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Editorial

John Beynon: the Purdue years

Characters! . . . mass spectrometry has probably had more than its share of strong characters and John Beynon must rank within the most exclusive group of these. An all-time list might include Aston, the Trinity man, a Hawaii skier in a period when most Englishmen had barely made it to Margate; Nier, the elf, beaming with happiness at science and the scientists he encountered and putting in six decades at mass spectrometry, and Joe Franklin, the quintessential southern gentleman, charming, urbane, an industrial scientist who moved smoothly from industry to academia and was the founder of ASMS.

A man of steely determination and great energy, John Beynon was someone who set a course and habitually achieved his goals. He was asked to become Professor of Chemistry at Purdue after Fred McLafferty resigned to take up a position at his alma mater, Cornell University. Fortunately, in those days appointments were the province of the Head of Department since John's background would not have gotten him past a screening committee . . . he had no formal education in chemistry and no previous experience in academics. Instead he came with credentials in physics and the penchant of those in that discipline to ask fundamental questions. He also came with a background in the business world from which he carried his belief in efficiency and the command structure of large organizations. This latter point was brought home to me very clearly on the occasion of a visit, perhaps in 1972, by the Director of Research for ICI (Imperial Chemical Industries), the UK and international chemical giant. I had thought that JHB's idea of a command structure was that he was the commander and the rest of us the commandees. That he himself could also play the role of commandee, somewhere other than at the top of the chain, was evident from his nervousness at the visit of his former boss and added to my appreciation of him.

Laboratories at the time under consideration were both very different and very much the same as now. The focused effort, the team work, the late nights, the thrill of discovery, all these seem to me completely unchanged. The tools were very different. Not only instruments but procedures were of another age. Publications were handwritten and then typed by a secretary so 3 rather than 37 versions seemed to be a reasonable maximum, measurements took orders of magnitude longer (although chemical reactions, solution preparations, and sample work-ups have not changed much, hence the great attraction of high throughput methods). For these reasons one tended to plan more carefully, and to interpret data more deeply. In our lab, most data acquisition was pen and strip chart recorder which gave one time to think during the measurement or in the case of the commercial instruments, UV light on photosensitive paper. This provided a record that showed wide dynamic range, allowed fast scanning and was permanent provided one did not examine the data too carefully. . . in which case it quickly faded away with additional exposure to light!

One of the projects [1] that I worked on with John Beynon, Jon Amy and David O. Jones, was an imaging detector . . . an electronic version of the photoplate detector which we fitted to the plane-of-focus of the CEC 110B instrument, a very high resolution magnetic sector instrument that used a photoplate detector. The glass photoplates were automatically read to produce high resolution data on a large number of ions. The handling of large quantities of data in analytical chemistry was a problem that appeared early and often in various forms of mass spectrometry. The off-line nature of the measurement had obvious disadvantages—putting in the photoplate backwards, for example, was an error that only became apparent much later. The electronic version of this detector represented one of the very first electronic imaging detectors. It was not commercialized or widely copied but did represent quite new concepts and technology. It involved ions-to-electrons-to-photons conversion. While operating the instrument it seemed rather magical to be able to visually observe (with the vidicon) the lines of the mass spectrum and to adjust the magnetic field and other parameters and see immediate responses in the output data.

The two main instruments during John's years at Purdue were the Hitachi RHM-2 instrument [2], a super-high resolution instrument of which just six were built because of an error in the ion optics that limited performance. Looking at this instrument, with its five large pumping systems, 3 m ion path length, large electric and magnetic sectors, one had an impression of great power. One also learned, based on its performance, a respect for ion optics. The error was fortunate perhaps, since it meant that the instrument *had* to be used for something other than standard high resolution experiments. This opened the way to ion kinetic energy spectrometry (IKES) and the use of instrument, after modification, to characterize ions based on kinetic energy distributions. A

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logical extension of this development was to mass-analyze the ions to be studied by IKES. The resulting instrument, termed the MIKES instrument (mass-analyzed ion kinetic energy spectrometer) was designed in 1971 and completed in 1973 [3]. The original design was a hand-sketch on a single sheet and, in view of the importance of this instrument and its progeny, the VG (subsequently MicroMass) ZAB line of instruments this is a significant piece of paper. The story of these instrumental developments and of some of the experiments done at that time appears elsewhere [4]. However, it is worth emphasizing the key role of Jon Amy and the philosophy of instrumentation that he developed.

Purdue was one of the US universities that expanded dramatically, especially after the World War II when returning servicemen were given unprecedented access to higher education. The Chemistry Department grew very rapidly to become one of the largest in the nation, both in terms of undergraduate and graduate student enrollment. At the time John Beynon came to Purdue, the number of graduate students was about 500, almost all of whom were white American males. (Purdue still has one of the largest Chemistry graduate programs but its complexion has changed dramatically, but that is another story.) Jon Amy, a graduate student with wartime service at sea as a radio officer, used his knowledge of electronics to build a microwave spectrometer for the late spectroscopist, Walter Edgell. He stayed on at Purdue and gradually built up what later became the Jonathan W. Amy Instrumentation Facility. More importantly, he built a modus operandus that included hiring instrumentation scientists and electronics staff to work directly with graduate students in designing and building new instruments. He also helped the Department to acquire the infrastructure needed to go along with this instrumentation emphasis. It took many years for most of the Chemistry Faculty to appreciate the value of Jon's ideas which were tolerated for many more years than they were celebrated. Jon Amy himself and Bill Baitinger worked closely with John Beynon and their contributions were essential to the changes in the RHM-2 and the construction of the MIKES instrument. Jon Amy passed on his philosophy of work and his deep insights into understanding systems (of which individuals represent just one particularly interesting example) to generations of Purdue graduate students, who are richer as people for having known him. His views are summarized in aphorisms like "We always have time to do a job twice, never enough to do it properly." and "No surprises" and "Let's understand what he/she wants."

The small group of people in the laboratory in those years included the late David O. Jones, a post-doctoral and imaginative scientist who with John Beynon invented electronic black-jack, poker and other games of chance. This arose in conversation at the Pig and Whistle after a lecture by the South African chromatographer, Victor Pretorius. John Beynon remarked that one of the frustrations of coming to Purdue was that he had just been granted a UK patent on "Games of Change using Solid State Logic Devices" and had not had a chance to build a prototype "Fruit Machine." This started the project of building games using integrated circuits and the products were trotted out at parties where they created quite a sensation. John and Dave rather innocently decided to look into commercialization and arranged a meeting in Chicago. I have no idea what transpired but the ebullient Dave Jones was subdued and John was very quiet when they returned. Apparently they had met a group of people from another field—turf is perhaps the better term—to whom this invention mattered a whole lot. A little over a year later electronic gaming machines started to appear in Las Vegas and Atlantic City.

Another invention that obviously gave John Beynon equal pleasure was the "automatic salad crisper." This was a bowl containing a little water to which one attached a vacuum pump and inserted the wilted lettuce; turn on the vacuum and "poof" the lettuce crisped up immediately, as the air in its veins was pumped away and replaced by rigid water. John's great joy in this story was the idea of doing the experiment with salad dressing instead of water... internalizing, so to speak, the dressing.

Richard Caprioli was a young post-doctoral at the time who continued his close association with John Beynon after being made an Assistant Professor of Chemistry in 1972. Richard had the peculiar idea that one could do mass spectrometry on small peptides and other biological compounds, and it is rather amusing to reflect on the comments of biochemists at the time as to the foolishness of this idea. However, this is also an idea that JHB did not embrace; his appreciation for organic molecules having been too firmly fixed by the ICI Dyestuffs Division's favorites which ran (obviously) to aromatics often with azo, amino and other less pleasant substituents. Had this topic caught John's imagination, the history of biological mass spectrometry might have been different. As is quite well known, mass spectrometry was not highly respected in organic chemistry circles either at that time or indeed later. It had about it too wild an aspect ... too much speculation ... to be consonant with the rapidly gelling orthodoxies of physical organic chemistry of the late 1960s and early 1970s. This is unfortunate, since both fields had much to gain from each other and the subsequent transfer of much mass spectrometry activity to analytical rather than organic chemistry underlined the loss.

John Beynon was very interested in the history of mass spectrometry, and wrote at length on the topic [5]. Few things gave him more delight than the connection to history that came with the discovery that a post-doctoral of J.J. Thomson's, and associate of Aston's (Dr. Albert Eagle) was living in retirement in the Manchester area. John arranged a meeting and came back to Purdue full of enthusiasm. His colleagues learned from this the importance of mass spectrometry's equivalent of the thin red line ... the connection from one generation in science to the next.

Spring in Indiana is not a gentle thing. It comes suddenly, leaves briefly and often reappears again. The associated storms can be quite violent. There were spring-time aspects to JHB's personality. He was always enthusiastic about new data, often very kind to people if they had any real personal problems, but harsh when his standards were not met or time was being wasted. This could take quite dramatic forms. I remember two deliveries to the waste basket that were especially notable. One was a set of solicited comments on a chapter of the book “Metastable Ions” from a rather eminent physical chemist. The response to the request for comments on the chapter was substantial—in fact it rivaled the chapter itself in length. It made frequent references to the deficiencies of the chapter, the approximations in the derivations, the lack of real understanding of modern kinetics, the simplistic notation of spectroscopic states, the lack of consideration of vibronic energy interconversion. John Beynon, Richard Caprioli and I worked on this for some time, Richard looking dazed, me trying desperately to steer us towards consideration of what had been many hours of work on the part of the reviewer and John, looking more and more like an explosion was imminent. It was. The 20–30 pages of reviewer comments sailed across the room and thudded into the trash can. I retrieved them (not immediately) and have kept them; they were an earnest effort, maybe too earnest, at being helpful but they rather missed achieving this. *Metastable Ions* [4], which was essentially written in the summer of 1972, went on to become one of the most widely-cited books in mass spectrometry.

The other projectile was a scientific instrument. John’s preferred method of doing experiments was to sit with a person at the instrument and to do the experiment together. It was a mode of operation that worked well in a small group and ensured that the direction of research was highly orchestrated. It also meant that the normal glitches were simultaneously encountered by the graduate student or other scientist and by John Beynon who had less tolerance for glitches than the rest of us. This particular day the glitch in question was a picoammeter that was supposed to read the current at an intermediate point in a multi-sector instrument that was being scanned in a complicated way ($E/2$ or was that $2E$? [6]; while V was scanned forwards and B was set . . . or was that scanned). These uncertainties along with lunch, or the poor quality of service in the West Lafayette area, or just being 50 combined to make the flicker and then failure of the picoammeter too much. It sailed!

Spring in Indiana can bring lightening too. The best lightning strike occurred, I think, in 1971. The Purdue Mass Spectrometry Center had been funded by the NIH as a national research resource and this funding was renewed for a limited period after Fred McLafferty’s departure. However, we learned that the funding would not be renewed further. John visited NIH and talked to the program administrator and his superior. Then the official letter of declination arrived. I suspect it was a good thing, since it meant that we could discontinue the mass spectrometry service aspect of the lab’s work and concentrate on whatever we found most interesting. At the time it seemed like a disaster and it certainly meant severe cuts in staff and operating budget. That afternoon, the group gathered as usual and the talk turned to “let’s get them.” Unfortunately “they” were immune from the usual academic slings and arrows . . . so we resorted to the Caribbean method . . . we made effigies and burnt them. Obviously lab safety rules would not allow modern groups with grievances to do likewise, but it was an effective procedure and all of us felt quite good about the NIH after that.

Spring of course was not the only season. John did not spend the whole year at Purdue but always spent the summer. These were times of great activity and productivity. As an industrial scientist, John was punctual and days began quite early for all of us. The evenings were often the period of most intense work but on quite a few afternoons John would take an hour or two and go out to do photography. His favorite subjects were insects and there is still a nearby pond on McCormick Road where he spent hours trying to capture damsel flies and other insects close up and on the wing. He was very good at this, as he was at everything he tried.

In considering the science done during John’s Purdue years, several things stand out. High energy collisions of ions which lead to dissociation or changes in charge state or both; hydrogen/deuterium exchange rates and the information provided on rotational frequencies; measurements of inter-charge distances in doubly-charged ions from kinetic energy releases. Structured metastable peaks (Fig. 1) and their understanding in terms of energy partitioning, stands out as a singular accomplishment. One of the most interesting of these cases is the simple system, H_3^+, which dissociates upon collisional activation to give a broad and a narrow peak of mass/charge 1, i.e., H^+. The explanation of this is

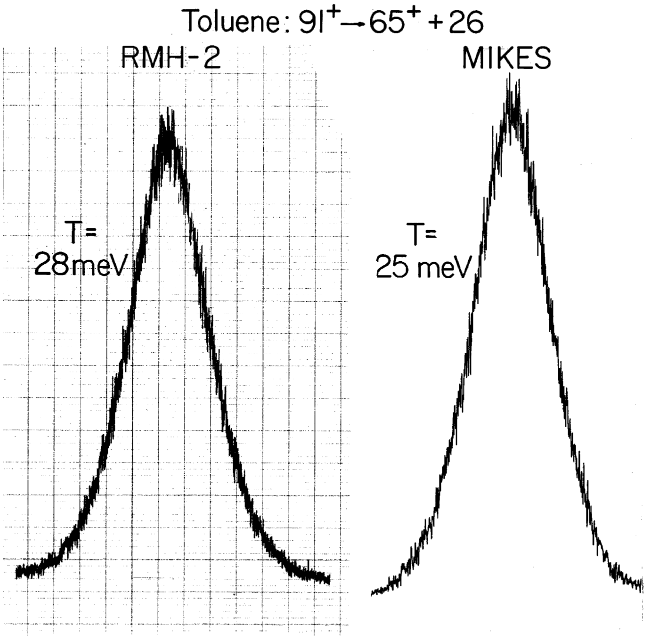

Fig. 1. Metastable peak shapes recorded on each of the major instruments discussed in this article, the commercial forward geometry RHM-2 instrument, manufactured by Hitachi, and the home-built reverse geometry MIKES instrument. The signals are for acetylene loss from the $[M - H]^+$ ion of toluene.

a pretty story of repulsive energy levels and vibrational predissociation [7]. What really took my breath away was the fact that John Beynon was convinced that there was additional fine structure in these peaks and said so! Later on, when apparatus of better energy resolution had been built at Swansea, the structure became indisputable. This sort of high risk leap was something that characterized John Beynon's approach to science. When he was sure of something he never hesitated to act.

John was a wit and raconteur, who owned a magnetic personality and was inspired by an audience. His stories, often repeated, were told with enormous care and conviction and had great punch lines. He was also an able sportsman, admiring the Rhodes Scholar President of Purdue more for his golf handicap (three, I believe) than for anything else he accomplished. I once had the pleasure of introducing John to a new sport, bowling. A good eye and natural coordination took him to a +200 game. He spent considerable time with me and my family and my second son, born in 1972 was named for him.

Looking back over the period under discussion, it still seems like a special time. The traditions that John Beynon established still continue strongly. Purdue is more than ever a center for mass spectrometry, with more than 40 Ph.D. students working on theses in the subject. The high intensity, high pressure era of John Beynon at Purdue set a standard the effects of which have been amplified rather than diminished over time.

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