CHEMICAL HERITAGE FOUNDATION

ALAN G. MacDIARMID

Transcript of an Interview
Conducted by

Cyrus Mody

at

University of Pennsylvania
Philadelphia, Pennsylvania

on

19 December 2005

(With Subsequent Corrections and Additions)

ACKNOWLEDGEMENT

This oral history is part of a series supported by the Center for Nanotechnology in Society (CNS), University of California, Santa Barbara, under the National Science Foundation Grant No. SES 0531184. Scholars and other people using this interview should acknowledge in all written publications. Any opinions, findings, and conclusions or recommendations expressed in this material are those of the interviewee and interviewer and do not necessarily reflect the views of the National Science Foundation.

This oral history series is an important resource for the history of nanotechnology, documenting the lives and career of key scientists and engineers that shaped and contributed to the contemporary practice of science and technology.

CHEMICAL HERITAGE FOUNDATION
Oral History Program
RELEASE FORM

This document contains my understanding and agreement with Chemical Heritage Foundation with respect to the participation of Alan G. MacDiarmid in the audio-recorded interview conducted by Cyrus Mody on 19 December 2005. I have read the transcript supplied by Chemical Heritage Foundation and returned it with my corrections and emendations.

1. The audio recording, corrected transcript, photographs, and memorabilia (collectively called the "Work") will be maintained by Chemical Heritage Foundation and made available in accordance with general policies for research and other scholarly purposes.

2. I hereby grant, assign, and transfer to Chemical Heritage Foundation all right, title, and interest in the Work, including the literary rights and the copyright.

3. The manuscript may be read and the audio recording(s) heard by scholars approved by Chemical Heritage Foundation subject to the restrictions listed below. The scholar pledges not to quote from, cite, or reproduce by any means this material except with the written permission of Chemical Heritage Foundation.

4. I wish to place the conditions that I have checked below upon the use of this interview. I understand that Chemical Heritage Foundation will enforce my wishes until the time of my death, when any restrictions will be removed.

a. X No restrictions for access.
NOTE: Users citing this interview for purposes of publication are obliged under the terms of the Chemical Heritage Foundation Oral History Program to obtain permission from Chemical Heritage Foundation, Philadelphia, PA.

b. ________ My permission required to quote, cite, or reproduce.

c. ________ My permission required for access to the entire document and all tapes.

This constitutes my entire and complete understanding.

(Signature) Signed release form is on file at the Science History Institute
Gayle Gentile

(Relationship to Participant) Wife

(Date) November 2, 2007

Rev. 1/01/07

Alan G. MacDiarmid, interview by Cyrus Mody at University of Pennsylvania, Philadelphia, Pennsylvania, 19 December 2005 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0325).

Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

ALAN G. MacDIARMID

1927	Born in Masterton, New Zealand, on 14 April
2007	Died in Drexel Hill, Pennsylvania on 7 February

Education

1948	B.Sc., chemistry, University of New Zealand
1950	M.Sc., chemistry, University of New Zealand
1952	M.S., chemistry, University of Wisconsin
1953	Ph.D., inorganic chemistry, University of Wisconsin-Madison
1955	Ph.D., inorganic chemistry, University of Cambridge

Professional Experience

	University of St. Andrews, Scotland
1955-1956	Assistant Lecturer
	University of Pennsylvania
1955-1956	Instructor in Chemistry
1956-1961	Assistant Professor
1961-1964	Associate Professor
1964-1988	Professor
1988-2007	Blanchard Professor of Chemistry
	University of Texas at Dallas
2002-2007	James Von Ehr Chair of Science and Technology, Professor of Chemistry and Physics
	Jilin University, China
2004-2007	Professor of Chemistry

Honors

1967	Philadelphia Section Award, American Chemical Society
1970	Frederic Stanley Kipping Award, American Chemical Society
1982	Madison Marshall Award, American Chemical Society
1982	Doolittle Award, American Chemical Society
1983	Royal Society of Chemistry Centenary Medal and Lectureship (England)
1984	Chemical Pioneer Award, American Institute of Chemists
1985	“Top 100” Innovation Award, *Science Digest*
1989	John Scott Award, City of Philadelphia

1993	Francis J. Clamer Award, The Franklin Institute
1999	Chemistry of Materials Award, American Chemical Society
2000	Nobel Prize in Chemistry (With Heeger, Shirakawa)
2001	Rutherford Medal, The Royal Society of New Zealand
2002	Member, Order of New Zealand
2002	Member, National Academy of Engineering
2002	Member, National Academy of Science
2003	Fellow, Royal Society of London, England
2004	Friendship Award, State Administration of Foreign Experts Bureau, P.R. China
2004	Establishment of the Alan G. MacDiarmid Laboratories of Polymer Research, Karnatak University, India
2005	Establishment of the MacDiarmid Institute of Innovation and Business, São Carlos, Brazil

ABSTRACT

Alan G. MacDiarmid begins the interview by discussing his childhood in New Zealand and goes on to describe how two books, both chemistry-related, sparked his interest in chemistry. Due to economic hardship, MacDiarmid juggled working and attending the University of New Zealand part time to complete his bachelor's and master's degree. Denied a scholarship to study in England, MacDiarmid came to the University of Wisconsin-Madison as a Fulbright Scholar to study inorganic chemistry. After obtaining a M.S. in 1952 and a Ph.D. in 1953, MacDiarmid left Wisconsin and finally got to fulfill his dream of studying at the University of Cambridge under H. J. Emeleus. Focusing on inorganic chemistry, MacDiarmid obtained a Ph.D. in 1955 and accepted a position at the University of Pennsylvania after a brief stint as assistant lecturer in the University of St. Andrews. MacDiarmid did his most seminal work at Penn, where he remained for fifty-plus year and is still a faculty member. His early research in America was funded by Cold War related projects overseen by government funding agencies such as the Air Force Office of Scientific Research and the Office of Naval Research. Then on a visit to Japan, MacDiarmid serendipitously met with Hideki Shirakawa, who was doing similar research on conductive metals. Over tea they discussed their work, and MacDiarmid invited Shirakawa to Philadelphia. It was there, collaborating with another Penn faculty member, Alan Heeger, that the three published influential works that led to the discovery of conducting polymers and their shared Nobel Prize in Chemistry in 2000. MacDiarmid, an inorganic chemist, emphasized the importance of inter-disciplinary research with Shirakawa, an organic chemist; and Heeger, a physicist. MacDiarmid describes how interdisciplinarity can advance current research and promote innovation. He concludes the interview by suggesting possible future research directions and the need to decrease dependency on fossil fuels.

INTERVIEWER

Cyrus Mody is an Assistant Professor of History at Rice University. Prior to that position he was the manager of the Nanotechnology and Innovation Studies programs in the Center for Contemporary History and Policy at the Chemical Heritage Foundation. He has a bachelor's degree in mechanical and materials engineering from Harvard University and a Ph.D. in science and technology studies from Cornell. He was the 2004-2005 Gordon Cain Fellow at CHF before becoming a program manager. Mody has published widely on the history and sociology of materials science, instrumentation, and nanotechnology.

TABLE OF CONTENTS

INTERVIEWEE: **Alan G. MacDiarmid**

INTERVIEWER: **Cyrus Mody**

LOCATION: **University of Pennsylvania**
Philadelphia, Pennsylvania

DATE: **19 December 2005**

MODY: Let's start from the beginning. What are the experiences that initially moved you towards chemistry or interested you in materials [engineering]?

MACDIARMID: My interest really developed in chemistry accidentally when I was about ten years old. Well, I was born in New Zealand and educated in the city of Lower Hutt, which is a suburb of the capital city of Wellington. When I was about ten years old, I rode my bicycle to the public library in Lower Hutt and went to the children's section. And I still remember very well that on the right-hand side doorway, bottom shelf, were the new books. There was one new book called *The Boy Chemist* (1). Today that would be a completely inappropriate title; it would be *The Young Person Chemist* instead. The whole book was devoted to experiments, so I signed it out constantly from the library for about a year and did most of the experiments in it using common chemicals around the house, or ones that I could purchase easily. But the thing that really got me interested, and I wish would be used more in education, was asking questions. The book would describe how to turn water into wine using acid-base indicators and colored indicators, or how to do invisible writing which you could then read by developing it with other chemicals. These things intrigued me, and left questions in my mind.

I'm very interested in education also, and I feel that we could combine that with the questioning part—for example we could introduce the general population to cartoon shows for a better scientific immersion. You could have Bugs Bunny holding, say, three balloons. One is red, one is green, and one is yellow. And Bugs Bunny lets one that is filled with air go, and it just sort of floats around and then slowly sinks. Then he lets another one, say the blue one which is filled with carbon dioxide or some heavy gas, go—and zoom, it sinks straight down to the floor. Finally let's have the yellow one, which is filled with helium, zooming upwards. So the question is, is it the color of the balloon that decides whether it hovers around or sinks to the ground? Or is it not?

Simple questions such as this can get the young people thinking. So I quite often feel that it is good to ask questions and not necessarily give the answers, but to give the leads where answers may be found. What I find is that if I've looked at a videotape movie, say a murder mystery, and for some reason or the other I never see the end, then that is the movie that my mind keeps coming back to. But if I see it from the very beginning to the very end, then it's a closed story in my mind, and I sort of forget it. I feel we really need to start in getting the kids to wonder, which was what got me going.

MODY: Was there anything else that drew you to chemistry? What did your family think of these experiments you did?

MACDIARMID: Well, my basic interest was there. My father [Archibald MacDiarmid] was trained as an engineer. On one occasion we were up in the loft pulling out some of his old books, and I found a book on chemistry which was probably written in the late 1880s. I liked the pictures and the drawings in it, so I would spend hours turning over the pages and not understanding a damned thing. But it got me interested in this whole area called chemistry.

Then during World War II, with New Zealand being a little out-of-the-way country in the bottom of the Pacific [Ocean], there was a great shortage of printing paper for cameras. And so I got some old books somewhere to learn how to make printing paper. I found out how to separate albumen from egg whites to make printing paper that printed up on some silver chloride that I made.

MODY: Did you sell those or did you use them yourself?

MACDIARMID: No, that was just to see if I could do it or not. But in order to get the silver nitrate for making these photograph plates, somebody gave me a part of a cutout of an old bob watch which was made from silver. Since my house was about a three-minute walk from the primary school, one lunch time I went back home and dissolved up some of the silver watch in nitrate acid, which went fine. Then I went back to school and I found my fingers were starting to turn a dark black color, and I thought, “Gee, I've got some terrible disease.” As you probably know, if you spill silver nitrate on your skin, it reacts with the sodium chloride in your sweat and precipitates silver chloride. Since silver chloride is photoactive, it turns your skin dark black. So I remember at intermission in the afternoon I went up to our teacher and said, “Sir, I have something very wrong with me.” And he asked, “What have you been doing?” To which I replied, “I went home at lunchtime and dissolved some silver in some nitrate acid.” He laughed and told me that I obviously spilled some silver nitrate on my fingers, and that made it black. And that it would all wear off. [laughter]

A lot of these little interesting things involving chemistry got me intrigued. This was in the middle of the big depression and my father, an engineer, was out of work for about four years, with four children in the family. He was in his fifties when I was born, so when I was sixteen he retired. He retired to the northern subtropical part of New Zealand, and I spent three years at high school, which was considerably shorter than most people. During that time I delivered newspapers to collect money. Before that, at grade school, I would deliver milk on a bicycle.

So anyway, we were always pretty poor as a family with five kids, with my father being out of work. I then accepted a job as lab boy in the chemistry department at Victoria University at Wellington. In those days it was just the University of New Zealand which is a little office building on a street in Wellington. And that was divided up into six university colleges, both in Wellington and other cities, and a couple of agricultural colleges. So the actual degree that we got was a degree from the University of New Zealand, through one of the colleges.

MODY: That's based on the model of the University of London?

MACDIARMID: Yes, exactly. A different examiner at one of the colleges would set the exam every year; and they were given on the same day at the same time in each of the major cities. Anyway, I accepted a job there as lab boy, which also doubled as a janitor. That involved cleaning out all of the dirty glassware that was left over by students, throwing wet sawdust down on the floor, and sweeping that up with a broom so the wet sawdust caught the dust. I did that at this old, wooden building, and then I was also a janitor at a student dormitory where I stayed at later. So I was always a part-time student.

MODY: Did you seek out the job in the chemistry department because of your interest in chemistry?

MACDIARMID: Yes. That was probably the case. I knew my father was going to retire, but he sort of stayed on his job until it came to the end of the semester. After that I was looking for where I might be able to get some work. So I felt maybe something with the university would be good if I could get in. So looking at the newspaper I saw the advertisement for lab boy and janitor at the chemistry department of Victoria University colleges that night. It had a very, very small salary. It was actually thirty shillings a week. [laughter] That was only enough to pay for my living expenses, and I had to get financial help from my parents. But then I am rather pleased to say that I left high school and started the university when I was sixteen years old. I only had three years of high school—usually people had at least one extra year, if not two, in some form. And the universities were essentially free if you passed the university entrance exam, which I did. From the age of seventeen onward I have supported myself financially with scholarships or fellowships, although I've borrowed money plenty of times and then returned it.

MODY: How many other students were part-time? How many others were supporting themselves like you? Was it quite unusual at the university?

MACDIARMID: Very unusual. The university was very small, with a total student population of I think 1,200 students in all departments. The chemistry department had one professor and one senior lecturer. The senior lecturer, Mr. [A. D.] Monro, had never been out of New Zealand, and the professor was a New Zealander who had gotten his degree from England. So this was unusual being part-time, doing janitorial work, working in the chemistry department, and in the student hostel. The janitorial work involved a lot, there was stoking up the central heating late at night, and also putting in coke into the big ovens in the kitchen. I'd go out to a party or a dance, and come back and change out of my fine clothing into hobo clothing to sift through the coke. And I remember one Saturday night I must have been at a good party, and I forgot about lighting the ovens and cooking things in the kitchen with the coke and, of course, by Sunday morning breakfast time, everything was out. So the whole dormitory had no food for breakfast. [laughter]

MODY: So did being part-time mean you stretched out your career at college then?

MACDIARMID: Well, most people usually do it in three years. I did the Bachelor's degree so I did it in four. And then stayed on and did my Master's degree for a two-year period rather than a one-year period. So everything was sort of slowed down a bit because of being part-time all the way through. But I learned so much.

As I always admit, I learned so much from being a lab boy and the lecture demonstrator. I prepared chemical samples and set up apparatuses for Mr. Monro in the prep room behind the freshman chemistry lecture room. And I remember on one occasion, at 9:00 am, just before Mr. Monro was going to give his lecture, I was tying something up and the bloody apparatus broke. Mr. Monro walked into the lecture room and, he used to always call me Mac, he said, "Mac, okay, where's the apparatus?" I told him, "I'm sorry sir, it broke." There was a deathly silence, and then he said, "*It* never breaks." To which I replied, "I'm sorry, I broke it." And then he just went straight into the lecture. [laughter] I've always remembered—*it* never breaks. But I learned a huge amount from him.

MODY: In terms of informal skills with materials or the formal language of chemistry as well?

MACDIARMID: There were two things. This was after I had been there for two or three years, and as a demonstrator I had to prepare apparatuses for lecture demonstrations. This means I had to perfect my manual skills in setting up apparatuses—more so than one would just taking regular lab classes, which of course I had to take as well. However, as a student, my lab classes were at night because I was working during the day. So this helped with the manual skills very considerably.

And also, Monro helped me develop as a person. He asked, "Mac, don't you ever wonder why I'm asking you to do this, or telling you to do that?" And I said, "I'm assuming, sir, that if you tell me to do it, then it is correct." He then told me to *never* assume anything he says is correct. So he helped me overcome my fear, awe, and trembling of this whole doctoral sort of thing, which was very helpful.

As a matter of fact, one of the samples that he got me to prepare for freshman chemistry was a sulfur nitride which formed lovely orange crystals. And so he chose this as a lecture demonstration, to show inorganic things are quite often highly colored. And said when I came to do my Master's work, I asked him if I might do it on the sulfur nitride, which I'd been working with. And he said, "Okay, if you'd like to, do it under my supervision." And then I got involved with my Master's thesis and changing colors with chlorine to greens, and purples and whatnot. And then that led to my getting involved in this lab here on these polysulfur golden crystals.

Later on in my career at Penn [University of Pennsylvania], I remember having discussions with Professor Alan [J.] Heeger in the physics department, and he said that Mort [Mortimer M.] Labes at Temple University had come up with some interesting things about a sulfur nitrogen compound. And I said, "Oh yes, I used to work on that for my Master's degree in New Zealand." He perked up his ears and says "Oh!", but I told him that was years, and years, and years ago. Then he asked me if I could make some, and I said no. Anyway, it was a long discussion. We finally made the crystals, and that then developed my interest in interacting with people outside chemistry, such as Alan Heeger in physics, who is one of the three of us that got the Nobel Prize[i]. So it actually goes back to my acting as a lab boy and then being promoted to demonstrator, to get things ready for some chemistry show-and-tell experiments.

MODY: What classes were you taking in other fields? Were you concentrating almost exclusively on chemistry or were you interested in other fields as well?

MACDIARMID: No. Chemistry was a very narrow sort of curriculum. Normally one would take three courses every semester, and I was taking two. The first one was always chemistry, and then for the second one I'd take the introductory classes in physics and math—I was lousy at math. The passing grade was fifty at the end of the year; I had two papers, and the average of those two papers had to be fifty. I often feel that Mr. Monro must have spoken to the people in the math department because I only got forty-eight on one paper and fifty-two on the other. So it came down to a passing grade. [laughter] He might actually have appealed to the math professor and said, "Well, MacDiarmid, he's no mathematician, but he seems quite good at chemistry, as good as a person may be."

And I remember that here at Penn forty years or so later I had one student from the Soviet Union. He was all right at the subject, and for personal and professional reasons, he didn't want to go back to the Soviet Union. So I worked more strongly in favor of him than I normally would, and helped him to get a green card [Permanent Resident Alien card] to the

---

[i] Alan J. Heeger, Alan G. MacDiarmid, and Hideki Shirakawa won the 2000 Nobel Prize in Chemistry for the discovery and development of conductive polymers.

states. This is embellishing the truth a bit, but I kept remembering the case of my forty-eight and fifty-two. So I gave him a glowing report and he got his green card and has now turned out to be director of research at a small company in California. So a little touch of human understanding touched me, I never knew whether my math score had been rigged, but there's always been that doubt in my mind that it could have been. So when I came to this particular individual, I thought, "Well look, life is more than just experiments. This is a good, honest, hardworking guy, so let me give him every benefit of the doubt." Later on I said to my wife that for the first time in my life, I think I've done something for which professionally I could be criticized, but I feel right about it inside.

MODY: What kinds of mentorship were your family, Monro, and other teachers giving you about a career? What did you see yourself doing?

MACDIARMID: Well, all of my vacations from high school I'd spend up in the northern part of New Zealand on citrus orchards with my brothers and sisters, and then my father retired. So with New Zealand being a farming country, I would have loved being a fruit farmer if I hadn't gone to the university. I was very interested in fertilizer and rainfall, and particularly in the grafting of trees. In other words, you could cut off a part of an apple tree and then graft it onto a branch of a peach tree, then you could have a tree producing apples and peaches on different branches. This intrigued me very much, but because of my ongoing interest in chemistry and having always done well in chemistry in high school, my science teachers said, "MacDiarmid, none of your siblings has ever gone to a university, but you should go." I thought I will try it once and see if I can get in.

And then after I got my Bachelor's and Master's degree as a part-time student, I wanted to go to England. England was always called "home," even though you were born in New Zealand. When asked about what you are going to do after finishing with university, it's common to hear, "Oh, I'm saving up to go home." Also, there were ships that sailed to England called "home boats." So I applied for an 1851 Science Exhibition Scholarship (2) to England but did not get it. After that I remember mentioning, while walking across a football field from the dormitory, to one of my friends that I was very disappointed. He says, "Well, there's this new sort of scholarship that's just come out from America, it's called the Fulbright Scholarship. Why don't you apply for it?" I told him that, "Look, I don't like anything American. I don't like American food. I don't like American music. I don't like American movies. I don't like anything about America. Nothing at all." And then he said, "Well, why not just call the American Embassy and ask them to send you an application form? You've been working really hard for six years now on your education, it can't do any harm." But I still thought that I'd kind of like to go to England, not America.

Anyway, I called and they sent me an application form. Upon finally filling it in, I got a telephone call that said, "This is the consul at the American Embassy on Capitol Hill and I've got some good news for you in regards to the Fulbright Scholarship. When can you come down to discuss this?"

When I got down there, it was sort of assumed that I was going to accept it. The consul asked me where I wanted to go to university and what I thought of all of this. Anyway, I had no idea at all so he suggested the University of Wisconsin might be a good place.

MODY: Why did he suggest Wisconsin?

MACDIARMID: Because he took out a map of the United States and pointed! [laughter] Actually, he asked what branch of chemistry I was interested in. I told him I was interested in inorganic chemistry and he said, "Well, I've checked out some schools' reputations, what about doing something with going there [University of Wisconsin]?" I told him that I'm sure the school is very nice, but I wasn't that interested in going. Finally he convinced me by saying that it is only for two semesters.

This was in the days before airplane travel was common. I packed two suitcases knowing full well that I'd be back in New Zealand after two semesters, so I left all the rest of my stuff in my clothes drawers—but the next time we returned was twelve years later! My clothes drawers had already been passed around many times. [laughter] It took about three weeks then to go from New Zealand to Wisconsin. These were the early days before the scholarship caught on—so I think there were two Fulbright students from New Zealand and four from Australia, and they sent us all first class by boat. First it swept down Australia to Wellington, then it headed up to Fiji and then from Fiji to Hawaii, and finally from Hawaii to Vancouver. And at Vancouver we caught a train that took a couple of days to cross Canada to Winnipeg and then down from Winnipeg to Madison, Wisconsin.

So this all sort of happened not totally by accident, but it definitely was not planned. My plan was to go to England, which I eventually got to later in my career.

MODY: And what did you find when you got to Wisconsin? Did you know at all what to expect, or who you wanted to work with?

MACDIARMID: I really did not know who I wanted to work with. But I do remember during the first six weeks of class, which involved either chemistry or physical chemistry, I had failed every exam miserably. [laughter] I started in September, and I remember it was gloomy and very cold. I walked around the block of the chemistry department about six times and thought, "Aaah, damn it, what is happening here? I had always done fairly well, and I have worked my butt off here." But I just couldn't understand what was wrong. Then it is very interesting, because it occurred to me all of a sudden. In New Zealand the educational system is modeled after the University of London—the examiner assumes when he starts grading or writing a paper that you've got zero points, and you are given points for everything you get right.

MODY: As opposed to the U.S. [United States] system.

MACDIARMID: Right, in the American system, which I've now used for fifty years, you assume the student has one-hundred percent and you take off points for everything that he gets wrong or leaves out. That's the system I still use today, but back to the reason I was doing poorly. I was studying in the same way in Wisconsin as I had been in New Zealand. There's a part of me that thought, "I don't understand this part, so to hell with that." I'll do a lot of reading which was not on the assignment, but that I find more interesting. And of course I got no credit at all for the outside reading, while a lot was taken off for everything that I goofed on in the lectures. So then I changed my method of studying and things went very much better.

It's an interesting difference between the North American system of education and the continental British system. I do not know yet which is better, because with the American system you can be assured that, knowing the university that the person comes from, he will know a little about everything but not too much about anything. Whereas in the British system, the initiative and the emphasis on outside reading is stressed more, but the final product can be much spottier. You can know a lot about certain areas, but some areas which may be very important you would know very little about. I've been using the American system myself for fifty years, and I am still at a loss to know which the better system is.

MODY: Right, it's like Sherlock Holmes not knowing that the earth goes around the sun. Well, were there mechanisms at Wisconsin to help you figure out those cultural differences? You talked in your Nobel autobiography (3) about the International Club, did that help?

MACDIARMID: Well, naturally I was very interested in foreign students, and I became the president of the International Club, which was the largest student organization on campus. And as president, I had an automatic position on the student board that ran the student union. So I learned then a lot about Robert's Rules of Order (4). [laughter] You had to conduct the meetings according to Robert's Rule of Order, both when being a member of the student union and acting in my capacity as president of the International Club. So I found this very, very interesting indeed.

Then the chemistry department apparently nominated me for becoming a Knapp fellow, which was named after an alumnus of the university. This happened right when the then current governor moved to another mansion, and his former residence was right on the shores of Lake Mendota. Since the University of Wisconsin is on the shores of Lake Mendota, we had the opportunity of studying at the old mansion. There were six of us from different disciplines, so we were given three beautiful rooms overlooking the lake. The mansion was renamed the Knapp Center, and we were called the Knapp Scholars. Also, when special visiting scholars came to the university, we got to quietly spend an evening chatting with them. Then my parents sent me information about a new Shell Oil Company fellowship that was being offered in New

Zealand, so I applied for that when I was at Wisconsin because I had always wanted to get to England. And I was able to get one of the scholarships to go to Cambridge [England].

But one of the interesting things about the Cambridge fellowship was that it was for a single male, yet my girlfriend [Marian Mathieu] and I got married at least two or three months before the fellowship was due to finish.

MODY: Well, let me ask you about your work at Wisconsin. At the end of those two semesters on the Fulbright [Scholarship], were you ready to begin working with an adviser?

MACDIARMID: After the Fulbright, I signed up with Norris [F.] Hall who was then carrying out research on radioactive tracer work. He helped me apply, as I recall, for a year on the University of Wisconsin research scholarship, when it was funded by the research foundation at Wisconsin. I helped in finding some key patents in something to do with the dairy industry, and some sort of pasteurization process. So when the Fulbright Scholarship phased out, I was eased into one of these research assistantships, which was great. And then of course, when I came to England I had the Shell Fellowship, so one thing sort of leads into the other very nicely.

MODY: How did you pick what you wanted to work on at Wisconsin? You knew you wanted to do something inorganic?

MACDIARMID: Yes. I knew that because I had done my Master's degree in New Zealand on inorganic stuff. And Norris Hall, who actually worked for Madame [Marie] Curie in France for a short time, was working on radioactivity. This was at a time, after the war, when radioactivity was dark and mysterious. Scientists want to know how one can use this to find out what ions, atoms, or groups were exchanging with each other. I worked on radioactive cyanide groups, making use of this strange thing called radioactivity, which was very intriguing.

MODY: And some of the work was on Carbon-14 [C-14], right?

MACDIARMID: Yes. This was completely using Carbon-14, initially.

MODY: And this would have been at about the same time that [Willard F.] Libby in Chicago [Illinois] is beginning to do radiocarbon dating?

MACDIARMID: Yes. On occasions I had been to various meeting when Libby was there, I guess he was doing his funded work on heavy water. He wanted to know how much of the deuterium being produced, i.e. when D_2O is produced instead of H_2O, was due to atom bomb testing? So I listened to him giving a lecture when he asked, “What is the best way of getting water of absolutely definite known date going back decades?” And then somebody suggested wine. So they bought, as chemical supplies, certain French vintage wine which apparently had a lot of trouble getting through U.S. customs. [laughter] And then for mass spec [spectroscopy] work they only needed a fraction of a cubic centimeter. So you take out a teaspoon full of rare vintage wine and what do you do with the leftovers? You waste it doing that! [laughter] So that was very much in the early days.

MODY: And was your work partly funded by the Atomic Energy Commission?

MACDIARMID: This was actually funded by a research alumni organization, the WARF [Wisconsin Alumni Research Foundation].

MODY: Oh, the WARF.

MACDIARMID: You’re familiar with the term?

MODY: Yes, it is well-known in the history of science as one of the first commercial ventures by a university.

MACDIARMID: That’s right, the Wisconsin Research Alumni Foundation. It is interesting that you’re familiar with that.

MODY: Yes, it was quite innovative in its day.

MACDIARMID: So I guess this was based mainly on these patents that were obtained at the University of Wisconsin where they were given to the alumni founders. It is a fantastic idea.

MODY: So your work on Carbon-14 was more on [radioactive] tracers?

MACDIARMID: Yes.

MODY: Were they tracers for medical purposes or something else?

MACDIARMID: Well, that was what we were actually doing. You could not buy cyanide ion which was C-14 in CN. All you could buy was calcium or sodium calcium carbonate, the carbon was C-14.

MODY: Where would you buy these chemicals from?

MACDIARMID: I forget where, but it was probably from Oak Ridge or some other source. Starting with calcium carbonate, I had to try to find out how to convert the carbon from calcium carbonate to cyanide. I found a way to do this by using sodium azide which is what exploded when heated. And I perfected the method for converting the carbon in the calcium carbonate to the carbon in the cyanide ion sodium with a positive ion, and you'd get some radioactive calcium carbonate if you do that. And then actually we take the complex cyanide which was potassium cyanide aqueous solution and then mix it with sodium cyanide aqueous solution to see under what conditions of heat or light that some of the radioactive cyanide would finish up in the potassium cyanide.

MODY: I see.

MACDIARMID: And then from that one can get the rate of exchange and postulate what might be happening.

MODY: So you were using it to characterize reaction dynamics?

MACDIARMID: Yes. That is right.

MODY: One more question about Wisconsin and your early career. Was there a real sense of excitement and of doing something new in working on radioactive materials in that era? Was there a sense that you were building new institutions?

MACDIARMID: Yes, there was a lot of input of older students coming from World War II. And of course, atom bombs and atomic energy had been stressed very much in the latter parts of World War II. So anything that had radioactivity in it was very interesting. But you know, I would say there's no greater emphasis on that than if I were to take courses in the human genome today. It was sort of interesting, but everybody goes to his own area of interest.

MODY: This is interesting. Can you tell me a little more about the Shell scholarship? Was Shell a major company? Was their New Zealand unit a major corporate office?

MACDIARMID: No. The Shell Company was really about the same size as any other company. But I guess their goal was to create goodwill through public relations. I know there were Shell Company Fellows from different parts of the British Commonwealth. And when we arrived in England, the Shell Company representatives from Holland [the Netherlands] invited us all over and took us on a guided bus trip. We stayed at the best of hotels and dined at the best of restaurants, and got to see various parts of Holland. So it was very much a public relations aspect I think, to show the Shell Company was not just a place where you generally buy gasoline—it was real people. Also, they brought the Shell Company Fellows from different parts of the world together and we got to meet, and have a very active program. We would have get-togethers in London every so often, and they were not just five-minute chats.

MODY: Right. Did you still have this feeling that going to the U.K. [United Kingdom] was going home in some sense?

MACDIARMID: Oh, no. I left Wisconsin in December, caught a ship across the Atlantic and spent Christmas day in the middle of the Atlantic. And when I walked into the chemistry lab in Cambridge my heart fell. I thought, "MacDiarmid, you have come to the wrong place." The windows were so old and had so much dust on them you couldn't see through it. It was cold, and we never received our tea until morning tea time at 10:30 am, and I actually lamented that to some post-docs. When we went out to lunch, we could never discuss research—that was considered to be unprofessional. When it was lunchtime, it was lunchtime. You discussed politics, music, or art. You can discuss anything, but not problems you're having with research.

And then it really hit me that the creativity of a university is in not the buildings. Of course, I knew that there were great people at Cambridge, so it is the people *inside* the buildings. You can have beautiful buildings, but if you do not have good people to put inside, you have absolutely nothing. One would like to have enough money to have beautiful buildings, and also excellent people to put in the buildings. Certainly if you have excellent buildings, it will help to attract some people. But if you only have a limited amount of money, do you put it in the people, or do you put it in the buildings? So my feeling is since none of us ever have enough money for both the institution and people, put the money into people. People are creative, buildings are not.

So that was what I thought about Cambridge: it had horrible, dirty, old, decrepit, and cold buildings, but the people were outstanding. They attracted outstanding people from various parts of the world. There are some post-docs, and people from within England too.

MODY: Who were the people that were particularly inspiring or encouraging to you?

MACDIARMID: One of them is Evelyn Ebsworth. He was a graduate student about two years ahead of me, and we had a little two-person lab. But a member of the parliament in our district had recently died, so he was running for parliament at the same time as he was doing his graduate work. Fortunately for science, he lost the election. [laughter] But then he rose to be [vice] chancellor of Durham University. And that position, of course, is almost like a president or the head of the university. All of the people that I knew seemed to rise in their countries. There's one person from India that rose to be a chancellor in Bangalore, India. And so I have kept in contact with some people a little bit during the fifty years, but some seem to have kicked the bucket. And most of them have retired except stupid MacDiarmid. [laughter]

MODY: What kinds of characterization equipment were you using then?

MACDIARMID: We were just using a beta counter or something similar, it was very unsophisticated. We used something simple to just collect a little round circle of silver cyanide ion, I think, from the solution, and then used the counter to get a count from the beta radiation given off. And that was the only thing. It wasn't until I went to Cambridge University that I had ever really heard of doing an infrared spectrum. And you're a chemical scientist I presume?

MODY: I have an undergrad degree in materials science.

MACDIARMID: Right, it was obvious from your response, saying the things you did. I remember I was working with [Harry J.] Emeleus at Cambridge on silicon compounds. I said to him, "would it be okay sir, if I find the infrared spectrum on this?" Neither he nor any of his students had ever done infrared, so he said, "don't waste too much time on it." [laughter] The chemistry department at Cambridge had an infrared spectrometer, there was certainly one in the department, and maybe it was the only one in the whole university. So I got these funny specs [spectrums], and I didn't know what they meant. I said, "All right, this is my thesis, seriously, I can't waste a lot of time interpreting it. The important thing is to get the basic procurements of the compound, and the element of that knowledge, to the professor."

So I introduced Emeleus' group, and the whole inorganic division, to the whole frontier of the infrared spectroscopy. Not that I necessarily knew anything about the whole thing. [laughter]

MODY: So how did you choose a lab to work in and a topic to work on?

MACDIARMID: Well, I chose Emeleus because he was the head of the department, and he had written a textbook, Emeleus and [John S.] Anderson (5), that I used back in New Zealand for my post-graduate work. I still have a copy in my office, and I remember I sat down with him and he asked, “Well, where would you like to work?” I replied, “I've been working on radiochemistry at Wisconsin, and I would like to work with Dr. Merrick,” who was a lecturer at Cambridge. And Emeleus—he called everyone laddie—said “Laddie, you’re too young to specialize, and you might want to get into a completely different area.” He said that for example, the area that he was interested in was silicon hydrides, because he had done post-doctoral work with Alfred Stock at Karlsruhe University, and Alfred Stock was the person who had been developing glass vacuum lines.

So he said, “You know, you’re too young to specialize. You want to try something different,” so he talked me into using glass vacuum lines and equipments. Emeleus had done his post-doc work for Stock, they developed this whole concept. Alfred Stock had done all of his work working with soda glass, a soft glass, which is very difficult to work with. So Emeleus, the head of the chemistry department at Cambridge, built my vacuum system because the technician of the glassware at Cambridge only worked with Pyrex and did not know how to work with soda glass. So Emeleus had learned [from Stock] and in turn taught us how to blow vacuum lines with soda glass. He also said, “Laddie, when you leave Cambridge, you can certainly work with Pyrex and you’ll probably find it much simpler.” So when I came to Penn, we had no glassware and I built my first vacuum line myself with hand torch and blowing into Pyrex glass.

I remember there was one very hot summer and there was no air conditioning in this old chemistry building here at Penn. So I was in the lab dressed in a pair of khaki shorts and using the blow torch—you held the rubber tubing in your mouth so you can blow into the glass. And John [G.] Miller, a senior faculty of the chemistry department who is rather pompous but a very sedate person, saw me up there sort of sweating bloody hell in a pair of shorts and blowing glass. He said to me, “Alan, is everything going okay?” And I said, “Yes, I think so. Thank you.” Then he said, “Look, before you go today why don’t you stop in and see me?” So I went to see him after I had gotten dressed. And of course, John goes, “You know, I’m not sure whether it’s quite right for a member of the faculty at Penn to be here in shorts and sneakers, and working in the lab. I understand why you would do this, but you know, it’s not sort of the usual thing that’s done.” So he was very, very tactful in explaining this to me, an uncultured guy, a young person from the colonies. [laughter]

One more thing about the States; I feel that the United States is a country of dog-eat-dog and you have to become highly competitive. If you are hard working, you’ve got a little bit of gray matter up there, then the possibilities are great. Whereas in certainly more European countries, you can be as smart as you like, but your accent sort of acts as a glass ceiling in advancement. This is what happened to me very much at Cambridge and when I was living in college. In these dormitories, each student would have their own jip—their own manservant. Mine would wake me up every morning, he’d come in and open the curtains and say, “Oh, it’s a very nice day out there. Today’s is going to be a great day.” He’d clean my shoes and wash the coffee cups. We would always eat dinner in the hall of the college, and the sons of the jips were the junior waiters of the students at the table every meal—breakfast, lunch, and dinner.

So one young guy I was chatting with found out I was from New Zealand, and I said, "Oh, some day when you're through with finishing here in the hall, why don't you come up to my room and have a cup of coffee? I've got some newspapers and things from New Zealand." He says, "Oh, I couldn't. I could not come to your room, sir." And I asked him why not when he was through with cleaning? But he said, "I cannot come up to your room to have coffee with you. I could not come to a room, sir." So I said, "Oh, okay. I'll bring you some newspapers and give them to you for next time." Because in this social strata if he were found having coffee with a young gentleman up in his room, he would be stepping out of his social class and be ostracized by his peer group. And, as young gentlemen, we were reminded that we should or should not do certain things. We were not students, women students were young ladies, and men students were young gentlemen. And we all had to wear gowns after sunset. One of the interesting things was that you must not show ungentlemanly behavior when you are wearing your gown. And two ungentlemanly behaviors in particular—one was to smoke a cigarette while you had your gown on. The other was to walk down the street holding the hands of a young lady. Things have changed by now! [laughter]

But this is one of the things about the United States that I found is great, in speaking about the sciences, is that pure ability matters the most. It is very much more so than other countries where the way you speak immediately indicate who you are. For example, when you speak English, back in New Zealand and England, depending on the accent with which you speak it shows whether you went to a socially snobbish school or not.

MODY: A snobbish school as in a prestigious university?

MACDIARMID: By school I mean grade school, high school and university. And I remember at Cambridge after my first two or three days there, I was at the railway station and two students got off the train and one said, "By Jove, it is jolly good to see you, old man. How is your mater and your pater?" I thought that he was just being funny talking about their mater and their pater, but they were being absolutely stoic. I think the U.S. is great in that aspect. [laughter]

MODY: Tell me some more about the silicon hydrate work at Cambridge then. What were you looking for? What were you making?

MACDIARMID: Well, we were actually looking—really I think it is fun to this day, I'll give you that—but just as one has methyl-m group [with carbon and hydrogen], then you can have the SiH_3 hybrid. And then you can have all sorts of metal compounds and silane compounds. And so you can have CH_{3X} for halites, and SiH_{3X}, and then you also have ethyl, C_2H_5

compounds. You have H_3CH_2, and these can be ethers or halites or cyanides or—and then I was the first person to make any of the silicon analogs of them—of ethyl compounds.

And it was sort of fun. In group four of the periodic table you have carbon, silicon, germanium, tin, and lead. So before I would write any paper or start any lecture I would—and this is a joke here—I would say that “since silicon lies immediately below carbon in the periodic table, and since so much is known about carbon, it would be interesting to see how silicon analogs of carbon compounds are similar to or different from carbon,” and then to try to explain the differences.

I remember on one occasion there was an Academy of [Natural] Sciences meeting down in Philadelphia [Pennsylvania], and I was giving a speech after lunch. My entire group was attending, and they were sitting in the front two rows. When I said “since silicon falls immediately below carbon on the periodic table…” they all started applauding. Afterwards my group gave me a fake award that said “Since silicon lies immediately below carbon in the periodic table.” I never used that phrase a lot after that. [laughter]

One of the interesting things was to decide how, or to what extent, the empty 3d orbitals in silicon—which could form pi bonds with oxygen, for example—were important in determining differences between carbon and silicon chemistry. But it was one of these new areas, like genome studies, and certainly people knew it was at the cutting edge.

MODY: Were there some of the characteristics of a new field? For example, new kinds of conferences, new sources of funding, new textbooks, and things like that?

MACDIARMID: No, there was no real special emphasis. The one special emphasis I found later when I came to Penn was the Russian satellite. That was a very important emphasis for us. There was such a scare for us with having the Russian Sputniks the size of grapefruits going around the world. I had a contract from the Office of Naval Research [ONR] and the Army. Research money was relatively easy to come by due to the enormous shock to the United States of having a satellite from Russia going around high above us. We were really spoiled in those days when research money was much easier to get. I feel sorry for the young researchers and junior faculty of today, because it was relatively easy in my day, even as an assistant professor, to get research grants. It is much tougher now.

MODY: Even though the military was interested, this research must have had just a very basic orientation in terms of developing these carbon analogs and finding out about how the orbital structure affected molecules, is that right?

MACDIARMID: That is right. It really is just straight-forward, although difficult, synthesis. It was difficult because most of the substances were spontaneously explosive and flammable in the air. As it turned out, a year ago doctors diagnosed me from suffering from benzene poisoning that I got when I was a graduate student back in New Zealand. Those were the days where if you had grease on your hands, you'd go wash it with benzene to get it off. Apparently the benzene goes for the bone marrow, which affects the production of red hemoglobin, white cells, and platelets. Now I have to get a blood transfusion every week or so from the hospital.

MODY: Do you have any stories about it being a more sort of free-wheeling time in the lab in general; something similar to blowing your own glass or washing your hands with benzene?

MACDIARMID: You mean in New Zealand?

MODY: No. Just in general, before the environmental health and safety offices and their rules.

MACDIARMID: The only thing interesting that I can remember happened at Cambridge University. There was some work going on with the fluorine industry and research on fluorocarbons. For people working with fluorine gas, there're special first aid kits that if you believed that you had gotten a spot of liquid fluorine or liquid HF [hydrofluoric acid] on your skin, you could immediately inject yourself with a hypodermic needle. The needle is already loaded with chemical devices that would counteract the poisonous chemicals. And so there was a lot of emphasis placed on using liquid HF, but not much on anything else—I can't remember any emphasis on goggles or rubber gloves of any sort at all. Absolutely none whatsoever. [laughter]

MODY: That must have been really dangerous, since you were probably working with silane and some other pretty nasty chemicals.

MACDIARMID: Yes, we were working with that a lot. Boy, you know the word silane, don't you? We had to make it ourselves, but then of course, silane is flammable in air depending on the ratio you had, so it was really dangerous. Back in those days of Russian satellites and rockets, I got a research contract from the Air Force on the possibilities of developing rocket fuel using silane and silane compounds—which was really ridiculous. But we were also looking for borenes and all sorts of possible propellants. For rockets, why not use something like SiH_4 with oxygen that can give SiO_2 and silicon oxygen bond which is very strong, and the hydrogen given H_2O and the hydrogen oxygen [bond] run very strongly. You'd work out some rationale here, but any funding that sort of has the possibility of being rocket fuel was relatively easy to come by.

MODY: So how long were you in the U.K. then?

MACDIARMID: I was at Cambridge University with a PhD degree for three years, and then I got a temporary junior lectureship at St. Andrews University in Scotland, at the Queen's College branch in Dundee [Scotland]. But the weather was so cold that my wife from Illinois was getting chilblains, which is a poor circulation problem where you get very itchy skin and blisters. So we searched around to get some warmer part of the world. I nearly took a job at the University of Witwatersrand in South Africa, but then at the same time I put in an application for a position here at Penn. Charlie [Charles C.] Price, then the head of the Penn chemistry department and president of the ACS [American Chemical Society] on occasion, knew Emeleus. They had gotten together and Emeleus said some nice things about me. Charlie Price had just come from the University of Notre Dame, and he hired me sort of sight unseen, which was very unusual. I think it was against the wishes of some of the more senior departmental people. So I came across from England with a very great salary of $4,800 a year, which of course was a pittance in part. This was as a demonstrator, and never in the fifty years since then has anyone been hired by the department as a demonstrator, but that was the lowest level you could go. [laughter] And then things worked out very nicely.

MODY: Were you determined at this point to stay as an academic?

MACDIARMID: I liked the feeling of academia. One of my good friends at the time from Cambridge, came to the U.S. with an appointment in biochemistry, and taught at the enormous salary of seventy-two hundred dollars a year. [laughter] This was enormous for me, but my feeling was that I had never been interested in money. I've always been interested in having the freedom of being able to do what I wanted.

MODY: So you started as a demonstrator. What did that entail?

MACDIARMID: That entailed working in lab quite often, it's similar to being a teaching assistant that I have now. After one semester it was suggested to me that maybe I could give some lectures in freshman chemistry. So then I would sit in a freshman chemistry lecture and write out the professor's lecture notes, and then the following day or two days later I would give the lecture myself to the overflow of the class. And then I was watched very carefully to make sure that I taught the right things. Although I remember in my first lecture, I wrote up on the blackboard S-U-L-P-H-U-R, and some smart-aleck in the class waved his hand into the air and said, "Oh, excuse me, but I think you've made a mistake." And I said, "I don't see anything." And he told me that I spelled sulfur wrong. [laughter] I just filled out for two semesters, after that they had enough fun of me and let me go ahead.

MODY: What was Penn like in those days? This was 1956? 1955?

MACDIARMID: Yeah. I remember when Emeleus was discussing Penn with me. I asked, "What sort of university is Penn?" He says, "Well, you know, it's one of the older Ivy League universities. It's not one of the best ones, but it's quite good. So as a foothold in the United States I think it was not a bad place to play." I stayed and all the while I had quite a few offers in the last forty-nine or fifty years. Really, I think it's a damned good place.

MODY: You had overcome your aversion to America by then?

MACDIARMID: Yes, I had an American wife and four American children. And I started to like American music, American food, and American movies. [laughter] One other thing, I found about how the United States is different from England from sharing an office with a person who grew up near and got his PhD degree at the University of Manchester, a red brick university. He said, "You know Alan, it's not fair. Because you went to Cambridge, many more opportunities are available to you than to me; assuming that we are similar in color." To me, I found that one of the nice things about the United States is that although the old school-ties do exist somewhat in law or business, where you can say, "I'll call my old friend, he was my roommate in school," in the sciences these ties don't cut any ice at all.

MODY: So after a year Penn had taken your measure and decided that you had the merit to be an associate professor?

MACDIARMID: Right. And then I went up the ladder from assistant professor to associate professor with tenure, then to a full professor, and then was the Blanchard Chair of Chemistry. And then the Nobel Prize came along. That came from work with Alan Heeger in the physics department and Hideki Shirakawa from Tsukuba University. Shirakawa was originally from the Tokyo Institute of Technology; he went on to Tsukuba University but also spent a year here at Penn.

MODY: So shall we talk a bit more about the development of your career? Let's quickly move through the early days up to the collaboration with Heeger. What was it like building a lab at Penn at the time? Did you have students from the very beginning? Did you have an empty lab space?

MACDIARMID: Yes, that was a slow business. The main thing was that I was given one small lab in the other building. I had to blow my own small glass vacuum lines since I had no students or technicians to help. And so when a possible student would come along and say, "I'm thinking of possibly doing research with you," I would say, "Okay, here's a vacuum line, you can use that. I'll show you how to use a vacuum line for silane and silicon hydrides and various things."

MODY: Did you start to attract students immediately?

MACDIARMID: Yes, pretty much immediately. Most of my first students are now retired, [laughter] but the list grew fairly quickly.

MODY: Pretty soon after you started, Sputnik happened, and this large inflow of money became available?

MACDIARMID: Yes, that happened pretty soon after I started. A lot of government agencies and other offices were interested in rocket fuels, so they supported the research a lot financially. They also invested in silicon hydride derivatives, due to the thought of hydrides as alternative rocket fuels. That funding changed when we got around to the conducting polymers, which was a completely and absolutely different area of science. The polymer research was very much due to Ken [Kenneth J.] Wynne who had been with the Office of Naval Research. He was and is quite an amazing person. At that time I was working on the polysulfur nitride—the sulfur nitrogen golden polymer. I came back from visiting the Kyoto University in Japan for three quarters of the year. And I told him I met this young Japanese person, Hideki Shirakawa, from the Tokyo Institute of Technology, and he showed me some silvery polymer which is called polyacetylene. I really wasn't sure what that was, but I planned to find out. To do that I asked if there was any chance that Ken could fund another post-doctoral appointment so I could try to get Shirakawa, who has given up work on polyacetylene, to work on this. Ken naturally asked me why, and I told him, "Well, you know, I've never seen this silvery polymer before." He laughed and asked, "Do you expect me to give you a new grant because it's something that's silvery? That's ridiculous…but why don't you write me a letter about it anyway?" So I wrote him a two page letter. And I thought that would be the end of that.

Later I got a phone call from him and he says, "Alan, I think you're crazy and I'm crazy, but why don't you write a formal proposal for twenty-one thousand dollars? You know nothing about organic chemistry, you know nothing about polymers. And you're asking me to fund something because of its color." I told him yes, that was basically it. [laughter] He's since retired and is at the Virginia Commonwealth University.

MODY: Was it easy to kind of get into these channels of defense funding? Were they seeking you out?

MACDIARMID: No. Ken Wynne supported me for about twenty-five years. And the way he ran his branch of polymer science in the Office of Naval Research was to create a minimum amount of red tape, and a maximum amount of freedom, and I think if anyone wants to see how a government-sponsored research organization should be run, they should see what Ken Wynne did.

MODY: I guess I'm wondering how you originally found those sources of funding, first with AFOSR [Air Force Office of Scientific Research], and then with ONR.

MACDIARMID: Most of this came through giving lectures at various ACS meetings. For example, I met my contract officer from the Air Force Office of Scientific Research because I had been giving some lecture on silicon hydride somewhere and he came up afterwards, gave me his card and said, "I and the AFOSR are interested in possible high energy fuels, et cetera. Give me a call sometime." The same thing happened with Ken Wynne when we were talking about polysulfur nitride, he said, "I'm in the polymer division of Naval Research, and that's interesting stuff you've got there. Could you give me a call sometime?" So the openings came primarily through giving of lectures on the work, and then people from the funding agencies who were scouting. One of the reasons they were going to various meetings was to find likely young people whom they could provide good support. It can be a win-win situation for both scientist and agency all the way through, similar to "what's good for you is good for me."

MODY: I want to continue talking about Ken Wynne and the funding a little bit. When you first started with Ken Wynne, were you a little unusual for the people he was funding—in not doing organic chemistry?

MACDIARMID: Yes, I think so, but let me go back one step. So much of the technological developments in any country depend on the people in a little cubicle of a building in some city. They are the ones who decide what research project should be funded or not funded. Now they knew to say okay in my case, but those people send your proposal out to various reviewers. These reviewers can then say, "Yes, fund," Or, "No, don't fund." But who selects the reviewers? Normally you would select reviewers who have similar feelings to yourself, and you can say, "Well, this proposal is nonsense, I'll go to Bill James who I know feels similar to myself on such nonsensical crap as this." Or, "Although this proposal is screwy, it's completely different than anything else, and John Doe, he's picked up on some screwy things at times."

Or you can argue that the decision is not up to reviewers, but by a panel of reviewers that changes every year. Okay, who chooses the panel? Of course, it's suggested by the former panel members when their time of duty is finished. So it depends, I think, on an individual person who makes the decision as to what to fund, or to decide on what reviewers a new proposal will be sent to, or who decides on the composition of the panel.

This is extremely important. I mentioned at one of these lectures that some of this work I had done by moonlighting—I was being paid to do research on the sulfur nitrogen area, but instead I was working on polyacetylene. After I received the Nobel Prize, the BBC [British Broadcasting Company] had a roundtable discussion with all of the Nobel laureates in the year 2000 in the various fields, and one question asked how each of us got started. People were giving reasons like pollution and other things, so I thought, "What the hell, I'm just a person from the colonies. I'll say the bloody truth." [laughter] And I said, "Well, as a matter of fact, most of our seminal work was done illegally, by moonlighting." Then I explained that moonlighting was doing something on the side that you're not really going to be paid for. This was in the library at King Gustav's Palace, where there was not enough room, so the wives and the family members were watching in a separate room. My wife, Gayl, was sitting beside Ruth Heeger, Alan Heeger's wife. And Ruth said to Gayl, "Oh! Alan [Heeger] shook his head and said that's terrible." [laughter] But it turned out in the next twenty minutes of conversation that most people, if not all, said that most of their seminal work was done on moonlighting. They were using personnel and equipment which were being paid for doing research on other subjects. The main reason was that if you submitted any of these crazy proposals, they will not get funded. [laughter] So you do moonlighting until you get some results coming along. All that's strictly illegal, but I think this is one of those things committing people in one way or another to explore crazy ideas.

MODY: So Ken Wynne's strength was that he could grease the wheels for that kind of activity?

MACDIARMID: Right. And what is more, he would even go out of his way with various people he was contracting. On one occasion he came up [from Washington, D.C.] personally and we visited one person at Rutgers University. Ken said, "I want you two to meet each other. I think maybe you could put some of your research interests together." As I always said to him, he was like an orchestra conductor. He had the people he supported, the members of the orchestra, and then he would try to bring people up and join them together. So I think that is the ideal sort of support in funding.

MODY: But at the same time did he give you a lot of independence? How often would you be in touch with him, and then what kinds of direction would he give you?

MACDIARMID: He didn't give us any direction. We would have a contractor's meeting once every year in Washington [D.C.], and we'd be given twenty minutes to present what we were finding out. And then Ken would have some of his colleagues from the Office of Naval Research, or from the Army, Navy, or Air Force, and they would discuss how to tackle the research, and coming up with novel ideas.

MODY: Were those useful for transmitting ideas among the different groups as well?

MACDIARMID: Yes. They were very interesting. I mean, other work was going on, and I think to myself, "Oh shit, I need time. Everybody else is doing fantastic, except you." I mean except for me. [laughter]

MODY: What were some of the differences from working with the Air Force and working with the Navy?

MACDIARMID: They were very similar. I think that Ken, in working with the polymer group, took a minimum amount of control as possible. On the other hand, you have the [U.S.] Department of Energy [DOE] where their control is fantastic. They want milestones of where you expect to be every six months. It was run like the government. [laughter] But they're great people.

But I wrote them a letter where about three weeks ago, after a meeting we had in Boston [Massachusetts]. I said that we need to have more knowledge of what's going on in the other groups in the hydrogen economy program. So that the left hand knows what the right hand is doing. I told them that they're not making full use of the technical manpower that they have. I feel they're taking too much control and it is being run like machinery, and I talked about stimulating new ideas and approach. On the other hand, ONR, AFOSR, and DOD [U.S. Department of Defense] are three organizations that are run very similarly. The problem at the moment is reducing the budget at a time when we need more money, and they're reducing it by imposing taxes. The DOE will give us a certain amount of budget and then use the government taxes of the ledgers to pay for the hurricanes and the Iraqi war. So interestingly the DOE budget is not taxed, but then there are government taxes added to other government budgets! That's one of the reasons I'm really finding other countries very much more advanced in certain ways that I feel are important.

MODY: Can you tell me a little about your involvement with the origins of the Laboratory for Research on the Structure of Matter, which I know both you and Professor Heeger were involved in?

MACDIARMID: Yes, that started about thirty-eight years ago, and it was a laboratory for research on the structure of matter. Bob [Robert] Hughes, who was a senior member of the chemistry department at that time, was an x-ray crystallographer. I was just a very junior member then, and I thought "synthesis" would be better, but Bob said the structure was the important thing. So it was called the laboratory for the structure.

MODY: Being a crystallographer, of course he would think that structure is more important. [laughter]

MACDIARMID: Right, so it was called LRSM, Laboratory for Research on the Structure of Matter, although it's more frequently called the "materials science laboratory." And that was funded first by DARPA [Defense Advanced Research Projects Agency], then by ARPA [Advanced Research Projects Agency] and now by the NSF [National Science Foundation]. However, the main method of operation remained the same—there's a director, an executive committee, essential facilities and seed programs, then more specifically projected programs.

In general, I think the concept is great. Individual faculty members put in joint proposals on work which could not be easily done alone, but can be done by a collaborative approach. It follows the concept of "one plus one makes more than two." They also have a seed program that outreaches for new concepts, by an individual or a group of researchers, for a one-year period, to just sort of see if something is feasible.

Finally, there's a very strong outreach program, we had a very active one in Puerto Rico in particular. It is for underprivileged people, African Americans, women, and people of Spanish descent, et cetera. The outreach program is a very important aspect. I think the overall program is great.

MODY: Let's talk about your work with Alan Heeger in the physics department. What organizations at Penn made that interdisciplinary research possible?

MACDIARMID: Well, it started off as industry related. And just from memory, I think it started about thirty-eight years ago. Bob Hughes and some others had heard that there could be money available for a laboratory doing research on materials science. Of course, forty years ago "materials science" was a buzzword, just as "nano-science" became a sexy word, and now "energy" is becoming a buzzword too. So I think they went to DARPA and said, "Well tell us more about the funding." They applied and the money came through. The university was very happy to be a part of that program.

MODY: What made things click with you and Heeger when you heard each other's presentations?

MACDIARMID: Well, we were working on silicon compounds at that time, some of which had an overall spherical symmetry. So I said to Alan at the coffee break, "We have these things, and apparently one was conducting and spherical." He said, "No, no, no, you don't want spherical things being conducting. And you want linear and two dimensional, not three dimensional…well, maybe we'll talk about it sometime." The coffee break was one of those practice sessions that all of us loathed. [laughter] We thought it was terrible, but it turned out to be extremely important, so the left hand knows what the right hand is doing in each department.

So we were sitting in my office, and then he said, "There's some interesting work that came out by Mort Labes at Temple University with $(SN)_x$, and it's fairly highly conducting." (6) And I asked, "What do you mean Sn_x? Everyone knows tin's a metal!" But he replied, "No, no, not Sn_x, but $(SN)_x$." [laughter] And so that's how we got started.

MODY: Well, what intrigued you enough about this to begin moving your research program into this area?

MACDIARMID: Mainly that Heeger asked me. I had told him that I'd made the precursor to $(SN)_x$ and he asked me if I could make it again. When he first approached me, I told him, "No. We would have to use a vacuum system, and vacuum systems are like toothbrushes—you don't change them. Once you put one chemical in them, it can contaminate the whole thing." So he asked, "Well, couldn't you just try it just once?" And one of my post-docs was working on silicon chemistry at the time so I did it once for him, and the first time it worked beautifully and we got beautiful golden crystals. But then we tried it again and again, for about three months, and we couldn't reproduce it. I was moonlighting and my post-doc was being paid to do the silicon chemistry, but luckily I had the energy to put in a proposal based on the research results. [laughter]

MODY: So were you only producing them for Heeger at that point, or were you actually interested in it yourself?

MACDIARMID: No. At first I was just doing him a favor. And of course, I was interested to see whether they were really highly conductive. Because Mort Labes, in his paper, had said that he had conductivities varying all over the place in his work, and they were not analytically pure. It's like the old story that: "Physical chemists do very accurate physical studies on impure materials, and synthetic chemists do very inaccurate physical studies on very pure materials." [laughter] I'd worked on the polymer test for the sulfur nitride, and when we got the results, they looked so pretty. Their color has really been the driving force in my life. This was, of course, a follow-through from the S_4N_4 work that I did as a lab boy.

MODY: So at what point then did you become interested in the sulfur nitride yourself as the main area of your research?

MACDIARMID: I've always loved the color, it looked just like gold. And I just got intrigued by this stuff that I'd worked on. I might also mention that the $(SN)_X$ was not a new compound, it was made by Margot Becke-Goehring at [the University of] Heidelberg in the early 1950s. So I was only trying to reproduce the reaction conditions that she had published. As a synthetic chemist, one sort of takes an innate pride in being able to reproduce someone's published work. And if you can't reproduce their work, you ask, "Are they wrong or am I wrong?" [laughter]

It's very interesting how different fads and fashions arise. Margot Becke-Goehring came out with this highly conducting stuff in the 1950s, but the world was not prepared for or thinking about highly conducting things in those days. And then Mort Labes, ten or fifteen years later, decided to try and reproduce her work—he got results with chemical purities and conductivities ranging all over the place. So then since I'd worked on something similar in New Zealand, I thought it would be interesting work when I saw these bright, shiny golden crystals.

MODY: And it was as much of a personal challenge as it was anything else?

MACDIARMID: Yeah. That is right. It was really a personal challenge. As a synthetic chemist, Margot in Heidelberg had a great reputation. And Mort Labes had gotten all of these varying results. I thought, "As a synthetic chemist, I should be able to solve, or at least clarify, the situation between these two groups of workers. I'd like to jump in the middle and see if what I get conforms with what Labes got, or what Margot got." It was an intellectual challenge, but once I saw these pretty colors I thought, "Oh boy. This is fun." [laughter]

MODY: Did you start to put the silicon hydride work behind you, or did that continue along for a while?

MACDIARMID: No. The hydride was when first I came from Cambridge, and then we started looking at silicon attached to transition metal carbonyls. And then we did things like trying tri-methyl silane carbonyl. These are in the days that organic metallic compounds and metal carbonyls were arousing a lot of interest. So I started off with silicon hydride work, and then transition metal carbonyls were arousing more and more interest as a possible catalyst and so then that made things like carbon CH_3 cobalt tetra-carbonyls. So then we thought, "Well, what about making silicon SiH_3, cobalt tetra-carbonyl and other carbon derivatives?" And it was much easier to work with the hydrogen replaced by organic roots and by using tri-methyl-silo instead of silo-SiH_3 to use three methyl-Si with a methyl group replacing each hydrogen. And so we published some papers on silicon transition metal carbon species. And all of these carbonyls were in colors. Color has driven me a lot, I like pretty things.

MODY: Well you were talking about fads of research topics before, what had changed between the original syntheses of polysulfur nitride and when you and Heeger took up the question again? What questions had changed that made it an interesting topic?

MACDIARMID: That's a good point. Margot Becke-Goehring in the early 1950s in Heidelberg had shown this high conductivity, but nobody was interested in conductivity at the time. It wasn't until a key thing developed, TTF-TCNQ. That stands for tetrathiafulvalene-tetracyanoquin, both of which are organic solvents. If you take an equal amount of solution of TTF and TCNQ and mix them together, you'll get crystals that have high electrical conductivity. So this then sparked a lot of general interest and Heeger was involved in its investigation. So what sparked the interest was probably the discovery that organic compounds can have charge-transfer salts with high conductivity. I think it's interesting when you have organic compounds with high conductivity, very much more so than inorganic compounds with high conductivity. So the TTF-TCNQ area held some indications, and Alan Heeger had published a paper suggesting some super conducting fluctuations in this. And sparked a huge amount of controversy—a paper came out with thirty different authors disagreeing with it (7)! [laughter]

MODY: So was it that these molecules provided a way to expand and complexify the study of electron transport? Was that the motivation to move from simpler materials to these more complicated organics?

MACDIARMID: I think the main thought was that the driving force was the whole physics community—how could you possibly have electrical transport of a metallic type in an organic material? Just a crazy thought. It's against anything known in the area of physics. I think this was the key thing. One can imagine having ionic conductivity, but having electronic? I know when we first published on the polyacetylene there was one very nasty letter in C & E [Chemical and Engineering] News which said that these people are just dealing with very high ionic conductivity, or capacitance effects.

MODY: So tell me about the process of working with Heeger and developing a collaboration where you learned each other's languages. What was involved in that process?

MACDIARMID: One of the key things with collaborative research is learning the language of the other person. One might say, "Okay, all chemists speak the same language," but they don't. Same goes for physicists or biologists. One of the key things here is learning the language of the other person. I think one of the greatest things that hold back interdisciplinary research is learning the jargon of the other area. In chemistry and physics, one would think of electrons in terms of mathematical equations while the other would think of them as being little red balls. One just has to learn to talk to each other. Then it comes to writing up a paper—if you're writing a paper with your own grad student or post-doc and there's a difference of opinion, you pull rank and say, "Okay, I think we should go this way." But if you're writing a paper with a person from a different department, of similar rank to yourself, you can't just say, "I want it this way," because then the other person will say, "Well I want it *this* way." You have to sort of work together. It takes three times as long to write up a paper with a person from a different department. [laughter] Interdisciplinary research is becoming more and more common, and it must be the way of the future, but it's very time consuming.

One of the key things was that we arranged to get together every Saturday morning. But we would not discuss the manuscript of a paper or any research proposals, we were just there to smoke cigarettes and have coffee and let our minds wander. We would ignore the telephone. For example, I would say, "Okay, let's talk. What is this dangling bond in the band gap? Oh, is it molecular? I don't have any idea what you mean." And he would answer, "You've got your band gap here. This is the conduction band, and this is the valence band." Then he would put down some equations on the backboard and say, "That means that this is a mid-gap state. And you can have electrons in it." And I said, "It's got electrons in it, sounds like a non-bonding molecular—if this is the pi anti-bonding. This is pi bonding. This is halfway, then this is a pi-non-bonding halfway between bonding and anti-bonding." And he answered, "Yes…I guess you could say that." [laughter] So we got together every Saturday, just to swap chemistry ideas and physics idea and teach ourselves a little bit about each other's discipline. It was fun.

MODY: But what about the mechanics of experimentation? Were materials going directly from your lab to his lab?

MACDIARMID: Yes, we were making the materials in my lab. But the trouble was that we would have a little bit of success, and Alan would go, "Oh, let me play with this. Let's do something." I would tell him no because I wanted to make sure we can reproduce it and make sure ourselves that it's looking right. In that way we got into the doping of polyacetylene in a very unexpected way.

MODY: I see, do you think you could continue with the theme of serendipities? How did you meet up with Hideki Shirakawa and learn about polyacetylene?

MACDIARMID: Well, that was the most important cup of green tea I've ever had in my life. [laughter] When I was a visiting professor at Kyoto University I was asked to give a lecture at Tokyo Institute of Technology. This was at the time of my silicon work and also of some sodium work in polysulfur nitride. After the lecture I was invited to have a cup of green tea with the head of the chemistry department and Shirakawa, a junior faculty member, was invited to join. And Hideki, as I found out later, didn't bother going to my lecture; but he came to tea because he was invited by the head of the department. So while having tea I took out a sample of the polysulfur nitride, and Shirakawa says, "I have something like that. It's silvery in color." When I said I'd love to see it, he left the table, went back to his lab and brought in a sample of polyacetylene. I didn't know what it was, so he told me it was polyacetylene. And since I was not an organic chemist, I asked, "What's the formula of polyacetylene?" So he tells me that he got it by polymerizing acetylene with a zinc catalyst. So I asked, "What's the conductivity? It's bright and shiny." And he replied, "Just about 10^{-5} siemens per centimeter. Like a semiconductor." So I said, "Oh, that's interesting, if I can get some money, would you spend a year with us?" That's why I wrote to Ken Wynne, to try to get Shirakawa to come over to the U.S.

We started work once Shirakawa got here, and we had some catalysts—aluminum oxide and titanium dioxide—present as ash when we did a carbon-hydrogen analysis. Together it was about ninety percent pure, but of course it should've been one-hundred percent. So we decided that we could get a sufficient wash to wash out the catalyst. We washed it and our sample got better and better, eventually the purity got up to about 98.6 percent; but the purer we got it, the smaller the conductivity was! We thought, "This is crazy. When you purify something, the conductivity should go up. Ah-hah! Maybe the titanium and aluminum catalysts are impure and are acting as dopants, making the conductivity higher." So that was the key thing. We found the purer it got, the less conductivity it had. For example, we found that $(SN)_x$ plus bromine vapor would give $(SNBr_{0.4})_X$, and that raised the conductivity by about one order of magnitude to that of iron. So we thought to see if we can use the same dopant we'd used for $(SN)_x$. When we did that, the conductivity shot right through the roof. Later on we found arsenic pentafluoride, which gave the polyacetylene an even higher conductivity.

MODY: So you had already been thinking of dopants before you got to the polyacetylene?

MACDIARMID: Yes, we'd published using bromine on the $(SN)_{x,}$ which brought the $(SN)_x$ to the conductivity level of iron (8). But it was not a large magnitude of increase.

MODY: But it wasn't automatic to think about doping with the polyacetylene until after you had tried these purification experiments?

MACDIARMID: That is correct, it wasn't until we found the more pure we made it, the smaller was the conductivity.

MODY: And was Heeger involved immediately with the polyacetylene work as well?

MACDIARMID: No. At first, we developed our own technique in measuring the conductivity of $(SN)_x$. But when we got into the polyacetylene, Heeger had much better equipment to measure the change in conductivity along with changes in temperature. We wanted to see whether the polyacetylene had the temperature profile of a metal or a semiconductor. If it were metal, as you decrease the temperature, the conductivity should increase. But if it's a semiconductor, the conductivity should decrease. So measuring the conductivity temperature profile was very important, and Heeger already had all the apparatus set up.

Also, he was much better versed in measuring magnetic properties than we were since we had no experience at all. Of course, it also helps that Alan has very good knowledge of the conduction processes and band structure in metals.

MODY: Do you want to tell me a bit about your own experience moving into organic chemistry with the polyacetylene? Was that a shock at all? Did you have to learn a whole new vocabulary?

MACDIARMID: Well, the only formal organic chemistry training I ever had in my life was just one course in New Zealand on introduction to organic chemistry. [laughter] So the real challenge is learning, puzzling over things, and asking questions.

MODY: Was Shirakawa helpful in that sense?

MACDIARMID: Yes. Shirakawa was trained as a polymer chemist, so he was very helpful. But I found really the interaction with the students in my group was the most helpful. I really shouldn't call them students, because they were like teachers.

MODY: And what did they teach you about?

MACDIARMID: I've learned so much from them. For example, right now I'm learning about agri-energy [agriculture energy] from one of my students, who is originally from Brazil. If it weren't for him, I would know very little about this whole energy study. So I just find that I learned from everyone. And also as I say, I made thousands of mistakes, and I hope I make thousands more in the future. [laughter] But I love the learning process.

MODY: What was some of the reaction to that famous 1977 paper on doping of polyacetylene? (9)

MACDIARMID: Actually it's rather interesting. For about fifteen to twenty years we—Heeger being a physicist and myself a chemist—were taken as mavericks, you couldn't really believe anything we said. Any reports with my name on it drew comments like, "This is a bumbling chemist who doesn't know anything about physics." And Heeger worked on magnetism for his PhD, as I recall, so he was considered as being way of out his depth. He had lost a lot of face by talking about superconducting fluctuations in TTF-TCNQ. So we were not believed by the physics group or the synthesis group. We did not belong to any standard group of people. And people would ask us to give talks at ACS and APS [American Physical Society] meetings sort of as amusing guys with all the crazy thoughts. [laughter] But we were really not accepted by any group. It really is funny.

It was only after we got the Nobel Prize that we were offered to join the academy, and even then we were cold-shouldered by everyone. [laughter] But then more and more people started getting interested, and more and more people started getting similar results. So slowly then people started saying, “Well, maybe there’s something worthy that these people are saying.”

MODY: But there must have been some small community that responded to this work.

MACDIARMID: Yes, there was a small, staunch community. For example John [D.] Miller, who’s at the University of Utah, was always a good strong supporter. So there was a group of people who felt our work was true, and they were interested to repeat the work themselves. And this is a small support group that kept on growing and growing.

MODY: How did those people find each other? Were they coming through you and then meeting each other?

MACDIARMID: Some people met through me, and some others met through a small group called ICSM, the International Conference on Synthetic Metals. It’s been meeting every two years now for fifteen years, and each time the group grew larger. It does not belong to any official organization, but every time we have a meeting there are always people from different countries that would like to host the next meeting. And lots of people would come, out of curiosity, to these ICSM meetings. They would hear the presentations and sometimes some of them would do experiments themselves. Others would tell more, and so the word spread. We started off in Boulder, Colorado with John Miller and I think about 115 people. And at the last one was in June [2005] with about eight hundred people.

MODY: At what point did people from industry become interested in possible technological applications? A couple of companies, particularly [the] Xerox [Corporation] and 3M, have worked with this, right?

MACDIARMID: Right. John Miller was employed by Xerox, and he’s a very farsighted person. I don’t know how official this is, but Xerox equipment had used polypyrrole conducting polymers for years; and at John Miller’s suggestion, they replaced that with organic conducting polymers. The organic polymers were better because it did not mechanically harm the drum, but it was still conducting so you can get electrostatic patterns on the drum.

In the beginning, a lot of companies thought that we could use this as a substitute for copper wire, but the conductivity and the air stability was not high enough. However, the field really bust wide open in 1993 with Richard [H.] Friend at Cambridge University discovered electroluminescence. By passing a low voltage, say around five to ten volts, through a conducting polymer, the cross film material would give out a visible radiation. I believe this occurred by accident on a Sunday evening in Cambridge. It was winter when it gets dark early, and a student by the name of Jeremy [H.] Burroughes was measuring some phenomenon with a piece of polyphenylene vinylene attached across some film. Burroughes came into the lab to check the progress on a Sunday evening when it was dark, and before he flipped on the light, he saw a green wire. When he walked over he found that it was this polymer film that was glowing. That was the beginning of electroluminescence. Today the whole electroluminescence field is based on a few volts of electricity across polymers that can give off different colors, according to the type of electronic polymer and what organic molecular compounds it had been sublimed on.

There are two types of applications that electroluminescence materials make very good sense for. One is automobile brake lights and traffic stoplights, and the second one is for flat screen LED's [light-emitting diodes]. LED's are much better than liquid crystal ones. With liquid crystal, you can't see it at a certain angle; whereas with LED, you can turn it almost at right angles to your field of vision and still see the picture.

I think this whole thing of electroluminescence really got the industry interested in the field. However, people also laughed because, in the beginning, electroluminescence observed in electronic polymers last in the air for only about an hour or two. So the liquid crystal people thought that they would be completely safe, since it'd never be of any commercial use. But now I think you can get electroluminescent material operating for about 40,000 hours. And then also a lot of work is being done on photovoltaics, from the point of view of sunlight conversion to electricity. And the efficiency has been constantly increasing, it's presently not good enough for commercial use, but I'm sure it will come.

MODY: Do you foresee anything else that is sort of up-and-coming with polymers?

MACDIARMID: People are now looking forward to electronic clothing with flexible material, and photovoltaic cells to charge capacitors and so forth. So this is another step forward with the whole aspect of electronic clothing. I think one cannot emphasize enough the enormous importance of industry. Once the industry feels maybe they've got a new product, they will stick at it and see what the problems are, and then overcome the problems. For example, one of the key things in electroluminescence is getting a good shield against atmospheric contamination. You needed a good amphibious shield to stop air and water vapor from diffusing in—and various approaches have been used. For example, we used several layers of films to lessen the chances of air diffusing through the pinholes of the film; of course, then the air found another pinhole to go around, but that slows down the diffusion process. Also, better chemicals are used to make better polymers to help eliminate air diffusion. But if one looks back, the role of plasticizers in many polymers has converted relatively useless technological polymer to something that can be really useful. So I think the input of industry is extraordinarily important, because they will push things to see where they go.

MODY: Tell me a little about the Electroactive Polymers group at NRL [United States Naval Research Laboratory]. Were they really trying to take your ideas and move them into Navy-relevant applications quickly?

MACDIARMID: Yeah. One of the thoughts was that one could use this for the transmission of information through sonic waves underwater. These waves could then be directional. In other words certain ceramics, like barium-titanate, could possibly have an interesting sort of piezoelectric effect—you put in an alternating field, and mechanically you'll get information and this can be directed through water. As a result then you can transmit information underwater in a directional mode. So from the point of view of an enemy, the transmission would be hard to find out. Also, we considered other applications like anti-fouling in naval vessels—we looked at the properties of various type of conductor paints which you hold at a certain potential that would discourage barnacles. To the best of my knowledge, none of these ideas turned out to be successful. But there was lot of interest in this area, so I saw lots of feeling in this direction and that direction.

So I would say the key things at the moment. Electroluminescence displays, flat screen displays in different colors, and photovoltaic are coming up. There are also various types of conducting polymers scanners. Heeger has done some very, very difficult work regarding that. There's also a lot of interest over photovoltaic and various types of conducting polymers in the forefront as the absorber of various things as energy collectors. A small company that's been out a couple of weeks in Texas is working on having polyvinylene carbon nanotubes in photovoltaics. My own feeling at the moment is that the whole area of electronic polymers is going to be industry driven. And in doing so, new scientific phenomena will be discovered. But it will be industry driven—which I think is great.

MODY: I noticed in a lot of the apparatus you were showing me from the 1970s that you were making batteries and storage devices with these polymers. Was that meant to demonstrate an industrial application, or merely to show off some of the material's characteristics?

MACDIARMID: The key thing was that we put in some early papers first to demonstrate, surprisingly, that organic polymers could act as storage material of electricity, and that the stored charge could be liberated on demand at a sufficiently high rate of power that it could be useful at some point.

MODY: I guess I'm interested in what you and Heeger were doing to build credibility for this. You talked about how you were very much outside both synthetic chemistry and physics. What were you doing to kind of get over those barriers?

MACDIARMID: We just tried to come out with research which was solid, so that people could not criticize it as being done sloppily. And I think our feeling was that good, solid chemistry and physics would stand the test of time. It's sort of interesting. One of the things that sometimes people say is that, "It's great to have papers in science." I didn't have papers in science, because my feeling is that I'm interested in *doing* certain things. Before publishing something, I make sure that other people in the group can reproduce the work without speaking to the person that has done it. We'll have a manuscript and say, "Okay Joe, can you reproduce the work there without speaking verbally to the author?" And my feeling is as Emeleus used to say to me—he said, "Theories come and theories go, but experimental facts go on forever."

So the feeling was I'll chug along at my own slow pace, trying to do reliable experimental work. Emeleus said to me when I left his group, "Laddie, do not go after the big fish. The big fish, if they're going to come, will come along over time. Just keep your face in front of the scientific public, and make sure that your experimental work is reliable and reproducible."

MODY: So were you choosing measurements to make based on interest and then trying to do them as reliably as possible to show that this field was credible? Or were you choosing measurements that would demonstrate credibility to the top communities?

MACDIARMID: Well, our key thing was to create credibility to ourselves. Can we reproduce it, or is it just due to something wired the wrong way? And when my group found that we could reproduce it, we'd ask Alan Heeger to see if he could really do a proper visible measurement. As an interesting side story, at the very early stage we found that we very much needed a good physicist. So I asked a certain physicist in the physics department if he would do some measurements. We arranged to have lunch together at Houston Hall, and as we were walking out the little side path and I was asking him if he could do some temperature studies for me. I don't want to say his first name so I'll call him Joe. And Joe said, "Alan, it's just a junk effect." So I replied, "Joe, if it's just a junk effect, and you know what the junk is, and you know how to put it in controllably and maybe take it out controllably, then you call it a doping effect." But Joe wouldn't have it, he kept saying, "It's just junk. It's crappy stuff. It's junk." [laughter] So then Alan Heeger entered and said he'd give it a shot. As a matter of fact, with one of our papers, Alan and Joe shared this small room trying to write up a paper together. And they ended up having a tremendous fight—one of them walked out of the room and said to me, "You'll have to make a decision. Whether he goes or I go." I thought about this and decided that I would feel more comfortable cooperating with Mr. Heeger.

And of course that worked out wonderfully because Alan and Hideki and I eventually received the Nobel. And after the Nobel Prize came along I had offers from quite a few places, and I almost accepted a full time position at the University of Texas in Dallas. Hai-Lung Dai, who was chair of the chemistry department at the time, was very strongly promoting my staying and keeping my Blanchard chair at Penn.

So I stayed on a quarter-time position at the University of Pennsylvania, keeping the Blanchard chair, my office and my secretary. And then I accepted a three quarter-time position at the University of Texas in Dallas. The reason that I stayed at Penn was that I had already immediately took the position in Dallas, but Hai-Lung Dai then got rustling with the administration to keep me and not let me go. I said to him, “Well, I’m already going, I've already committed myself.” He said, “Look, you can’t commit yourself full time to Texas. What about three-quarters time in Texas and keep some time here?” And I agreed.

I feel that Penn is a great institution, I look upon it like an elephant—it is slow moving, and it’ll never do anything very good or very bad in a big hurry, it is as reliable and as solid as the Rock of Gibraltar. On the other hand the University of Texas in Dallas is very much more like a mountain goat, jumping nimbly from one peak to another, and it may fall into a big abyss along the way. But I could have stayed on full time here at Penn, and like old soldiers never die, I would have only faded away. But I liked living my life with excitement. I like risk. I like being daring. I used to do mountaineering in New Zealand. I still put in about sixty-hours of work a week. And I like to be living.

Many people ask what I want to get out of life? I say it’s not a matter of what I want to get out of life, it’s what do I have time enough to do to put into life? I carry around a list of things I want to do, and I don’t have enough time as it is at the moment to do all of them. Then things developed very greatly, as one can see. There are various laboratories in the world named after me. For example, there is one in Wellington, New Zealand, another in Changchun, China, and one which I’m very much involved with in Brazil. And this one up in Karnataka, India and this one was just dedicated about four or five weeks ago which I didn’t have time to go for the dedication. I also received the Friendship Award, the highest award given by the Chinese government to a foreigner, for my work and interest in education and development in China.

MODY: So you’re still conducting active research? Where do you do it now?

MACDIARMID: I have a research group here and in Texas. Two weeks ago, we just put in video equipment so we can hitch up with video from the lab in Texas and the lab here [in Penn]. I like to remind people that I was fifty years old when I started research which resulted in the Nobel Prize; until then it’d always been silicon work of various sorts. Then by accident I got involved with Alan Heeger, purely because we were having a dress rehearsal for our materials science lab. Each of the faculty members was listening to other faculty members discussing what they were going to do, and I was interested in his comments on some organic collectively conducting materials.

MODY: Right. Tell me a little about some of the work that I know you were interested in, and I know also was associated with the NRL program on electroactive polymers and similar molecular electronics idea of the spun fibrils of the organic conductors.

MACDIARMID: Yes, actually we were the first people to apply this to conducting polymers along with the electronics. And we published a couple of papers on that (10). I think electro-spinning is a very interesting way of getting nano-fibers. Although the conventional method is more convenient but those fibers, while having the same diameter as the electro-spun ones, are not as long. I think the average diameter of those is around about thirty-two nanometers. So I think electro-spinning has got a lot of very interesting aspects to it. They're actually using some of the electro-spinning techniques now in making alcohol bio-diesel, although it is using the context in a completely different way.

MODY: What was the motivation to begin making these spun fibers?

MACDIARMID: Just fun. Some people had already done some very nice work in electro-spinning. So we thought, "Aaah, why don't we try to electrospin electronic polymers?" But electro-spinning started off in the early 1930s with sealing wax. A person was playing around with sealing wax at a high potential, and found you got these thin fibers. But I think there are real possibilities, one of my former students, Younan Xia, with Chinese origins, at the University of Washington has done a very nice thing. Instead of having one flat sheet, he had two electrodes very close together. And then he can take two electrodes that ground with 35,000 volts, and he gets fibers that he can process. So one of the things you get is fibers of all lines and parallel. Of course, that is getting closer to carbon nanotubes, which is a rather fascinating area. I work closely with Ray [Raymond H.] Baughman at the University of Texas in Dallas in that area, and it worked out very well.

MODY: What else do you think is important in the area of conducting polymers?

MACDIARMID: So in trying to think of any other interesting aspects, one of the key things is from the point of view of the future; we do not yet know why different conducting polymers give different conductivities. Also, we do not yet know whether conducting polymers can be made that have a higher conductivity than the best electric conductors like copper, silver, and gold. There are various theories, but no upper limit has been suggested by a physicist or chemist. To me, it would seem that having highly aligned crystalline material is necessary. Furthermore, from the point of view of superconductivity, we know that organic charge transfer compounds can have superconducting transition temperatures. I think the highest is about twelve degree Kelvin at the moment, but there is no reason to believe that you cannot get superconducting organic polymers. None have yet been made; however, I feel it is highly likely that superconducting organic polymers can be made. I don't think anyone can try to find it on purpose. Research wise you just keep an eye out for it, but the Holy Grail will be discovered more or less accidentally while looking for some other property. You never really *expect* to find it, but if you do find it you'll grab it quickly. [laughter]

My own feeling is that we will get much higher conductivity. There's no reason why superconductivity at some given temperature in organic polymers should not be discovered. So I think the sky is still the limit. And I think that the research in the future in this field will be driven by industrial technological developments. And somewhere along the line, either by accident or by doing an experiment for a different purpose, superconductivity or very high conductivity will be observed, at least there's no known reason to say it cannot be.

MODY: Tell me a little about the field of molecular electronics. I've heard people refer to you as one of the grandfathers of molecular electronics. And some of the early people who said that they were doing molecular electronics, like Mark [A.] Ratner, Ari Aviram and Forrest [L.] Carter all reference your polyacetylene work. Was that a community that you were close to?

MACDIARMID: I think the term "molecular electronics" means a different thing to every person that uses the term. [laughter] The main thought is centered around getting PN rectifying junctions together, with a Schottky junction, and therefore, a semiconductor. Now the other thought is that you can get, say a PN junction, and put two molecules together. I know people have suggested that they've done this experimentally, but when you get to that stage, there are so many questionable parts about the experimental techniques. When you focus down to a single molecule, how do you keep up with the electronic equipment to measure them? I think when people talk about molecular electronics they tend to be talking about two perhaps rather complex organic molecules, or even one molecule, with one end certainly being at P and the other end N type. Still, one molecule that will act as a PN junction will of course have many applications. I think that the term molecular electronics means something different to everybody that uses the term. But there I get very lost.

The thought of having complicated electronic equipment calculators the size of the head of a pin is quite possible, although at the moment I don't see how. But I think it is also partly science fiction. Think of Buck Rogers having his wrist watch radio, which was science fiction then, but is now quite reasonable.

So the following is one of the things I've thought about quite a lot. You make a PN junction, and you'll have two flat surfaces. It'll involve retro-spinning and have a flat surface. This is one of the things where I'm interested in it having onion-skin layers. Here you not only have something one wire thin, which could be a nanofiber, but you also have layers of different P or N dopes, like polymers, or thin films of metals to get PN junctions. So if you had some metal, then a circular layer of polymer all the way around it, then when you tap it you could have the P injunction. Or you can have multi-layers, and have them offset from each other so you could then tap separate ones.

But then the problem becomes—as you get smaller and smaller, how do you make contact with the outer world without the proper equipment? My thought is that instead of soldering, one should consider a non-mechanical contact way of tapping with the outer world. So this is from the point of view of having some sort of micro P junction or something in the thread there. I think that we must put much more emphasis on considering how to make contact with the outside world. We are all very accustomed to field effect and making these whole electric fields or magnetic fields, but we need to do more concentration on non-mechanical contacts. Sometimes I think we need to look back at the crude work we've done in the past and see how using new concepts, new techniques, new types of apparatus, and new chemicals can build on old concepts and techniques.

MODY: In your Nobel lecture you have a section on nanoelectronics. Were you thinking about how to look at electron transfers through smaller and smaller amounts of these organic conductors from the beginning? Or did that come later?

MACDIARMID: I think this became more and more of interest when we got into the electro-spinning, and finding out that we could make smaller fibers. And then we asked, "Can we use the smallness for anything?" Anyway, we have been doing some really good stuff. There are two areas that we're interested in now. One is nano-science where we're really doing research at the moment. We have some electrically conducting metallic form pure fibers. We can also make small hollow spheres which we call eggs and eggshells. The work in getting these spheres to form in solution should be appearing in print for the first time in a journal in about two or three weeks. These are very small and are considered to be in nano-science. The diameter of one of my hairs is measured by calipers at about 50,000 angstroms. The nano-science area starts when you get to sizes smaller than about one hundred nanometers. The smallest wavelength of visible light is under four hundred nanometers and so you're looking at something much, much smaller.

Then we go on to energy, which is my real area of interest at the moment. I'm interested because it involves highly interdisciplinary chemistry, physics, biology, and agriculture research. You know that Brazil has been in the past a leader in the renewable energy, but it can keep its leadership position for only about another two years. Six million tons of ethanol is being manufactured in Brazil, and that is quite remarkable. How can Brazil continue in the future to move the world in the bio-fuel area? One possible solution that I'm pushing at the moment is using sawdust and wood waste to put that into bio-alcohol through enzymatic hydrolysis. A lot has been said about that, including by the [U.S.] Department of Energy. Then there's bio-diesel, which we get by squeezing nuts with enormous weights. But as I pointed out in my lectures in Brazil, what do you with the stuff after you squeeze out the oil? And do you use soybeans that are meant to feed cattle? That is fine, but what I want to do is convert *all* of that into bio-diesel. And according to Bill [William C.] Ford [Jr.], the Ford Motor Company plans to manufacture a quarter of a million cars in the U.S. that runs on bio-alcohol next year. That is quite amazing.

Also, I predict in the future that we will have a switch to fuel cells. As I like to say, Swiss watches were magnificent and beautiful pieces of mechanical machinery. Then suddenly electronic watches came along and zoom, they almost disappeared. So I feel in another two decades, people will say that at the beginning of the 21st century, these magnificent automobiles were fantastic and wonderful pieces of machinery. Cars of the future will not have engines. They'll have fuel cells, run on bio-diesel or bio-alcohol. It'll have an electric motor that'll make the wheels go around, but we won't have any of this reciprocating engine business. Now I'll tell you that fuel cells must go down a hydrogen route. I have a recent contract with the Department of Energy on producing hydrogen from ethanol. We're working on this in the lab here, and trying to effect this. In principle, we've got a lot of available hydrogen, and it's better than fossil fuel. And by the year 2015, various methods of chemical hydrogen storage will become available.

So I think the following areas are important—one is basic research in nano-science where we've got some completely new stuff coming out. The other is in international science, economics, technology, and sociology—whatever you want to call it. I think scientists as a whole need to take very much more interest in how their science is used, where the world is going, and how we can help things.

Now there's a new thing that it's I think too early to make it official, but there's a new group of people I would like to take the time to get involved with. It's called the MacDiarmid Institute for Excellence in Science and Technology [The MacDiarmid Institute for Global Research Excellence, Inc.]. This is a non-profit institute, and it was incorporated last week in the state of Delaware (11). The name "MacDiarmid" will be copyrighted and the purpose of this organization is to bring together scientists from all over the world, particularly those interested in basic research, which may lead to new technologies which would be helpful to people in the different countries.

I feel that it's incorrect when people say you can't do basic polymer research and also dabble in technology at the same time. And we have this as proof—in the twenty-five years that our basic research was setting high standards, a Nobel Prize standard, we also have twenty-five issued patents. So you can be involved in two things simultaneously.

Another issue is that you can do first class basic research in two general areas: one general area where there's no obvious immediate technological use in science, and a second area where there is a *possible* technological use in science. Now if you do good quality basic research in either direction, why not choose the avenue where maybe there's some potential technological use in science? Obviously, if you do good basic research in general, somebody will find a technological use occasionally. But most people, companies, or countries cannot wait for a hundred years to get the technological use.

So life is exciting. I'm very excited about this new area of energy. Petroleum [use] has got to go down today, and we must grow our own energy. There'll be many different types of energy in the future. There'll be hydroelectric energy and solar panels placed out across the water, et cetera. The energy for anything in society will depend on the country, the type of conditions, and various other things. You see, this is to me very interesting. Just on November the 16th [2005] I gave two main lectures in Brazil. And after the first one, [Richard] Branson, who owns four airline companies including Virgin Atlantic Airways, announced he is considering making the bio-alcohol himself and having his whole fleet of planes run on bio-alcohol. I said, "If an individual entrepreneur can do this, why can't a whole country?"

MODY: Right.

MACDIARMID: And then with this electrospinning here, can we use this for anything? Or is it just nice to say we've got a bunch of stuff that's 1/500th of the diameter of someone's hair. Okay. That's fine. But can you put it to any use? For instance, can we use it in solar energy research? Somewhat by accident I became very, very interested in one small section in Emeleus' and Anderson's (5) textbook which talked about the effect of electrical discharges on gases. So we then set up an electric discharge apparatus which is exactly the same as the ozonizer that we used in a first year organic chemistry lab. When you pass air through the electrical discharge you would get the smell of ozone; sometimes you smell this on Xerox machines. The ozone then reacts with the double bonds in carbon compounds to form ozonide which then can hydrolyze with water and convert the double bonds into COH groups which you can analyze better. So we made up our own ozonizer and used a 15,000 volt old neon sign transformer for the power source. This is about fifteen years ago, and we were able to get SiH_4, silane, and things like dimethyl-ether reacting with each other to give end carbon silicon bonds. This is going back to old, old literature.

As a matter of fact, one of the things I was stressing in Brazil was the principle of going back to some of the old literatures. For example, this goes back to high school chemistry, any chemical reaction can have say A plus B to gives products, and you have to input activation energy. If you're dealing with an exothermic reaction, let's say an oxygen hydrogen fuel cell, for example, the reaction first gives out heat, then it finishes up with H_2O. So we say anything that burns in air is thermodynamically unstable in the air, therefore it will react, and then release energy as heat by electricity, or a combination of energy. This is high school chemistry. One other thing is that we know alcohol burns in air; therefore it's thermodynamically unstable in air. In this case your reaction will start off with air and alcohol, and finish up with CO_2. And if you start off with bio alcohol, every molecule of carbon dioxide has been taken in from the air by photosynthesis to make this.

Now normally in chemistry, how do you make two things react? One thing you can do is to cook it up by boiling it. In other words, you get over the activation energy, assuming that what you're doing is an exothermic reaction. So what happens when you actually boil something and heat it together? You raise it to a higher vibrational state. Or if you expose it to

UV [ultra-violet], or sunlight in the good old days, or either raise it too high of a vibrational state, you'll break the bond and get free radicals. Now what happened in the ozonizer is you expose the oxygen in the air to a very intense electric field which involves very little flow of electricity. The test right here is very important. We have several papers using sums of the list of discharge, or an electric field. So that is one of the methods we're using here in the electrical field. That's the same as a field effect transistor or a ChemFET [chemical field effect transistor]. The field of a transistor is a sensor of something airborne, let's say an impurity. You could alter the chemistry of your sensor path by using an electric field, which we already constantly do in any electronic equipment.

So this is one of the things that really I'll say at the moment is the "et cetera." In other words, I wouldn't put it on any transparencies for a lecture. But, it is an electric field, so it can then begin a silent electrical discharge where you pass air through. That is what you get through an ozonizer or when you smell the ozone when you're using a Xerox machine. You're not breaking the bond, so you're just getting the oxygen to go to a higher vibrational level. That way you get a vibrationally activated O_2 molecule which can pair either with another vibrationally excited O_2 molecule, or with a ground state O_2 molecule. So for an electric field, if you have a discharge, you can get a breaking of bonds. But we're not interested in breaking bonds. We're interested in using an electrical field with high voltage but low, if any at all, current. We're using apparatus where thirty-five thousand volts goes through micro-amps, which is even smaller than milliamps.

I feel that this is type of reaction has an enormous future. We've been using quite a lot of old textbooks which we got through inter-library loans, published in 1960 or 1950, referring back to papers of fifty years before. Now we are looking at some of this old stuff, but we have newer types of apparatuses and newer types of detection methods. So I feel that with the use of an electrical field, we can try to activate the oxygen in the air or activate the alcohol to power a car. People will say, "Yeah, but you're putting energy into the ozonizer." Sure, but the ozonizer we use is a small, conventional one. It is two-hundred watts, and the headlights in cars now already use that much—we're only talking about two one-hundred watt light bulbs. Moreover, in any of the modern cars, you use energy to start the car, you use energy when you're driving at night with headlights and taillights, and you use energy to drive the electric motors that circulates the air. So in other words, you already use that much energy even when all of the energy is created by the gasoline. Similarly, I believe you can have a fuel cell providing energy. We haven't produced one yet, but if we can get everything to work, we can make one. And yes, we need to expend some energy in order to get this fuel cell activation in its peak, but we already expend energy for all the electrical stuff, so it can be done. That is where we are right now, and we'll spend the next ten years investigating this whole silent electric discharge array, but this is a whole new energy area. When I first started, materials science was the buzzword. Then more recently nanotechnology, which is still a fascinating field, but I feel that the big field now is energy. Not necessarily bio-energy, but energy with no pollution of water, et cetera.

A long time ago, this person, Zoltan Kiss, who developed a solar energy company in Flemington, New Jersey, was visiting and I showed him some of these tubes which were in the ozonizer. He asked about some deposit that was in the tubes, and I told him that I didn't know what it was. That interested him very much, and it turned out to be multi-silicon. Actually it is silicon sub-hydride, so effectively it's not silicon, it's $SiH_{0.3}$.

I became very much involved with his company, the Chronar Company in the Princeton—Flemington area, in doing research. Our work led to a patent (12) on an electrical discharge apparatus for which you could take, say for example the semiconductor SiH_4, pass it through and get then Si_2H_6, Si_2H_8, and all these various things. Previous to this, in order to make a thin layer of silicon solar cells, you would need to take a plasma discharge of silane. But now they were selling the Si_2H_6 in cylinders that they made according to our method. Then you just pass this through a heated tube, and by a simple chemical beta deposition, you can get a nice silicon and multi-silicon solar cell material.

All these industrial applications and consulting are nice, but of course the money always meant very little. As a matter of fact, I was doing some consulting at the time for Zoltan Kiss. And he'd write me a twenty-dollar check and say, "Don't cash it until next month." [laughter] And then he'd say he couldn't pay any consultant fees, which was a lot of money, so he gave me one share [of stock] of his private company. And it was written on a piece of paper, and so I put it in the clothes drawer, along with with my clean socks. A few months later I got a letter from their attorney saying that the company had gone public, and my one share of stock had split into quite a few shares of a public company. Then I said to my wife, "Where the hell did I put that piece of paper?" I wouldn't have completely thrown it away, but I did the next best thing to it. We finally found it in with the socks. [laughter] The company did quite well, and we sold some of the stock from that and bought a small condominium. It was surely interesting it came from this piece of paper that I thought was worth absolutely nothing. But I've never been interested in money. I've been interested in being able to do what I want to do insofar as possible.

So the tie-ins were interesting. The first tie in from the sulfur nitride business, and my Master's thesis in New Zealand, to the polysulfur nitride electrical conducting polymer, which then later led to the organic polymers. Also, there's the tie in from the silane work from Cambridge, leading to the experiment of electrical discharge coming from the textbook by Emeleus and Anderson to give a method for making semiconductor raids by silane which could then be composed thinly fairly easily into most of the solar cells. This progress of one's own work is a really amazing thing in science.

[END OF INTERVIEW]

NOTES

1. A. Frederick Collins, *The Boy Chemist* (New York: The Odyssey Press, 1944).

2. The Royal Commission of the Exhibition of 1851 Science Research Scholarship.

3. Editor Tore Frängsmyr, *Les Prix Nobel*. *The Nobel Prizes 2000*, (Stockholm: Nobel Foundation, 2001). http://nobelprize.org/chemistry/laureates/2000/macdiarmid-autobio.html (accessed March 28, 2006)

4. H. M. Robert, S. C. Robert, W. J. Evans, D. H. Honemann, T. J. Balch, *Robert's Rule of Order*, *Tenth Edition* (New York: Perseus Books Group, 2000).

5. H. J. Emeléus and J. S. Anderson, *Modern aspects of inorganic chemistry* (Princeton: Van Nostrand Publishing, 1960).

6. V. V. Walatka, M. M. Labes, J. H. Perlstei, "Polysulfur Nitride – One-Dimensional Chain With a Metallic Ground-State," *Physical Review Letters* 31 (1973): 1139-1142.

7. G. A. Thomas, D. E. Schafer, F. Wudl, et al, "Electrical-Conductivity of Tetrathiafulvalenium-Tetracyanoquinodimethanide (TTF-TCNQ), *Physical Review B* 13, no. 11 (1976): 5105-5110.

8. C. K. Chiang, M. J. Cohen, A. F. Garito, A. J. Heeger, C. M. Mikulski, A. G. MacDiarmid, "Electrical Conductivity of $(SN)_x$," *Solid State Communications* 18, Issues 11-12 (May 1976): 1451-1455.

9. C. K. Chiang, C. R. Fincher Jr., Y. W. Park, A. J. Heeger, H. Shirakawa, E. J. Louis, S. C. Gau, A. G. MacDiarmid, "Electrical Conductivity in Doped Polyacetylene" *Phys. Rev. Lett.* 39, Issue 1098 (October 1977): 1098-1101.

10. I. D. Norris, M. M. Shaker, F. K. Ko, et al. "Electrostatic fabrication of ultrafine conducting fibers: polyaniline/polyethylene oxide blends" *Synthetic Metals* 114 Issue 02 (August 1 2002): 109-114.

11. The MacDiarmid Institute for Global Research Excellence, Inc. was incorporated in the state of Delaware on 21 December 2005.

12. Alan MacDiarmid and Zoltan Kiss, "Amorphous semiconductor method." U.S. Patent #4,459,163, filed 5 March, 1982, and issued 10 July, 1984.

INDEX