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SCIENTIFIC REMINISCENCES

Reminiscences of my life in science

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Abstract With a great deal of humility I attempt in the following to recall important events in my life and in my scientific career of more than five decades. I am not through yet. I continue to do research though, I admit, not with the energy and fervor I once had. Still, I hope to contribute to science in meaningful ways.

Key words: reminiscences — geophysics — planetary physics — science career

1 EARLY YEARS

It all started in the Bronx, New York. I was born there in 1939 just as World War II was beginning. I do not have many memories of this period but I do distinctly recall the euphoria and celebrations that accompanied the war's end. I attended P. S. 6 (Public School 6) through grade 8. Then Brooklyn Technical High School in Brooklyn, New York through grade 12. I can trace my interest in mathematics and science back to elementary school. One memory I retain is being sent to the principal's office for reading an advanced algebra text while in English class.

When it came time to attend high school there were three special high schools in New York that required entrance exams and were very competitive, Bronx High School of Science, Peter Stuyvesant High School in Manhattan, and Brooklyn Technical High School. All three excelled academically. I had my choice among them and decided on Brooklyn Tech because of all the impressive engineering shops it had. I never regretted that decision. I had to ride the subway to get to Brooklyn Tech, an hour's ride each way. It kept me from participating in extra-curricular activities but it offered time to get homework done during the trip. Brooklyn Tech was an all male school at the time. The college preparation I received there was outstanding, so much so that when I entered college I received credit for having already had almost the entire freshman year of required courses.

Choosing a college presented a challenge for me. I would need a scholarship to attend a distinguished university. I remember sitting for numerous scholarship exams. Somehow, I received an appointment to the U. S. Naval Academy from the congressman of our district. How that came to pass I no longer remember, but it was quite an honor for someone of my background to attend the Naval Academy. I also received an NROTC (Naval Reserve Officer's Training Corps) scholarship that would pay for my tuition and books and supply a modest monthly allowance at a university of my choice. All I had to agree to do was to participate in naval training at the university, spend my summers on naval training cruises, and serve as a naval officer for three years upon graduation. I traveled on my own for the first time from New York to Annapolis to see what the Naval Academy was like. It may have been the first time someone interviewed the Academy rather than vice-versa. I also underwent a rigorous and exhausting series of medical tests at the St. Albans Naval Hospital in Queens (the hospital has long been closed). The experience was more draining than the scholarship tests I had to sit through. Fortunately, I passed the medical exam to insure both my NROTC scholarship and my Naval Academy appointment. So, the Navy would have me one way or another. My choices came down to either Cornell University or the Naval Academy. I couldn't deny my desire to become a scientist and decided to go to Cornell. It was one of the best decisions in my life. It was difficult at the time to turn down an appointment at the Naval Academy. Who would do such a thing? I faced the consternation of my parents and my congressman who selected me ahead of probably many others to receive the honor of the appointment. So, in the Fall of 1956 I was off to Cornell to begin my scientific career as an Engineering Physics major (a five year program).

Engineering Physics at Cornell was a rigorous and demanding major. It consisted of 5 years of courses in mathematics, physics, chemistry and engineering and some liberal arts electives. Preparation for a future career in science could not have been better. I was able to finish the 5 year program in 4 years thanks in part to credits I received for freshman courses that I had already taken at

Brooklyn Tech. However, I did not take my degree after 4 years because I had an obligation to serve in the Navy for 3 years after my graduation and the Navy had allowed me 5 years to finish my Bachelor of Engineering Physics degree. I took advantage of this opportunity with the help of some understanding faculty members who gave me temporary incompletes in a couple of courses so I could defer graduation for a year and pursue a Master of Aeronautical Engineering degree during my fifth year at Cornell. When I finally left Cornell in 1961 I did so with both the Bachelors and Masters degrees in my pocket.

I used the year in Aeronautical Engineering to fuel up on courses in fluid dynamics. I wrote a thesis titled "Linearized Oscillations in a Plasma" (Master's Thesis, Cornell University, 1961). My thesis advisor was a young Assistant Professor by the name of Donald Turcotte, who, unknown to me at the time, would turn out to be a lifelong colleague, collaborator and friend (more on this later). My thesis formed the basis for the paper "Interaction of low frequency electromagnetic waves with a plasma, D. L. Turcotte and G. Schubert, *Phys. Fluids*, **4**, 1156–1161, 1961", my first paper.

I met another person at Cornell who would also have a major influence on my professional career. Peter Goldreich and I both enrolled in Engineering Physics at the same time in 1956. Had I chosen the Bronx High School of Science instead of Brooklyn Tech we would have met years earlier. We became good friends and were study companions throughout our undergraduate years. We were also traveling companions, driving almost every weekend between Ithaca and New York to be with our future spouses. We had a few harrowing encounters during these trips that came close to ending our yet blossoming careers. Joyce and I were married in 1960 and lived in Ithaca during my fifth year at Cornell.

Another person of note from Cornell, Thomas Gold, comes to mind. He worked on numerous problems in astrophysics and, among other successes, deduced that the short period pulsed radio sources discovered in 1968 were associated with the rate of rotation of a neutron star. I originally sought to do my masters thesis with Gold who was interested at the time in the nature of the lunar regolith. The subject did not interest me then and as I look back I was probably somewhat in awe of this famous astrophysicist. I recall that in my first conversation with him he asked me how good I was, saying that he was not interested in working with any but the best of graduate students. I wasn't sure if I fit that category and looked for a thesis advisor elsewhere. Some things turn out for the best for my quest brought me to Donald Turcotte. By the way, Peter Goldreich obtained his Ph. D. under Thomas Gold.

A principle that I now attribute to my experience at Cornell is that good ideas in science are the most valuable asset a researcher could possess. While technical skills are needed to carry ideas to fruition, the ideas are the most important. It does not matter if all the ideas turn out to be correct. At least some of them should, of course, but one should not shy away from pursuing them all. Another lesson I carried away from Cornell is that one should seek the simplest possible solution to a problem. Complexity does not necessarily contribute to understanding. Approximations that simplify a problem to its essential elements are to be used whenever possible. These ideas have stayed with me throughout my research career.

When it came time to leave Cornell I was commissioned as an Ensign in the U. S. Navy and was obligated to serve for three years. That length of time away from my academic pursuits is not what I wanted so I sought some form of duty with the Navy that would allow me an opportunity to pursue graduate studies while serving out my obligation. That opportunity presented itself when I was able to apply for an assignment with the U. S. Naval Nuclear Power School at the Mare Island Naval Shipyard in Vallejo, California. The School was part of Admiral Hyman Rickover's nuclear program, in effect a branch of the Navy entirely under Rickover's administration. Selection for this assignment was highly competitive and I endured an entire day of "interviews" in Washington, D. C. including a difficult one with the Admiral himself. Fortunately, I was successful and could look forward to a career in science and engineering as a naval officer. The one downside to all of this is that I had to extend my obligatory service to four years, but I thought that was a reasonable thing to do in light of the opportunities I would have as part of the nuclear program.

The Nuclear Power School was the place where officers and enlisted men headed for duty on nuclear submarines would learn about the physics and engineering of the power plants that ran their boats so they could operate them safely and efficiently. In addition to the normal duties of a naval officer I had to teach four hours a day. I taught nuclear reactor physics, mathematics, thermodynamics, fluid mechanics and heat transfer, and case studies (discussions of possible "accidents" or malfunctions and how to deal with them). While more engineering than science the, atmosphere at the School was similar to what would be found in specialized graduate programs in colleges and universities. Some of the students were officers of high rank including those who would become commanders of their boats. I remember feeling a bit awkward teaching officers so senior to me but I took solace in the fact that I knew the subjects better than they did. During the years I was at Mare Island (1961–1965) there were two Nuclear Power Schools, the other one at New London, Connecticut. Today, these Schools have been replaced by a combined facility under the Naval Nuclear Power Training

Command in Goose Creek, South Carolina. Students there receive college credit at civilian universities for the courses they have taken.

Vallejo is located very near Berkeley and while I was at Mare Island I was determined to study for a Ph. D. at U. C. Berkeley (UCB). Without consulting the commanding officer of the Nuclear Power School, I enrolled at UCB in engineering. How I was going to teach four hours a day every day of the week and do the administrative and advisory work of a naval officer and still be a graduate student at UCB seems impossible in retrospect, but I pulled it off. I believe I am the only officer in the Nuclear Power Program to have earned a Ph. D. while serving. At some point the Commanding Officer discovered what I was doing and read me the riot act but did not prevent me from continuing my studies.

I chose to major in Aeronautical Sciences since my Master's degree in Aeronautical Engineering at Cornell gave me a few steps up on the Ph. D. program. However, all the courses I took were mathematics, physics and fluid dynamics. They were an excellent foundation for my later research. The first course I took was electrodynamics. I couldn't make the scheduled time of the class at UCB, but it was also offered in the evening at the Lawrence Livermore Laboratory in Livermore, California. I drove several nights a week from Vallejo to Livermore, almost 100 miles round trip, in order to take the course. They were long and exhausting trips. The instructor was Allan Kaufman then a staff physicist at Lawrence Livermore Laboratory and later a Professor of Physics at UCB. We used a pre-publication version of the text Classical Electrodynamics by John David Jackson. It stands out in my mind as one of the most valuable of all the physics courses I have had. I recall not doing well on the first exam but with the encouragement of Allan Kaufman I persevered and eventually earned an A in the course. Many years later I ran into Allan at a meeting of the American Geophysical Union. I felt good that he remembered me as a student.

My Ph. D. thesis advisor was Gilles M. Corcos, a fluid dynamicist with a notable feeling for the subject. I studied the structure and dynamics of the laminar or viscous sublayer of a turbulent boundary layer, the region immediately adjacent to a wall or no-slip boundary wherein the flow becomes laminar due to the reduced velocity near the wall. I developed a model in which the viscous sublayer is driven by means of a fluctuating pressure that is independent of distance from the wall and imposed by the external flow. The equations solved are boundary-layer approximations to the Orr-Sommerfeld equations. They constitute a non-homogeneous system and were solved by convergent power series evaluated on an IBM computer run by punch cards. I recall many drives between Vallejo and Berkeley carrying boxes of punch cards I hoped would not spill. The solutions exhibit the strong role of viscosity throughout the sub-layer and provide a model endowed with many of the experimentally known features of turbulence near a wall. My thesis (A Linear Analysis of the Effect of a Wall on a Given Turbulent Flow, Ph. D. Dissertation, University of California, Berkeley, 1964) formed the basis of the paper "Schubert, G. & Corcos, G. (1967). The dynamics of turbulence near a wall according to a linear model. *Journal of Fluid Mechanics, 29*(1), 113–135, doi:10.1017/S0022112067000667". I received the Ph. D. in Engineering (Aeronautical Sciences) from the University of California, Berkeley in 1964.

I obtained my Ph. D. in only three years while serving full time in the Navy at the Nuclear Power School. While I am proud of this achievement I did miss out on the experience of interacting with other graduate students, with faculty other than my advisor, and with visitors who gave frequent seminars. The experience was similar to the one I had at Brooklyn Tech.

Though I received my Ph. D. in 1964 I could not pursue my academic career right away. I still had one year more to fulfill my obligation to the Navy. During that time I was promoted to Lieutenant, a naval rank not usually attained in three years. I suppose it was an inducement to reenlist at the end of my fourth year but I had different plans. I applied for and received a NAS-NRC (National Academy of Sciences-National Research Council) postdoctoral fellowship to study at DAMPT (Department of Applied Mathematics and Theoretical Physics) at the University of Cambridge in England. So, in the summer of 1965 Joyce and I and our two young children (Todd and Michael) were off to England and my first real academic position.

2 START OF MY ACADEMIC CAREER

Living and studying in Cambridge was an experience I remember quite pleasantly. We lived in a semi-detached house dating at least back to before the Second World War. It required wearing a lot of sweaters and sitting by the gas fire. The house was located near Grantchester Meadows and we enjoyed many walks through the meadows to a tea house in Grantchester. We were also nicely situated near DAMPT, just a 10 minute walk across the Silver Street bridge. We had a car, but driving it represented a challenge since the steering wheel was on the wrong side for driving in England and of course I had to accustom myself to driving on the left. Nevertheless we took every opportunity to see almost all of England venturing as far as Wales and Scotland and crossing the Channel to France.

My contact in DAMPT was L. E. (Ed) Fraenkel a now famous mathematician elected as a Fellow of the Royal Society in 1993. In 1965, at the time of my visit, he was working toward his Ph. D. at the University of Cambridge.

He was awarded the degree in 1968. I didn't come to Cambridge with a particular research problem in mind. Ed put me on to a problem involving the nature of viscous flow in a cusped corner, a problem that had already been solved for a sharp corner by Keith Moffatt. I found that the flow in the cusped corner consisted of a sequence of eddies of rapidly diminishing strength. The results were published in the Journal of Fluid Mechanics (Viscous flow near a cusped corner, G. Schubert, *J. Fluid Mech.*, **27**, 647–656, 1967). The writing of these reminiscences inspired me to contact Ed Fraenkel. I hadn't had any connection with him from the time I left Cambridge in 1966. I was pleased when he responded to my query "Of course I remember you well. You did a good piece of work on the problem of viscous flow near a cusped corner.".

DAMPT had an impressive faculty when I visited. Indeed, it was probably the leading center of theoretical fluid dynamics. George K. Batchelor was Head of Department and the stellar cast included such people as Keith Moffatt, Francis Bretherton, T. Brooke Benjamin, Adrian Gill, and S. A. Thorpe. A turning point in my life occurred while I was at DAMPT. My good friend from Cornell, Peter Goldreich visited and gave a talk on the orbital dynamics of the Moon discussing research he later published in Reviews of Geophysics (P. Goldreich, History of the Lunar Orbit, *Rev. Geophys.*, **4**, 411–439, 1966). I was fascinated and inspired by his presentation and decided, probably even before his talk was finished, that I too wanted to do planetary physics. Peter was at UCLA at the time and on his recommendation I was offered an Assistant Professorship in the then Planetary and Space Sciences Department (later to metamorphose into the Geophysics and Space Physics Department and still later to undergo additional name changes culminating in the present name Department of Earth, Planetary and Space Sciences). I also had an offer of an assistant professorship in an engineering department at New York University but I was hooked on planetary science and have never regretted my decision to pursue it at UCLA. I arrived at UCLA in the summer of 1966.

3 EARLY YEARS AT UCLA

Like fluid mechanics at DAMPT in Cambridge, UCLA's faculty was pre-eminent in the fields of geophysics, planetary physics, and fluid dynamics. Among the faculty and researchers I joined were George Wetherill, William Kaula, David Griggs, Leon Knopoff, Peter Goldreich, Willem Malkus, Fritz Busse, Jack Whitehead, and more that I should have probably mentioned. Some of them were members of the Institute of Geophysics and Planetary Physics (IGPP), an elite group of researchers who were exempt from teaching. It was several years before I was invited to join the IGPP. Despite its excellence the IGPP has not survived to the present perhaps because of the egalitarian culture of UCLA. At the time I arrived at UCLA the Director of the IGPP was Willard F. Libby, a Professor of Chemistry and an eccentric who won the Nobel Prize in 1960 for his work in developing radiocarbon dating. The fluid dynamics group headed by Willem Malkus was particularly active and in addition to Busse and Whitehead included Alan Newell and Victor Barcilon. We had a joint seminar with the fluid dynamicists at UC San Diego, quite an amazing happening considering the distance between the campuses.

4 GOLDREICH-SCHUBERT INSTABILITY

By the time I got to UCLA in 1966, Peter Goldreich, who facilitated my appointment, had left UCLA for Caltech. Nevertheless, the first research I did was a collaboration with Peter on the stability of differential rotation in the radiative zones of stars. Robert H. Dicke, the famed Princeton physicist, had interpreted his measurement of a solar oblateness of 5×10^{-5} as evidence that the radiative interior of the Sun was in rapid rotation with a 1.8-day rotation period, much faster than the slow surface rotation period of the Sun which he assumed was confined to the convective zone. However, we found that Dicke's proposed angular velocity profile would be unstable (Differential rotation in stars, P. Goldreich and G. Schubert, *Astrophys. J.*, **150**, 571–587, 1967; Rotation of the sun, P. Goldreich and G. Schubert, *Science*, **156**, 1101–1102, 1967.) The operative instability is a double diffusive one similar to the thermohaline instability in Earth's oceans. The instability in the oceans arises because the diffusivity of heat is much larger than the diffusivity of salt. In the Sun, the instability occurs because the thermal diffusivity is much larger than the momentum diffusivity (kinematic viscosity). The idea for the double diffusive instability in the Sun actually originated with the thermohaline instability which Peter learned about during a summer in Woods Hole Oceanographic Institution with Willem Malkus. This is certainly one of the research contributions of which I am most proud. The instability has become known as the Goldreich-Schubert instability or the Goldreich-Schubert-Fricke instability for the work on the phenomenon later contributed by Klaus Fricke.

In the first lecture to astrophysicists that Peter gave on this work he illustrated the angular velocity instability in the Sun by carrying out an experiment in real time using two beakers of water, one containing warm salty water dyed red, and the other containing cold fresh water. When the warm salty water was carefully poured over the cold fresh water the configuration was initially stable because temperature more than compensated for the salt and the beaker had light red water on top and clear, more dense water on the bottom. Soon however the double diffusive instability set in, small red droplets of water would sink because efficient thermal diffusion wiped out the stabilizing density difference due to temperature and the water in the beaker quickly mixed. This was probably the first and only astrophysics lecture to feature a live experiment. It is a superb example of how concepts from one area of physics can carry over into another. I performed this experiment many times in my teaching of geophysical fluid dynamics at UCLA.

5 LUNAR RIVERS

My first office at UCLA was in Slichter Hall immediately adjacent to the offices of Richard Lingenfelter and Stanton Peale. At the time, the Moon was the focus of planetary science (The Apollo program of lunar exploration was in full gear in 1967–1968. The first lunar landing took place in 1969.). We had limited understanding of the processes shaping the lunar surface. People even argued about whether features were volcanic or impact in origin. One of the enigmatic features was the lunar sinuous rille, a channel that could meander over large distances on the Moon's surface. Lingenfelter, Peale and me studied the possibility that the channels were cut by the flow of water and we published several papers on the subject. It is now accepted that the sinuous rilles are volcanic in origin, much like collapsed lava tubes on Earth. Though we were wrong in retrospect about the origin of the rilles, our work carefully explored how water behaved in vacuum and how it could flow under an ice cover and erode a channel. Perhaps a lot of what we did might be relevant to the channels on Mars, features that are generally believed to have formed by flowing water. Of course, we do not know if any of the Martian rivers flowed under an ice cover or whether they flowed as terrestrial rivers in a more clement environment. The possibility of ice-covered rivers might be more suitable for the outer planet moons having subsurface liquid water oceans. My colleagues and I never explored these possibilities. Soon after I arrived at UCLA Peale left for a position in the Physics Department at UC Santa Barbara where he had a distinguished career studying the orbital and rotational dynamics of solar system bodies. Although we never published together after he left UCLA we stayed in close contact as colleagues and friends.

6 SUPER-ROTATION OF THE VENUS ATMOSPHERE AND THE MOVING FLAME

In 1967 it was learned, by analyzing the motions of cloud features in ultraviolet images of the planet Venus, that at least the upper atmosphere was rotating in the same direction as the solid planet but at speeds much greater than the solid body rotation rate. From that time to the present, the search for the explanation of Venus' atmospheric super-rotation and other dynamical phenomena occurring in its atmosphere, has been a mainstay of my research. In 1967 I had the idea that the super-rotation might be caused by the overhead motion of the Sun that is in the opposite direction to the observed motion of the planet and its atmosphere. I collaborated with Jack Whitehead who was a researcher at UCLA at the time and we showed, by rotating a Bunsen burner beneath a pan of liquid mercury, that the mercury rotated in the opposite direction to the Bunsen burner. The observation became known as the moving flame effect (Moving flame experiment with liquid mercury: Possible implications for the Venus atmosphere, G. Schubert and J. A. Whitehead, *Science*, **163**, 71–72, 1969.) It has now been shown that the overhead motion of the Sun indeed plays a significant role in driving the super-rotation. The transfer of angular momentum in the Venus atmosphere occurs through the motions induced by solar-driven thermal tides, while the momentum transfer in the laboratory experiment is due to thermal diffusion. Because of this the moving flame idea never received much credit from atmospheric scientists, perhaps rightly so, but the realization that the motion of the Sun had an important role to play in driving the super-rotation is something I look back on with pride. My Venus research has many other tales to tell but I will proceed chronologically to discuss my science.

7 FIRST STUDIES OF MANTLE CONVECTION

At about this time I ran into my Master's thesis advisor, Donald Turcotte, at a meeting of the American Geophysical Union in Washington, D. C. Amazingly, unbeknownst to each other we had both left the field of aeronautical engineering in favor of geophysics. That chance meeting turned into a lifelong friendship and close scientific collaboration in which we shared ideas and coauthored many papers and books. We both brought a strong knowledge of continuum physics and fluid dynamics to our collaboration and ideas flowed nonstop. It was the beginning of the plate tectonic revolution and one could obtain deep insights into geological and geophysical problems with simple thermal and fluid dynamic models. How lucky could one get? At the beginning of my research career planetary science began to blossom and a new paradigm transformed the Earth sciences. Opportunities to contribute on the ground floor in both areas were abundant.

My first investigation along these lines addressed the question of whether mantle convection, the presumed driving mechanism for continental drift and plate tectonics, could be occurring in the interiors of other planets such as Venus, Mars, and the Moon (Stability of planetary interiors, G. Schubert, D. L. Turcotte, and E. R. Oxburgh, *Geophys. J. Roy. Astron. Soc.*, **18**, 441–460, 1969). We (myself, Don Turcotte, and Ron Oxburgh, now Lord Oxburgh) carried out an analysis of the thermal stability of a layer of fluid heated from below with a viscosity increasing exponentially with depth, as might be the case in a planetary interior. We found all the planetary interiors studied to be thermally unstable implying the likelihood of thermal convection or mantle convection in all the terrestrial planets. Today, it is generally accepted that this is the case, although the surface expression of mantle convection is plate tectonics only for the Earth.

8 LUNAR ELECTROMAGNETISM AND THE APOLLO PROGRAM

Mantle convection would be a major focus of my research throughout my career, but so would planetary physics. In 1969 there was so much excitement created by the Apollo exploration of the Moon that it would have been foolhardy not to be involved. UCLA happened to be at the center of much of this excitement because the space physics group, under Paul J. Coleman, Jr. and including Christopher Russell, Larry Sharp, and Bernard Lichtenstein (one of my first Ph. D. students) was deeply involved in the Apollo magnetic field experiments. The group also had good connections with a team at NASA Ames Research Center (Charles Sonett, Palmer Dyal, Curtis Parkin, David Colburn, Bruce Smith, and John Mihalov). The team also included Kenneth Schwartz, a consultant from the private sector. Charles Sonett and Palmer Dyal were Principal Investigators (PIs) of the Apollo 12, 15 and 16 lunar surface magnetometer experiments, and Paul Coleman was PI of the Apollo 15 and 16 sub-satellite magnetometer investigations. I was a co-investigator on the Apollo 16 lunar surface magnetometer and the Apollo 15, 16 subsatellite magnetometers. These investigations used magnetometers on the surface of the Moon and in orbit around the Moon to investigate the characteristics of lunar crustal magnetization and lunar internal electrical conductivity. The crustal magnetization is a permanent magnetic field while the Moon experiences time-varying magnetic fields as it moves through the solar wind and the Earth's magnetosphere. The time varying magnetic field induces electrical currents in the Moon that in turn have associated magnetic fields (Faraday's law of induction). My role in all these experiments was to provide the theoretical basis for interpretation of the magnetic field data. The determination of the crustal magnetic field is relatively simple because of its permanence but the separation of the measured time varying magnetic field into its components (induced and driving) is much less straightforward. The latter provides information about the electrical conductivity of the material carrying the induced currents, the rocks of the lunar interior. From the distribution and magnitude of the crustal magnetization we learn about the nature of the magnetic field that magnetized the rocks (presumably the early lunar dynamo). Knowledge of the electrical conductivity inside the Moon tells us something about the composition of the rocks and the temperature in the Moon. The Apollo era electromagnetic exploration of the Moon was the first time that magnetic fields were used to explore the crusts and interiors of planetary bodies other than Earth. It was a privilege to be at the center of this effort. Since then magnetometers on spacecraft have made important discoveries throughout the solar system including crustal magnetization on Mars (Mars once had a dynamo operating in a liquid part of its core) and subsurface liquid water oceans on outer planet satellites such as Europa and Enceladus. I have also been privileged to participate in several of these spectacular discoveries.

Of the many papers I co-authored on lunar electromagnetism one is particularly intriguing to this day. The paper was published 44 years ago. It reported evidence from the surface magnetometer at the Apollo 15 site of polarized magnetic field fluctuations that we interpreted to be caused by an electrical conductivity anomaly associated with Mare Imbrium (Polarized magnetic field fluctuations at the Apollo 15 site: Possible regional influence on lunar induction, G. Schubert, B. F. Smith, C. P. Sonett, D. S. Colburn, and K. Schwartz, *Science*, **183**, 1194–1197, 1974). The validity of this possible discovery has never been confirmed; there have been no additional measurements of this kind on the lunar surface. If true, the existence of an electrical conductivity anomaly at Mare Imbrium and perhaps other maria would have important implications for the composition of the materials in the mare regions and the origin of the maria themselves.

9 RICHARD YOUNG

At about this time I continued to study the fluid flow induced by a moving thermal source with my first Ph. D. student Richard E. Young. We were motivated, of course, by the potential application to Venus. Our first paper together was "The 4-day Venus circulation driven by periodic thermal forcing, G. Schubert and R. E. Young, *J. Atmos. Sci.*, **27**, 523–528, 1970". This was the beginning of a long professional collaboration to study the Venus atmosphere. After obtaining his Ph. D. and a postdoctoral appointment at the National Center for Atmospheric Research, Richard took a position at the NASA Ames Research Center. He rose to become the entry probe chief scientist on the Galileo mission to Jupiter. Richard was one of a number of my former students to achieve great success in his scientific career. He was also one of a number of my students with whom I have had a lifelong friendship and a continued research collaboration well beyond their graduate student years. I have indeed been fortunate to have had the privilege of working with many exceptional students. Richard passed away in 2013.

10 MANTLE CONVECTION AND PHASE CHANGES

One of the most important contributions of my research career occurred during the time I was heavily engaged in studying lunar electromagnetism, but the contribution had to do with mantle convection. In the late 1960s there was great debate about whether mantle convection took place in the form of separate circulations confined to the upper mantle and the lower mantle or took place as a single circulation occupying the entire mantle (layered vs. whole mantle convection). The debate was not settled for many years. The mantle is the rocky region of the Earth's interior between the base of the crust and the top of the core at a depth of nearly 2900 km. The upper mantle extends to a depth of about 660 km. The lower mantle lies beneath the depth of 660 km. It is now known that an endothermic subsolidus phase change from spinel to perovskite and magnesiowüstite occurs at the depth of 660 km to separate the two layers of the mantle. It was thought by many that the properties of this boundary somehow served as a barrier to mantle convection stopping the subduction of the lithosphere (plates) from penetrating further into the lower mantle. Another significant phase change involving the exothermic conversion of olivine to spinel occurs within the upper mantle at a depth of about 410 km.

The behavior of a mantle convection system containing a phase change was simply not understood in the 1960s. My esteemed UCLA colleague Leon Knopoff in his 1964 paper in Reviews of Geophysics (The convection current hypothesis, Vol. 2, No.1, February 1964) wrote "We conclude, therefore, that the phase transition, summarily assumed to lie at 600 km but more realistically spread out in the region between 400 and 1000 km, acts as positively a barrier to convection…". I was able to show that this conclusion was incorrect and I was the first to elucidate the actual behavior of the phase change in a convection environment. I shared my understanding with colleagues Donald Turcotte and Fritz Busse. The work was published in several landmark papers: Phase change instability in the mantle, G. Schubert, D. L. Turcotte, and E. R. Oxburgh, *Science*, **169**, 1075–1077, 1970; Phase changes and mantle convection, G. Schubert and D. L. Turcotte, *J. Geophys. Res.*, **76**, 1424–1432, 1971; Convection in a fluid with two phases, F. H. Busse and G. Schubert, *J. Fluid Mech.*, **46**, 801–812, 1971; and Structure of the olivine-spinel phase boundary in the descending lithosphere, D. L. Turcotte and G. Schubert, *J. Geophys. Res.*, **76**, 7980–7987, 1971. The essential point previously unaccounted for was that a phase boundary in a fluid undergoing convection is displaced upward or downward depending on the nature of the phase transition and temperature perturbations in the vicinity of the phase change. A phase change can either enhance or inhibit convection. The olivine-spinel phase transition at 410 km depth enhances mantle convection while the spinel-perovskite transition at 660 km depth inhibits it.

The role of phase transitions in mantle convection has since been studied by numerous authors including my former graduate student David Yuen (Role of phase transitions in a dynamic mantle, G. Schubert, D. A. Yuen, and D. L. Turcotte, *Geophys. J. Roy. Astron. Soc.*, **42**, 705–735, 1975.). My colleague Paul Tackley who, as a graduate student, incorporated the physics of phase transition behavior into a global numerical model of mantle convection (Effects of an endothermic phase transition at 670 km depth in a spherical model of convection in the Earth's mantle, P. J. Tackley, D. J. Stevenson, G. A. Glatzmaier, and G. Schubert, *Nature*, **361**, 699–704, 1993; Effects of multiple phase transitions in a three-dimensional spherical model of convection in the Earth's mantle, P. J. Tackley, D. J. Stevenson, G. A. Glatzmaier, and G. Schubert, *J. Geophys. Res.*, **99**, 15,877–15,901, 1994; Mantle dynamics: The strong control of the spinel-perovskite transition at a depth of 660 km, G. Schubert and P. J. Tackley, *J. Geodynamics*, **20**, 417–428, 1995.) David Bercovici, my former graduate student and now a distinguished professor at Yale University also elaborated on how the 660 km phase change acts in mantle convection (On the penetration of the 660 km phase change by mantle downflows, D. Bercovici, G. Schubert, and P. J. Tackley, *Geophys. Res. Lett.*, **20**, 2599–2602, 1993.). It is now accepted that mantle convection is a one-layer system although individual descending slabs may be temporarily stalled at the 660 km transition.

There is a shallow phase transition in Earth's mantle involving the change from gabbro to eclogite that also plays an important role in mantle convection. I studied the reaction rate of this transition and its effects on descending slabs with my colleague Thomas Ahrens (Gabbro-eclogite reaction rate and its geophysical significance, T. J. Ahrens and G. Schubert, *Rev. Geophys. Space Phys.*, **13**, 383–400, 1975; Rapid formation of eclogite in a slightly wet mantle, T. J. Ahrens and G. Schubert, *Earth Planet. Sci. Lett.*, **27**, 90–94, 1975.).

11 WILLIAM KAULA, THE MOON AND VENUS

My research on the Moon involved more geophysics than just lunar electromagnetism. My UCLA colleague and longtime mentor William M. Kaula was a team leader for the laser altimeter on Apollo 15, 16 and 17. I joined him in an effort to learn as much about the Moon's interior as could be gleaned from knowledge of its topography. We discovered that the Moon had a center of mass-center of figure offset, quantified its magnitude, and interpreted its origin in terms of nearside-farside crustal thickness differences. We also used the topography data to characterize the features on the lunar surface and, together with other supporting lunar data, deduced aspects of the Moon's internal structure, e.g., core size, and the interior processes responsible for the origin of the center of mass-center of figure offset. Mars also has such an offset, as does Earth, and together with another colleague, Richard Lingenfelter, I interpreted the Martian offset as due to convection in the planet's interior (Martian centre of mass – centre of figure offset, G. Schubert and R. E. Lingenfelter, *Nature*, **242**, 251–252, 1973).

I worked with William Kaula again some years later in the early 1990s. We were both members of the radar and gravity investigation groups on the Magellan mission to Venus. The Magellan spacecraft carried a synthetic aperture radar to map the surface of Venus. It orbited the planet for about four years beginning in August of 1990. Magellan obtained the first high quality radar images of Venus' surface revealing, in almost photographic detail, what the eye would see if the obscuring atmosphere could be stripped away. The spacecraft also provided global data on Venus' topography and gravitational field and some atmospheric data as well. This rich collection of observations enabled the first in-depth studies of the nature of the planet's volcanic and tectonic features and inferences about the structure and dynamics of the interior. I contributed to these studies for many years.

12 LITHOSPHERIC FOUNDERING ON VENUS

Two of the Magellan-inspired papers of the many I coauthored are particularly worthy of note for the new concepts they put forth. The Magellan images confirmed clearly that there was no signature of global plate tectonics on Venus. However, the images also revealed features called coronae that were not previously seen on any other planetary surface. My former Ph. D. student and lifelong colleague David Sandwell and I presented evidence that regional subduction in the form of retrograde lithospheric foundering was occurring along the margins of at least some coronae. We published two papers in 1992 proposing this idea: "Evidence for retrograde lithospheric subduction on Venus, D. T. Sandwell and G. Schubert, *Science*, **257**, 766–770, 1992" and "Flexural ridges, trenches and outer rises around coronae on Venus, D. T. Sandwell and G. Schubert, *J. Geophys. Res.*, **97**, 16,069–16,083, 1992, Mantle plume-induced subduction on Venus". These papers not only proposed that there was subduction on Venus but they also argued that the subduction was a consequence of upwelling mantle plumes. The "centers" of coronae were above the plumes that thinned and weakened the lithosphere resulting in volcanism in the interiors of coronae. Along the edges of the disrupted lithosphere where it was thicker and heavier the lithosphere could founder and sink into the underlying mantle and eat its way into surrounding lithosphere in a process identical to that along some subduction zones on Earth known as retrograde lithospheric subduction. The idea that mantle upwelling could be the cause of mantle downwelling was a new one. The evidence for all this was the arcuate shapes of coronae margins similar to the planforms of terrestrial subduction zones, the flexural nature of topographic profiles across the margins, and the gravity anomalies associated with the coronae (Gravity over coronae and chasmata on Venus, G. Schubert, W. B. Moore, and D. T. Sandwell, *Icarus*, **112**, 130–146, 1994.) The insights we acquired at the time of Magellan have been reinforced by recent studies of gravity and topography of some coronae and laboratory experiments demonstrating that upwelling indeed causes downwelling in laboratory fluids (Experimental and observational evidence for plume-induced subduction on Venus, A. Davaille, S. E. Smrekar, and S. Tomlinson, *Nature Geoscience*, **10**, 349–355, 2017.)

13 MORE MANTLE CONVECTION AND SOME PLANETARY THERMAL HISTORY

Throughout the 1970s, and continuing to the present, I devoted much of my energy to studying the related topics of geodynamics, mantle convection, and the thermal evolutions of the planets. My main collaborators in these efforts were Donald Turcotte, David Sandwell, David Yuen, Claude Froidevaux, Richard Young, Luce Fleitout, Patrick Cassen, Abdel Zebib, and Tilman Spohn. With Donald Turcotte I studied the frictional heating of the descending lithosphere, the influence of viscous dissipation in Bénard convection, and the role of phase transitions in mantle convection, already discussed above. David Sandwell and I investigated lithospheric flexure at subduction zones (a phenomenon we later found to occur on Venus as already discussed) and the relationship between geoid height and lithospheric age using SEASAT altimeter profiles across the Mendocino Fracture Zone. David was one of the first to use satellite data to study geodynamics, particularly, processes occurring on the ocean floor. David Yuen, Claude Froidevaux, Luce Fleitout and I studied the thermal and mechanical structure of the oceanic lithosphere and asthenosphere and the shear deformation zones along major transform faults and subducting slabs. With Abdel Zebib I modeled many aspects of thermal convection in spherical shells with application to the mantle in mind. Richard Young, Patrick Cassen and I explored the thermal histories of Earth, Mars and the Moon.

14 THE DAVES

I have already discussed some of the research I carried out with my former graduate students David Bercovici, David Sandwell and David Yuen. I'll take this opportunity to show one of my favorite photos with them

Fig. 1 Photo at the dinner of my 60^{th} birthday celebration in 1999 at the meeting of the American Geophysical Union. My graduate students David Yuen, David Bercovici, David Sandwell and David Baker are standing behind me (from left to right). Also standing at the right is the distinguished planetary scientist David Stevenson (joining the Daves). David Stevenson and I collaborated on a number of papers over the years dealing with the thermal histories of the planets. Seated to the right are Paul Roberts and his wife Maureen. Paul is a distinguished fluid dynamicist and a UCLA colleague. Though we have interacted extensively over the years we published only one paper together (Instabilities in a fluid layer with phase changes, P. Roberts, G. Schubert, K. Zhang, X. Liao, and F. Busse, *Phys. Earth Planet. Inter.*, **165**, 147–157, 2007).

(Fig. 1). The picture was taken at a dinner held in conjunction with a special session at the meeting in 1999 of the American Geophysical Union to celebrate my 60^{th} birthday (Fall 1999 American Geophysical Union Special Session, Dynamics of the Atmospheres and Interiors of the Terrestrial Planets: A Celebration of Gerald Schubert on his 60^{rmth} Birthday). These Davids were not the only graduate students named David that I had the good fortune to work with. There was also David Baker (also shown in Fig. 1); together we studied convection in the cloud level atmosphere of Venus.

15 STYLE OF MANTLE CONVECTION

In the early 1980s, Tilman Spohn spent several years with me as a postdoctoral researcher. We met at a Royal Society meeting in England organized by Keith Runcorn to discuss progress made in understanding the Moon. Runcorn, who spent much of his time on lunar geophysics, was a regular visitor at UCLA often spending weeks with those of us involved in interpreting the magnetic field data acquired during the Apollo era. One of the research topics Tilman and I worked on was the style of mantle convection, one-layer or two-layer, a hotly debated subject at the time. In a 1981 paper (Two-layer mantle convection and the depletion of radioactive elements in the lower mantle, G. Schubert and T. Spohn, Geophys. Res. Lett., 8, 951–954, 1981) we presented a strong case for single layer or whole mantle convection based on considerations of the distribution of radioactive heat sources in the mantle and the measured heat flow at the Earth's surface. We demonstrated that only a small percentage of the mantle's total content of radioactive elements could be in the lower mantle if the region underwent steady convection separate from the upper mantle. Geochemical models of a depleted upper mantle and an undepleted lower mantle had relied heavily on the assumption of separate upper and lower mantle convection cells. We argued that steady two-layer mantle convection required a depleted lower mantle and an undepleted upper mantle to match the observations of surface heat flow and mantle viscosity at odds with the geochemical model. The constraint on the abundance of radioactive elements in the lower mantle depended on the existence of at least one thermal boundary layer at the upper mantle-lower mantle interface with thickness comparable to that of the surface thermal boundary layer. Since the temperature drop across an interface boundary layer is limited by the relatively small difference between the solidus temperature and the upper mantle temperature, the amount of heat that could be transferred across this layer, if it existed, would be small compared with the surface heat flow. Gerald Wasserburg, the eminent Caltech geochemist, told me that this work was a major contribution to the debate on the style of mantle convection, a critique I look back on with considerable pride.

16 RELATIVE CONTRIBUTIONS OF COOLING AND RADIOACTIVE HEAT PRODUCTION TO THE GEOTHERMAL HEAT FLOW

Another contribution I consider among the most important of my research career involved the relative contributions of cooling of the Earth and radiogenic heat production in the Earth to the measured heat flow from the Earth. Knowledge of the relative contributions allows determination of the amount of radioactive elements in the Earth. This had long been debated and it still is. Lord Kelvin thought that the heat flow from the Earth was caused by the cooling of the planet from some high temperature. With the discovery of radioactivity it was realized that Kelvin's supposition could no longer be valid (It also gave a ridiculously young age for the Earth.). Eventually, with the acceptance of mantle convection, the pendulum swung in the opposite direction and it was assumed that the geothermal heat flow was due entirely to radioactivity in the Earth's mantle and that the amount of radioactive elements in the Earth could be determined by equating the radioactive heat production to the measured heat flow at the Earth's surface. I studied this problem and was able to show that there could not be a simple equilibrium between convective heat loss from the Earth and radioactive heat production. Instead, cooling of the Earth would have to contribute to the observed heat loss. The result meant that the radioactive heat content of the Earth could not be known until the relative contributions to the surface heat flow were determined. While some informed estimates of the relative contributions can be made, it is only certain that they must lie between 0 and 100%. My first attempt to publish this work was met with a rejection by Harmon Craig, an esteemed geochemist who was then editor of Earth and Planetary Science Letters. I eventually published this with colleagues in the Journal of Geophysical Research (Whole planet cooling and the radiogenic heat source contents of the Earth and Moon, G. Schubert, D. Stevenson, and P. Cassen, *J. Geophys. Res.*, **85**, 2531–2538, 1980.) The experience taught me that challenging the norm is difficult to do, perhaps especially in science.

17 GEODYNAMICS

By the late 1970s, Don Turcotte and I had spent more than a decade applying our knowledge of thermodynamics, heat transfer, fluid mechanics and solid mechanics to explain the world of geology. We appreciated that all of geology was a product of plate tectonic activity and that geologic phenomena could be understood using simple fluid and solid mechanical models. We realized that we were at the forefront of a new science, geodynamics, and that our background and experience positioned us to make major contributions to the field. It became apparent to us that geologists would need the tools of geodynamics to understand the objects of their labors. Those skills were not taught in the geology programs of that time yet they would be essential for practicing and future geologists. Thus was born the textbook Geodynamics by D. L. Turcotte and G. Schubert, first published by John Wiley and Sons in 1982. There followed two more editions published by Cambridge University Press in 2002 and 2014. The book teaches the subjects of fluid mechanics, solid mechanics and heat transfer and applies them to explaining geologic phenomena. The applications derive from many of our own research papers. I think it fair to say that Geodynamics is a classical textbook that is considered essential reading by geologists and geophysicists even 36 years after it was first published.

18 PIONEER VENUS

Before getting too far afield from my work in the 1970s and 1980s I need to backtrack a bit and talk about my role in the Pioneer Venus Multiprobe and Orbiter missions. The Pioneer Venus Orbiter mission inserted a satellite into orbit around Venus on December 4, 1978. The Pioneer Venus Multiprobe mission carried four entry probes on a separate spacecraft from the Orbiter spacecraft and inserted them into the Venus atmosphere on December 9, 1978. The orbiter and entry probes carried numerous experiments to characterize the atmosphere and surface of Venus. I was an Interdisciplinary Scientist on these missions and Head of the atmospheric dynamics working group. My Pioneer Venus colleagues included Richard Young and Alvin Seiff from NASA Ames Research Center, who I would later work with on the Galileo mission to Jupiter, and my students Anthony Del Genio and Curt Covey. The Pioneer Venus mission provided extensive new information on the geology of the surface and the structure and circulation of the atmosphere. I summarized and synthesized the results in a major paper that appeared in the first University of Arizona Press book on Venus (General circulation and the dynamical state of the Venus atmosphere, G. Schubert, in *Venus*, D. M. Hunten, L. Colin, T. M. Donahue, and V. I. Moroz, eds., University of Arizona Press, Tucson, Arizona, 681–765, 1983.). Among the ideas I put forth at the time was a prediction that the meridional circulation of the atmosphere might consist of layered "cells" and a prediction that the atmosphere and solid planet would exchange enough angular momentum to cause a variation of several minutes in the rotation rate of the planet. More recent observations and general circulation models have supported these predictions.

19 POROUS MEDIUM THERMAL CONVECTION

There were yet other lines of research that I pursued in the 1970s and 1980s. One was the nature of thermal convection in porous media, with application to geothermal systems. Joe M. Straus of the Aerospace Corporation (a former UCLA graduate student who obtained his Ph. D. under Fritz Busse) and I wrote a long series of papers on this topic. We studied how porous medium convection was influenced by the temperature and pressure dependences of the transport properties of water and how the coexistence of steam and liquid water modified convection. We applied the results of our two-phase studies to vapor-dominated geothermal systems finding that liquid water could overlie steam in such systems. We calculated the thermodynamic properties of steam-water-CO_2 mixtures (Thermodynamic properties for the convection of steamh-water-CO_2 mixtures, G. Schubert and J. M. Straus, *Am. Jour. Sci.*, **281**, 318–334, 1981) and incorporated them into our models of geothermal systems. We explored the basic fluid dynamics of porous medium thermal convection including the style of three-dimensional convection, its heat transfer characteristics, the sequence of transitions encountered with increasing convective vigor in time dependent convection, and the route to chaos in porous medium convection. In the latter study we were joined by my former postdoctoral scholar Shigeo Kimura.

20 GRAVITY WAVES AND ACOUSTIC WAVES IN THE ATMOSPHERE

In the mid-1980s I began to study the propagation of gravity waves and acoustic waves in the atmospheres of the Earth and planets like Venus and Jupiter with particular attention to how the waves modified the nightglow from excited atoms and molecules in the upper atmospheres of these bodies. Observations of these emissions can reveal a lot about the propagating waves and the structure and temperature of the atmosphere they pass through. While there were many observations to interpret, the theory required to do so was not adequately developed. I was joined in this effort by Richard Walterscheid, also at The Aerospace Corporation and a former UCLA graduate student (I served on his Ph. D. thesis committee) and Michael Hickey (Embry Riddle Technical University). This very productive collaboration has lasted to the present. We have studied the propagation of small-scale acoustic-gravity waves in the Venus atmosphere, gravity wave driven fluctuations in the Earth's OH nightglow, airglow fluctuations driven by tides and planetary waves, gravity wave heating and cooling in Jupiter's upper atmosphere, wave generation in Earth's atmosphere driven by tropical convection and tsunamis, and wave heating and Jeans escape in the Martian upper atmosphere, to name but a few of our many projects.

21 AT THE HEBREW UNIVERSITY IN JERUSALEM

In 1982 I took a year long sabbatical in the Geology Department at the Hebrew University in Jerusalem. I look back on it as one of the best years in my research career not only for the science I accomplished but also for the lifelong friendships I made and the opportunity to live and explore perhaps the most interesting and inspiring place on Earth. My wife Joyce and my daughter Tamara were with me and each weekend we visited a site of historic and biblical significance. Living in a non-English speaking country has always been a challenge for us and this time it was particularly difficult for Tamara who attended an Israeli high school. But in the end she learned a lot of Hebrew and made some good friends. At the university I tried particularly hard to interact with as many people as possible. I probably spent more time walking the halls and pestering people for conversation than I have ever done at UCLA. Among the colleagues I mostly worked with were Zvi Garfunkel, Arthur Reymer, and Ze'ev Reches. Zvi and I studied the upwelling of mantle material along the Dead Sea and Salton Trough-Gulf of California transforms. We also published an important paper (Mantle circulation and the lateral migration of subducted slabs, Z. Garfunkel, C.A. Anderson, and G. Schubert, *J. Geophys. Res.*, **91**, 7205–7223, 1986) in which we surveyed and identified those slabs undergoing retrograde subduction and determined how lateral slab migration influenced mantle circulation. Arthur and I worked together on the problem of how the continents grew over geologic time. We determined the rate of continental growth by measuring the crustal volumes of island arcs and constrained the times over which the island arcs formed. We published these results in an influential paper "Phanerozoic addition rates to the continental crust and crustal growth, A. Reymer and G. Schubert, *Tectonics*, **3**, 63–77, 1984". We also showed that certain segments of continental crust underwent rapid growth (Rapid growth of some major segments of continental crust, A. Reymer and G. Schubert, *Geology*, **14**, 299–302, 1986) and developed a model to relate continental growth to sea level changes (Continental volume and freeboard through geologic time, G. Schubert and A. P. S. Reymer, *Nature*, **316**, 336–339, 1985). I believe these papers made important contributions to the as yet ongoing debate about continental growth. My interactions with Ze'ev bore fruit in a couple of papers published after I had returned to UCLA. Ze'ev's interest in geology and behavior of faults led to the paper " Modeling of periodic great earthquakes on the San Andreas Fault: Effects of nonlinear crustal rheology, Z. Reches, G. Schubert, and C. Anderson, *J. Geophys. Res.*, **99**, 21983–22000, 1994".

22 THE GALILEO MISSION

In the mid-1990s I began a fruitful collaboration with John D. Anderson who was at the Jet Propulsion Laboratory at the time. We were both involved in the Galileo mission to Jupiter. I was an interdisciplinary scientist and John was responsible for use of the radio science data to determine the gravitational fields of the Jovian moons and to infer their interior structures from those data. The Galileo spacecraft was launched in 1989 and arrived at Jupiter at the end of 1995. It sent a probe into the Jovian atmosphere and orbited Jupiter 34 times passing by the moons Io, Europa, Ganymede and Callisto. The mission ended in 2003. Galileo carried an instrument suite that included a camera and a magnetometer, two instruments that I would become closely involved with in addition to the radio science and probe experiments.

John and I (and others) published a series of papers reporting the gravitational coefficients of the Galilean satellites (Io Europa, Ganymede and Callisto) and interpretations of what their interiors were like. We found that Io has a large metallic core (Galileo gravity results and the internal structure of Io, J. D. Anderson, W. L. Sjogren, and G. Schubert, *Science*, **272**, 709–712, 1996), Europa has a predominantly water ice-liquid outer shell and a deep interior that could be a mixture of metal and rock or pure metal (Europa's differentiated internal structure: Inferences from two Galileo encounters, J. D. Anderson, E. L. Lau, W. L. Sjogren, G. Schubert, and W. B. Moore, *Science*, **276**, 1236–1239, 1997), Ganymede is differentiated into a metallic core surrounded by a rocky mantle which in turn is surrounded by an icy shell (Gravitational constraints on the internal structure of Ganymede, J. D. Anderson, E. L. Lau, W. L. Sjogren, G. Schubert, and W. B. Moore, *Nature*, **384**, 541–543, 1996), and Callisto is essentially undifferentiated (Gravitational evidence for an undifferentiated Callisto, J. D. Anderson, E. L. Lau, W. L. Sjogren, G. Schubert, and W. B. Moore, *Nature*, **387**, 264–266, 1997).

23 ROTATION RATE OF SATURN

Some years later John and I used the data on Saturn's gravitational field obtained by the Cassini spacecraft along with Pioneer and Voyager radio occultation and wind data, to infer that Saturn rotated more rapidly than previously assumed based on magnetic field plus kilometric radiation data. That period was 10 hours, 39 minutes, and 22 seconds. We inferred a period of 10 hours, 32 minutes, and 35±13 seconds, a spin more rapid by nearly 7 minutes (Saturn's gravitational field, internal rotation, and interior structure, J. D. Anderson and G. Schubert, *Science*, **317**, 1384–1387, doi: 101126/science.1144835, 2007). This more rapid spin implies slower equatorial wind speeds on Saturn (than was previously assumed) and winds at higher latitudes that flow both east and west, as on Jupiter. I later collaborated with Peter Read and Timothy Dowling on a paper that inferred Saturn's rotation period from analysis of its potential vorticity (Saturn's rotation period from its atmospheric planetary-wave configuration, P.L. Read, T.E. Dowling, and G. Schubert, *Nature*, **460**, 608–610, 2009). We inferred a period about 2 minutes longer than that of Anderson and Schubert (2007) but still less than the long-wavelength radiation period. Because its magnetic field is almost perfectly axisymmetric and exactly aligned with its rotation axis Saturn's bulk rotation period is still uncertain.

24 BACK TO THE GALILEO MISSION

In addition to my role as an interdisciplinary scientist on the Galileo mission I was a co-investigator on the Atmospheric Structure Experiment on the Galileo Probe, a member of the Gravity Investigation Team, and an associate member of the Solid State Imaging Team. All these roles made it possible for me to directly participate in many of the exciting discoveries of the Galileo mission, one of the most scientifically prolific missions in all of solar system exploration.

The Atmospheric Structure Experiment on the Galileo Probe measured the vertical profile of temperature in Jupiter's atmosphere (Thermal structure of Jupiter's upper atmosphere derived from the Galileo Probe, A. Seiff, D. B. Kirk, T. C. D. Knight, L. A. Young, F. S. Milos, E. Venkatapathy, J. D. Mihalov, R. C. Blanchard, R. E. Young, and G. Schubert, *Science*, **276**, 102–104, 1997) and the depth profile of the zonal winds (Wind speeds measured in the deep Jovian atmosphere by the Galileo probe accelerometers, A. Seiff, R. C. Blanchard, T. C. D. Knight, G. Schubert, D. B. Kirk, D. Atkinson, J. Mihalov, and R. E. Young, *Nature*, **388**, 650–652, 1997). Significantly, we found that the zonal wind speed increased dramatically with depth in a region of wind shear between 1 and 4 bar and then persisted at high velocity down to at least the 17-bar level. The question remains just how deep the cloud level zonal winds extend. The recent Juno mission to Jupiter did not provide a conclusive answer to this long standing question (Origin of Jupiter's cloud-level zonal winds remains a puzzle even after Juno, D. Kong, K. Zhang, G. Schubert, and J. Anderson, PNAS, Vol.115, No.34. 8499–8504, 2018, (`http://www.pnas.org/cgi/doi/10.1073/pnas.1805927115`).

The images acquired by Galileo were revealing of Solar System objects and processes hitherto unseen. It was exciting to participate with members of the Imaging Team in their reports of these discoveries. The Team was led by Michael J. S. Belton (Kitt Peak National Observatory) and included many distinguished planetary scientists, too many to list here. The Team made interesting discoveries as Galileo travelled to Jupiter: the first close-up observations of the asteroid Gaspra, the first detection of a moon (Dactyl) of an asteroid (Ida), and the serendipitous observation of the impact of the fragments of comet Shoemaker-Levy 9 into the Jovian atmosphere. Once at Jupiter, the Team studied the Galilean satellites, the population of small satellites, the Jovian ring system, and the Jovian atmosphere. I was closely involved with the analysis and interpretation of Galileo's observations of extensive volcanism on Io (Active volcanism on Io as seen by Galileo SSI, A. S. McEwen, L. Keszthelyi, P. Geissler, D. P. Simonelli, M. H. Carr, T. V. Johnson, K. P. Klaasen, H. H. Breneman, T. J. Jones, J. M. Kaufman, K. P. Magee, D. A. Senske, M. J. S. Belton, and G. Schubert, *Icarus*, **135**, 181–219, 1998) and of the geologic features on the Galilean satellites indicative of the existence of subsurface water oceans on several of them, particularly on Europa (Evidence for a subsurface ocean on Europa, M. H. Carr, M. J. S. Belton, C. R. Chapman, M. E. Davies, P. Geissler, R. Greenberg, A. S. McEwen, B. R. Tufts, R. Greeley, R. Sullivan, J. W. Head, R. T. Pappalardo, K. Klassen, T. V. Johnson, J. Kaufman, D. Senske, J. Moore, G. Neukum, G. Schubert, J. A. Burns, P. Thomas, and J. Veverka, *Nature*, **391**, 363–365, 1998).

Before leaving this brief discussion of my experience with the Galileo Imaging Science Team I should mention that I had worked together with Michael Belton early in my career on the interpretation of the features seen in the ultraviolet images of Venus taken by the Mariner 10 spacecraft (Cloud patterns, waves and convection in the Venus atmosphere, M. J. S. Belton, G. R. Smith, G. Schubert, and A. D. Del Genio, *J. Atmos. Sci.*, **33**, 1394–1417, 1976). This connection probably facilitated my later interaction with the Galileo Imaging Science Team, an experience that was one of the most satisfying of my participation in NASA missions. Members of the Team were welcoming and sharing of their data and functioned without animosity, a tribute to the Team members and their leader. This was particularly impressive to me because I had been an original member of the Team and left it because I did not take well to the heavy load of mission planning. Nevertheless, I was welcomed back into the fold later in the mission.

Probably at the top of the list of amazing and surprising discoveries by the Galileo spacecraft were those made by the onboard magnetometer. The Principal Investigator of the Galileo Magnetometer instrument was my UCLA colleague Margaret Kivelson. Though I was not a formal member of the Magnetometer Team I was invited to join Margaret and the Team in the interpretation of the magnetic field measurements. This was another prime example of the unselfish cooperation in scientific research that characterized the Galileo mission. The Galileo magnetometer discovered that Ganymede had a magnetic field (Discovery of Ganymede's magnetic field by the Galileo spacecraft, M. G. Kivelson, K. K. Khurana, C. T. Russell, R. J. Walker, J. Warnecke, F. V. Coroniti, C. Polanskey, D. J. Southwood, and G. Schubert, *Nature*, **384**, 537–541, 1996) an observation consistent with and confirming of the existence of a metallic core inside Ganymede inferred from the gravitational data (The magnetic field and internal structure of Ganymede, G. Schubert, K. Zhang, M. G. Kivelson, J. D. Anderson, *Nature*, **384**, 544–545, 1996). Not only did Ganymede have a metallic core but the core had to be sufficiently molten to support an active dynamo. In addition to Earth and planet Mercury, Ganymede is the only other terrestrial-like body in the solar system with a present-day magnetic field. The Galileo magnetometer made another startling discovery, global liquid water oceans beneath the surfaces of Europa and Callisto (Induced magnetic fields as evidence for subsurface oceans in Europa and Callisto, K. K. Khurana, M. G. Kivelson, D. J. Stevenson, G. Schubert, C. T. Russell, R. J. Walker, S. Joy, and C. Polanskey, *Nature*, **395**, 777–780, 1998). The liquid water oceans were detected by the realization that certain magnetic field fluctuations measured by Galileo were produced by electric currents induced in the oceans by time variations of Jupiter's magnetic field sensed by the satellites due to their orbital motion and the tilt of the Jovian magnetic field. Later, a similar analysis of magnetic field fluctuations measured by Galileo in the vicinity of Io indicated that it had an internal magma ocean (Evidence of a global magma ocean in Io's interior, K. K. Khurana, X. Jia, M.G. Kivelson, F. Nimmo, G. Schubert, and C.T. Russell, *Science*, **332**, 1186–1189, doi: 10.1126/science.12014252011, 2011).

25 THE MANTLE CONVECTION BOOK PROJECT AND LOS ALAMOS

In 2001 a decade-long book writing project came to fruition with the publication of "*Mantle Convection in the Earth and Planets*, G. Schubert, D. L. Turcotte, and P. Olson, Cambridge University Press, Cambridge, England, 2001, 956 pp.". This was a major milestone. We attempted to cover the entire subject of mantle convection and even with three coauthors the task was daunting. Mantle convection was and is a rapidly changing field, but the book has lasting value especially for its presentation of the basic fluid dynamics involved in mantle dynamics.

Since the subject of mantle convection has just come up again it is an opportunity to mention the mantle convection workshop that was held at the Los Alamos National Laboratory for a number of years in the late 1980s. The workshop was sponsored by the Los Alamos branch of the Institute of Geophysics and Planetary Physics of the University of California. Chick Keller led the Institute and enthusiastically supported the workshop. We met for several weeks each summer. We had discussions and presentations but mostly we established collaborations and carried out research in real time. It was during these workshops that I established close connections with Gary Glatzmaier, Peter Olson, Charles Anderson and Bryan Travis, colleagues with whom I co-authored many papers on topics in mantle convection. Visiting Los Alamos, which I did throughout the year outside of the workshops, also provided an opportunity to enjoy the remarkable scenery in that part of New Mexico, to ski at the resorts of Taos and Santa Fe, and to experience southwestern cuisine at the excellent local restaurants.

26 THE TREATISE

A book project that in retrospect represents a challenge I would not even consider taking on today was the *Treatise on Geophysics*, a major reference work published by Elsevier in 2007. A second edition was published in 2015. The Treatise is a multi- volume (11 volumes in all) exposition of all aspects of geophysics. I was Editor-in Chief and assembled a distinguished team of editors to oversee the individual volumes dealing with Deep Earth Seismology (Volume 1), Mineral Physics (Volume 2), Geodesy (Volume 3), Earthquake Seismolgy (Volume 4), Geomagnetism (Volume 5), Crustal and Lithosphere Dynamics (Volume 6), Mantle Dynamics (Volume 7), Core Dynamics (Volume 8), Evolution of the Earth (Volume 9), Physics of Terrestrial Planets and Moons (Volume 10), and Resources in the Near-Surface Earth (Volume 11). It amazes me that my editorial team and I managed to make the Treatise happen given the large number of contributors involved and the publication deadlines that had to be met. In the end I believe it was all worthwhile given the lasting value of this extraordinary work in the face of new developments in the different fields.

27 KEKE ZHANG AND GIANT PLANET INTERIORS

In the mid-1990s I began a serious and long lasting collaboration with Keke Zhang a Professor at the University of Exeter. I had known Keke a long time before that. He was a graduate student at UCLA earning his Ph. D. under my colleague F. H. Busse. I served on Keke's Ph. D. thesis committee. Our joint work has covered a variety of topics in magnetohydrodynamics, thermal convection in rotating systems, and dynamo theory with applications in planetary physics and astrophysics. In about 2010 we (Keke, I, and his then student Dali Kong) embarked on a study of the shapes and internal structures of rapidly rotating planets and stars and the external gravitational signatures of these objects (Shapes of two-layer models of rotating planets. D. Kong, K. Zhang, and G. Schubert, *J. Geophys. Res.*, **115**, E12003, doi:10.1029/2010JE003720, 2010). We had a major long-term goal in mind, interpretation, in terms of internal structure and wind systems, of the gravitational measurements to be made by the spacecrafts Juno and Cassini in their orbits around Jupiter and Saturn. We developed a rigorous and exact theory to determine the external gravitational field of a rapidly rotating body with arbitrary internal structure. This problem had never been solved though it had been studied by renowned physicists and mathematicians for decades. The problem had always been dealt with by employing approximate methods based on small departures from sphericity of the shapes of the bodies. Our solution involved a self-consistent perturbation approach in which the leading-order problem accounted exactly for rotational distortion, thereby determining the basic shape, internal structure, and gravitational field of the planet (The shapes, internal structures, and zonal wind systems of rapidly rotating planets and stars and how these properties are revealed in the gravitational signatures of the bodies, K. Zhang, D. Kong, and G. Schubert, *Ann. Rev. Earth Planet. Sci.*, **45**, 419–446, doi:10.1146/annurev-earth-063016-020305, 2017). The Juno and Cassini missions have now returned the gravitational data we had looked forward to and we are now in the process of applying our theory to their interpretation.

Before leaving the discussion of my work on giant planet interiors I must mention the collaboration I have had with Ravit Helled. She was a postdoctoral researcher with me for several years beginning about 2008. She is now a Professor in the Center for Theoretical Astrophysics and Cosmology at the University of Zurich. We co-authored a number of papers on the interior structures and shapes of giant planets and exoplanets, the formation of giant gaseous protoplanets, and the spin rates of Jupiter and Saturn.

28 ZVI BEN-AVRAHAM AND PLATE TECTONICS

A very different line of research opened for me in about 2005 when Professor Zvi Ben-Avraham (Tel Aviv University and University of Haifa) spent a half year sabbatical at UCLA. The major focus of our work was understanding the state of stress and faulting in Africa, a project we carried out with my UCLA colleague Peter Bird (Patterns of stress and strain rate in southern Africa, P. Bird, Z. Ben-Avraham, G. Schubert, M. Andreoli, and G. Viola, J. Geophys. Res., 111, B08402, doi:10.1029/2005JB003882, 2006). We examined fault orientation data and confirmed the dominant pattern of NW-SE directed most compressive horizontal principal stress widely known as the "Wegener stress anomaly". It had earlier been attributed to ridge push generated by the South West Indian Ridge. Instead, we found that the Wegener anomaly is actually caused by NE-SW extensional tectonic stress resulting from the resistance of unbroken lithosphere to the relative rotation of the Somalia plate away from the Africa plate. We also studied the formation of deep basins along strike-slip faults such as the Dead Sea fault, the causes and global consequences of abrupt changes in plate motion, and the possibility of rapid slab detachment at the collision of the Ontong Java plateau with the northern Melanesian arc.

29 BACK TO VENUS

In recent years most of my research has focused on Venus' atmosphere and the interiors of the outer planets. I have probably already said enough about my work with the outer planets, but I did fail to mention a paper I published with David Bercovici in which we explored using the techniques of helioseismology to learn about the interior of Jupiter (Jovian seismology, D. Bercovici and G. Schubert, *Icarus*, **69**, 557–565, 1987), likely the first discussion of this possibility.

Let me add some words about my recent studies of Venus. I have been fortunate to work with Sebastien Lebonnois, Directeur de Recherche and CNRS senior scientist at Laboratoire de Meteorologie Dynamique, Sorbonne Universités Paris and Thomas Navarro, a postdoctoral researcher with me. I have also benefitted from my role as an Akatsuki Participating Scientist in the Japanese mission to Venus. With Sebastien Lebonnois and others I have studied the dynamics of the Venus atmosphere as revealed in numerical General Circulation Models. We have investigated the nature of the planetary boundary layer at the surface of Venus (Planetary boundary layer and slope winds on Venus, S. Lebonnois, G. Schubert, F. Forget, and A. Spiga, *Icarus*, **314**, 149–158, `https://doi.org/10.1016/j.icarus.2018.06.006`, 2018) and have offered a possible explanation for the unstable temperature gradient measured by the VeGa-2 probe at Venus's surface (The deep atmosphere of Venus and the possible role of density-driven separation of CO_2 and N_2, S. Lebonnois and G. Schubert, *Nature Geoscience*, **10**, 473–477, doi:10.1038/NGEO2971, 2017).

The Akatsuki spacecraft made a major discovery in 2017 that has dramatically altered our understanding of Venus's atmosphere. It found a 10,000-km-long meridional structure at the top of the cloud deck of Venus that appeared stationary with respect to the surface and was interpreted as a gravity wave induced by flow over Venus's equatorial highlands. This indicates a direct influence of the solid planet on the whole Venusian atmosphere despite dissimilar rotation rates of 243 and 4 days, respectively. In a seminal study led by Thomas Navarro (Atmospheric mountain wave generation on Venus and its influence on the solid planet's rotation rate, T. Navarro, G. Schubert, and S. Lebonnois, *Nature Geoscience*, **11**, No. 7, `https://doi.org/10.1038/s41561-018-0157-x`, 2018) we determined how the mountain waves (it is now known that there is more than one) are generated and discovered that they substantially contribute to the total atmospheric torque that acts on the planet's surface. We estimated that the mountain waves, along with the thermal tide and baroclinic waves, could produce a change in the rotation rate of the solid body of about 2 minutes per solar day. This interplay between the solid planet and atmosphere might explain some of the different rotation rates (equivalent to a change in the length of day of about 7 minutes) measured by spacecraft over the past 40 years. Understanding the unusual rotation rates of Venus's atmosphere and solid body has been a long term goal of mine. I believe we have shed some light on the physical processes involved in the exchange of angular momentum between the atmosphere and solid body.

30 CONCLUDING COMMENTS

It is probably more than time to bring these reminiscences to a close. I must acknowledge my good fortune to have been in on the ground floor during the plate tectonic revolution and the first visits to many of the planets and moons of our solar system. It has been a golden age of exploration and discovery for me. If only it would be possible to witness what lies ahead as we continue to expand our knowledge of our solar system and the stellar and planetary systems that lie beyond.

It has been a challenge for me to write these reminiscences. It is not easy to decide what to include from a list of nearly 570 papers and a career spanning many lines of research over a period of more than 50 years. I have tended to highlight papers that I thought were particularly original with new ideas and first discoveries. At the outset of these reminiscences I mentioned how important new ideas were to my research. However, what I perceive as important contributions may not be how others would evaluate my work. So, despite my efforts not to do so, I have probably failed to mention some important papers and some people with whom I've had significant collaborations. If you have read this and fall into that category, my apologies.

I have been fortunate indeed to have worked with so many outstanding individuals, undergraduate students, graduate students, postdocs, and colleagues. They all make me proud as I witness their achievements and scientific contributions. Most importantly, many of these associations have turned into long lasting friendships. Much to my satisfaction, I continue to do research and publish with students and postdocs from decades ago. At the end of these reminiscences I have appended a list of the students and postdocs with whom I've worked.

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R. E. Young, B. M. Cordell, P. J. Thomas, B. R. Lichtenstein, B. L. Horning, D. A. Yuen, A. D. DelGenio, R. J. Terrile, A. Tovish, E. Fishbein, L. E. Roth, C.C. Covey, D. T. Sandwell, K. Ellsworth, M. N. Ross, M. Newman, P. J. Thomas, D. Bercovici, P. McGovern, Z.-P. Sun, R. D. Baker, II, D. Limonadi, W. B. Moore, J. T. Ratcliff, S. W. Asmar, E. P. James, J. P. Devaux, S. Musotto, J. L. Palguta, C. Milbury, J. Austermann.

Postdocs

T. Spohn, S. Kimura, R. C. Paliwal, M. Ozawa, G. Baer, A. Guest, H. F. Parish, X. Zhan, R. Helled, T. Navarro.

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