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ORIGINAL PAPER

John Bockris at Imperial College

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My first contact with John Bockris was in October 1945 in the final year of my degree course at Imperial College, when he gave a 21 lecture course on Electrochemistry. This was immaculately prepared and delivered. His only notes for each lecture were contained on a 13 cm by 8 cm index card and he never hesitated or stumbled in his exposition. He was the youngest lecturer in the Chemistry Department at that time and this was his first year in that position. However, it was not the first time he had lectured. He came to Imperial College to do a Ph.D. in 1943 from Brighton Technical College, where he had studied for his first degree which was an External B.Sc. from London University (External degrees were of a much higher standard than Internal ones because the students had no contact with the examiners before taking the exams). His supervisor at Imperial was H.J.T. Ellingham who had become known for his free energy/temperature diagrams, but who, by that time, had little interest in research. John was, therefore, left much to his own devices which resulted in a thesis on six different subjects. While he was working on these, he got a job teaching chemistry in Acton Technical College (in West London). It was there that he honed his lecturing style. All this was done in the midst of World War 2 and he had fire-watching duties in the nights. From this, it is already evident that he was a man of enormous energy and of course, still is.

Dedicated to the 85th birthday of John O'M. Bockris.

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By 1946, the College was able to expand its research activities. John had already started research with Stanislaus Ignatowitz, a Polish Chemist who had escaped from the Soviet Union where according to John "he had been put to work in a salt mine, used in place of a donkey, to turn a rotor working a grinder". He eventually took part in the research at high temperatures. Harold Egan had been a student at Acton who had won a Beit Fellowship to support his work for a Ph.D. He joined John at Imperial in the Autumn of 1946. Students who were already at the College and had graduated that summer went round the Department to talk to those of the staff who were active in research. Brian Conway and I both decided that John's research plans were the most interesting and he accepted us. The team at that stage was completed by Ahmed Azzam who came from Cairo. Harold worked on salting out and salting in while the other three of us worked on various aspects of hydrogen overpotential. Harold and I worked in the room which was also John's office. It was a long semi-basement room with windows at one end opening into a sort of open corridor outside. John's desk was across that end and Harold and I each had a bench on one of the long sides of the room. Brian and Ahmed were in another room near the back of one of the big lecture theatres. Brian worked on hydrogen overpotential and especially the effect of minute traces of poisons [1]. Ahmed, following some work in Moscow, built an apparatus to measure the rate of hydrogen evolution at very high current densities [2]. John's supervision was very thorough, perhaps a reaction against his own experience of being virtually unsupervised. He used to arrive each day with a little notebook containing a list of points he wished to discuss with each of us and, of course, we could raise any problems that we had

encountered. My first work was a continuation of John's work on hydrogen overpotential studying a range of metals' behaviour in methanolic solutions. The metals were sealed into glass tubes using various cements and it was soon evident that the surface in contact with the solution was by no means clean. At this time, John had access to work in Frumkin's Laboratory in Moscow which showed that reliable results could be obtained only in a scrupulously clean system. This was confirmed by Brian's later work when he studied the effect of poisons on such systems.

In April 1947, a Faraday Discussion on Electrode Processes was held in the University of Manchester. This was an excellent opportunity for us to get to know some of the other people working in this subject, and also for John to present some of his work. He presented two papers on the solvent effect, one on hydrogen [3] and one on oxygen [4] overpotential based on his thesis work. He also made several contributions to the discussion. Unfortunately, some of the most interesting contributors, Frumkin, Levich, Ershler, and Heyrovsky, were unable to come to the meeting. Looking back on it, there are several very backward looking papers, amongst the more interesting ones. The Imperial College Electrochemistry Group assembled a table of standard electrode potentials which was published together with the other papers from the Discussion [5].

At this time, John decided that we must be more careful about the cleanliness of our systems and that I should work on mercury electrodes with attention to the purity of the materials and finally pre-electrolysis to remove the last traces of contamination [6]. This involved changing the mercury at 2-h intervals, so I stayed overnight, sleeping on John's desk[1]. I also used a dropping mercury electrode which, by accident, I made an anode. This resulted in the formation of a tube of anodic product from which the mercury continued to drop. John was excited about this and we sent a short paper to Heyrovsky who was also excited and published it in Czechoslovakia [7]. By the spring of 1948, John decided that I had enough for a thesis and should write it. He read it as I wrote and made numerous helpful comments. He was also my Internal examiner, as was usual then, and the External was J.A.V. Butler. This was the first thesis presented by one of his pupils, so I think he was more nervous at the oral exam than I was, but, fortunately, all went well.

By 1948, the group had expanded and John decided that we should have a group photograph

This must have been taken towards the end of 1948. Besides the students, it shows J.F Heringshaw, a member of staff who became a joint supervisor, with John, of Martin Fleischmann who started in 1947 working on palladium electrodes and the diffusion of hydrogen through thin Pd foils. Allan Wetterholm came from Sweden for 6 months to study hydrogen evolution on mercury for which he took over my apparatus. J.Bowler-Reed continued Egan's work on salting-in and -out. John Tomlinson started the work on high temperature melts. Hannah Rosenberg (aka Hedda Linton) started work on the temperature dependence of the rate of hydrogen evolution from methanolic solution eventually including the effect of solidification of the mercury cathode [8]. She and Brian studied the conductivity of protons in aqueous and non-aqueous solutions leading to a theory of this process [9]. Edmund Potter made a more detailed study of hydrogen evolution on nickel cathodes [10].

[1] Once I put a Tesla coil down by the woodblock floor and its discharge caused the cracks in the floor to light up, due to the mercury spilt by the previous occupants of the room. I think that the reason none of us suffered from mercury poisoning was that the windows did not fit well so that there was good ventilation, also the floor was not very clean and the evaporation of the mercury was slow.

John was always keen that the group should relax occasionally in a social gathering such as a picnic:

A picnic at Balcombe in Sussex; from left to right: Ignatowics, Conway, Parsons (in Azzam's fez); Hannah Rosenberg, Potter, Azzam, Egan, Tomlinson, Bowler Reed.

John with Hannah Rosenberg and another friend (not an electrochemist)

In September 1948 I was appointed to a three-year Assistant Lectureship at Imperial College during which I had to give a course of lectures on Thermodynamics and the Phase Rule to students on the Chemical Engineering course. This meant that I was formally independent of John and carried on my own research in a small laboratory. John respected my independence and our friendship continued without problems. We published jointly the work in my thesis [11, 12, 13]. He asked me to be a joint supervisor of Hannah on the temperature dependence work [8]. His group expanded rapidly until he left for the University of Pennsylvania in 1953, and in the whole of his time at Imperial, he lists 25 students who got a Ph.D. under his supervision; of these, six were under joint supervision. Despite all this activity, he decided to translate Kortűm's Elektrochemie into English [14]. This proved more difficult than he expected, although he was fluent in German as a result of spending summer holidays with his father in Germany before the war. The difficulty was not in the translation but in the way some of it was presented and he rewrote some sections including the whole chapter on Thermodynamics. He asked me to prepare two additions: on experimental methods and a collection of relevant data. In October 1950, there was a meeting of the Deutsche Bunsengesellschaft in Karlsruhe and we decided to attend, as we would like to find out what was happening in electrochemistry in Germany. I asked the Administrator of the Chemistry Department whether they would support our visit. He said "Why do you want to go? If they are any good they will come here!", so, we financed ourselves and made interesting contacts there. John, of course, got more out of the meeting because of his knowledge of German. We were treated as distinguished representatives of British Electrochemistry [15, 16]. I remember that we were invited to a dinner with all the important German members of DBG where they told many jokes; I could follow everything except the point. After the meeting we went to Tűbingen to visit Kortűm partly to discuss the translation.

Not content with this major translation and revision, John decided to found a new series of electrochemical reviews: Modern Aspects of Electrochemistry. Volume 1 appeared in 1954 [17] with Brian Conway's assistance. A Russian translation appeared in 1958 and since at that time Russia was a great centre of electrochemistry, it is worth quoting from the forward of that edition by Ya. M. Kolotyrkin, "The attention of Soviet readers is called to the book of Bockris which is destined for a wide circle of students interested in problems of electrochemistry, thermodynamics of aqueous electrolytes, physical chemistry of polymers and physiology. The book is not a monograph in the complete sense of the word, but is a collection of review articles devoted to the present state and advances made in recent years in a series of fields of contemporary theoretical and experimental electrochemistry. There are five articles (chapters) and each of these is written by a prominent specialist in the corresponding region of electrochemistry.....Professor Bockris, the author of Chapter 4 (Kinetics of Electrode Processes), has done a

difficult and fruitful job in collecting a large number of original studies carried out by different scientists at different times and in various countries. In the scope of the literature considered, this chapter is the most complete of all reviews appearing in this field in recent years.....Much valuable information on the energy and entropy of solvation of electrolytes and individual ions and also on the hydration numbers is contained the chapter written by Conway and Bockris "John asked me to write the chapter on Equilibrium Properties of Electrified Interfaces while I was still at Imperial College but I left before I could finish it. John read it in manuscript and wrote detailed criticism on the text. This was an invaluable help to me in clarifying my ideas and expressing them in the text.

At the end of 1950, as I was beginning the third and final year of my appointment as an assistant lecturer, it was suggested to me that I should apply for a post-doctoral fellowship in the Dundee College of the University of St Andrews. I was successful in this and moved to Dundee early in 1951. During my time there, when John was still at Imperial, we had an extensive exchange of correspondence. I still have a large bundle of letters. We criticised each others' papers in detail and I think both gained much from this. In April 1951, John went to the USA for a scientific tour of the East Coast. He started in New York visiting New York University and Brooklyn Polytechnic, then to Pittsburgh at the Carnegie Institute and the Bureau of Mines. He then went to the Electrochemical Society's Spring meeting. He wrote two long letters about his impressions which are often very funny. He was critical of the ECS meeting which he compared unfavourably with the Faraday Discussions. But he met interesting people: Hiskey and Post, Carl Wagner, Uhlig, and David Grahame. He characterised the lower end of the meeting by the quote which he maintained was true "Well, folks, we have Al here tonight to try to say a bit about corrosion. Al don't know too much about corrosion, but he's been tryin' to put a bit together from the books". Also, in Washington he visited the Naval Research Laboratory where he had interesting discussions with Schuldiner on hydrogen evolution. He then went to Amherst to have more detailed talks with Grahame, which somewhat reduced his earlier impression of him; he was too "thermodynamic" and rather strait-laced. Finally, he went to M.I.T. where he met Debye and again Wagner. Talking to Debye "was rather a colossal experience. He is the only great man who hasn't disappointed me. He really is terrific in clarity and depth of thought and ability to push away in a minute all but the essentials of a problem. His "fire power" is of a different order from that of any other scientist I have met yet" Wagner did not compare well with him "he refuses anything but the thermodynamic method although he seems to be dealing with a bewilderingly large number of problems at once".

John was also struck by the general American view that war with Russia was imminent and that they were ready for it. On the other hand, they were unanimously welcoming and friendly towards him.

This is probably one reason that he decided to accept an invitation to be a visiting professor at the University of Pennsylvania in Philadelphia in 1953. Another reason was the low probability of promotion at Imperial College. In those days, there was only one professor in a Department. In fact, at Imperial there was one Professor of Organic Chemistry and one for Inorganic and Physical. In addition, F. C. Tompkins had joined as Assistant Professor of Physical Chemistry in 1947, so the prospect was that John would have been stuck as a Lecturer for some time however brilliant his achievements. He kindly suggested that I might replace him during his temporary absence, but this was not accepted by the Department then, or again when his American professorship became permanent. Although electrochemistry continued at Imperial College, it was not on the scale of John's Group.

To be a member of John's Group at Imperial College was a stimulating and enjoyable experience. I am delighted to have the opportunity of recalling it on the occasion of his 85th birthday.

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