomogeneous isotope broadening. Phys. Rev. Lett. **90**, 186402 (2003)
20. M. Steger, A. Yang, D. Karaiskaj, M.L.W. Thewalt, E.E. Haller, J.W. Ager III, M. Cardona, H. Riemann, N.V. Abrosimov, A.V. Gusev, A.D. Bulanov, A.K. Kaliteevskii, O.N. Godisov, P. Becker, H.-J. Pohl, Shallow impurity absorption spectroscopy in isotopically enriched silicon. Phys. Rev. B **79**, 205210 (2009)
21. M. Steger, A. Yang, M.L.W. Thewalt, M. Cardona, H. Riemann, N.V. Abrosimov, M.F. Churbanov, A.V. Gusev, A.D. Bulanov, I.D. Kovalev, A.K. Kaliteevskii,

O.N. Godisov, P. Becker, H.-J. Pohl, E.E. Haller, J.W. Ager III, High-resolution absorption spectroscopy of the deep impurities S and Se in ^{28}Si revealing the ^{77}Se hyperfine splitting. Phys. Rev. B **80**, 115204 (2009)
22. D. Karaiskaj, M.L.W. Thewalt, T. Ruf, M. Cardona, Dependence of the ionization energy of shallow donors and acceptors in silicon on the host isotopic mass. Phys. Rev. B **68**, 121201 (2003)
23. M.LW. Thewalt, M. Steger, A. Yang, N. Stavrias, M. Cardona, H. Riemann, N.V. Abrosimov, M.F. Churbanov, A.V. Gusev, A.D. Bulanov, I.D. Kovalev, A K. Kaliteevskii, O.N. Godisov, P. Becker, H.-J. Pohl, J. W. Ager III, E.E. Haller, Can highly enriched ^{28}Si reveal new things about old defects? Physica B **401**, 593 (2007)
24. M. Steger, A. Yang, T. Sekiguchi, K. Saeedi, M.L.W. Thewalt, M.O. Henry, K. Johnston, H. Riemann, N.V. Abrosimov, M.F. Churbanov, A.V. Gusev, A.K. Kaliteevskii, O.N. Godisov, P. Becker, H.-J. Pohl, Photoluminescence of deep defects involving transition metals in silicon: new insights from highly enriched ^{28}Si. J. Appl. Phys. **110**, 081301 (2011)
25. M. Cardona, T.A. Meyer, M.L.W. Thewalt, Temperature dependence of the energy gap of semiconductors in the low-temperature limit. Phys. Rev. Lett. **92**, 196403 (2004)
26. K.-M.C. Fu, T.D. Ladd, C. Santori, Y. Yamamoto, Optical detection of the spin state of a single nucleus in silicon. Phys. Rev. B **69**, 125306 (2004)
27. A. Yang, M. Steger, D. Karaiskaj, M.L.W. Thewalt, M. Cardona, K.M. Itoh, H. Riemann, N.V. Abrosimov, M.F. Churbanov, A.V. Gusev, A.D. Bulanov, A.K. Kaliteevskii, O.N. Godisov, P. Becker, H.-J. Pohl, J.W. Ager III, E.E. Haller, Optical detection and ionization of donors in specific electronic and nuclear spin states. Phys. Rev. Lett. **97**, 227401 (2006)
28. M. Steger, K. Saeedi, M.L.W. Thewalt, J.J.L. Morton, H. Riemann, N.V. Abrosimov, P. Becker, H.-J. Pohl, Quantum information storage for over 180 s in a ^{28}Si "semiconductor vacuum". Science **336**, 1280 (2012)
29. K. Saeedi, S. Simmons, J.Z. Salvail, P. Dluhy, H. Riemann, N.V. Abrosimov, P. Becker, H.-J. Pohl, J.J.L. Morton, M.L.W. Thewalt, Room-temperature quantum bit storage exceeding 39 minutes using ionized donors in silicon-28. Science **342**, 830 (2013)
30. M. Cardona, M.L.W. Thewalt, Isotope effects on the optical spectra of semiconductors. Rev. Mod. Phys. **77**, 1173 (2005)

Interactions

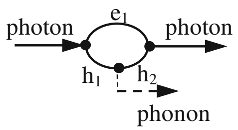
Bernard Weinstein

Physics interactions: As a young Ph.D. student in Manuel's early Stuttgart group trying to understand resonance Raman scattering, it was natural to approach the new results we measured on the one- and two-phonon resonances in GaP through the eye of Feynman Diagrams, freshly learned a year earlier from my courses at Brown University. This was fine as a starting point. But when I sat down with Manuel in his office to discuss the findings, we used a different sort of diagram that clarified many details of the physics. And so, as sketched here, Manuel transformed the familiar bubble diagrams into an approach grounded firmly in the physics of the medium.

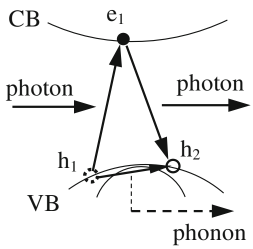
In this picture, the symmetries, wave functions, and state-densities of the interacting electron and phonon states immediately become apparent, enabling easy focus on the selection rules, strength, and dispersion of the strongest resonant scattering terms. The approach also directly connected the resonance Raman profiles to the dispersion of the piezobirefringence coefficient, and related the electron-phonon interaction to the effects of applied strain on the band structure, a subject studied extensively by Manuel earlier using conventional and modulation spectroscopies. The students and post-docs in Manuel's group at this time learned much about the physics of semiconductors from these *medium-based* light-scattering diagrams. From this stance, Manuel taught us to properly anchor our analysis of the interactions to realistic properties within the semiconductor medium.

Human interactions: Some years later, I visited Manuel at the MPIF on the way to the 1986 ICPS meeting in Stockholm. The Institute was sending a large contingent (30–40 people) by rail, and I was traveling by like means, so Manuel suggested I take the same train, which I was happy to do. We boarded separately. The MPIF group climbed en masse into a standard car, while the train conductor directed me to the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_40



Generic Feynman Diagram for one-phonon Raman scattering.



Cardona's *medium-based* view of the same Raman process near the E_0-gap of a semiconductor.

reserved seat paid for by my company. After we were underway, I walked down to the MPIF car. There I found Manuel and Inge crowded into a compartment with students and postdocs. The talk was lively and loud, as refreshments of one strength or another were passed around. It was clear that Manuel and Inge enjoyed being a part of this group. They could easily have traveled in a different manner, but chose to be with these young people in an environment (a *medium* as it were) for *valued human interactions*, enriched all the more by the lens of their collective experience. And amidst it all, Manuel was busily at work transforming matrix elements to verify selection rules for a paper in progress.

Eventually I went back to my quiet compartment, and later on Manuel and Inge visited me there. I cannot recall the conversation exactly, but we spoke for a long time about many things. Probably we reminisced about some of the students and postdocs who worked with Manuel at Brown and at the MPIF during my time in his group, Fernando Cerdeira, Peter Yu, Francisco de La Cruz, Jean and Marianne Renucci, Jeff Lannin, Wolfgang Richter, Nigel Shevchik among *many* others. Nigel had tragically succumbed to cancer not too many years earlier, and Manuel and Inge were sure to have voiced concern for how his wife Ellen and their children were getting along. Perhaps we joked about the month's supply of kosher meat stored by me and my wife Helen in the Institute freezer, which spoiled along with Manuel's gray tin samples when the MPIF cleaning staff mistakenly pulled the plug over the weekend. The question, "Was the food or the samples the greater loss?" surely was again posed, but never finally answered. Or we may have retold the story of the wine-museum tour, when Manuel *loudly proclaimed to all present* the text of a sign that included my family name Weinstein in its description of wine developed to its fullest sweetness. Perhaps we discussed politics, like the actions of Ronald Reagan who had recently ordered the bombing of Libya and was entangled in *Irangate*, or the discrimination against minorities and guest workers in Europe by right wing groups. Manuel's knowledge and perspective of politics and history was exhaustive and compassionate. A few years later, at another ICPS meeting, we walked together on a tour of the old Jewish cemetery in Warsaw, pausing at the statue that honors the educator Dr. Janusz Korczak who refused passage from the Ghetto without his students.

Although all of these human encounters with Manuel are beyond the description of any diagrams, none are needed for his students, colleagues, family and friends to know how significantly our lives have been enriched by our interactions with him.

Manuel Cardona

Félix Yndurain

I met Manuel Cardona in 1970, when he came to give a seminar at The Cavendish Laboratory invited by Volker Heine. I was working for my Ph.D. thesis on semiconductor's surface states under the supervision of Federico García Moliner in Madrid and in collaboration with Volker Heine in Cambridge. I already knew the pioneer work of Manuel on optical properties and band structures of semiconductors. His joint works with Fred H. Pollak on the $\mathbf{k}\cdot\mathbf{p}$ method as well as the calculations of semiconductor's surface states and complex band structures with C. Chaves and Norberto Majlis (interrupted abruptly by the Argentina Coup in 1966) were already a reference for me. I remember perfectly well that seminar, and that special way in which Manuel closed his eyes in order to concentrate, which so many times afterwards I have been a witness of, I remember his clarity, his depth, and the "push" that seminar meant to me for my thesis works. Since then, and especially after my return to Madrid from my Berkeley Postdoctoral in 1976, I had an excellent relationship and friendship with Manuel, in which always, and even today, I feel in debt with him. I also recall my first visit at the Max Planck Institute in Stuttgart; I remember his office full with a thousand papers, books and scientific journals, etc. But what impressed me most of all was that in order to talk physics he grabbed an enormous A3 size page pad and started writing and sharing with me the subjects he was working on and the problems that he thought were most interesting and could interest me. We were there for a long time, and it marvelled me then, how a person like him, could dedicate so much time to me. It was then that I learned the two characteristics I came to admire more in Manuel: his generosity with time and his generosity with his knowledge, which he shared with others in a very natural manner. He always had time (I never knew where he found it) to talk about physics problems and to share his ways of understanding them. I learned very much from him and his deep knowledge on experimental as well as on theoretical aspects always fascinated me. He could talk on perturbation theory and the electron self-energy as well as on the intriguing Raman spectrum that someone had just measured.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_41

I always sought out (and found) his help and advise. Around 1980 we wanted to organize a Festschrift in honour of Nicolás Cabrera and we did not know where to begin. I called Manuel and immediately he launched a campaign to get an excellent Philosophical Magazine volume (Vol. 45, 1982) in honour of Nicolás Cabrera. In another occasion, we wanted to launch a research program with Spanish electrical companies on the new superconductor materials, and with no doubt I took a plane to Stuttgart and Manuel helped me once again to set the basis of a research programme that would involve both the electrical companies and the Spanish Administration. That was Manuel, always generous, at work as well as with his family life and his friendship. I visited him many times in Stuttgart during short periods and also long ones. Him and Inge (Inge and him) were excellent friends and hosts. Dinners at the Cardona's meant talking on a thousand subjects among which Manuel's immensurable knowledge on history, geography, art, ceramics, you name it, and his great sense of humour always surfaced. He unconditionally shared all. I remember a specific dinner at his house with my wife Ana ad our son Jaime, then 10 years old and interested in fossils. Manuel gave as a magnificent lesson on fossils an when we arrived at the hotel, he called us to remind us where in the area were the best museums on the subject, which we then visited, he knew and described in detail the content of them. In that same sense, I recall vividly once we were visiting with him the Colegiata of Santillana del Mar, in the north of Spain, we were a group attending a workshop in Santander, and we had a guide that was explaining the marvellous Romanic collegiate, but after a while we all decided to follow Manuel because he explained the Colegiata much better than the professional guide, and besides, he was much more fun.

As José M. Calleja said, Manuel Cardona is *irrepetible,* unrepeatable. His scientific work and contribution is vast and measureless, and with no doubt, there is a before and after M. Cardona in the development of Condensed Matter Physics. But this is so, not only because of his contributions were monumental, but because of his ability to train and help scientists. In Spain he helped us immensely, very much so. The Spanish scientific community is in great debt with Manuel Cardona.

All I Really Need to Know About Semiconductors, Research and More, I Learned as a Graduate Student of Cardona

Peter Y. Yu

The author Robert Fulghum published in 1988 a book with a rather humorous title: "All I really need to know I learned in Kindergarten". The book became an instant success and was on the New York Times bestsellers list for 2 years. This book became a success because it pointed out a simple and yet true fact: what we learn when we were young will influence us for the rest of our lives. This is definitely also true for my career in physics. What I learn in graduate school determined the course of my career and hence also my life. I was lucky that I learned from a master in my graduate school. Manuel Cardona was more than a thesis advisor to me; he was a mentor who changed my life.

When I first applied to the Physics Department of Brown University as a graduate student in 1966, I was so ignorant of the world that I thought Rhode Island (the State where Brown is located) was a real island like Hong Kong where I grew up. One thing I did know was that I wanted to study semiconductors which had become important technological materials after the discovery of the transistor, the injection laser and the Gunn diode. I was very fortunate in that Manuel was my teacher for the Introductory Solid State Physics course during my first year at Brown. He was a very engaging and lively teacher. In addition, to waving his arms a lot, I remembered him as the only person I have ever met who would close one eye when thinking hard. His approach to physics was a breath of fresh air to me since my undergraduate physics courses in Hong Kong tended to be mathematically rigorous and theoretically oriented. The reason was that during the 1960s Hong Kong has a very limited amount of modern equipment in the Physics Department so it was easier to emphasize theory in the course work. After I have started research in Manuel's group, he offered a special graduate course on Optical Properties of Semiconductors. It was a very useful course for me so I took careful notes of all his lectures. Even today I keep those notes although the binder I used to hold those notes has come apart long time ago. Later on at Berkeley, I decide to develop a course on Semiconductor Physics based on this course Manuel taught at Brown. When I was approached by Springer Verlag to publish my lecture notes, I asked for his advice. After all, I learnt all the materials from him! He was very enthusiastic about the idea and gave me so many excellent suggestions that we ended up co-authoring the book. It turns out to be our longest collaboration. The first edition appeared in 1996 while the fourth edition came out in 2010. It has been translated

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_42

into Japanese, Chinese and Russian. I have to admit that Manuel was more than a co-author. He was also our salesman! Once he was invited to lecture in Mexico and he found out that the book was not available in Mexico. As is typical of Manuel, he would not let a small obstacle like that to stop him. He carried 25 copies of the book in his luggage and sold them all in Mexico!

In the 1960s Hong Kong has only one university modeled after the British system. There was usually only one professor per department (the rest of the staff were either lecturers or senior lecturers) who was also the head of the department. To talk to the professor we had to make an appointment through a secretary. In the US universities, I discovered that the professors were much more approachable. But Manuel was more approachable even by US standard. His office door was always open. Once I was working on a formula involving the spin-orbit interaction and ran into a problem. Failing to solving the problem after many trials, I dropped into his office unannounced and asked for his help. He was probably in the middle of writing a paper at his desk. He stopped his work, listened to my problem and promptly helped me to solve it.

At Brown, Manuel would often stopped by the laboratory and worked side-by-side with his students and postdocs on their experiments. He taught me how to use the evaporator to prepare thin film samples. Once I ran into a problem with the far-infrared optical setup in the lab (at that time nearly all the setup were constructed by the department machine shop unlike the turn-key, computer-controlled commercial equipment nowadays). Manuel went with me over to the Chemistry Department at Brown and used their Cary spectrometer setup to finish the measurement.

In 1969–1970 while I was a second year graduate student at Brown, Manuel received a John S. Guggenheim Foundation Fellowship which allowed him to spend a year at the German Electron Synchrotron (known also as DESY) in Hamburg, Germany. I was fortunate to have a fellowship from Brown in that year so I was able to work with him at DESY for 6 months learning how to perform experiment with synchrotron radiation. During that period I literally worked with him every day. During some of our "graveyard shifts", we would meet at the beam line (nicknamed the "bunker" since it was underground) at night after a whole day of sample preparation for the measurement. By morning my mind would be so tired that it refused to work. But Manuel would keep going until the next shift scientists arrived to kick us out. When we did not have machine time, Manuel would suggest a theory problem for me to work on to keep me busy. This was my first trip to Europe so everything was new and exciting to me. Manuel and Inge treated me like a member of their family. I remember that one weekend they drove me to visit the old Hanseatic League city of Lübeck by the North Sea. Along the way we stopped to visit Inge's parents who were living in Hamburg. I also get to know first hand their three children: Michael, Angela and Stefan. When my wife and I got married at Brown in 1971 the entire Cardona family attended our wedding!

The Cardona Family in front of the Manning Chapel of Brown University on May 22, 1971

Their children have kept in touch with us even after they grew up and left home. I remember that one summer Stefan came to visit us at Berkeley. He carried my son (about 2 years old) on his shoulder while we went sightseeing along the Telegraph Avenue (famous for its street artists) in Berkeley! This past summer Angela and Klaus visited us in California with their daughters. I think everyone who has worked with Manuel has found the entire Cardona family to be most warm and welcoming.

Ten Years with Manuel: 1989–1999

A Short Report About a Long and Rewarding Time in the Department of Manuel at Max-Planck-Institut für Festkörperforschung in Stuttgart

Jörg Zegenhagen

Ten Years with Manuel

I first heard about Manuel Cardona and the *Max-Planck-Institut für Festkörperforschung* (MPI-FKF) in Stuttgart during my Ph.D. work at the Hamburg Synchrotron Radiation Laboratory (HASYLAB) at *Deutsches Elektronen Synchrotron* (DESY) in Hamburg, Germany. In 1980, the lucky Ph.D. and Diploma students working with synchrotron radiation had the pleasure to broom clean the foundations of the new HASYLAB experimental hall under construction. We were rewarded by being given the opportunity to move into brand-new offices in the newly erected HASYLAB office building. On the first floor there was the office of a scientist from Manuel's department, Robert Johnson who was very busy setting up a photoelectron spectroscopy station named Flipper. (Worthy of note, this station was, virtually unchanged, more than 30 years in operation until 2013, when the DORIS storage ring was turned off). In 1984, bestowed with a Ph.D., or correctly speaking a doctor *rerum naturalium* (Dr. *rer. nat.*) as it is called in Germany, I went to the US, where I continued hearing about MPI-FKF and Manuel Cardona and started to realize how famous the man was. I met Manuel in person in 1989, when he was giving a talk on high temperature superconductivity (HTS) at Brookhaven National Laboratory (BNL) in a packed seminar room. It was the high time of HTS and nobody working on this subject at BNL wanted to miss what Manuel Cardona would have to say about HTS and phonons. As a hands-on experimentalist, he was spicing his talk with a little demonstration of the Meißner effect. Wearing thick gloves, he was cooling a piece of $YBa_2Cu_3O_7{-}\delta$ with liquid nitrogen below the transition temperature. Setting it onto a U-shaped magnetic slide, the levitating $YBa_2Cu_3O_7{-}\delta$ piece was practically frictionless. swinging back and forth until it warmed up an came to rest. It was a good method of getting the attention of warming up the audience and getting everybody's attention.

At that time, I had decided to go back to Germany and Robert Johnson had informed me that Manuel was looking for senior staff. He had also dropped my

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_43

name and Manuel wanted to talk to me. When we sat down in the lobby of the National Synchrotron Light Source at BNL, Manuel started our conversation with the remark that good people were so very hard to find. Well, with the help of Robert it hadn't been too difficult for him to find me. Anyway, I decided to take it as a compliment. The job description that Manuel outlined was very attractive, we parted in mutual agreement and I joined his department at MPI-FKF in September.

Manuel had arranged attractive start-up funds, which allowed me to bring the first scanning tunnelling microscope at MPI-FKF into operation, an ultra high vacuum (UHV) machine that has produced numerous nice papers. However, Manuel still "owned" an old X-ray photoelectron spectroscopy system (VG ESCA), dating back almost to his start at MPI-FKF, i.e. the founding of the institute in 1969, to which he obviously had some sentimental affection. Thus, he was keen that I inherited the instrument. It had not been in use for quite a while. With the excellent technical support of Wolfgang Stiepany we managed to get it to work after a few months. Not long after, I received a panic call that water was coming out of the lab.

Corrosion had eaten away the brazing of the water cooled X-ray anode; the tip had broken off, and through the opening water was flooding the UHV chamber, the analyser and the oil diffusion pumps. (In older times, oil diffusion pumps were chosen when high pumping speed without noise and magnetic fields was required.) The mechanical fore-pumps dutifully continued pumping now the oil-water suspension into the exhaust lines at the ceiling from where it rained down into the lab. Cleaning up the lab took days. In a heroic effort, Wolfgang cleaned the whole UHV system, bathing it, including the electron analyser, in trichlor. It took months to get the ESCA system into operation again and to take the first spectrum from gold—when the high voltage supply failed. At this point, even Manuel agreed to retire this beloved and ancient piece of equipment.

We then started a very fruitful project on the growth and characterisation of epitaxial films of high temperature superconductors, fruitful already in view of the generous funds which we could acquire with Manuel's help and good name. With the new equipment we could grow the 90 K superconductor $YBa_2Cu_3O_7{-}\delta$ under ultra-clean conditions, unravel the very early stages of growth and produce films of YBa2Cu3O7$-\delta$ with record values of the critical current [1, 2]. A lasting memory from that time is our visit to Beijing and the Forbidden City (see Fig. 1), where I had the pleasure of being given a private lecture by Manuel on ancient pottery and china/porcelain in general.

Early on, Manuel had started to explore isotope effects in HTS using Raman and IR spectroscopy. These studies provided a wealth of information and the finding that phonons and thus BCS theory could not account for the pairing. As a spin-off of this work with isotopes, Manuel kicked off a strong activity in the investigation of isotope effects in materials [3].

Crystals made of different isotopes of the same material exhibit at low temperature slight differences in lattice constant [4]. Manuel had found a publication with X-ray measurements on the lattice constant difference of germanium isotopes. He had good reasons to doubt the accuracy of the results and suggested to me to repeat the measurements. Studying the corresponding paper in detail, I realized that

Fig. 1 At the Forbidden City in Beijing, China during the conference on materials and mechanisms of high temperature superconductors, February 28 to March 4, 1997. *Left* Manuel, the chairman, and me; *right* Where is the chairman?

with the technique used it was very difficult to minimize experimental errors. It was likewise difficult to obtain the isotopic pure crystals of germanium needed for the experiment.

A guest scientist from Russia, Alexander Kazimirov, had the clever idea to use a new approach for the measurements. A material grows on a crystal of the same material but with a different isotopic composition perfectly lattice matched at sufficiently high temperature, i.e. above the Debye temperature which is 360 K for Ge. However, upon cooling down, the epitaxial layer will strain to adopt its own lattice constant. An X-ray standing wave (XSW), produced by reflection from Bragg planes from the underlying crystal, reaches through the epitaxial film and represents a perfect yardstick [5] (cf. Fig. 2). Using an epitaxial film with N lattice planes enhances the effect N fold, since the change in surface layer position with decreasing temperature equals N times the individual change in lattice plane spacing. The position of the surface layer(s) can be measured by the photoelectrons excited by the maxima of the XSW. Growing a perfect pseudomorphic epitaxial film is more difficult than one might naively expect, but we succeeded and determined the dependence of the lattice constant of Ge and Si on the isotopic composition with very high precision [6, 7]. A later high resolution X-ray backscattering study confirmed these XSW results perfectly [8].

The scientific life in Manuel's department was very busy, with many visitors, papers constantly circulated by Manuel and two seminars every week. Not too long after I started, Manuel surprised me with presenting me with a bottle of wine to the occasion of my birthday. The present was accompanied, however, with the kind request that I should please attend the seminars regularly, what I did from then on. Unforgettable are the annual departmental meetings, coined the Cardona days. Former members were keen on joining and we always had some guests. Later, the meetings were held at Ringberg castle at Tegernsee [9]. Every postdoc, graduate and undergraduate student of Manuel's department had to report about their work. The short talks were typically followed by some questions and at the end

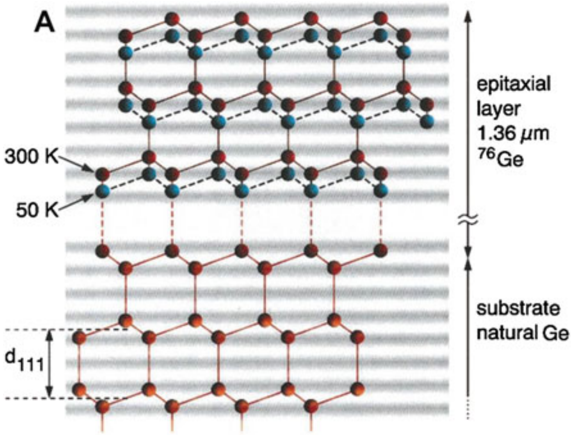


Fig. 2 Schematically: X-ray standing wave and the isotopic effect on the lattice constant

unavoidably by a longer instructive blackboard tutorial by Manuel on the specific subject.

I am grateful for the time I could spend at MPI-FKF in the department of Manuel, since I learned so much (e.g. what polyglot and bibliographical knowledge means). When I left the MPI-FKF in 1999 for the European Synchrotron Radiation Facility (ESRF) in Grenoble, Manuel was very supportive (and continued to be so when needed). He pushed hard that "my" whole lab equipment could be transferred to the ESRF, where it continued to educate Ph.D. students and postdocs and to produce excellent science.

References

1. T. Haage, J. Zegenhagen, H.-U. Habermeier, M. Cardona, Phys. Rev. Lett. **80**, 4225 (1998)
2. T. Haage, J. Zegenhagen, J.Q. Li, H.-U. Habermeier, M. Cardona, Ch. Jooss, R. Warthmann, A. Forkl, H. Kronmüller, Phys. Rev. B **56**, 8404 (1997)
3. M. Cardona, in: *Advances in solid state physics*, vol. 34, ed. by R. Helbig (Vieweg, Braunschweig/Wiesbaden, 1994), p. 35
4. H. London, Z. Physik. Chem. Neue Folge **16**, 302 (1958)
5. J. Zegenhagen and Alexander Y. Kazimirov, in: *The X-ray standing waves technique; principles and applications*. World Scientific (2013)
6. A. Kazimirov, J. Zegenhagen, M. Cardona, Science **282** (1998) 930
7. E. Sozontov, L.X. Cao, A. Kazimirov, V. Kohn, M. Konuma, M. Cardona, and J. Zegenhagen, Phys. Rev. Lett. **86**, 5329 (2001)
8. H.-C. Wille, Yu.V. Shvyd'ko, E. Gerdau, M. Lerche, M. Lucht, H.D. Rüter, and J. Zegenhagen, Phys. Rev. Lett. **89** (2002) 285901.
9. A "medieval" castle built from 1912 to 1973 by the Bavarian Duke Luitpold of Wittelsbach, inherited by the Max-Planck-Gesellschaft in 1973. http://www.schloss-ringberg.de

Interactions of Theory and Experiment in Raman Scattering

Roland Zeyher

I met Manuel Cardona for the first time at New York University (NYU) where I was a postdoc of J.L. Birman. At that time J.L. Birman was interested, besides of other topics, in the microscopic theory of resonant Raman scattering and the problem of additional boundary conditions in polariton theory.

Manuel told me that he will accept an offer for a directorship at the Max-Planck-Institut für Festkörperforschung in Stuttgart. On invitation of H. Bilz I also planned to go to this institute after my postdoctoral time at NYU and thus hoped for interesting discussions and collaboration with Manuel. I did not become disappointed and had over many years fruitful discussions on many different topics in solid state physics, such as, Raman and Brillouin scattering, electronic band stucture, the electron-phonon coupling and high-Tc superconductors. In the following I will describe two projects in inelastic light scattering in more detail, where we closely interacted with each other.

During my postdoc time in New York J.L. Birman got an invitation to write a review article on light scattering. During writing we became aware of an obvious shortcoming of the microscopic, fully quantized theory of light scattering in extended solids. This theory predicted for the intensity of the scattered light an explicit dependence of ω'/ω (ω and ω' are the incident and scattered frequencies of the light, respectively). On the other hand it was known from light scattering from molecules and the semiclassical theory in solids that the cross section should obey a $\omega'^3\ \omega$ law. We could not find a solution to this problem so it went out of our focus. A few years later I told Manuel of this problem and he immediately engaged himself to find a solution. After several unsuccessful trials (at one time, we suspected that the gauge invariance may be violated in the microscopic theory) we found a way to re-express the microscopic expression for the cross section in such a way that the $\omega'^3\omega$ pre-factor appeared explicitly. The key to this was the derivation of a f-sum rule for displaced atomic positions and then to expand the resonant and non-resonant Raman contributions in terms of atomic displacements. As a result the infinite sum over conduction bands was re-summed yielding a fast convergent series plus an explicit $\omega'^3\omega$ pre-factor. With Manuel's help I could convince the

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_44

second author of our SSC letter [1] of the correctness of our explanation. I also was impressed by Manuel doing all the algebra by himself independently from my calculation. At the end we had just to compare the results and were then sure that the calculation was correct.

A deeper and still controversial problem is the so-called additional boundary problem. It arises in polariton theory where the exact eigenmodes (polaritons) of the coupled exciton-photon Hamiltonian are considered. One obtains two dispersive, collective branches. The lower branch increases linearly with momentum well below the exciton, like usual light, and approaches with increasing frequency and momentum the exciton branch from below. The lower polariton branch thus consists mainly of a photon part at small momenta and of an exciton part at large momenta. The second branch starts a zero momentum a little above the exciton energy and approaches towards higher frequencies the straight line of high-frequency photons. For incident light below the exciton only the lower polariton branch can be excited and the boundary conditions from Maxwell's equations are sufficient to determine the amplitudes of the reflected electric field and of the polarization wave inside the medium. If the frequency of the incident light is larger than the exciton energy both polariton branches may be excited because momentum conservation does not hold at the surface. In this case Maxwell's boundary conditions are not sufficient to determine all amplitudes and one needs an additional boundary condition (a.b.c.). The incident light thus creates inside the crystal a coherent state with two different polarization waves and the ratio of the polarization waves is determined by the a.b.c. The determination of the a.b.c. has a long and controversial history and even now the problem is not really solved. The underlying difficulty is that the a.b.c. is determined by the way the exciton is exactly reflected at the boundary. Approaching the boundary the exciton as an extended bound state of a hole and an electron gets deformed in a complicated way and ceases to exist very near the boundary. In Ref. [2] it was argued that resonant Brillouin scattering should show in general four and not one peak in the scattered light because of the presence of two polarization waves and that the measured intensities of the lines could be used to determine experimentally the a.b.c. Unfortunately the light scattering experimentalists in New York and also in Stuttgart were very skeptical whether one could detect these predicted lines because they would sit on a huge background of Rayleigh scattering. Actually, these additional lines in Brillouin scattering were found by chance by Ulbrich and Weisbuch in 1977.

Manuel was first also skeptical whether these lines were observable in real systems. But after 1977 he got very interested in this subject, not so much in the controversial a.b.c. problem but in getting information from such experiments on the band structure, exciton masses and the electron-phonon coupling. He recognized that most of this information can be obtained without knowledge of the a.b.c., just from conservation of energy and, inside of the crystal, of momentum. On the other hand the experimental spectra, for instance in CuBr and CdTe, showed many more lines and were much more complex than in our original proposal. Manuel recognized that a more realistic description of the exciton was necessary to explain the spectra. The Hilbert space of exciton states involves two spin states for the electron times 4 spin states of the hole described by a J = 3/2 spin operator. The exciton Hamiltonian in this 8 dimensional space can be expressed by invariants formed from products of the electron spin operator **σ**, the hole spin operator **J,** and the center-of-mass wave vector **k** of the exciton. The prefactor of these invariants represent exciton parameters. Manuel and his coworkers worked out the excitation spectrum of the exciton along the main symmetry direction, calculated the corresponding polariton dispersion curves and determined the exciton parameters by comparing the theoretical predictions with the experimental data. Looking, for instance, at Figs. 4–6 in [3] or Fig. 6 in [4] one is deeply impressed by the careful and clear analysis of the data, the fantastically good agreement between experiment and theory and the clear-cut conclusions achieved in these papers.

References

1. R. Zeyher, H. Bilz and M. Cardona, Solid State Commun. **19**, 57 (1976)
2. W. Brenig, R. Zeyher, J.L. Birman, Phys. Rev. B **6**, 4617 (1972)
3. D.-P Vu, Y. Oka, M. Cardona, Phys. Rev. B **24**, 765 (1981)
4. J.C. Merle, R. Sooryakumar, M. Cardona, Phys. Rev. B **30**, 3261 (1984)

Manuel Cardona's Contributions to Semiconductor Technology: A View from Industrial and Applied Physics

Stefan Zollner

A speaker at a conference last year said that Manuel Cardona "knew more about the basic nature of semiconductors than anyone in the world". As a student in Stuttgart, it certainly felt that way. The combination of Manuel's knowledge and wisdom, the experience of his students and visitors and other scientists in the institute, and the well equipped library made Heisenbergstrasse a wonderful place for Ph.D. thesis research. I also made personal connections there, which have influenced my career in industry and academia for more than 20 years.

While Manuel's contributions to basic solid-state physics have been well documented and will certainly be addressed elsewhere in this book, it is also worthwhile to stress Manuel's contributions to semiconductor technology. Our world is very different today compared to the 70s, when Manuel began his long tenure in Stuttgart. Computers are now commonplace and we increasingly rely on smartphones and tablets for everyday activities like booking travel, online purchases, gathering information, getting directions, etc. Manuel and his students and visitors made very significant contributions to the semiconductor process technologies, which have enabled these high-tech products. It took, of course, thousands of micro-innovations to create these technology platforms. A few of these, however, including some very important ones, clearly carry Manuel's fingerprints.

The industrial contributions of an academic scientist start with training students who choose industrial careers. Manuel's own career began in industry, at RCA in Princeton and Zurich, and included many visits to the industrial labs at IBM and Xerox. Today, these industrial physics labs are mostly in the past, unfortunately, but the legacy of Manuel's students in industry continues. Manuel actively encouraged his students and postdocs who sought an industrial career. A few names worth mentioning are Peter Lautenschlager (aerospace), Diego Olego (Phillips Healthcare), Tobis Ruf (Bosch), Uwe Schmid (software), Jens Kircher (Bosch), Thorsten Heyen (Wacker, Infineon), Ran Liu (Motorola), Andreas Goebel (Acorn Technologies), Wolfgang Kauschke (Lucent Alcatel), Gerhard Fasol (Hitachi), Stefan Zollner (Motorola, Freescale, IBM). I am proud to say that I am continuing the tradition of training students for industrial careers: My first Ph.D. student Lina S. Abdallah, after writing a thesis on spectroscopic ellipsometry of Ni-Pt alloys, just took a position with Intel in Oregon as a lithography engineer.

K. Ensslin and L. Viña (eds.), *Manuel Cardona*,
DOI 10.1007/978-3-319-20343-0_45

Manuel Cardona liked to stay in touch with his students in industry. He visited Ran Liu and me at the Motorola Process and Materials Characterization Lab in Mesa, AZ, around 2000, while we prepared for the International Conference on the Physics of Semiconductors (ICPS, his favorite conference series), held in Flagstaff in July 2004. He encouraged us to include industrial topics and issues in the planning of the 2004 ICPS, following a long tradition from the 70s and 80s, which, unfortunately, has gone out of fashion at the ICPS. While other scientists frowned upon the March meeting activities of the Forum on Industrial and Applied Physics (FIAP, a unit of the American Physical Society), which I represented as Chair and later as APS Councillor and Executive Board Member, Manuel was a frequent active and passive participant in our sessions. He had a keen interest in the applications of the basic semiconductor science he understood so well. At the 2002 APS March meeting in Indianapolis, when we expected vacuum-ultraviolet (157 nm) lithography to push the limits of CMOS scaling, Manuel gave the opening invited talk "Vacuum ultraviolet spectroscopy: Classical examples and recent results" in a FIAP invited session on "VUV Optical Science and Measurements". He clearly understood the issues we were struggling with, especially the stress-induced birefringence and spatial dispersion of calcium fluoride just below the band gap. He offered important insights and pointed out issues (which eventually killed the VUV lithography technology).

Manuel Cardona received his Ph.D. in applied physics at Harvard in 1959, following a thesis with Bill Paul on the pressure and temperature dependence of the dielectric constant of semiconductors. (Since German universities abandoned academic regalia in the late 60s, Manuel's Ph.D. students in Stuttgart were allowed to wear Manuel's Harvard gown during the parties following their thesis defense. These parties were an important element of the culture at the MPI and allowed for integrating a very diverse group of scientists.) It is not surprising that pressure, strain, alloy composition, and temperature became important themes for Manuel's research throughout his career. Manuel knew how to manipulate materials through external stimuli to unlock their secrets.

Strain is also an important component of modern complementary-metal-oxide semiconductor (CMOS) technologies. To achieve scaling of CMOS devices following Moore's Law, it is necessary to reduce the channel resistance with each generation. By applying a process-induced stress to the channel, it is possible to split bands and modify the effective masses of electrons and holes. For electron-based NMOS devices a strain along the (100) direction splits the three equivalent directions into a doublet and a singlet. For the proper sign of the stress, the singlet becomes the ground state, which reduces intervalley scattering, the dominant scattering mechanism under high electric fields. Similarly, stress will split the heavy and light hole bands. Choosing a suitable stress, it is possible to populate the holes in the light hole band, which has a lower effective mass and a higher mobility than the heavy hole band in Si.

The most powerful method to achieve stress is to dry-etch a divot in the silicon source-drain areas of devices, followed by selective epitaxy of a silicon-germanium (for the PMOS) or silicon-carbon alloy (NMOS) using chemical vapor deposition.

The difference in the lattice constant between the Si:Ge (or Si:C) alloy and pure Si causes a uniaxial stress in the channel, which reduces the channel resistance. These effects can easily be understood using **k·p** theory (another one of Manuel's favorite topics) and are incorporated into commercial device simulators. (Another method to reduce device resistance is with ultrahigh doping by ion implantation followed by laser annealing, one of the thesis topics of Luis Viña.)

Right across the hall from his office on the seventh floor, Manuel had a cabinet containing his famous sample collection. Among the most valuable pieces were bulk silicon-germanium crystals grown in the 1950s at RCA. Even today, these would be very unique pieces. Nobody grows such alloys anymore as bulk crystals. Silicon-germanium alloys were crucial to our understanding of the band structure of silicon and germanium. Manuel and his students and guests (especially Isabel Alonso, Josef Humlicek, and J.B. and M.A. Renucci) established a database of basic properties of silicon-germanium alloys, which formed the basis for the introduction of silicon-germanium alloys into mainstream cellphone production at Motorola in the late 1990s. Manuel was also a heavy user of the crystal growth laboratory at the institute, especially after the discovery of high-temperature superconductivity.

Manuel also collaborated with scientists who had epitaxial crystal growth capabilities, especially Gerhard Abstreiter (and later Karl Eberl) on silicon-germanium and with Klaus Ploog on compound semiconductor heterostructures. Pseudomorphic growth of epitaxial layers produces biaxial stress, which causes shifts and splitting of phonon and electron energies, a common theme for Manuel's students.

During the late 1980s, Manuel's *Abteilung* could roughly be split into five groups by their experimental methods (Raman, ellipsometry, photoemission) and their class of materials (semiconductors and oxide superconductors). As a student, I stayed away from oxides as much as possible, but they have now become the focus of my research at New Mexico State University. I was not introduced to the spectroscopy of oxides until I joined Ran Liu at Motorola in Mesa, AZ, in 1997. During this time period, semiconductor companies started to contemplate replacing silicon dioxide as the gate oxide material in CMOS devices with complex metal oxides. These oxides have a higher dielectric constant than SiO_2 and thus reduce the leakage current (by quantum mechanical tunneling) through the gate oxide. Ran Liu and I had the benefit of being aware of 40 years of research by Manuel on the dielectric constant and its relationship with the vibrational and electronic degrees of freedom of materials. We published many papers on Raman spectroscopy and ellipsometry of metal oxides, especially $SrTiO_3$ and HfO_2. One of the blueprints for our work was a paper by Ran Liu (written as part of his thesis in Stuttgart) on the phonon symmetries and frequencies of $YBa_2Cu_3O_7$, one of the most highly cited physics papers in 1988.

Raman spectroscopy was certainly the workhorse in Manuel's laboratories in Stuttgart. Thanks to the efforts of his group, Raman spectra are well understood for many classes of materials. With the arrival of commercial easy-to-use Raman spectrometers (especially by Renishaw), Raman spectroscopy became an important metrology technique for the semiconductor industry. Manuel's most highly cited journal article addresses the hydrogen-related Raman spectra in amorphous silicon, an important example of the Raman metrology that was used heavily at Motorola factories for process control. Ran Liu also developed a technique, where he could determine the Si–Si Raman frequency with an accuracy of 0.01 cm^{-1} and a spatial resolution of 0.3 micrometers (employing UV optics in the spectrometer, mirrors and detectors with UV coatings, and the 325 nm line of a He-Cd laser). This allowed two-dimensional stress mapping of process induced strain in CMOS devices, an important result for device and process simulations.

In summary, Manuel Cardona's research had a comprehensive, lasting, and important influence on the development of semiconductor technology and scaling of CMOS devices following Moore's Law. Some of his students and postdocs (like Meera Chandrasekhar) may not even have been aware of the impact of their research. Industrial semiconductor engineers do not usually look for the provenance of a scientific result, but my classmate Ran Liu and I would often recognize Manuel's influence on our work at Motorola.

The references below list my choice for the top ten industrial physics papers authored by Manuel Cardona. Some of these papers are among Manuel's most highly cited papers. It is clear that his research has had a very profound impact on semiconductor technology.

References

1. M.H. Brodsky, M. Cardona, J.J. Cuomo, Infrared and Raman spectra of the silicon-hydrogen bonds in amorphous silicon prepared by glow discharge and sputtering. Phys. Rev. B **16**, 3556 (1977) (1527 citations in Google Scholar (#1))
2. F.H. Pollak, M. Cardona, Piezo-electroreflectance in Ge, GaAs, and Si. Phys. Rev. **172**, 816 (1968) (786 citations in Google Scholar (#3))
3. F. Cerdeira, C.J. Buchenauer, M. Cardona, F.H. Pollak, Stress-induced shifts of first-order Raman frequencies of diamond-and zinc-blende-type semiconductors. Phys. Rev. B **5**, 580 (1972) (756 citations in Google Scholar (#4))
4. M. Cardona, Optical properties and band structure of $SrTiO_3$ and $BaTiO_3$. Phys. Rev. **140**, A651 (1965)
5. R. Liu, C. Thomsen, W. Kress, M. Cardona, B. Gegenheimer, F.W. De Wette, J. Prade, A.D. Kulkarni, U. Schröder, Frequencies, eigenvectors, and single-crystal selection rules of k = 0 phonons in $YBa_2Cu_3O_{7-\delta}$: Theory and experiment. Phys. Rev. B **37**, 7971 (1988)
6. L. Vina, M. Cardona, Effect of heavy doping on the optical properties and the band structure of silicon. Phys. Rev. B **29**, 6739 (1984)
7. M. Cardona, W. Paul, H. Brooks, Dielectric constant of germanium and silicon as a function of volume. J. Phys. Chem. Solids **8**, 204 (1959)
8. J.B. Renucci, M.A. Renucci, M. Cardona, Volume dependence of the Raman frequencies of Ge-Si alloys. Solid State Commun. **9**, 1651 (1971)
9. P.Y. Yu, M. Cardona, Spatial dispersion in the dielectric constant of GaAs. Solid State Commun. **9**, 1421 (1971)
10. D.E. Aspnes, M. Cardona, Strain dependence of effective masses in tetrahedral semiconductors. Phys. Rev. B **17**, 726 (1978)