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THE FOUNDING OF THE INTERNATIONAL SOCIETY OF ELECTROCHEMISTRY☆,☆☆

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At the age of 26, I had been a Faculty Member of the Chemistry Department at the Imperial College of Science and Technology in London for only 4 years when I received a call from Sam Hoar at the University of Cambridge. He asked me to accompany him to Brussels. It was a mysterious phone call, and I wasn't sure what the object of the visit was to be — I knew only it was something about the formation of a Committee on Electrochemistry.

Later on, I learned that I was to meet Marcel Pourbaix (Photo 1), Professor Charlot and Valensi, and a number of other French and Belgian scientists who were to be the founders of what later became known as the Comiteé International de Cinetique et de Thermodynamique Electrochimique (CITCE), the forerunner of ISE.

ELECTROCHEMISTRY: 1949

The atmosphere and background of electrochemistry at this time was dominated by the dead hand of Nernst. Overpotential was regarded as a kind of disease suffered by gas electrodes. There must clearly be something wrong causing "the deviation". Perhaps it was gas bubbles, blocking the surface?

Electrochemistry in 1949 was an old and breaking science. In Europe it was dominated by industry and thoughts, e.g. of aluminium. In England and America, the textbook and reading was dominated by solution theory. Books of the time (e.g. that of Glasstone) contained mostly chapters about pH, conductivity, equilibrium and solutions, with one chapter on "overvoltage" ("The excess pressure to form a bubble").

Who were the leading names in 1949? The most mentioned was that of Nernst. Wagner and Traud were, of course, often mentioned by Pourbaix's presence because of his interest in corrosion. Debye and Hückel were names mentioned much in universities when one talked about electrochemistry. The most frequent exam question related to activity coefficients.

Erdey-Gruz and Volmer, whom I see as the fathers of electrode kinetics, and Frumkin, were seldom mentioned.

THE FIRST MEETING IN BRUSSELS: 1949

Who was present at the first meeting in Brussels? It was, of course, Marcel Pourbaix, who organized it, and T.P. Hoar from Cambridge, at whose invitation I was present.

Then, the Belgian, Juillard and the Frenchmen, Charlot and Valenci. Apart from these, the leading persons were Defay, Gierst, Breckpot and a Madame Boute, who functioned as secretary.

Three electrochemists, well known at the time, were later added: the Italian, Piontelli, the American, Van Rysselberghe and the German, Lange.

The personalities of these founders are easy to sketch. Pourbaix was dynamic, jolly, dominating, positive — always smiling and dashing about with tremendous verve, the heart of CITCE, its creator.

Hoar was a kind of balance to Pourbaix because he was so steady, practical and formal, speaking slowly in a very British way, full of dignity and definiteness.

Gierst was young and enthusiastic, experimentally inclined.

Valensi was a tireless and enthusiastic person, but had an unfortunate habit of not realizing when the time for the lecture ended and going on for an extra hour or more, until the audience was down to two or so.

☆ Talk given at the 40th meeting of the International Society of Electrochemistry, Kyoto, 1989.

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Photo 1. Marcel Pourbaix in his mid-sixties. (Picture sent by his son in 1989.)

Coming to the more human points of this first meeting, there were, of course, language difficulties. (English, then, did not have its present dominance.) We had French, German and English. Hoar always spoke English only, though he spoke slowly and clearly so that it was easy for the Europeans to understand him. Pourbaix spoke a mixture of French and English; Lange spoke German, Piontelli in Italian and French (and sometimes English). I spoke English and German and, when necessary, French.

Often, we would speak in our native language, and the other person would understand and reply in his native language. We got on fairly well like this, except when everybody was shouting in different languages at once, which was rather frequently.

One remembers tiny incidents. We arrived on Foundation Day at Pourbaix's large house in the evening. There seemed to be seven or eight of us. I remember standing all together by the fireplace, more or less in a line, when all the lights in the house went out. Unfortunately Pourbaix didn't know where the fusebox was. We used matches until finally Mrs Pourbaix came home and went immediately to the box and mended the wire.

What was the thrust of CITCE in that initial meeting? It was the thermodynamics of corrosion. It would be reasonable to say that the group was not an Electrochemical Society which would be recognized today. It was dominated by Pourbaix and his ideas. Certainly many of the things he talked about were of interest to electrochemists, but everything was turned to questions of corrosion.

"La lutte contre la corrosion" Pourbaix would cry with the missionary's light gleaming in his eyes, and with enormous verve he would rush from person to person, encouraging us to fight against this dreadful evil which would obviously lead to the breakdown of Europe at least, if not the whole world, unless it could be saved by the use of his diagrams.

From these early discussions came the idea that there might be something more than corrosion, that it was related to this amorphous branch of knowledge called "electrochemistry".

At this first meeting, I suggested that the Group be called the "International Society of Electrochemistry", but Pourbaix did not like this perhaps because he thought it would detract from the view of corrosion and thermodynamics.

There are two other things I would like to bring out which struck me in retrospect as anomalous (but I was only 26).

The first was the attitude towards Frumkin and the Russian school. With Parsons and Conway, I had already come to the conclusion that Frumkin's school was in fact at that time well ahead of us in London.

But this wasn't a view which was popular at all in Brussels in 1949 and any mention of Frumkin was met with a lip-curving question mark. "La theorie de Frumkin?" It was too kinetic and avoided the *real* basic stuff, thermodynamics.

In 1949 Pourbaix was saying that he wanted to *introduce* thermodynamics, that there wasn't *enough* thermodynamics in electrochemistry. The field was too experimental, it should be based on thermodynamic theory.

I pointed out in the other direction, thermodynamics had been indeed overstressed in electrode process chemistry, it was surely impossible to apply thermodynamics to reactions far from equilibrium.

But Pourbaix used to put these thoughts aside "If it is for the gas, ah — then we can add an overvoltage", was his always brightly and optimistically expressed opinion.

So, there was a contretemps between Pourbaix and myself in those early days but my view was not popular, and people were suspicious of kinetic treatments as not *exact*. *Models?* But these could be *wrong*[1]

[1] There was in those days in Chemistry a feeling that a theory had to be correct "forever". The concept of scientific theories as working stuctures, useful for a time, was not present.

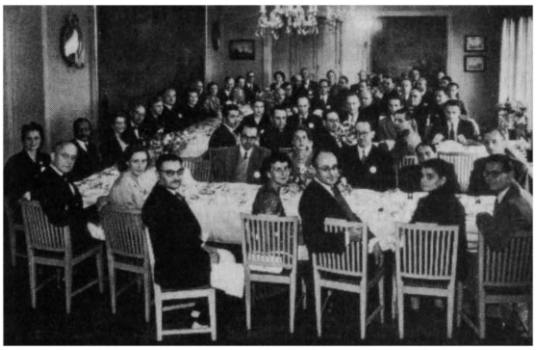

Photo 2. Photograph from 1952 meeting. Front row: Lange, Valensi, Hoar and Tragherdt.

MEETINGS: 1950–1954

I will now mention the succeeding four or five meetings of CITCE as it grew from babyhood to childhood. There was a 1950 meeting which took place in Milan and in Lake Maggiore in Italy at the Villa Bellinzona, a small palace on the lake.

I remember from that meeting particularly the contributions of Lange. He had driven down from Erlangen in Germany in a large Mercedes convertible. He talked much about electrochemical potentials and how the two terms, chemical and electrical, were not clearly separated, that the separation was a formality.

This meeting saw the formation of a committee on nomenclature, on which I served and had many amusing hours in the succeeding years, largely because of the attempts by the Europeans to change all electrochemical terminology. (Lange even wanted to change the symbol for the Boltzmann constant to B). A typical change called for was overvoltage to ''overtension'', *etc.*

1951 saw a small meeting in Berne at which I was not present.

1952 was a meeting organized by myself and Hoar in London and Cambridge, respectively (Photo 2). Here, the stress was primarily on nomenclature, but by the time we got to Cambridge in the presence of Agar, kinetic discussions were emerging, the tip of the iceberg. U.R. Evans, one of Pourbaix's major idols, was present part of the time.

In 1953 we were in Stockholm. There was stress on industrial electrolysis, with much discussion of economics, which is the first time I remember economic considerations being the subject of debate at CITCE.

The last of the meetings I shall mention was Poitiers in France. My memories are dominated by leading French members of CITCE, Valensi and Charlot. This year, I saw the phenomenon of Valensi lecturing for hours without stopping and the audience fading to two. Of course, Charlot presented electroanalytical themes, which in those days was essentially polarography.

THE EVOLUTION OF CITCE TO ISE

The evolution of ISE from CITCE was much debated, and it wasn't until 1971 that the name was changed. Pourbaix did not want the change. He seemed to want to continue to stress the thermodynamics, and to have electrochemistry as a kind of ancillary background subject for corrosion.

His influence had been so large that it was difficult to make the change. Finally, we agreed it should be ISE, but Pourbaix wanted to add ''CITCE'' after it, and I made up jokes about people would say ''I'm a member of ISE(*CITCE*)''. My view didn't really need much argument but Pourbaix had been (was and indeed is) The Father, and hence an activation barrier had to be crossed.

ATTITUDES IN THE EARLY DAYS

It's strange now when we see thermodynamics as a *background* for dealing with equilibria, to realize that in 1949, it was mainly Moscow that spoke kinetics and the rest of the electrochemical world[2] was thermodynamically inclined. The big change came with Vetter's Book of 1955.

[2] Although of course there were Laboratories (London, Gottingen, Berlin, Hokkaido) where the new brew was cooking.

Secondly, there was a greater difference between European electrochemistry and the Electrochemistry of England and America than now. The stress was upon electode processes, in Europe and at CITCE, and solutions were to do with *physical* chemistry, not electrochemistry. In those days, in England and in the United States, the stress was on ionics, and electrode processes was a witch's brew of gas bubbles, misunderstood signs, and unexpected potentials of mystifying origins ("It changes with the current density! Perhaps an iR drop?").

But I think there was a very positive spirit in 1949–1955. Pourbaix was, you might say, polyvalent, in the sense that he wanted to make everybody happy, and if you thought $x = y$, that was fine. If you thought it was $x = z$, that was fine, too. He was a great man for talking about giving and not getting.

This brings me to mention the origins of *Electrochimica Acta*. The early CITCE papers were published in a *Compte Rendu*. However, publication was not always very successful—I refer rather to printer's strikes and not to the machinations of referees who were in fact the Editor, Piontelli. I had a strong part in creating *Electrochimica Acta* and it was my repeated stimulation of T.P. Hoar that finally got us to Robert Maxwell and Pergamon Press. At last (1955) we had a "proper journal". T.P. Hoar was the Chief Editor until 1972. Hoar's view was that *he* was the best referee and he used to edit papers in great detail, correcting the English to bring clarity if perhaps not the Electrochemistry. I, on the other hand—the American-based Editor—did send papers out, normally to one referee and then a second if the first review was negative. I left the U.S. for Australia in late 1971 and a number of authors, those papers Hoar had rejected, persuaded Maxwell to replace Hoar with Thirsk.

CONCLUSION

Finally, my part in all these things, the development and evolution of CITCE and ISE in these early days, was largely that of a critic. I tried to pull CITCE away from its preoccupation with thermodynamics and corrosion to kinetics and quantum mechanics. This did not make me very popular: the impression seemed to be that there was insufficient support for my position and that I was a bit of a spoil-sport. In spite of all, I look back to that day in 1949 when Hoar asked me to come to Brussels to meet Pourbaix and his colleagues as one of the more important in my life, and I appreciate the meeting of ISE in Japan because it is a final confirmation of the growth of CITCE away from its origin as (effectively) a Committee on Corrosion to being truly and completely *the* International Society of Electrochemistry.

Thank you.