A Short History of Heavy Ion Beam Probing

R. L. Hickok, *Fellow, IEEE*

IN the late 1950s, when controlled thermonuclear research was declassified, I was working for the Research and Development Laboratory of the Mobil Oil Company. At that time Mobil considered itself an Energy Company not just an Oil Company. They asked me to look into this potentially new energy source, so I made a visit to the major labs (Princeton, Oak Ridge, Los Alamos, and Livermore), came back and submitted a report. The report described the various experiments that were going on and concluded that if Mobil really wanted to keep abreast of what was going on in this field that it couldn't just sit by and read reports but that it would have to actively participate in the research. The response from Mobil management at that time was that they were willing to support a modest research program that would allow us to interact with the major national research labs and they asked me to propose an appropriate program.

One of the conclusions that I had come to after visiting the major labs was that there was a need for new diagnostic techniques for studying high temperature plasmas. My background was in classical and low energy nuclear physics and we also had available at Mobil a 2 MeV accelerator. Consequently I proposed that we develop a diagnostic for measuring plasma density by probing a deuterium plasma with a deuterium beam and measuring the proton production from the D-D nuclear reaction. Fortunately I was lucky enough to point out that this would be a rather slow measurement due to the small cross section of the D-D reaction and that eventually we should go to an atomic reaction rather than a nuclear reaction in order to make use of the larger cross section to speed up the measurements. Unfortunately, I wasn't smart enough to propose going directly to an atomic reaction.

Mobil responded by saying "go ahead" and agreed to put up \$100 K to establish a plasma laboratory (\$100 K was alot of money in the late 1950s). I again visited the various national labs and discussed the proposed research program. I remember in particular the response from P. R. Bell who was at Oak Ridge at that time. I don't remember his exact words but it was something like, "I don't know if what you propose is any damn good, but I support your effort because we need more bright young men in this field" (I was fairly young at the time). As a result of the visits to the national labs, we decided to build a clone of the hollow cathode arc being developed at Oak Ridge for the DCX experiment and to probe it with a deuterium beam.

Two things happened about this time, we hired F. C. (Buzz) Jobes and Mobil suggested that we should try to obtain partial government support for the program. It wasn't a question of money (at that time Mobil had more money than they knew what to do with) but the thought was that we would have closer ties with the national labs, if we had government support. The AEC was the Government Agency that was supporting most of the fusion research at that time. When we applied to the AEC we were told that they didn't think the idea was any good and, if it was, they could develop it better in their own laboratories. We finally obtained 50% support from AFOSR. At this point (early 1960s) there was myself, Buzz Jobes, Jack Marshall working half time and two technicians working on the project. Mel Gottlieb was a consultant and a very valuable contributor to the project.

By the mid 1960s, Mobil's plasma was operational and we demonstrated that it was possible to measure the density by probing a deuterium plasma with a deuterium beam and measuring the protons from the D-D reaction. As predicted this was a very slow measurement. One day after we had been carrying out a D-D measurement, I suggested that we switch over to an H_2^+ beam and look for an H^+ signal. The H^+ was loud and clear and that was the last of the nuclear measurements [1]. One of the first things we did using the molecular breakup of the H_2^+ to measure the density of the hallow cathode arc plasma was to study a coherent instability. Our frequency response wasn't sufficient to directly measure the instability so we used a trick. A Langmuir probe was used to detect the instability and we locked in on the Langmuir probe signal. The Langmuir probe gave a signal from a fixed spatial location but the beam probe signal was swept across the plasma giving us 2D spatial resolution. In essence, we made a stroboscopic map of the plasma cross-section. This was the first detailed mapping of a plasma instability.

It was published in *Phys. Rev. Lett.* in 1969 [2] and this had some rather amusing results. The article was selected by APS for inclusion in their year end summary of outstanding achievements in physics and was subsequently selected for distribution by the United States Information Agency. This resulted in it being distributed to a large number of semi-technical trade magazines who then printed articles on their interpretation of what we had done. One or two of them provided a reasonably accurate description of our work but most of the ones we saw were completely wrong and didn't have any idea of what we were doing. Since we probably saw only a small fraction of the stories that were circulated, we suspect that the vast majority were equally confusing.

It was pretty obvious that we didn't need a 2 MeV H_2^+ beam to carry out these measurements, the same results could be obtained with a low energy heavy ion beam. Buzz Jobes however, pointed out something that wasn't quite so obvious:

Manuscript received April 15, 1994; revised May 20, 1994.

The author is with Rensselaer Plasma Dynamics Laboratory, Rensselaer Polytechnic Institute, Troy, New York 12180-3590 USA.

IEEE Log Number 9403856D.

0093-3813/94\$04.00 © 1994 IEEE

if we used a low energy heavy ion beam, then we might be able to directly measure the plasma space potential simultaneously with the plasma density. To demonstrate this, we grounded the hollow cathode arc plasma through the secondary windings of a transformer and drove the primary with a 60 cycle signal. This worked but we suspected that everybody would think that what we were looking at was 60 cycle pickup. So we went out and bought an expensive high fidelity audio amplifier and replaced the 60 cycle transformer with an audio transformer. Now we were able to drive the space potential at audio frequencies and clearly showed that the effective space potential followed the drive signal while the density remained constant. [3] The high fidelity audio amplifier didn't particularly like this arrangement since the plates of the output tubes glowed red hot.

Some time in the late 1960s (I think it was in 1968) PPPL was converting the Model C Stellarator to the ST Tokamak. Harold Furth called me and wanted to know if we could measure the plasma current density with the crazy ion beam probe we were playing around with. I told him we hadn't thought about it, but maybe we could. He suggested that we get together and talk about it. We did some simple evaluations that suggested that the displacement of the beam out of the plane of injection should provide some information about the tokamak's poloidal field. We reached an agreement with PPPL to design and install a heavy ion beam probe on ST. Our time was being paid for by Mobil and AFOSR and all of the equipment was supplied by PPPL.

The primary purpose was to try and measure the plasma current density distribution. We were able to show that if ϕ was an ignorable coordinate, then the ϕ velocity at that detector was directly proportional to the poloidal field B_p at the point where the secondary ion was created and that the ϕ position at the detector would provide an accurate approximation to B_p[4]. We did observe toroidal displacement of the beam as it was swept across the plasma, but we were not able to interpret the experimental results as a measure of the current density distribution. The initial results were very tempting. If we ignored the toroidal variation of the vacuum field then the toroidal displacement of the secondary ion beam provided a very reasonable description of the plasma current density distribution. More detailed analysis showed that the fringe field on ST was responsible for most of the displacement. Thankfully, Buzz and I did a very hard analysis of the data and decided that we didn't have a measurement of the current density distribution. In this sense the first HIBP measurement on a confinement experiment was a failure since its main objective was to measure the current density distribution. On the other hand, it was a success in that in provided the first information on the radial potential distribution and the first spatially resolved measurements of Mirnov oscillations. In collaboration with J. Hosea and A. Dellis we were also able to show that the Mirnov oscillations propagated at the electron diamagnetic velocity when corrected for the $E \times B$ drift [5].

Two things happened in late 1970 and early 1971. The first was that the AEC fusion program informed me that they wanted to see this research program continue and if we did not obtain sufficient funding from AFOSR that we should apply to the AEC (this was very gratifying since we were originally turned down by the AEC); the second was that Mobil decided that they were an oil company and not an energy company. As a consequence, they cancelled the fusion research program and Buzz and I were fired (in Mobil's terminology our jobs were eliminated). Buzz joined the staff at PPPL and I took a faculty position at RPI. Buzz and Joel Hosea continued operating the HIBP on ST and carried out some very interesting measurements on the time evolution of the plasma space potential.

There was an existing plasma laboratory at RPI, but it was primarily concerned with space plasmas (not plasma space potentials). With the help of Bill Jennings, I re-oriented the RPI plasma lab towards fusion plasmas. At first we concentrated on the further development of HIBP measurements using hollow cathode arcs as target plasmas. We were able to show that it was possible to measure T_e, at low temperatures by using two different probing ions [6]. The first measurements of plasma space potential fluctuations were obtained and identified as a Kelvin-Helmholtz instability driven by a reversal in the radial electric field [7]. During this period we also obtained support from the fusion program in AEC.

In the early 1970s we also undertook the task of installing a beam probe on the LITE (Laser Initiated Target Experiment) experiment at United Aircraft in Hartford, CT. The LITE experiment consisted of suspending a LiD particle in a baseball magnetic field and then blasting it with a laser to form a plasma. This plasma was then to serve as a target plasma for neutral beam injection, but at this time there was only the target plasma. This research was supported by the fusion program. As it turned out this was our first and only failure of HIBP. There were two reasons for this. First, we did not recognize that the strong gradients in the relatively small baseball magnet would cause strong defocussing of the ion beam. Once this was realized a linear field electrostatic lens was installed in the beam line to refocus the beam in the plasma [8]. The second problem was that the plasma temperature was much lower than predicted. Our design was based on a predicted T_e of 100 eV and that a minimum T_e of 25 eV was required for any meaningful HIBP measurement. It was eventually determined by the UT research staff that the electron temperature was less than 10 eV. At this temperature the predicted HIBP signal was well below the noise level. John Stufflebeam was our on-site Research Associate for the LITE collaboration.

In the mid 1970s Ken Connor joined the faculty at RPI. About the same time I received a request from Ray Dandl to design an HIBP for the ELMO Bumpy Torus at ORNL. Ken took the lead in carrying out this program. This was the start of a long, successful (our evaluation) and very congenial collaboration with Oak Ridge. We were an intimate participant in the experiment and Ray Dandl was an enthusiastic supporter and collaborated in our efforts (Ray designed and built the lock-in amplifiers that were used as our front end detectors). This experiment worked very well. Early Langmuir probe measurements had shown that the space potential became more positive as you moved in from the edge towards the high temperature electron rings. We confirmed this, showing that there was a positive potential peak at the electron rings but that there was a negative potential well in the center [9]. This caused a complete revision of the theoretical explanation of the operation of EBT. Pat Colestock and later Spencer Kuo were our on-site Research Associates on the EBT experiment. (Personal note—EBT was a very interesting plasma experiment and I believe that the scientific community could have learned a substantial amount of plasma physics from this device and that it was a shame that it was shut down).

Also in the mid 1970s we started to look for a better target plasma than the hollow cathode arc for the advanced development of HIBP. DOE suggested (I think it was ERDA at the time) that we look at the VERSATOR Tokamak that Bob Taylor developed at MIT. With Bob's help we built a similar device at RPI known as RENTOR. Since we were supported by the diagnostic group within DOE we were told that RENTOR was simply a target plasma and we should not try to carry out any tokamak physics, in fact we were excluded from the "Small Tokamak Users Group". We agreed with this dictum in principle, but in practice we also carried out some interesting physics experiments [10, 11] (we had to since, we were training graduate students). RENTOR was shut down in the mid 1980s.

In the mid to late 1970s we were also asked to design a beam probe for the center cell of TMX. As part of the request, Fred Coensgen said he wanted to be able to point his finger at somebody in case it didn't work and I said fine, just give us the credit when it does work. It worked. We measured the density and space potential radial profiles under a variety of operating conditions and detected some low frequency oscillations at the edge of the plasma that propagated at the $E \times B$ drift frequency. [12] Near the end of the TMX experiment we operated the energy analyzer in an open circuit mode (all previous HIBP measurements used a slow feedback loop to keep the secondary ion signal centered on the split plate detectors). In the open circuit mode we obtained much higher frequency response and for the first time we were able to look for high frequency, up to 500 kHz fluctuations. All subsequent HIBP operated in the open circuit mode. Gary Hallock, even though he was only a graduate student at the time, was our on-site representative for the TMX collaboration.

Next our group was asked to design beam probes for the TEXT and ISX-B Tokamaks. TEXT required a 500 keV beam and ISX-B a 160 keV beam. Due to serious delays in the delivery of the 500 kev accelerator, the ISX-B system came on-line first. On ISX-B we measured the radial space potential profile under ohmic, neutral beam co-injection, neutral beam counter-injection and balanced neutral beam injection. We were able to show, in collaboration with A. J. Wootton and R. C. Isler from ORNL, that the MHD ion momentum balance equation was valid [13]. The first multi-point simultaneous measurements of high frequency fluctuations of $\tilde{n}$ and $\tilde{\phi}$ were obtained on ISX-B and we were able to show that, to within the experimental accuracy, they could account for all of the cross field particle transport [14]. Unfortunately ISX- B was shut down before we had time to exploit the full potential of the HIBP measurements. This work, however, was the start of a long, enjoyable and successful collaboration with A. J. Wootton (I hope Alan agrees) that continued when Alan took over as Director of TEXT and more recently as Head of the Fusion Research Center at UT-Texas. Gary Hallock, now a full fledged Research Associate was our on-site representative for the ISX-B experiments.

TEXT was one of the most satisfying experiments to be associated with. We had the full cooperation of the TEXT staff, first under the direction of Ken Gentle and later under the direction of Alan Wootton. Our first priority was on measuring fluctuations and their relation to fluctuation induced transport. We were able to show that in the edge region (ρ>0.8) that fluctuation induced transport could account for all of the cross field particle transport but not all of the energy transport [15]. We also showed that the Ergodic Magnetic Limiter (EML) produced a major change in the radial space potential profile and caused a poloidal variation in the amplitude of the density fluctuation [16]. HIBP measurements in the interior of the plasma (< 0.8) indicated that the average wave number k was around 1 cm^{-1}. This was in an apparent disagreement with FIR scattering measurements which indicated a much higher value for k. It was suggested that the low k values indicated by the HIBP measurements were due to errors introduced by finite sample volumes or primary and secondary beam modulation as they were attenuated by the high fluctuation amplitudes in the outer region of the plasma. We have carried out extensive evaluations of both finite sample volume and beam attenuation on the evaluation of the wave vector from HIBP measurements and concluded that both could lead to smaller values of the wave vector, k, but that they could not account for the observed k measurements. This was confirmed by independent evaluation by Ross and Sloan of the TEXT scientific staff [17].

The primary on-site Research Associate for our work on TEXT was Paul Schoch, who is now an Associate Professor at Rensselaer and directs our work on TEXT Upgrade along with another Associate Professor—Tom Crowley. This new system, which has recently become operational, is the largest beam probe built to date and required some extensive development work, particularly for the analyzer. Tom Crowley describes some of the more significant features in his paper in this issue [18]. After our work on EBT and ISX-B came to an end, we were able to continue our fruitful collaboration with Oak Ridge by designing and operating a beam probe on ATF [19]. The very complex magnetic field structure of this interesting device offered some unique design challenges that were met successfully so that we were able to make both potential profile and fluctuation measurements. Once again, as with EBT, we were able to demonstrate the usefulness of the heavy ion beam probe on a device whose life was cut short by funding problems. Stellarators remain a promising confinement concept that is being pursued at quite different levels in the US, Japan, and Europe. Beam probes are being designed or are planned for some new stellarator devices.

I know that I have not written the complete story of the beam probe and that I have not mentioned the contributions of many people—especially students—who I have had the pleasure to work with over the years. I have tried, primarily, to tell the story of the early years and how we got where we are today, leaving the details of present work to the papers in this issue.

However, it would not be right to end without recognizing the two research engineers who made it possible for much of our equipment to get built and work over my years at Rensselaer: Jack Lewis and John Schatz.

REFERENCES

[1] R. L. Hickok, "Plasma density measurement by molecular ion breakup," *Rev. Sci. Instrum.*, vol. 38, p. 142, 1967.

[2] F. C. Jobes, J. F. Marshall and R. L. Hickok, "Plasma density measurement by ion-beam probing," *Phys. Rev. Lett.*, vol. 22, p. 1042, 1969.

[3] F. C. Jobes and R. L. Hickok, "A direct measurement of plasma space potential," *Nucl. Fusion*, vol. 1, no. 10, p. 195, 1970.

[4] R. L. Hickok and F. C. Jobes, "Ion beam probe for ST tokamak," *AFOSR Scientific Report*, AFOSR TR-72-0018, 1972.

[5] J. C. Hosea, F. C. Jobes, R. L. Hickok, and A. N. Dellis, "Rotation and structure of low-frequency oscillations inside the ST Tokamak," *Phys. Rev. Lett.*, vol. 30, p. 839, 1973.

[6] R. E. Reinovsky, J. C. Glowienka, A. E. Seaver, W. C. Jennings, and R. L. Hickok, "Ion beam probe measurements of electron temperature," *IEEE Trans. on Plasma Sci.*, 2, 250 (1974).

[7] J. C. Glowienka, W. C. Jennings, and R. L. Hickok, "Spatially resolved measurements of plasma instabilities using a heavy ion beam probe," *Appl. Phys. Lett.*, vol. 28, p. 485, 1976.

[8] J. H. Stufflebeam, W. C. Jennings and R. L. Hickok, "Heavy ion beam probe plasma diagnostic system for the deep magnetic well LITE device," *IEEE Trans. on Plasma Sci.*, vol. 6, p. 130, 1978.

[9] P. L. Colestock, K. A. Connor, R. L. Hickok, and R. A. Dandl, "Direct measurement of plasma space potential on Elmo Bumpy Torus," *Phys. Rev. Lett.*, vol. 40, p. 1717, 1978.

[10] P. M. Schoch, R. L. Hickok, and W. C. Jennings, "Comparison of the radial potential profile measured in a tokamak to predictions of stochastic magnetic field theory," *Phys. Rev. Lett.*, vol. 55, p. 2417, 1985.

[11] K. Saadatmand, T. P. Crowley, and R. L. Hickok,"Pressure and potential fluctuation measurements in the interior of a tokamak plasma ," *Phys. Fluids B*, vol. 1, p. 164, 1989.

[12] E. B. Hooper, G. A. Hallock, and J. H. Foote,"Low-frequency oscillations in the central cell of the TMX tandem mirror experiment," *Phys. Fluids*, vol. 26, p. 314, 1983.

[13] G. A. Hallock, J. Mathew, W. C. Jennings, R. L. Hickok, A. J. Wootton, and R. C. Isler,"Space potential distribution in the ISX-B tokamak," *Phys. Rev. Lett.*, vol. 56, p. 1248, 1986.

[14] G. A. Hallock, A. J. Wootton, and R. L. Hickok, "Space potential and density fluctuations in the ISX-B tokamak," *Phys. Rev. Lett.*, vol. 59, p. 1301, 1987.

[15] J. C. Forster, P. M. Schoch, R. L. Hickok and W. C. Jennings, "Study of density and potential fluctuations in the TEXT tokamak with a heavy ion beam probe," *IEEE Trans. Plasma Sci.*, this issue.

[16] S. C. McCool, J. Y. Chen, A. J. Wootton, M. E. Austin, R. D. Bengtson, D. L. Brower, W. A. Craven, M. S. Foster, R. L. Hickok, H. Lin, Ch. P. Ritz, P. M. Schoch, B. A. Smith, X. Z. Yang, and Y. Z. Zhang, "The effect of magnetic perturbations on edge transport in TEXT," *J. Nucl. Mater.*, vol. 176, p. 716, 1990.

[17] D. W. Ross, M. L. Sloan, A. J. Wootton, P. M. Schoch, J. W. Heard, T. P. Crowley, R. L. Hickok, and V. Simcic,"Effect of beam-attenuation modulation on fluctuation measurements by heavy ion beam probing,"*Rev. Sci. Instrum.*,, vol. 63, p. 2232, 1992.

[18] T. P. Crowley and the Rensselaer Plasma Dynamics Laboratory Team, "Rensselaer heavy ion beam probe diagnostic methods and techniques,"*IEEE Trans. on Plasma Sci.*, this issue.

[19] S. C. Aceto, K. A. Connor, J. G. Schwelberger, and J. J. Zielinski, "Measurements of the space potential of electron cyclotron heated plasmas in the Advanced Toroidal Facility," *IEEE Trans. on Plasma Sci.*, this issue.

Robert L. Hickok (SM '74,–F'80) received the B.S. degree from Rensselaer Polytechnic Institute in 1951, the M.A. from Dartmouth in 1953 and the Ph.D. from RPI in 1957. He was a postdoc at Yale until 1959. After that he worked at Mobil Research Labs, where he began experiments with heavy ion beam probes as a plasma diagnostic tool. During part of this period, he worked as a visiting staff member at the Princeton Plasma Physics Laboratory. Since 1972, he has been on the faculty at RPI. His present position is Active Professor Emeritus.