

## CHAPTER XIV AWARD ADDRESS\*

NEIL BARTLETT, University of California at Berkeley, Berkeley, California

### ROBERT A. WELCH AWARD — NEIL BARTLETT

(G. T. Seaborg: "I have the honor to present Dr. Neil Bartlett, internationally renowned scientist, a man who has conducted meritorious research in chemistry. He is being recommended on the basis of his synthesis of compounds of the noble gases and the consequent opening of broad new fields of research in inorganic chemistry".

Roger J. Wolfe: "I, Roger J. Wolfe, on behalf of the trustees of The Robert A. Welch Foundation, confere upon you Dr. Neil Bartlett the third Robert A. Welch Award in Chemistry on the basis of your meritorious chemical research. It is my pleasure to hand you a check in the amount of your award, and a certificate endorsed by the trustees and officers of The Robert A. Welch Foundation. Please accept my most sincere congratulations".

W. O. Milligan: Dr. Bartlett may I add the congratulations of all of us in this room, including my own.)

Dr. Bartlett\*: Ladies and Gentlemen, Friends. I am, of course, overwhelmed, as you might imagine, by this great honor, but I have to say, immediately, that I recognize myself as being an exceedingly lucky fellow. No matter what the merits of my case may, or may not have been, I have clearly had persuasive proposers and they have obviously labored mightily on my behalf. I am indeed grateful to these unknown supporters for their efforts.

Like Dr. Löwdin, I am under some pressure to be brief. Although it has been a remarkable, absorbing day it is late and I must therefore proceed quickly with my talk. I would like to talk to you about a little bit of research — in fact about the research which has been recognized by this award. I believe that the story will also illustrate some of the points that Professor Löwdin has made earlier.

In 1956 I was a graduate student, and I was supposed to be working on sulfur compounds and in particular on sulfur fluorides, but I was a curious fellow and for a complicated set of reasons I found myself, late in that year, working on platinum

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compounds. By sheer chance I made, I believe it was on November 5, 1956, a readily volatile fluorine-containing compound of platinum. This excited me because there were, at that time, no such compounds known. Although I was drawing my graduate work to a close this was clearly something that I should look into as soon as possible. I did try to look into it immediately but the problem proved to be too big for the few weeks remaining in my graduate program. So, I kept it very much in mind, and when the opportunity came of moving to the University of British Columbia in Vancouver, Canada, I took this problem with me.

I was fortunate in Canada in being supported by the National Research Council of Canada, and I have to say that I felt remarkably free to do what I liked to do, and I have to say also that today I have a sense that not too many people have the kind of freedom that I had in Canada at that time. Of course I wrote research proposals, and as you might expect, I did not include this problem, because I didn't know what the material was and had no good theoretical basis for proposing it for a research. Nevertheless, I took the problem on. I didn't worry about that, because I wasn't constrained to work solely on the problems that I said I would work on. I want to acknowledge that at this time I also had support from the Research Corporation (which is a smaller foundation than the Welch Foundation, and supports science in not only the United States but in Canada also). I benefited enormously from the Research Corporation support at the University of British Columbia. Although the money was not great in quantity (\$3,000 or so) it did not constrain me to work on tightly defined lines and along with the National Research Council support it was possible to tackle the problem in a thorough manner. It took two or three years to get to the heart of the matter.

This platinum compound is a remarkable material. When we put it in water, for example, it liberated all manner of things, including light, ozone, platinum metal and other decomposition products. Within three years, my first Ph.D. student, Derek Lohmann, and I had the problem pretty well licked. We knew the chemical composition of this material, and it turned out to have the chemical formula  $\text{PtF}_6\text{O}_2$ . It was, in fact, a salt of the dioxygenyl cation and the hexafluoro-platinate anion — both new ions. Platinum hexafluoride, you see, was the most powerful oxidizer that anyone had ever come across. It had oxidized the oxygen molecule.<sup>4</sup> Here we were then, at the end of 1961.

I talked to my chemist colleagues, at The University of British Columbia, about this material and they were most interested, but I detected an undercurrent. I thought maybe some did not really believe my conclusion, because I was asked occasionally if I was sure that everything fit the ionic formulation. We had been at

great pains to prove the  $O_2^+PtF_6^-$  salt and all of the properties, physical and chemical fit splendidly. But these doubters were a challenge and I had to respond to them. I had to find a more dramatic case than the oxidation of oxygen, to win over the doubters. It occurred to me early on that I should try to oxidize helium. On reflection, I realized that this would be impossible, because I also knew that the platinum hexafluoride gas would not even oxidize nitrogen, because that's the way we had been making our compound. We had been carrying platinum hexafluoride in nitrogen gas. Since helium is very much harder to oxidize than nitrogen, I dismissed helium from further thought. It wasn't until February 1962 — I don't unfortunately know the exact data, but it took a gestation period of 6 weeks to 2 months from the  $O_2^+PtF_6^-$  solution — when the next important event occurred. I was preparing a lecture; I was looking up something — exactly what I have forgotten, and as I turned the pages of the text book I noticed a diagram which illustrates a relationship which I should have remembered, because I learned it in high school. It reminded me that the ionization potentials of elements depend on atomic number and that the heavier elements have low ionization energies. I immediately knew what to do — look up the ionization potentials of the noble gases. If you trace the ionization potentials down the noble gases from helium through to radon, you see that the ionization potential falls from about 24 eV (an enormous amount of energy) down to a little over 10 eV (less than that of molecular oxygen) by the time you get to radon. Radon was obviously the thing to oxidize, but only Glenn Seaborg and his expert colleagues can handle that element, because it is an alpha emitter.

The next choice was xenon, and by sheer coincidence xenon's ionization potential was only 1 kcal per mole away from that of oxygen. With the thought complete, I rushed out of my office and tried to find a colleague of mine who was an expert in matrix isolation spectroscopy. He used to use noble gases all the time, and knew their behavior in detail. By chance he was in the corridor outside my office. So, I rushed up and asked him, "Do you have some xenon?" He was in conversation with another colleague; he turned and said, "why do you want xenon?" I responded, "I want to oxidize it". In fact, he laughed. I don't blame him, but it immediately confirmed my suspicion that I had not convinced him that platinum hexafluoride was the most powerful oxidizer of all.

I ordered some xenon. By this time my first student had left me and I had two inexperienced students with me, so I had to get down to it and do the experimental work myself. So, I made a very simple apparatus out of glass and quartz, and set to mix the xenon with platinum hexafluoride. I finally had everything ready by a Friday evening, in March. In fact, I have checked my notebook and it was around about 8:00 p.m. and I was worrying about my poor wife, whom I imagined would be wondering

where in the devil I had got to. My students had crept away, but I had everything set up. It was a very simple apparatus with the red platinum hexafluoride gas separated from the xenon gas (which is a colorless gas) by a very thin glass diaphragm. I had arranged with a magnet to lift some little spheres of nickel through the xenon gas, so as to smash the diaphragm in order to mix one gas with the other. As I broke the diaphragm, there was an immediate reaction between the xenon gas and the platinum hexafluoride gas. It was essentially the same kind of reaction as had occurred some weeks earlier when I had mixed platinum hexafluoride with oxygen.

So I rushed out of the laboratory to spread my news — and there wasn't a soul there! Well, there we are it was for me a tremendously exciting discovery.

Let me now mention another matter. Many people ask, "what use is it?" I am a little embarrassed by that question. I won't labor the point because I am convinced, as convinced as I am that I am standing here, that in time, this discovery will have practical value. We already know that radon can be scrubbed from air by xenon salts which oxidize the radon. Dr. Lawrence Stein at The Argonne National Laboratory has also shown that dioxygenyl salts take radon out of the air and liberate oxygen in the process. Radon is a natural contaminant of the air in uranium mines. There is, therefore, even here a possibility of practical application.

I know that basic research will pay off. It's important that we should think in the long term and I am afraid that today we are thinking a little too much in the short term, a little too much in the short term about everything. However, I won't harp on such serious matters now.

To end, let me tell you a story involving a Great British scientist (since I am British, I must use a British example). The scientist is Michael Faraday. Michael Faraday, was a great experimentalist, and a man of humble beginnings, but a man of great perception and great experimental skill. In 1831, he discovered the principle of the dynamo and a few years later the Prime Minister of that time, Sir Robert Peel learned of this remarkable discovery of Faraday's. He went along to Faraday's laboratory at the Royal Institution in Albemarle Street in London, and asked to see the little device that Faraday had built. Faraday showed it to him, and Peel said (and you all know what is coming), "And of what use is it?" And Faraday said, "I know not. But I wager that your government will eventually tax it." Long after, when Faraday and Peel were both dead, the electrical power industry became large enough to attract the eye of the Treasury and was indeed taxed.

I see a glint in W.O's eye, I had better quit the stage. I thank you for listening, and give my thanks and best wishes to The Robert A. Welch Foundation.



Dr. Neil Bartlett