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Celebration of Inorganic Lives Interview with F. Albert Cotton

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F. Albert Cotton

1. Introduction

An interview with F. Albert Cotton, W.T. Doherty–Welch Foundation Chair; Distinguished Professor and Director, Laboratory for Molecular Structure and Bonding at Texas A&M University; Member, National Academy of Sciences.

Major Awards: National Medal of Science, Welch Award, ACS Priestley Medal, Wolf Prize, Lavoisier Medal.

Interviewed by Richard D. Adams, Arthur S. Williams Professor of Chemistry, University of South Carolina, Columbia, SC 29208, USA.

2. Interview

Adams: *Could you tell us a little about your childhood years? When did you realize that you were very interested in science and want to make it a career? When did you realize chemistry was the most important field of science for you and why chemistry?*

Cotton: My childhood was a very happy one. Although my mother was widowed when I was two, she had a very strong character. Even though she had to work full time—and more, because the great depression was on—she took wonderful care of me. We lived in a row house in a working class neighborhood where there were lots of other boys with whom I played games and enjoyed myself. However, I always had intellectual interests they didn't share. School never challenged me and didn't much interest me until about eighth grade when I had a science teacher who lent me her college freshman chemistry book, which I really enjoyed. Despite various distractions, such as jazz guitar, I was pretty well locked in on chemistry from about the sixth grade.

When I graduated from high school I received a scholarship to Drexel University (then Institute) where my father had studied engineering and I enrolled as a chemical engineering major. In my second year I realized I wanted to be a chemist, not a chemical engineer. Unfortunately, Drexel, at that time, did not offer a degree in chemistry, so, in the middle of the glut of GI bill students (1948) I had to try to transfer. Temple University took me in and this was one of the most fortunate things that could have happened. The small chemistry faculty was of very high quality, and I spent a wonderful 2 years there. Also, the environment of a real university instead of a technical school was very beneficial. I took courses in history, art, esthetics and did a minor in German.

When I arrived at Harvard I had no idea what inorganic chemistry was all about. My intention was to be a physical chemist or, more likely, a physical organic chemist. I passed all the qualifying examinations and thus was free to take whatever courses I chose. The man I thought I would most likely work for was the physical organic chemist, P.D. Bartlett, and as it happened, he was the one to whom I was assigned for counseling on what courses I should take. On his

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advice, I signed up for thermodynamics and a course in inorganic chemistry, because I was lacking in background in both these areas. Thus it was that I first encountered Geoffrey Wilkinson, a new assistant professor who was teaching the course in inorganic chemistry for the first time. Before a few months had gone by, I had become fascinated by the course and impressed with Geoff, and so began my career as an inorganic chemist. By the following spring, the ferrocene era had begun and I was in on the ground floor, my first work being to determine the heat of combustion and from that the heat of formation of ferrocene.

My 4 years at Harvard included 8 months in Europe, working with Geoff during his sabbatical in Copenhagen and touring Germany, Switzerland, France and England. By late 1954 it was clear that I would have my thesis finished by June of 1955 and it was time to think about a job.

Adams: *Why did you choose Harvard for your graduate studies in chemistry and what led you to focus on inorganic chemistry?*

Cotton: I didn't really choose Harvard; I was told to apply there by my mentors at Temple University. I also applied to several other places, including Washington University in St. Louis, because I thought I might be interested in radio- or nuclear chemistry. I got an early acceptance with a teaching fellowship from Washington University and I rushed to share the good news with Professor William T. Caldwell, who was also Dean of Arts and Sciences. He congratulated me and then said, "but, of course, you will be going to Harvard." It seems he was a friend of Woodward's, had previously sent him a student who did well, and he knew I would be accepted, and, shortly thereafter, I was. This was one of the turning points in my life because of my experience there and the friends I made, as well as my later frictionless move to MIT.

In those innocent days, the way good places hired good young people was by informal recommendation. Geoff Wilkinson simply picked up the phone, talked to a couple of his friends at MIT and it was a done deal. No advertisements, no paperwork, no protestations about being an equal opportunity employer, etc. A few years later, based entirely on my personal recommendation, Arthur C. Cope interviewed Dietmar Seyferth, liked him, and hired him. Was this a good way to do things? If you use the practical criterion of whether Cope could have done any better by present day technology, the answer is obvious. He got two future National Academy members with an absolute minimum of bureaucratic arglebargle.

Adams: *Why did you leave MIT for Texas A&M?*

Cotton: I was offered a better job in a better environment and a political climate I preferred. That answer, of course, is too simple, but I'd rather not, at this juncture, rehash all the details. Suffice it to say, it was a move I have never regretted.

Adams: *It is well known that you are a very hard worker. Why do you work so hard even at the age of 70?*

Cotton: I work hard because, as Noel Coward once said, "work is more fun than fun." In addition, I want my coworkers to work hard, and I don't believe that you can "lead your troops from the rear," so to speak. I come in on Saturdays because I expect them to come in on Saturdays. Also, my own standards for what I publish could not be met if I didn't put in lots of hours. Not one word of any of the approximately 1500 papers I have published was not either written by me (which is most of them) or carefully reviewed and approved by me.

Adams: *Why did you choose academics over industry for a career?*

Cotton: The thought of going into industry never occurred to me for a nanosecond. I like teaching and I like thinking about what I want to think about. I have never worried about money nor cared to improve somebody's widget just because he will pay me to do it. I am not knocking people who do that; I just never wanted to do it myself.

Adams: *Could you tell us what you think two or three of your greatest research achievements in chemistry are and explain briefly why?*

Cotton: From my earliest days, that is, the latter 1950s, I had often wondered why there were practically no examples of compounds with metal–metal bonds. With the determination of the structure of the $Re_3Cl_{12}^{3-}$ ion in what had previously been formulated simply as $CsReCl_4$, I was led to postulate for the first time the existence of metal–metal multiple (in this case double) bonds. Further experimental work with rhenium(III) chemistry quickly led to the $Re_2Cl_8^{2-}$ ion and my proposal that it was the first known example of a quadruple bond. When I showed that it could be easily converted to $Re_2(O_2CR)_4Cl_2$ compounds, it immediately occurred to me that the '$Mo(O_2CCH_3)_2$' which has been described shortly before by Wilkinson (who proposed several different structures for it—all of them wrong) must be $Mo_2(O_2CCH_3)_4$ with a quadruple bond and that $Tc_2Cl_8^{3-}$, also reported about the same time, had to have a quadruple bond with one additional weakly antibonding electron. In 1968, when my postdoc Jurij Brencic and I achieved the conversion of $Mo_2(O_2CCH_3)_4$ to $Mo_2Cl_8^{4-}$, the proof of the concept was complete. Many coworkers and I have gone on, over the years, to make several thousand structurally and electronically related compounds containing V, Cr, Co, Nb, Tc, Ru, Rh, W, Os, Ir and Pt, and with bond orders ranging from 1 to 4. To me, the demonstration that in addition to the classical (i.e. Werner's) coordination chemistry of the transition metals there is another previously unsuspected chemistry of the transition metals, has been immensely enjoyable and satisfying. Having done that, I am jealous of no man.

I might mention that these M–M bonded compounds continue to afford tremendously interesting research opportunities. The δ-bond itself affords a unique opportunity to understand the two-electron bond, as Dan Nocera and I recently showed in an *Accounts* article. There are important applications of such compounds in catalysis and chemotherapy (especially the dirhodium compounds) and, as summarized in a recent *Accounts* article I wrote with Chun Lin and Carlos A. Murillo, the dimetal compounds allow the elaboration of a vast array of supramolecular structures.

There are several other major achievements I might mention. One is the demonstration that there is a vast field of fluxional organometallic compounds including (as you know well, Rick, because you did your PhD on them) polynuclear metal carbonyls. This is a fundamental aspect of organometallic chemistry in which I and my coworkers had the opportunity to do most of the pioneering work, including the very first use of NMR spectroscopy not only to show that a rapid spontaneous intramolecular arrangement occurs, but to find out the mechanism by which it occurs. This is taken for granted today, but Alan Davison, Jack Faller and I did it for the first time, in 1966.

I will specifically mention only one other achievement: the discovery of what are now commonly called agostic hydrogen atoms. The first explicit recognition, full characterization and correct explanation of this very important phenomenon were reported in a series of papers I published in the period 1972–1974. It was not until nearly a decade later that others found further examples. I am saddened that reviews have been written in which the original discovery is relegated to footnotes.

Adams: *You have written several highly successful texts for inorganic chemistry. These must have required a great effort. Could you tell us a little about the motivations for producing these great works.*

Cotton: Writing books is, at least for me, easy, but the complete process from initial idea to the appearance of the book is very onerous. The actual writing (1) is followed by (2) preparation of a typed manuscript, (3) proofreading of the said manuscript, (4) working with the so-called artists that publishers employ to get all the figures drawn correctly, (5) correcting first proofs, (6) correcting page proofs, (7) making a final check on the accuracy of all the references (over 4000 in the 6th edition of *Advanced Inorganic Chemistry*). Finally, just when, in a state of catatonic exhaustion, one is ready to relax, comes (8) a reminder that there is an index to make.

My motivation in writing my books was always the same: to teach. While I enjoy lecturing, I enjoy even more trying to communicate what I know and love to as many others as possible. I have done this by writing at every level, from my high school text to the monograph Dick Walton and I wrote on *Multiple Bonds Between Metal Atoms*, with undergraduate texts on inorganic chemistry and my group theory book in between. I am proud that everything I have written except the high school book (which did sell about 350 000 copies while it lasted) remains in print and in use to this day.

Adams: *What gives you your greatest satisfaction(s) out of research and out of teaching?*

Cotton: I have gotten equal satisfaction out of both aspects of my career: teaching and research. I think it is probably obvious why I derive satisfaction from my research, but let me say a few words about teaching. It disturbs me that, in the US at least, 'teaching' tends, implicitly, to be construed to mean undergraduate teaching, most particularly at the freshman level. I don't deny for a moment that these activities are very important and that doing them well is not a walk around the block, but I think that the importance and the challenges involved in teaching at the graduate level should also be more recognized than they are. Just as there are undergraduate teachers who take a deep interest in their work and excel at it, so there are professors who do the same at the graduate level.

I have had well over 120 graduate students, a few of whom have simply not been qualified and couldn't earn a PhD, but of whom 106, so far, have become PhDs. In a significant number of cases, these were young men and women who required a lot of mentoring, and I take great satisfaction in having been able to give them the guidance they needed. In a few other cases the challenge has been to see that a huge talent was as fully nurtured as possible. I have always been as interested in the students doing the research as in the research itself. Every student is different and it is a task I have always welcomed to find, in each case, the right balance between being helpful on the one hand and making the student learn to be independent and self-confident on the other. I think that there is a tendency in this country not to recognize that this kind of teaching is no less important and demanding than teaching a good freshman course.

Adams: *You have had a long and friendly relationship with Lord Lewis of Newnham. Would you be willing to tell us a little about how this began and why you have been such good friends?*

Cotton: Jack Lewis is one of the most remarkable people I have ever known and the most remarkable person I have ever known well. He is only one of many brilliant scientists I have been privileged to know, but he is unique in his range of human qualities as well. I have never known anyone who has done so many things so well, and with such grace.

Jack and I first met in the mid-1950s, just after I had started at MIT. Geoff Wilkinson had taken the chair in inorganic chemistry at Imperial College, London, in 1955 and Jack was one of the first lecturers he had hired. I visited Geoff regularly and that's how Jack and I met. Our ways of doing inorganic chemistry had much in common: we tried to apply physical chemistry as rigorously as possible while not forgetting that preparative chemistry was also vital. Jack soon moved to University College where Ron Nyholm was in charge, but our friendship was already well established.

One high point in our relationship was the Spring of, I believe, 1960, when he came to MIT as a visiting professor and our families became well acquainted. Jack's career advanced rapidly from then on. He became a Reader at UC, then professor at Manchester, then professor at UC, then professor in Cambridge, where his abilities were fittingly recognized by his being asked to work with the benefactor David Robinson in establishing Robinson College. Jack's work as head of a Royal Commission on the Environment led to his elevation to the Lords, where he now plays a leading role in all questions relating to science. I think Jack's extraordinary success in life results from the fact that he possesses a talent called judgment, which resembles absolute pitch; it is very rare, and you either have it or you don't.

Adams: *What are some of your plans and goals for the future?*

Cotton: Broadly speaking, I plan no near-term changes. I feel fine and enjoy what I am doing as much as ever. My current research group of about 15 (about the average over the past decades) is a very good one and we are working on problems that interest me keenly.

Back in about 1993 I was a member of an NRC committee that was set up at the behest of congress to advise on whether the age cap of 70 years, which then applied to faculty in institutions of higher education, should be lifted or continued. Our hearings and internal discussions involved strong differences of opinion, but we finally recommended removing the age cap, and that's what congress did. Frankly, I have the feeling they would have removed it even if we had recommended the opposite.

The doomsayers, who argued that if the cap were removed universities would bear a crushing burden of useless old people and be unable, for lack of space and resources to hire young people, have been shown to be wrong. In my opinion, our major problem in the professorate is not old hangers-on who can't cut it any more but won't go away, but rather people who have run out of gas in middle age who hang on because they have nowhere else to go.

I am very happy to be one of the beneficiaries of our present enlightened system in which people who are still enthusiastic, energetic and creative go on contributing instead of automatically being chucked out. The system is pretty well self-regulating. Those who aren't good at what they do and did it only because they needed to earn a living are always delighted to quit once they can no longer increase their retirement benefits by staying on—and provided also that their retirement benefits are adequate, as is the case for professors. I think what we have now is a win–win situation.

The most annoying thing anyone can say about me is that I've mellowed. However, I rarely hear that and I try very hard not to give anyone grounds for thinking so.

Adams: *Thank you very much Al for your time, insight and words of wisdom. We all wish you the best in you in your future activities and we are looking forward to a continuation of great research results for your laboratories.*