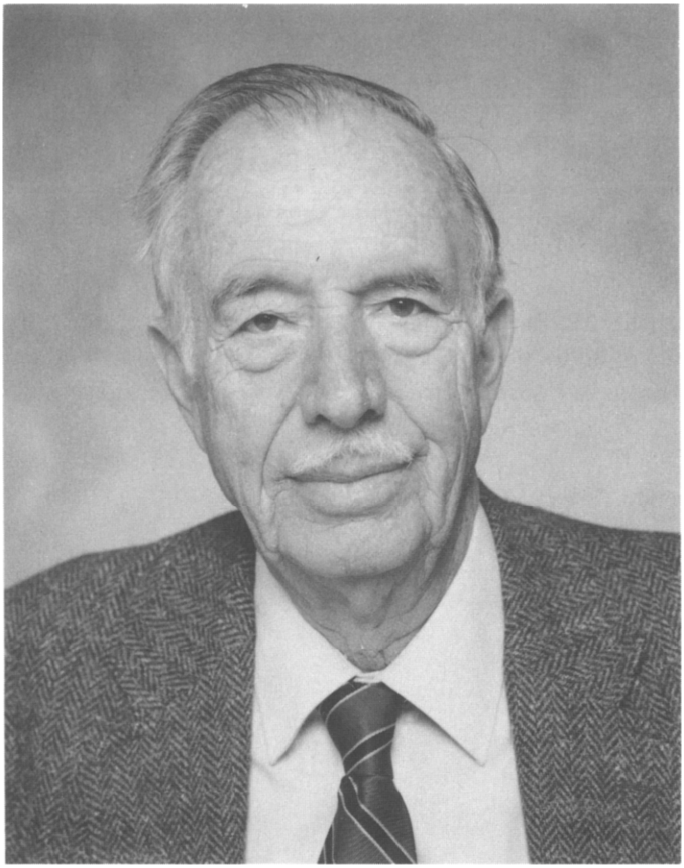
Coordination Chemistry Reviews, 110 (1991) 1–16
Elsevier Science Publishers B.V., Amsterdam



Arthur W. Adamson

Art Adamson is Emeritus Professor of Chemistry at the University of Southern California in Los Angeles, California. He was born in Shanghai, China in 1919 and educated at the University of California (Berkeley) and at the University of Chicago, from which he received his Ph.D. in 1944. After a brief period at Oak Ridge National Laboratory working on the Plutonium Project, he joined the faculty at the University of Southern California in 1946 where he has been ever since. He was chairman of the Chemistry Department at U.S.C. from 1972 to 1975.

Arthur has received numerous honors and awards, most notably three major awards given by the American Chemical Society: The Kendall Award in Colloid or Surface Chemistry (1979); the ACS Award for Distinguished Service in the Advancement of Inorganic Chemistry (1982); and the ACS Award in Chemical Education (1984). Other notable honors include the Unilever Professorship at the University of Bristol, U.K., the Tolman Medal of the Southern California section of the ACS, the creative Scholarship and Research Award of the University of Southern California, the Alexander von Humboldt Senior Scientist Award, and the Harry and Carol Mosher Award of the Santa Clara section of the ACS. He has been designated Distinguished Emeritus Professor by the University of Southern California.

Professor Adamson is often referred to as the "father" of inorganic photochemistry. In addition, he has also made outstanding contributions to the field of surface chemistry. He has contributed over 250 publications to the literature and has authored four books, *The Physical Chemistry of Surfaces*, *Concepts of Inorganic Photochemistry*, *A Textbook in Physical Chemistry*, and *Understanding Physical Chemistry*, some of which have been translated into Russian, Chinese, and other languages. Over the course of his long and fruitful career, he has been associated with almost 90 graduate students, postdoctoral fellows and visiting faculty. He was the founding editor of the ACS journal *Langmuir*, and has been Plenary Lecturer at innumerable symposia.

A CELEBRATION OF INORGANIC LIVES

INTERVIEW OF ARTHUR W. ADAMSON
(University of Southern California)

ROBERT BAU

R.B. Art, perhaps we could start off with an account of your personal background.

OK, I was born in Shanghai, China. No, my father was not a missionary: he graduated in civil engineering from Iowa State at Ames and was sent by the YMCA to China. There, he supervised the construction of several YMCA buildings, a notable one being in Shanghai ... it's there to this day as a government youth center. The building method was a novel one. Shanghai lies on a river delta and sits over hundreds of feet of sand and soil. Rather than sink pilings that deep, as had been the custom, my father supported the building on a sunken concrete raft. His first wife died in childbirth and he later met and married my mother in China. I was a fairly late child. I'm told that I crossed the Pacific five times on visits to my mother's family in the San Francisco Bay region, but only remember dimly the final trip at age six. My memories of China are fragmentary but I was certainly brought up in the atmosphere of an old China family. My father was subsequently appointed the construction engineer for the Jerusalem YMCA, now a landmark building across from the famous King David Hotel. Lack of suitable schools in Palestine led my mother and me (and, for a while, my half-brother) to live in Switzerland; there, I went to French-speaking schools at the age of ten, for three years. When the family returned to the States in 1933, I started high school in New Jersey. Interestingly, my three years of Swiss schooling covered five years of American education. Maybe things haven't changed all that much in the matter of quality of education in this country! It was then the height of the great depression and the family moved around a good deal. I graduated from a Long Beach high school in 1935 at the age of 16. I have dwelt on this family history, I suppose, because it explains how it was that I went to some twenty different schools before entering college! This moving around prevented my forming lasting school friends and for this reason, as well as by temperament, I was a loner and rather shy. I was an avid reader as a child, including the Tom Swift books and *Popular Mechanics*, but it was my high school teacher that cemented an interest in chemistry, an interest that has been with me all my life. It was a benefit not to be uncertain as to what I wanted to do.

R.B. I was just wondering, since you mentioned that your high school teacher started your interest in chemistry, whether there was any particular incident that might have triggered this interest?

It was probably my last year in high school that I was taking chemistry; no, she wasn't charismatic or outstanding in any way, but was a good competent teacher. What I do remember is an experiment I did. I got some crude oil from Signal Hill, put it in a wash basin and set it on fire. I had a bottle of dilute acid with a container of sodium bicarbonate inside and equipped with a stopper and a spout. When I inverted the bottle, the reaction produced CO_2, forcing out the water along with some foaming agent . . . I was able to put out the fire – just barely! Anyway, that's my memory of my first experience in experimental chemistry.

R.B. After that, you started your undergraduate studies at Berkeley. Could you tell us something about that?

I spent a semester at a local junior college because I was a mid-year graduate, but then I went on to UC Berkeley in the fall of 1936. The four years there were impressive. I well remember Joel Hildebrand and his lecture demonstrations and, later, W.F. Giauque whose thermodynamics class was half-populated with very intimidating graduate students. Other memories include going, in my senior year, to seminars presided over by cigar-smoking G.N. Lewis. Melvin Calvin was a TA in my organic course; Meryl Randall had a penetrating voice and one asked not whether one had seen Randall, but rather whether one had heard him! The inorganic course under Wendell Latimer featured a cigarette which switched from one side of the mouth to the other as he paced back and forth. I did learn a lot from him, apart from watching that cigarette. There was an air of great excitement in those days – cyclotrons, radioactive isotopes, etc. I did a senior research project under Willard Libby. It was an exchange study involving colloidal MnO_2 and the work was mentioned in a footnote in one of Libby's publications. I joined the Chemistry Club; I think at one time I was President of it. We used to sell pies around the Freshman Lab building to raise a little money. That was my story at Berkeley.

R.B. What made you decide to go on to graduate school?

I am sure it was the senior research project and the atmosphere at Berkeley that led me naturally to want to go to graduate school and somehow I fixed my mind on the University of Chicago, I don't remember exactly why. There was a problem, however; Latimer ran a very tight ship in telling seniors where they could go to graduate school if they wanted a recommendation from him for a TA-ship. I forget what his choice was for me, but I didn't like it and was able to bypass this operation of his by virtue of a small inheritance which, along with waiting tables, carried me through the first year at Chicago. Again, out of ignorance of the usual procedure, I took and passed the Preliminary Examination during that first year and was thus eligible for research before having picked a research director. After talking around some, I was impressed by George Boyd's enthusiasm and work with monomolecular films and became, I think, his first Ph.D. student. George had been a student of W.D. Harkins and since space was tight – what's new about that? – I set up my film balance in Harkins' laboratory. Harkins made daily inspections of work in progress and as he passed my set-up, never failed to remark that most of the equipment was his. I came to know George Jura, then a senior graduate student in the same laboratory and, later, on the faculty at Berkeley. The money problems eased; a TA-ship and then a Fellowship provided enough to live on, about $1500 a year, and to marry.

R.B. I gather then, that Virginia and you were married while you were a graduate student?

That's right. I had met and wooed Virginia Dillman back in Berkeley and returned there in 1942 and we were married. Things were changing quickly, what with Pearl Harbor and World War II. Boyd joined the Manhattan Project and so did I, working on plutonium purification by ion exchange. This was after some soul searching as to whether to enlist or to accept deferment. Our laboratory in Chicago was at that time in the West Stands, over the first "pile" or nuclear reactor and we did notice a rising level of background radiation. The group moved to Oak Ridge, then a sea of red mud and, for a young couple, adventurous living. The first of three children was born. I completed my Ph.D. thesis on the theory and practice of ion exchange and passed the Final Examination in Chicago in 1944. I well remember James Franck as a wonderful gentleman on my Committee and a person who would ask a seminar speaker simple basic questions which often turned out to be very penetrating. Other memories are of T.F. Young ("Tuffy"), Frank Westheimer and Maurice Kharash. The war ended. I knew I wanted to go into academic work and sent out applications. I turned down a couple of offers to decide to go to the University of Southern California, partly because we wanted to get back to California. The academic salary was a good one, $3600 for a nine-month year. I've been at USC ever since and have always felt that the choice worked out well for me.

R.B. I guess when you started at USC, things were very different ... funding was not the same as it is now and you had to start in a new area of research.

Well, circumstances were fortunate again. My thesis work and immediate expertise were classified so I couldn't work in that area and was compelled to change my field. The training in handling radioactive materials gave me the thought of using radio isotopes to obtain ion-self diffusion coefficients in aqueous solution and also the self-diffusion coefficient of water using deuterium labeling. I will mention one of my early Ph.D. students, Ray Irani, because of his unique career. Ray is now President of Occidental Petroleum and presently the top man, following the death of Armand Hammer. A second idea was that of studying ligand exchange with cyano complexes using ^{14}C-labeled cyanide ion. Parenthetically, the literature method for making labeled $Na^{14}CN$ was laborious and we invented a preparation from $Ba^{14}CO_3$ using sodium azide (the material I think now used in automobile airbags). During the course of the exchange studies, we made what the literature said was $K_4Co(CN)_6$ but found instead that the compound analyzed to be $K_3Co(CN)_5$. Most unusually, it was green in solution and purple as a solid. This was in 1951 and, for a while, some publications graciously called this compound "Adamson's salt". The potassium salt crystallized in randomly stacked flakes and could not be used for crystallography, but the barium salt looked promising. There was a twinning problem, however, and I guess that you, Bob, can appreciate the difficulty. Later, Larry Dahl's group found a useable crystal and in 1972 we published a joint paper on the structure. It turned out that the solid compound was diamagnetic through Co–Co bonding and that it dissociated in solution to become paramagnetic. Our early papers, incidentally, supplemented, to some extent, the inner–outer sphere ideas of Henry Taube.

You mentioned the early situation, Bob. There was very little research money in those days but I got a small Research Corporation grant and then, at first jointly with Wayne Wilmarth, one of the first Office of Naval Research contracts. The Navy was interested in developing closer ties with scientists much as had happened between the Army and nuclear scientists. I started to go to meetings and one of the early ones was the 1953 International Conference on Coordination Chemistry, the first true ICCC by my reckoning. I remember Lars Gunar Sillèn and his earthy Scandinavian folk sayings and Latimer's beautiful paper on redox reactions, unintelligible to the Europeans because of his clipped speech. I met Jannick Bjerrum there and the next year, my wife, three daughters and I spent our first sabbatical leave in his laboratory. This was supported by one of the early National Science Foundation sabbatical leave grants. Fred Basolo turned up to share the same lab with me, also on a sabbatical leave. We and our families came to know each other and have been friends ever since. I learned more about Werner complexes; in fact, Fred and I published a joint paper on an aquation kinetics study.

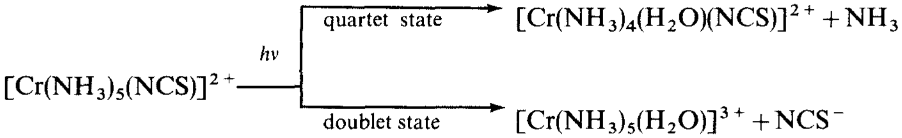
R.B. Was it around this time that your interest in inorganic photochemistry began?

You might say so, but in a way it was accidental. In one of our exchange studies, it was with the $[Mo(CN)_8]^{4-}$ complex, we had the puzzling result that the exchange with cyanide ion was zero-order both in complex and in free ligand. What we finally realized was that there was new fluorescent lighting – not too common in those days – and that we were getting photochemistry. In fact, we even estimated a quantum yield! Also, I had learned in Bjerrum's lab that many ammine complexes were sensitive to sunlight and, remembering the octacyanide complex, got the idea of doing physical-chemical and kinetic photolysis studies on coordination compounds. At first we used a home-made bolometer but then calibrated aqueous Reinecke's salt, *trans*-$K[Cr(NH_3)_2(NCS)_4]$, as a reliable actinometer and a much more sensitive one than the old uranyl oxalate system. Moreover, it was applicable to much longer wavelength light than the newer ferrioxalate actinometer. Well, the photochemistry proved to be a fascinating topic and it was a field not crowded with other researchers, so that interesting systems abounded. We felt like a dog in a meat locker in not always knowing where to bite next. As a comment, the development of ligand field theory was going along apace then and I do think that both it and our photochemical work would never have gone as far, or as rapidly, without the recording spectrophotometer. Before its advent, data could be obtained only laboriously with the point-by-point capability of the classic model DU Beckman instrument.

R.B. It must have been at this point that your photochemistry research really started to take off.

It was a prolific and exciting period. Now, it occurs to me that I'd better comment on a problem in talking about this. Many excellent graduate students and post-docs are involved in the work as well as in surface chemistry, and I would like to give them specific credit, yet to do so selectively would be unfair and to be complete would just be too long. Accept, Bob, that the credit is there, but I'll only be giving it indirectly. We, a real we and not a royal "we", determined a lot of quantum yields; but the Copenhagen experience and that of a second sabbatical in 1962 in Ron Nyholm's laboratory, nourished an abiding interest in mechanisms and reaction pathways. Photochemical reactions of coordination compounds had generally been thought to be photocatalyses, that is merely to be accelerations of thermal reactions. We found out that, especially with chromium ammines, the photochemical solvation or aquation reaction might be antithermal. That is, the photolabilized ligand was not always the thermally labile one. This led, in due course, to a formulation of some rules, that came to be known as "Adamson's rules", whereby we could generally predict photochemical products. We could also pretty well predict which isomer would be formed if there was more than one possibility and approximate quantum yields as well as their wavelength dependence. The rules generated interest... I know of some persons whose careers seemed largely devoted to confirming or disproving the rules.

Another stage came along in 1967. Gerald Porter and Hans Schläfer at Frankfurt wrote a landmark paper on the fluorescence and phosphorescence of chromium(III) hexaurea and later, in 1971, Cooper Langford and Noel Kane McGuire published a paper showing that emission from chromium(III) complexes was common. Emission had not been much noticed before then, since it occurred at a longer wavelength than would be expected by analogy to organic emitters. Well, all this got us interested in relating emission to photochemistry. The Office of Naval Research helped us get a big naval gun, as we called it: a twice-amplified, neodymium glass laser capable of giving two joules in a single frequency-doubled pulse. We wanted a single-pulse system since photochemistry would be occurring and signal averaging of repeated pulses would not work; also, noise would be less and the system allowed interactions between emission and product appearance rates to be seen. We found patterns of behavior, and again some rules relating emission lifetime to ligand properties. One of the goals in our work was to identify the excited state that was involved in the chemical reaction. The early supposition, especially of the Frankfurt group, was that, with chromium(III) ammines, the emitting low-lying doublet state responsible for the phosphorescence was also the reactive stage. This was somewhat in analogy to organic photochemistry and triplet state reactivity. Adamson's rules, however, made the best sense if the first quartet excited state were the reactive one, and this conclusion was again the subject of much interest and of various attempts to prove or disprove it. Theoretically minded people, such as Jeff Zink at UCLA, came to our support. Where two different photochemical substitution reactions occurred, it seemed necessary to assign one to the quartet and the other to the doublet state as, for example:



$$[Cr(NH_3)_5(NCS)]^{2+} \xrightarrow{h\nu} \begin{cases} \xrightarrow{\text{quartet state}} [Cr(NH_3)_4(H_2O)(NCS)]^{2+} + NH_3 \\ \xrightarrow{\text{doublet state}} [Cr(NH_3)_5(H_2O)]^{3+} + NCS^- \end{cases}$$

In this and other cases, it would have been very easy to get into polemics. This never appealed to me; I preferred to leave a few footprints and to move on. However, throughout this work, we did emphasize that chemical reaction occurred from thermally equilibrated excited states, rather than from prompt excited state or prompt hot ground state species. The word *thexy* was coined to denote such thermally equilibrated states. A thexy state has its own structure, reactivity, thermodynamic properties, etc. It is a chemical isomer of the ground state and a perfectly good chemical species.

R.B. In addition to the large amount of work you did on chromium complexes, what about complexes of other metals?

Chromium complexes were fun to work with because emission and photochemistry provided interacting probes, useful in elucidating mechanistic details and because their thermal substitution chemistry was well known. We did look at many other families of complexes, however. Cobalt(III) ammines and acidoammines were prone to photoredox decomposition, and we published in 1958 an accepted cage mechanism to explain how the decomposition could be accompanied by extensive ligand substitution of the parent complex. Such complexes seemed photoinert to longer wavelength visible light, that is to light in the first ligand field band, and well away in wavelength from the charge transfer band, but we found that there was actually a rich photosubstitutional chemistry although at a very low quantum yield level. To make the measurements, we made an early use of an argon ion laser and the reaction cell was a 10 cm path length, 1 mm diameter micropolarimeter cell. The fact that the quantum yields were low was of no great theoretical interest since it merely meant that radiationless deactivation was a good competitor. What we did find was that the photochemical pattern was distinctly different from that of the chromium(III) ammines. We also looked at rhodium(III) ammines and complexes of platinum, iridium, iron, manganese and other metals, as well as those with a variety of chelating ligands, including some sulfur- and selenium-bonded ones. As an example of a chelation effect, *trans*-$[Cr(en)_2Cl_2]^+$ photoaquates easily, yet the seemingly analogous *trans*-$[Cr(cyclam)Cl_2]^+$ is quite photoinert. We were proud that, using our rules-related experiences, this photoinertness was actually predicted in one of our research group discussions and I mean predicted.

An important series was with carbonyl complexes of the type $M(CO)_5L$ where M is a group VI metal and L a substituted pyridine. Following an observation of low-temperature emission by Mark Wrighton and his group, we found room-temperature emission as well and could develop mechanistic excited state schemes relating emission and photosolvation of L. An idea that developed out of a luncheon conversation at the Faculty Center was that of photocalorimetry, the measurement of the heat of an overall reaction, but one that was photochemically driven. The point was that, by doing it this way, we could obtain clean reactions where the desired thermal reaction either didn't occur or did not occur cleanly. The first photocalorimeter was developed with Arnd Vogler at the University of Regensburg during a sabbatical leave supported in part by an Alexander von Humboldt Fellowship. Other ventures were in studies of chemiluminescence. An outstanding example and the basis for a *Journal of Chemical Education* article is the amazingly strong chemiluminescence on reducing $[Ru(2,2'\text{-bipyridine})_3]^{3+}$. There was a series of papers on photochromism, notably of metal dithizonate complexes.

R.B. What was the story behind one of your major discoveries, that of the intriguing photochemistry of $[Ru(2,2'\text{-bipyridine})_3]^{2+}$*?*

Well, maybe not major in terms of time spent, but yes it's quite a story. I should back up a little first. Sensitization had not been observed with Werner complexes before, yet we felt that it should be possible. We were uncertain about it and to sort of "copper our bet", our first attempt in 1968 was with a cobalt(III) ammine having a stilbene-containing ligand. The idea was to have the potential sensitizer in the same molecule with the metal ion. This worked, and the success led in the same year to finding sensitized decomposition of other ammines using solute sensitizers such as benzophenone; so that good kinetic, that is Stern–Volmer, plots could be obtained. Now for the story of $[Ru(2,2'\text{-bipyridine})_3]^{2+}$, which we came to call "Rubipy". A new arrival to our group brought some interesting information on the emission properties of such bipyridine complexes and, with our discovery of sensitization in mind, we found that Rubipy could, in fact, act as a sensitizer for $[PtCl_4]^{2-}$ aquation. Another post-doc rather quickly found that Rubipy could sensitize the redox decomposition of cobalt(III) ammines but with the unexpected appearance of the ruthenium(III) complex as a product:

$$[Ru(bipy)_3]^{2+} + [Co(NH_3)_5Br]^{2+} \xrightarrow{h\nu} [Ru(bipy)_3]^{3+} + Co^{2+} + \text{ligand products}$$

We concluded that excited state electron transfer, rather than energy transfer, had occurred and proposed this in 1972. It is amazing, in retrospect, how controversial this idea was, to the point of holding up publication on our communication. We had to fight to get it published. The correctness of our proposal was fully established by subsequent work in our laboratory, but perhaps especially in those of others such as Norman Sutin. Now I should say that the idea of excited state electron transfer came easily to us because of earlier studies on photoelectron production with ferrocyanide and molybdenum octacyanide ions and other complexes. And I might add that, once we realized what was going on, it was not a big step to start talking about hypothetical "water splitting" reactions. Aquated electrons reduce water as presumably could a suitable reaction partner in the redox reaction with Rubipy, and the ruthenium(III) trisbipyridine product would oxidize water ... so in lecture and seminar talks in those days, I was putting up hypothetical water-splitting schemes. Anyway, excited state electron transfer opened up new vistas for many people. The purists, you might say, had a new regime of high driving force electron transfer reactions to apply Marcus theory to. A lot of others saw the golden possibility of storing solar energy by means of photochemical reactions producing high-energy products or by photogalvanic systems. By now, there must be thousands of papers dealing with Rubipy and related complexes.

While we spent some time with these systems, we didn't concentrate on them, but went on to other things. Perhaps I should expand on that footprint analogy. Chemistry to me has been an adventure. I felt like an explorer sailing along a strange new coastline, exploring bays and tributaries and climbing up valleys here and there to experience new vistas, but never settling down to map any one territory fully. We made footprints here and there and blazed a trail or two but left the colonization to others. In one respect, this philosophy worked out to the advantage of those from my group who went on to academic careers. They could pursue their acquired expertise and knowledge without fear of being in competition with the parent group. In a sense, they carried that particular subject area with them as an inheritance.

R.B. I'm not sure that most readers of Coordination Chemistry Reviews *realize that you are one of the few scientists around that has managed to wear two hats successfully: that is, to be an outstanding researcher in two totally different fields. Perhaps you could tell us something about how you got started in surface chemistry.*

Yes, it would be missing a part of me not to say something. As you know, my original Ph.D. work was on monomolecular films and this did give me a lasting interest in surface chemistry. When I came to USC, one of the courses I taught was on surface chemistry, using notes from the course at Chicago. Incidentally, faculty actually taught two courses at a time in those days and didn't think too much about it. Anyway, after giving the course a few times, it was apparent that N.K. Adam's classic 1938 *Chemistry and Physics of Surfaces* was badly out of date and this led me to put together the first (1960) edition of *The Physical Chemistry of Surfaces*, drawing mainly on my lecture notes. Again I was fortunate. This was a book that appeared at the right time. There was a strongly developing interest in surface chemistry and, in fact, the appearance of that first edition intercepted some putative writing by others. At any rate, the book was successful. A much better second edition was prepared in 1966 while at the University of Bristol as Unilever Professor. Later editions followed and the current fifth edition appeared in the spring of 1990. There have been several translations, notably into Russian and Chinese. I have known persons who have used that book through all of its editions, first as a student and then as an instructor. I might comment that the financial reward for even a successful graduate level text is not enormous but the professional, and call it psychic, benefits are impressive.

R.B. Could you summarize some highlights in your surface chemistry research?

I will be brief. I did some early stuff on ion exchange, out of the Oak Ridge experience, but then became interested in vapor adsorption and surface area measurements, I think partly from writing that first edition. One of the things we did was to develop the means of analyzing adsorption isotherms so as to obtain the distribution of adsorption energies for a heterogeneous adsorbent surface, a method that found some vogue. The matter of heterogeneity also had a topological aspect, first noted in that 1960 edition in which it was explained why the apparent surface area of a solid would likely depend on the size of the probe used. Finer probes, such as smaller adsorbent molecules, would fit into the irregularities inaccessible to larger ones. This aspect has now developed massively in terms of the fractal dimension idea, or that of "self-similar" surfaces, by people such as David Avnir. I noticed also that physical adsorption isotherms in the small multilayer region were superimposable and again, in the 1960 edition, offered what I called a universal isotherm. This concept was a precursor to J.H. de Boer's so called *t*-plot, which was useful in analyzing pore size distributions in porous solids.

Later, in an attempt to get at the nature of the adsorption forces, we moved into a series of studies of physical adsorption on molecular solids, most notably ice. We concluded that surface restructuring could occur even with weak adsorption forces. The work led to looking at the relationship between physical adsorption of a vapor and contact angle and wetting behavior of the bulk liquid from that vapor on the same adsorbent surface. This involved using ellipsometry and a variety of low-energy surfaces such as Teflon and polyethylene. It also led us to conclude that significant vapor adsorption could occur even in non-wetting systems, and to point out the problem that this situation posed to simple models that were being used for contact angle phenomena. Also, using single crystal ice as the surface, we were led to postulate two identifiable classes of systems we termed A and B. These two classes are somewhat of an analogy to A and B or hard and soft metal ions and ligands. For type A systems, there was little surface restructuring while for type B, the effect was important and affected the contact angle behavior. Now, the above ideas ran counter to a popular assumption that surfaces are structurally inert in physical adsorption and that there was essentially no vapor adsorption in a contact angle situation. Again, I avoided polemics and we went our own way. People had made these assumptions as a matter of convenient simplification of modelistic treatments and became wedded to them, but there is finally a growing consensus that surface structure and restructuring of adsorbed films can indeed be very important in physical adsorption and wetting. Incidentally, the advent of the scanning tunneling microscope has helped in this respect. To wind it up, I have now moved into the intriguing matter of irreversible adsorption from solution. Adsorption forces are typically not large and the effects seemingly must be ascribed to cooperative phase changes in the adsorbed film. I have been working on a model that can replicate the phenomenon.

A final comment is that it was my practice to hold joint weekly meetings of the photochemical and surface chemical student groups so that each heard about what the other was doing and I have been told, particularly by students that went into industrial work, that this dual exposure was very helpful to them. Either their problem assignment was changed or shifted in a way that the photochemists found their exposure to surface chemistry very helpful, and the surface chemists benefited from their contact with photochemistry.

R.B. One of the causes you pushed vigorously was the idea of an ACS journal in surface chemistry. Can you say a few words about the conception and your first editorship of Langmuir*?*

I'd like to very much. This is something I worked hard on. I felt that the ACS, as part of its general mandate to be of service to chemistry, should have journals in all important areas, not just the *Journal of the American Chemical Society*, *Journal of Physical Chemistry* and the like. I was glad to accept when I was invited to become the founding Editor of the new journal, *Langmuir.* The journal was successful; it grew from 800 pages in 1985 to some 2000 pages in 1990 and from a bimonthly to a monthly publication. Also, it was never in the red financially and this, as well as its scientific success, undoubtedly encouraged the ACS Publications Committee to proceed with the establishment of the several subsequent new journals that have now appeared. By 1989, I had reached 70 and Emeritus status and felt it was time to ease away from what was, for me, the very time-consuming obligation of the editorship. I did not want or ask for a second five year appointment. I enjoy being Editor Emeritus now and watching *Langmuir* develop further under the good leadership of the new editor, Bill Steele of Penn State.

R.B. One thing that has never ceased to amaze me is that, in addition to your intensive activity in two different areas of research, you have always had an active profile in the American Chemical Society as well as in the USC Chemistry Department.

Well maybe I liked committee work, although that's hard to believe. Anyway, I have felt an obligation to our professional society and have paid my dues gladly. I was Chairman of various committees of our local Southern California Section and eventually Section Chairman and then, for 16 years, a Counselor to the National ACS. Parenthetically, I was instrumental in promoting the formation of the Division of Inorganic Chemistry out of what had been the Division of Physical and Inorganic Chemistry. Maybe some readers will remember that earlier arrangement. I was asked to organize and chair the inaugural symposium of the new division – this was at the Miami ACS meeting in 1957. I well remember that meeting. Glenn Seaborg was one of the speakers and he was very irritated at having to turn up his voice level several notches because the sound system had failed. Moreover, the room was overflowing with people and was very hot. Yet it was the inaugural symposium. Well, anyway, I was active also on the Executive Committee of the Division of Colloid and Surface Chemistry, eventually as Chairman. While I never really aspired to high office, certainly not enough to work hard for it, it was an honor to have run for a Divisional Directorship once and to have been nominated by the ACS Nomination Committee for the office of ACS President.

I suppose that my most enduring commitment was as a member of the Publications Committee. I opposed page charges then and still do, as being a tax on the producer rather than on the user; there were friendly but divergent exchanges with Bryce Crawford and a letter to *Chemical & Engineering News.* On another matter, it took over five years of prodding and two task force reports that I organized, finally to maneuver the idea of a new ACS journal in surface and colloid chemistry. This was carried out despite ACS concerns about new ventures, and concerns that it might not be financially profitable. I might say also, that it took considerable pushing to get the idea past some political flack. The result finally was *Langmuir*, the name incidentally being the inspiration of an old friend and colleague, Karol Mysels.

Another "dues" paid to the profession was being Chairman of my Department for three years, from 1972 on. I have various memories of that – enough said. Probably the best thing I did was to extract money from the Administration, inaugurate lab fees that generated, for a while, a nice flow of money into the Department, eventually to be cut off as the Deans woke up to what was happening. I've often explained to people that the chairmanship was something *I wanted* to have done. I wanted the experience but I didn't want to make a career out of it. Perhaps I should mention, because people do ask, that quite possibly the stresses involved in that chairmanship stimulated a bladder cancer. While handled successfully, this ailment did involve radiation treatments which had a local aging effect, which eventually required some further "plumbing" operations. So much for that.

R.B. In addition to the book on surface chemistry that you talked about earlier, you've also written several other textbooks. Now, for most people, writing just one book is about all that one can expect in the course of a scientific career. It's remarkable that you have been able to produce three major textbooks.

Well, I've loved teaching and, I might say, teaching is to me the true source of eternal youth because it borrows steadily from the vitality of the young people flowing through the courses. Over the years, I've taught freshman chemistry, quantitative analysis, physical chemistry, radiochemistry, surface chemistry, senior inorganic chemistry, inorganic reaction mechanisms and inorganic photochemistry. The physical chemistry course was important to me as a foundation course of chemistry and yet one that I felt was too biased toward spectroscopic and quantum mechanical aspects. Many chemistry departments seem to have an excess of people in such fields and about the only undergraduated courses they can teach are physical chemistry or, worse, freshman chemistry. This concern led to several books. There was first a paperback called *Understanding Physical Chemistry,* made up of actual examination and quiz questions and their worked-out solutions. To any aspiring young authors reading this, let me say that the book was unusual and five publishers rejected it before Bill Benjamin, a very venturesome guy, picked it up. It worked out well. It went through three editions although by now it has faded, perhaps because as teaching loads grew lighter, I did not teach physical chemistry each year and the source of new questions dried up. The lesson to the young author is: persist. The best complement I think that I've ever had on the book was that instructors *avoided* mentioning it to students so as not to compromise their source of examination questions!

Later, there was a textbook on physical chemistry, a regular textbook which tried to give a more catholic treatment than usual of the subject. It's now in a third edition. Another teaching book was one edited with Paul Fleischauer, *Concepts of Inorganic Photochemistry.* Here, a variety of well-known inorganic photochemists contributed. I've occasionally pursued a point of interest in *Journal of Chemical Education,* the most notable, perhaps, was a paper called "A camel is a camel", lambasting the SI system of units. A lighter topic was "Dermatometry for coeds", which showed how to tell the alcohol content of a mixed drink from the contact angle on skin. I've had a lot of fun with that one and found that expounding the principle provides the quickest way I know of to be holding the hand of a newly met female.

R.B. Those of us who have known you for a number of years are familiar with your collection of home movies, recently transcribed onto videotape, of notable inorganic chemists. Could you expand on this a little bit?

Sure. Some readers will be aware of this ongoing hobby, making sound movie interviews of people, mostly coordination and surface chemists. I started this over twenty years ago and the collection includes notables such as Ronald Nyholm, C.K. Ingold, Eric Rideal and N.K. Adams. Lots of others are still living; it would make too long a list to be given here. There's also a historic collection of interviews on what was called "anomalous" or "poly"-water. All this is now on video tapes and one set is with the Center for the History of Chemistry. It was fun getting these interviews and even more fun replaying some of the older films, especially to appreciative scientific audiences. I suppose I can mention another hobby activity, the building up of a collection of what we called ligand-field or photosongs. These are photochemical ditties generally composed in a group over drinks at a meeting here and there. The printed copy is available on request and again, I have a video tape of a group of capable singers singing these songs.

R.B. I think graduate students and post-docs who have studied in the Southern California area have very fond memories of the annual summer conferences in photochemistry that were held between your group and those from Cal Tech, UCLA, UCSB and UCSD.

You're quite right. This got started in 1975, well, essentially with some joint seminars with Jeff Zink's group and that of Harry Gray. Out of this developed a series of informal photochemistry meetings, with the first one under USC auspices on Catalina Island (at the USC marine research station). What I think is special about this series is that the program consists entirely of papers by graduate students and post-docs. There are no invited speakers and there is no general audience, just the working stiffs! The meetings are annual and grew to include the UCSD and UCSB groups. I think these meetings have been memorable for the graduate students and post-docs involved.

R.B. Even though you're formally retired, you still maintain quite an active profile. In closing, could I ask you to comment about your ACS awards and to tell us what your future plans are?

Well, I have received recognitions and have felt fulfilled by them; I might say especially because they've come in all three fields of importance to me, photochemistry, surface chemistry and chemical education. I have might have gone higher up the recognition ladder had I chosen to concentrate in some particular field, but as the footprint analogy explains, this simply was not in accord with my temperament, nor do I think I would have had as much plain enjoyment if I had tried to concentrate in one area. Anyway, I'm Emeritus now, I do keep a small research program going, with some outside support, on irreversible adsorption. Activities include tennis, mostly doubles now rather than singles and I have fun running an outdoor model train which does its own switching. Virginia and I have seven grandchildren to be fond of and one great grandson; we are more active in Church than before and we go on more sightseeing trips around our own country. I suppose that like MacArthur, I'll be fading away but slowly I hope and with good cheer.