My Career in Fusion Research

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1. High school 1964-1966:

My first exposure to fusion research was at the 1964 New York World's Fair, which I visited at least twice as a 16-year-old high school student growing up on nearly Long Island. General Electric and Walt Disney had a public "Fusion Demonstration", which was based on a theta pinch similar to the Los Alamos Scilla machine [1]. It was an impressive device with a 100 kJ capacitor bank which reportedly made about 1 million DD neutrons in a 6 µsec pulse every 10 minutes or so. This is better than many private fusion startups are doing in 2023. I don't remember being at the exhibit itself, but its loud "boom" and its optimism about fusion energy most likely impressed me.

At Mepham High School in Bellmore N.Y. we had to write a career report in the 11th grade, and I did mine on being a physicist. I had never met a physicist, so I visited nearby Brookhaven National Lab during their Open House and asked a man at a booth about physicists. As I recall, he gave me a small official-looking orange booklet about Fusion, probably written by the AEC for public relations. It was very interesting and I kept it for many years (I couldn't find it online).

In the 12th grade I found in the high school library the 1958 book "Project Sherwood: The US Program in Controlled Fusion" by Amasa Bishop. This was a great book which captured the spirit and the challenges of the early fusion program. I particularly liked the illustrations, which showed drawings of strange machines such as the Ixion. This book made me want to have a career in fusion research.

2. MIT (1966-1968):

I went to college at MIT in the fall of 1966 and was at first very impressed and excited about the people and the place. I had never been away from home before. But by the end of the first semester I was bored with classes and distracted by the confusing times. Sometime towards the end of my first year I went to visit an MIT professor who worked in fusion (probably David Rose) to ask for advice and/or some student work. He told me to come back when I was a Junior. I never made it back and dropped out of MIT in early 1969.

3. Stony Brook (1970-1972):

I finished my undergraduate degree in Physics at SUNY Stony Brook, which was near my home on Long Island. I enjoyed the physics courses and was encouraged by my teachers, especially my advisor Prof. Janos Kirz. There was no fusion research at Stony Brook, but I worked with their High Energy Physics group part-time at the Brookhaven AGS (a large accelerator). Fusion interested me more than high energy physics, so I applied to graduate school to study plasma physics.

4. Cornell (1972-1974):

When I got to Cornell for graduate school in the Physics Department in 1972, I found out that fusion research was done only in Applied Physics and Electrical Engineering. So sometime in my first year I sought out Prof. Hans Fleischmann in Applied Physics, who seemed to have the most interesting fusion experiment at Cornell. He and his group shared the High Voltage Laboratory off campus with other pulsed power plasma projects. Hans' goal was to make a field-reversed ring of high energy electrons to confine a plasma in an FRC or "Field Reversed Configuration".

The first day I went to the High Voltage Lab the lab engineering manager thought I was the new janitor and had me cleaning up the place. After this was clarified, I started working on Hans' new experiment "Christa" to create an MeV electron beam for injection into a magnetized vacuum chamber. This was a short-pulsed version of Christofilos' ASTRON of the late 1950's, and it did (after I left) generate a field-reversed configuration [2]. My immediate supervisor on this project in 1972-3 was Hal Davis, who was a very kind and patient mentor.

My favorite professors for courses at Cornell were Hans Bethe and Ed Salpeter, who had opposite teaching styles. Bethe put Jackson's Electrodynamics on the podium and went through it page-by-page, making occasional corrections and eating many Tums (it seemed). Salpeter put a short list of statistical mechanics texts on the blackboard the first day, and then talked without notes about what interested him (like Feynman). Ravi Sudan taught two semesters of plasma physics, writing long equations on the board even before we walked into the lecture room.

My favorite course at Cornell was the Physics Laboratory, which was held a large room with many small experimental set-ups. I was one of the few students to take this course twice since I liked it so much. Here I designed my first experiment related to plasma physics: a rotating metal disk with an oscillating electromagnet on one side and a magnetic pick-up coil on the other. How much of the magnetic field was "dragged" around by the various conducting disks ? The results agreed simple theory except for aluminum foil, which I later found out had trace metal impurities which affected the conductivity.

After two years of course work I asked Hans Fleischmann to be my thesis advisor. He was a German-born professor with very high standards and a serious manner. His many graduate students learned to "go through" their work very carefully with him, always on the look-out for ambiguous data or sloppy thinking. I learned from Hans how to be skeptical and critical and have total honesty as a goal. Hans said that he couldn't sleep at night if he cut any corners.

Hans knew John Clarke, who had recently become the head of fusion research at ORNL and who wanted to bring graduate students there. Hans had a choice of whether to send me or my office-mate (Stan Luckhardt) to do a Ph.D. thesis on the ORMAK tokamak. I think Hans preferred to work with Stan at Cornell (Stan was much better at theory than I was), and so I was sent to ORNL with Hans as my (remote) thesis advisor. This was the start of my three-year thesis project on ORMAK.

5. Oak Ridge (1974-1977):

The Oak Ridge tokamak ORMAK was located in a large high-bay building 9201-2 at the Y-12 site of ORNL. This was the second building constructed during WW2 for the separation of uranium by calutrons [3]. There were some remnants of the calutron racetracks still in the building, and in one corner was the old DCX fusion device with huge electrodes like Frankenstein's lab. ORMAK was one of the first tokamaks outside Russia, and it was just beginning to run routinely when I got there in the summer of 1974. The ELMO bumpy torus was next to it.

I was the first student to do a Ph.D. thesis on ORMAK. The lab atmosphere was very different from Cornell or MIT, since most of the physicists were not interested in students and there were few people my age. I had an excellent local thesis advisor Heinz Knoepfel, who was visiting from Frascati to work on runaway electrons, and a fellow graduate student Don Spong from the Theory department. Lee Berry was the young, enthusiastic, and inspirational leader of the group. One of the few staff members who was friendly was Charles Bush, who I worked with at PPPL 20 years later.

My thesis on high energy runaway electrons was a good topic since it was largely unexplored and outside the main research of ORMAK. These were electrons accelerated up to 10 MeV by the tokamak toroidal electric field and detected by the hard x-rays they made when they hit the wall. We bought a large 5" diameter NaI crystal detector and put it inside an existing 6 ft long lead x-ray collimator, and aimed it at the tungsten outer limiter. My first co-authored publication with Heinz came out only a year later [4].

Half-way through my thesis work Heinz returned to Italy and Dave Swain joined ORMAK and became my local thesis advisor. Dave was a careful experimenter and was also very good at theory. He was easy-going Southerner from MIT, but often very skeptical. He forced me to prove that I analyzed the hard x-ray data objectively. It was hard to count x-ray pulses on Polaroid camera film.

One day in 1976 I saw everyone standing around and looking at a wisp of smoke coming from the top of ORMAK. A coil had failed and that was the end of ORMAK and the beginning of my thesis writing. Fortunately, I had finished my main experiment on runaway electron diffusion, and the writing took only about 6 months. Dave helped me to keep the length under control, and Hans helped with quite a lot with criticism, some heated. The thesis was typed and retyped by hand by a friendly ORNL secretary.

The last step was the oral Ph.D. exam at Cornell organized by the Physics Department. This was doubly difficult since I hadn't been on campus for three years and Hans wasn't a member of the Physics department. It helped a lot that Lee Berry came up from ORNL to support me at the exam. It seemed to me that the committee wasn't very impressed, but I passed.

6. UCLA (1977-1980):

I was happy to get a post-doc at UCLA since I wanted to live in LA. The job was at the tokamak lab of Bob Taylor. This lab consisted of Bob, a physicist Lena Oren from Israel, another post-doc (Sam Talmadge), a very capable engineer (Zoltan Lucky), and a few good technicians and students. Bob and his group had already built two tokamaks in a high-bay lab in the UCLA engineering building: Microtor and Macrotor. I mostly worked on Macrotor, which was about the same size and field as NSTX. Many years after I left Bob and his small team built a huge low-field tokamak bigger than JET in a new building in Westwood (the Electric Tokamak).

Bob was a 1956 Hungarian refugee and the closest thing to a genius I ever worked with. He seemed happiest working with his hands, but he was also great at engineering, electronics, computer programming and his own version of plasma theory. Bob enjoyed confusing his colleagues with obscure language and paradoxical statements. He tried to be a stand-up comedian during his talks and usually succeeded in getting some laughs. He was the opposite of Hans Fleischmann.

I came to UCLA in 1977 with the idea of measuring plasma turbulence in these small tokamaks, partly based on a suggestion from Jim Callen of ORNL to try Langmuir probes. Bob drew a simple circuit for me to build for measuring the probe current, but I didn't have any experience in electronics. At ORNL that work was done by the technicians. Bob initially thought I was an idiot.

By the time I got to UCLA Bob was already well known for inventing "Taylor discharge cleaning" at MIT, which used repetitive short plasma pulses to clean the inside walls of the tokamak. Without such wall cleaning a large low-powered plasma like Macrotor could not be sustained due to impurity radiation. Bob also tried for many years to develop RF heating, but it was never very successful.

We got the first Langmuir probe signals from the Macrotor edge in 1978 using a storage scope connected directly to the machine. On the shot timescale of ~50 msec the probe current looked like fuzzy white hash with a nearly 100% fluctuation level. This was a surprise to us both. We soon tried B-dot coils in glass tubes stuck into Macrotor and saw more turbulent hash. This resulted in our paper "Small scale Magnetic Fluctuations Inside the Macrotor Tokamak" [5]. This paper tried to link the magnetic fluctuations to the theory of stochastic magnetic field transport, which was popular at the time.

Working with Bob had its ups and downs. I liked doing detailed measurements and writing papers, but Bob was not interested in either. He wanted to build machines and invent new ways to make them work better. Bob was probably the only person in the world who privately built and sold small tokamaks like Microtor, but he never talked about this at UCLA.

Bob and Lena Oren were trying to measure the wall impurity content by biasing an electrode inside the plasma to change the ion flux to the wall. This led to Bob's famous "biased H-mode" experiments on Macrotor in which the particle confinement improved significantly with electrode biasing. This was in 1979 and the ASDEX "H-mode" hadn't been discovered yet, so these results were not widely appreciated or even published until much later [6]. Bob thought the confinement improvement was caused by rotational smoothing of the turbulence, but not specifically due to rotational shear. It is interesting that I was making turbulence measurements on the same machine at the same time but didn't try to connect the biasing results to the turbulence. This was in part because the biasing experiments were led by Bob and the fluctuation experiments were led by me, and we each had our own territory.

This territoriality led to the end of my post-doc with Bob. One day in early 1980 I came into the lab and found that my instrumented limiter to measure heat and particle flux was removed from Microtor to make way for some experiment of Bob. This upset me and I decided to leave UCLA to escape the dominating personality of Bob. I was also getting divorced at the time and wanted a change of scene.

7. Caltech (1980-1984):

Coincidentally, sometime about 1980 Roy Gould of Caltech asked me to work as a Senior Research Fellow on his small Caltech Research Tokamak. Roy had previously discussed small tokamaks with Bob Taylor but designed this one himself, mainly to study turbulence with probes. Paulette Liewer, a good plasma theorist, was already in Roy's group working on a review paper about turbulence in tokamaks, as were 3 graduate students and one good engineer. Caltech is only about 30 miles from UCLA so it was an easy move.

I was very happy with this move due to the great reputation of Caltech and the freedom I would have to do independent work there. Roy had just been appointed Dean of Engineering and Applied Science and was looking for someone to help coordinate the lab. For the last two of my four years there the tokamak lab was just across the hall from my office, in a beautiful new low-rise building with a courtyard garden just outside my office window (Watson lab).

My 4 years at Caltech was the happiest period in my career. It was the exploratory phase of tokamak edge turbulence, and I could make unlimited measurements with the probes. It was great to have Paulette there to help interpret the results, and Roy came by once or twice a week to discuss results and plans (the graduate students were officially his). Roy seemed mainly interested in physics and not as much motivated by a fusion reactor as were Hans and Bob.

We were interested in the 2-D structure of the turbulence, and so I built an 8x8 probe array made from the gold pins of coax cables cemented into a ceramic square, and our engineer designed a one-of-a-kind data system using analog delay lines. Initially we had no way to make a movie of the data, so Roy printed single frames and we flipped them like a deck of cards. We saw moving blobs [7], and maybe also blob vortex motion (which we never found from data analysis).

A memorable event was the Caltech Physics colloquium which Paulette gave on tokamak turbulence about 1983. This was an audience with high standards, especially Feynman, who always sat in the middle of the first row. I was very nervous since she was showing some of our Caltech tokamak data. She did very well, but I wouldn't have done that kind of talk myself.

By 1984 I had finished most of what I wanted to do with probes on the Caltech tokamak. Paulette's turbulence review was nearly done, and my long-term future at Caltech was not clear (I was not on a tenure track). Sometime that year I got a call from Jim Strachan at PPPL asking me to join the fusion product group at the new TFTR tokamak. This was a good career move, but I was not happy to leave California.

8. TFTR (1984-1997):

The rest of my career was spent at the Princeton Plasma Physics Laboratory (PPPL). This was a great workplace for me since there were state-of-the art fusion devices and many excellent physicists and engineers. I mainly worked on TFTR up to 1997, but I also had some freedom to pursue side projects of interest to me (see below).

My initial job at PPPL was defined by my supervisor Jim Strachan: to build "lost alpha" detectors for TFTR. He knew about my previous work from reading my papers on runaway electrons (he had worked on this topic in Australia). Alpha particles are 3.5 MeV helium nuclei created by the D-T reactions in the plasma core, and they need to be well confined to create high gain or ignition in future tokamaks like ITER.

TFTR was the largest fusion device in the US and was very intimidating compared with the almost table-top tokamak at Caltech. It was frightening to stand next to TFTR in the large "test cell", since it was so massive and complicated. My mother (when she visited) called it a "plumber's nightmare". I was now a small cog in a very large machine.

Jim and his excellent graduate students had already measured the loss of charged D-D fusion products in earlier PPPL tokamaks like PLT, but now we needed a detector which could survive the high neutron flux in TFTR D-T experiments, which were planned for 1986. Jim already had the idea of using simple phosphor screens, since the more sensitive silicon detectors would not work in the expected neutron flux. My job was to implement this idea.

The first "lost alpha" detector was installed at the bottom of TFTR and operated during the D-D run of 1987 [8]. This measured the 1 MeV tritons and 3 MeV protons from D-D fusion reactions and worked fairly well. The D-T run had been postponed and so there was time to implement 3 improved detectors along the bottom of the vessel and one movable detector. The movable midplane detector was designed and operated by Rejean Boivin, who was my first Ph.D. student at PPPL. He did a great job, graduated in 1991, and went on to a have good career in fusion at MIT and GA.

Beginning about 1986 there were many discussions and planning meetings concerning "alpha particle physics", largely motivated by the PPPL director Harold Furth. Alpha physics had become a major reason for doing D-T on TFTR, since the original goal of Q=1 or "breakeven" by then seemed unlikely. The most interesting topic concerned alpha particle-driven TAE instabilities, which were initially found to be theoretically unstable in the expected TFTR D-T plasmas [9]. I found out about these instabilities by meeting Frank Cheng during an international alpha particle conference in Sweden in 1988. At that time there was surprisingly little communication between the experimental and theory groups at PPPL.

Doug Darrow became an outstanding full-time collaborator on the TFTR lost alpha detectors starting about 1990 and continuing through the end of TFTR. The detectors were installed and worked well for D-D fusion products until 1991, when a change in the RF limiter configuration cause overheating and melt damage to them. This was a low point in my career at PPPL, but we got considerable help from the engineers and management to redesign and fix them. After their protective mushroom covers were improved the detectors operated well through to the end of TFTR.

The best days for me at TFTR were those of the first D-T experiment in 1994 ("DT-1"), which was advocated by Jim Strachan to specifically test the lost alpha detectors before starting other D-T experiments. We could see immediately the alpha loss data "live" on a TV in the control room as the alpha particles hit a scintillator screen inside the detector at the bottom of TFTR. The first alpha loss signals were dim but clear, and we observed an absence of the "delayed loss" seen in D-D plasmas (later identified as collisional loss of 1 MeV tritons). So that was a great relief after 10 years of work.

The years 1994-1997 were very busy with the TFTR D-T experiments. The machine ran from 7:30 am to 11:15 pm every weekday and Saturday mornings. Doug and I got a lot of good lost alpha data and wrote many papers. By the end of the TFTR run in 1997 I thought we had done all we could with lost alphas, and I was also very tired. Most people on TFTR were unhappy about its shutdown, which involved major budget cuts for PPPL, but I was happy to move on.

9. Alcator C-Mod and NSTX (1998-2018):

After the TFTR shutdown in 1997 the PPPL physicists were encouraged to collaborate with other labs until the new machine NSTX could be finished. I wanted to work at the Alcator C-Mod tokamak, since it was not far away and I still liked MIT. Also, just after TFTR shut down Rob Goldston became PPPL director, and he asked me to be the head of the Plasma Science and Technology Department, which I agreed to do (and did do) for 5 years at about half-time.

My main research during these years concerned edge turbulence imaging, which I felt was the next step after the 2-D probe work at Caltech. This imaging was initially based on a fast Kodak camera run by Ricky Maqueda of LANL on TFTR, later replaced with faster cameras from Princeton Scientific Instruments and Vision Research. I collaborated with Ricky for this whole time on NSTX, and with Jim Terry at Alcator C-Mod (who had previously collaborated with TFTR). We developed the gas puff imaging (GPI) diagnostic technique in parallel on both NSTX and C-Mod.

To me the most interesting physics issue was the L-H transition, which was not well understood in the late 1990's and (in my opinion) is still not understood. I wanted to see what happened to the edge turbulence at this transition. Supposedly the transition was caused by an increased poloidal flow shear, but I was very always skeptical about this. The most recent results from 2021 confirm that increased flow shear does *not* precede the transition in the NSTX GPI images [10], but I still don't know what does cause it.

Another topic of interest was the 'blobs' formed near the separatrix and propagating into the scrape-off layer (SOL). I had seen blobs in the probe array on the Caltech tokamak, but the GPI on both NSTX and C-Mod showed blobs more clearly than had ever been seen before [11].

These blobs became more interesting due to the theory of Sergei Krasheninnikov in 2001 and more important due to the high heat flux predicted for the SOL in ITER, to which they contribute.

The best NSTX GPI data came in 2010 just before the NSTX coil failure, and no GPI data has been taken since then. We planned several GPI hardware upgrades after 2010, such as additional viewing locations (Munsat), optical zoom to see small-scale structure (Mandell), Helium line ratio GPI to measure density and temperature fluctuations (Agostini), and an APD array for faster and more sensitive GPI signals (Terry). But none of these was done since NSTX-U was not working (and still is not working in 2023).

I visited MIT about one week a month during 1998-2012. The travel was tiring but I enjoyed the visits. The GPI hardware on C-Mod was slightly different; for example, we solved an in-vessel optical fiber browning problem by installing custom quartz imaging bundles, which worked well for several years. The best C-Mod GPI data came about 2012 after we installed a 2nd GPI view near the bottom of the vessel. This allowed us to compare the outer midplane with the "X-region" views, which showed similar structure. C-Mod was shut down by DOE over 2013-2016 and my collaboration ended. I continued to work on NSTX GPI data analysis until 2022.

10. Fusion Energy Sciences Advisory Committee (1996-1998):

I don't know why I was appointed to this FESAC committee. It was interesting to meet researchers from other parts of the fusion program and to feel important, but the committee didn't seem to have any control over the direction of the fusion program. As I remember it, the leaders set the agenda and the budget/program decisions were made behind the scenes. I was not appointed to a second term, and that was fine with me.

11. NAS/NRC Plasma Science Committee (2001-2004):

For some reason I was put on the Plasma Science Committee of the NAS/NRC. The main event during this time was a review of US participation in ITER, which was handled by a sub-committee. But just after that review started the DOE management asked the sub-committee to approve the US rejoining ITER based on political decisions higher up ("the train is about to leave the station…"). The sub-committee gave DOE the answer it wanted during the middle of its review, which was disappointing to me. I thought it needed a more critical review.

12. ITER:

I found out by asking ITER experts at the FESAC meetings about 1997 that ITER could not make any net electricity even if it had electrical generation systems (which it did not). In February 2001 I tried to confirm this through an email to David Campbell, the Head of ITER Physics at the time. He initially didn't know the ITER "electrical Q" himself, but eventually found out from the engineers and replied to me in July 2001 that: "You will see that your original estimate of 0.4 is not significantly in error, although I think that with some optimization of the scenario, a number above 0.5 could be achievable".

This was very disillusioning, because it meant that ITER was not even close to a fusion reactor, and that everyone in the fusion program had been either deceived or deceitful about it. ITER had been advertised as "demonstrating the scientific and technical feasibility of fusion energy". Until then my main motivation was to make a fusion reactor. After then I was just motivated by my own research, and I went along with the deception.

13. Plasma Science and Technology Department (1997-2001):

Rob Goldston had asked me to be the head of the PS&T department of PPPL just after he became director in 1997. This is the small-projects part of PPPL, similar to a University physics department. I agreed to do this half-time for 5 years while starting my own new research on GPI at NSTX and C-Mod. I had never previously been a manager and now I was responsible for about 8 very independent physicists, including the former PPPL director Ron Davidson and the head of the Princeton Graduate plasma program Nat Fisch (who had projects in the PST department).

My approach was to interfere as little as possible in the work of this department and to focus on maintaining good relationships with the upper management, specifically Rich Hawryluk. I held a PST Department meeting once a week and had a meeting with Rich once a week. I enjoyed the new authority for a while and tried (but largely failed) to bring in new projects and funding.

Part of this job was riding down to the DOE office in Washington with other managers for the annual OFES budget presentations. These I found boring and far from the reality of research. I was not much interested in being a manager, which make me a good interim head for the PS&T department at PPPL. Phil Efthimion was my sucessor and he has been doing this well for the past 20 years.

14. Teaching (1998-2005):

Nat Fisch asked me to teach the graduate course in experimental plasma physics at PPPL, and I agreed, but only if the teaching was shared among several colleagues. This worked out OK for seven years and I enjoyed giving about 8 lectures per year. Since each one took about 30 hours to prepare, after a few years I didn't want to do any new ones and retired from this job after 2005.

I also supervised 3 Ph.D. Thesis Students (Rejean Boivin, Hans Herrmann, Max Karasik), and had a first-year experimental student for most years over 1990-2018. As the head of the "Off-site university research" program, I also developed an undergraduate plasma lab based on glow discharge tubes (see SP14 below). I also gave two lectures on turbulence experiments in the PPPL Turbulence graduate course, and twice gave the NUF (now SULI) introductory lecture on fusion (see https://w3.pppl.gov/~szweben/Course/course.html).

15. Physics of Plasmas (2004-2015):

Out of the blue in 2004 Ron Davidson asked me to be Resident Associate Editor of Physics of Plasmas. I had previously been an associate editor for plasma physics for PRL (2003-2006), but this job was much more serious, involving about an hour a day at the PoP office at PPPL. I was also paid extra for it. I learned a lot about good work habits from Ron, who had retired from the Director job at both PPPL and MIT. He wanted to clean off the desk of new manuscripts by noon. I enjoyed the PoP work very much, but eventually I ran out of gas and asked to retire in 2015, which I think helped trigger Ron's own retirement later that year.

16. NIF bet and IFE:

When I was teaching at PPPL I gave a lecture on IFE, so I was as skeptical about IFE as MFE. At the APS-DPP meeting in 2009 I had lunch with John Perkins of LLNL and his girlfriend. At that time people at NIF were predicting a fusion yield of 10 MJ and ignition within three years, mainly based on computer simulations. I bet John $100 that NIF would not get 1 MJ within 3 years and won that bet in 2012, since by then it made less than 0.1 MJ. Recently John emailed me that NIF had finally made 1.3 MJ, 12 years after the bet.

Sometime around 2010 I had an idea of starting ignition with a high-powered laser focused onto the tip of a very sharp needle of DT ice. If alphas could start a propagating burn, there might be no limit to the energy gain with a tapered conical target. For a couple of days this made me very nervous, since if it worked it might ignite a big explosion using relatively little equipment. I didn't talk to anyone about it, and after a few days I decided it was too unlikely to pursue. I guess this thought has occurred to almost everyone in IFE, but it was scary nonetheless.

17. Review papers:

I enjoyed working on a few review papers over my career. I was a co-author of the review by Alan Wootton et al on "Fluctuations and anomalous transport in tokamaks" (Phys. Fluids B 1990). I wrote with TFTR co-authors a review on "Alpha particle physics experiments in TFTR" (NF 2000), a detailed review of "Edge Turbulence Measurements in Toroidal Fusion Devices" (PPCF 2007), and the experimental parts of "Convective transport by intermittent blob filaments - comparison of theory and experiment" (PoP 2011). Also "Gas puff imaging diagnostics of edge plasma turbulence in magnetic fusion devices" with Jim Terry, Daren Stotler, and Ricky Maqueda (RSI 2017), and a tutorial on "Plasma Mass Separation" with Renaud Gueroult and Nat Fisch (PoP 2018), which was my last review paper.

18. Relation of theory and experiment:

I was never good at plasma theory, but I tried to read theoretical papers and enjoyed talking with theorists. I felt that plasma turbulence theories were either too complicated or too oversimplified, and at best I only partially or vaguely understood their relationship to what I was measuring. I did try in all my papers to make comparisons with theory, but usually the agreement was qualitative, at best.

For one paper I did focus on an explicit comparison of the GPI results with edge turbulence theory. This was for C-Mod GPI data and the GEMR simulation code of Bruce Scott. Bruce was an interesting character, and it took over a year to get his attention to run the C-Mod simulations. The results were generally within a factor-of-two, but the experience left both Bruce and I thinking that this was incomplete and there was much more to be done [12].

19. Successes:

Most of my success came from developing and using plasma diagnostics and making detailed measurements such as edge turbulence or alpha particle loss in tokamaks. It helped that I worked on the same topics for many years. I started optical imaging of edge turbulence with a 1983 paper on the Caltech tokamak. I coined the word "blob" in 1985 to describe coherent SOL turbulence structures and my last paper on optical imaging of blobs was in 2022. I worked continuously on lost alpha particles for 15 years. I'm most proud of the GPI movies made in collaboration with Ricky Maqueda of NSTX and Jim Terry of MIT [11]. To me these movies are still beautiful and mysterious.

21. Disappointments:

The biggest disappointment in my career was not being able to find the cause of the L-H transition by analyzing the GPI images. As early as the year 2000 I was convinced that that the transition was *not* caused by an increase in poloidal flow shear, as commonly supposed, since this was not visible in the GPI movies. It was disappointing that I could not shake the prevailing dogma about this despite trying for over 20 years. I still hope that some new experiments and/or theory will discover the true cause for the L-H transition.

Another disappointment was not finding any collective alpha particle instabilities in the TFTR D-T experiments. I had "discovered" Frank Cheng's work on TAEs and advocated doing D-T based on his early estimates of TAE thresholds, which turned out to be much higher than initially expected. Toward the end of TFTR a small alpha-driven TAE mode was found, but it had no significant effect on the observable alpha loss.

My biggest disappointment in general was finding out that ITER could not make any net electricity (see "ITER" above). This convinced me that MFE was not going to work.

22. Retirement (2018-2022):

I was slowing down and went to 80% time in 2016 and took the "buy-out" to retire in May 2018. Yet I continued to go into PPPL nearly full-time for about two more years, and continued working at home for another two years. I was happy to phase out slowly. I did a few papers in 2018-2021 and submitted my last first-author paper in 2022 [13].

Side Projects:

My father was an amateur inventor and made a list of many thousands of creative ideas over his long life. This helped inspired me to think creatively outside my main-line research interests. For most of my career I had a main project on tokamak experiments and at least one other small side project.

Below is a list of these side projects. Many of them involved graduate student collaborators, some came through the PPPL University Support program, and some were supported by internal PPPL funding (LDRD) with the goal of attracting external funding. However, none of these projects succeeded in obtaining significant outside funding.

SP1. Local divertor: My first side project was an experiment about 1983 to test a small localized toroidal divertor coil at the outer midplane of the Caltech tokamak. This was one of several ideas

I had to help solve the SOL heat flux problem. The coil did have a strong local effect and Paulette helped explain the results with some theory [14]. However, this was not practical in larger tokamaks due to the large forces involved, so it was never repeated.

SP2. Central Ignition: My most optimistic idea to help develop fusion was the ‘central ignition’ scenario for TFTR, based loosely on central ignition ideas for laser fusion. I thought that alpha heating at the core of TFTR might be enough to start a propagating burn and ignition. I wrote a paper with analytical estimates and sent it for publication about 1985. It was the only paper I ever wrote that was rejected by a journal – the reviewers thought it was too speculative. I then co-wrote another paper with Martha Redi and Glen Bateman on the same subject but with numerical transport simulations, which was eventually accepted in Fusion Technology [15].

SP3. Alpha Storage Regime: I also explored possible alpha particle effects short of ignition in TFTR in terms of the “alpha storage regime”, essentially the low-density supershot regime in D-T [16]. The alpha population appeared to be large enough to excite alpha particle instabilities based on the simplest estimates of the instability thresholds. This name was not kept but the idea helped to start “burning plasma physics” and motivate the D-T experiments.

SP4. Neutral beam Imaging: Sometime toward the end of TFTR there was a call for new diagnostic ideas, and I gave a talk about neutral beam imaging of edge turbulence in TFTR. This was based on an existing “diagnostic neutral beam” and the plasma TV system. The beam was just barely visible on TFTR [17], but this led to the development by others of BES at PDX at PPPL.

SP5. Runaway avalanche: My thesis advisor Hans Fleischmann from Cornell visited me at PPPL for two summers about 1991 to study runaway generation during disruptions in TFTR. I helped slightly with his runaway avalanche paper with Jayakumar (who I never met) [18]. Hans told me he discussed this work with his old friend Marshall Rosenbluth, who later wrote his own paper on the subject without referring to Hans’ work (which upset Hans). Hans also wrote a long paper which was rejected by Nuclear Fusion but was made into a PPPL report [18].

SP6. Tritium measurement: Tritium retention in the TFTR vacuum vessel was an issue for the DT run, and I had the idea of measuring tritium on the wall inside the vessel by using neutral gas in the vessel as an ionization chamber to measure its beta decay rate [19]. It worked surprisingly well, but it was not very useful since it didn’t measure tritium below the surface layer, as defined by the range of the betas.

SP7. 2-D LIF: Just after TFTR I had another idea to do 2-D imaging of edge turbulence using LIF. The atomic physics was way beyond my scope, but a few colleagues were interested and did estimates for NSTX in an RSI paper [20]. This was later explored in detail by Fred Levinton and students, but it never worked well enough to make a good 2-D turbulence image.

SP8. Arc furnace: In 1994 my former colleague from Caltech Paul Bellan suggested that we collaborate on a project to stabilize industrial-scale arc furnaces. Arc furnaces up to 100 MW are a major source of recycled steel but are very unstable (and literally very noisy). In 1995 I obtained LDRD funding to build a very small test furnace and took on a Ph.D. student Max Karasik for a thesis on this topic. We almost got a 3-year contract with ABB in Switzerland to study ways to stabilize furnaces, but the University at the time was not supportive of such industrial research. Max did a couple of nice papers [21] and finished his Ph.D. in 2000, but the arc furnace project ended shortly thereafter without any external funding.

SP9. Phosphor imaging: As part of the "Off-site University Research Program" at PPPL (of which I was the manager), I collaborated with equipment and visits on a project at UCSD. The project was to make 2-D images of turbulence in the PISCES device using a phosphor end screen. This had several interesting challenges, but the graduate student did get some results and wrote a paper [22]. This diagnostic was recently revived for use on TORPEX with good results.

SP10. Thomson scattering: During the initial development of GPI in 2000, I had the idea of doing 2-D edge turbulence imaging using high-power Thomson scattering for density and temperature fluctuation measurements in NSTX. In collaboration with the Thomson guys at PPPL and a laser guy from LLNL, we determined that this could be done with a 1-3 kJ amplifier from the Nova laser [23]. However, the cost was estimated to be well over $1M and this was not practical for NSTX.

SP11. Plasma NMR: A novel side-project was to investigate whether nuclear magnetic resonance (NMR) could be used as a diagnostic of tokamak magnetic fields. This was a new idea, as far as I know, although the similarity of the NMR and ion cyclotron frequencies had been noted previously. I contacted Prof. Will Happer of Princeton for discussions about NMR and one of his group members was a co-author of the paper [24]. However, there were many difficulties in the nothing came of it.

SP12. Magnetic field measurement: For TFTR I had been using small radioactive alpha particle sources to calibrate the lost alpha detectors, I thought it might be possible to use such sources as an ion beam for measurement of the internal magnetic field of a tokamak. This would be especially attractive for small tokamaks since the hardware would be simple and cheap. However, the source/count rate was too small to be practical [25].

SP13. Edge minority heating: When collaborating with C-Mod about 2004 I had the idea of trying to trigger an L-H transition by intentionally creating fast ion loss from the edge using RF heating. Ion loss was supposed to be one method for creating an edge electric field which caused the H-mode. I got help from the RF group and others at C-Mod to run the experiment, but it didn't show any effect on the L-H transition. I then got help with the analysis and tried to explain the results. However, someone at C-Mod did not support publication, so this was only presented in an RF conference paper [26]. This was the only time when I did a tokamak experiment and didn't get it published.

SP14: Plasma Lab: During 2005-2006 after a visit to PPPL of Prof. Carl Helrich of Goshen College, I designed an undergraduate plasma lab to be sent there as part of the University Support program. This lab was based on two DC glow discharge tubes, a few simple diagnostics, and a rather nice lab manual (https://w3.pppl.gov/~szweben/Course/Lab/). The equipment was put together by a PPPL technician Mike DiMattia who went to Goshen to set it up. This was quite a lot of fun and it was working at Goshen at least up to 2012. Tim Stoltfus-Dueck came to PPPL from Goshen as a graduate student, but I don't know if he used the lab.

SP15. Biased electrodes: Many tokamak experiments since Bob Taylor's at UCLA tried electrode biasing to improve the confinement, but I wanted to see the local effects of biasing on the edge turbulence in NSTX. I tried biasing of small electrodes in the GPI view at the midplane and also at the bottom divertor plate during 2009-2012. This required considerable cost and effort in the engineering of the electrode hardware, cabling, bias controls, and small power supplies. The results were exhaustive [27], but not promising for plasma control purposes. It is now amazing to me that NSTX and I spent so much time and effort on this project.

SP16. Plasma ball: Perhaps the most fun I had on a side project was with the "plasma ball", in collaboration with Prof. Michael Burin at Cal State San Marcos and students at PPPL and CSSM. The goal was to understand how commercial plasma balls worked, and the experimental results are still the most detailed available [28]. We learned many things over several years of experiments at PPPL and CSSM (2008-2015), but were not able to explain them with theory since that required complicated numerical simulations. I thought this would be very popular and well cited work, but there have been very few citations (we did not publicize it well).

SP17. Moving limiter: One of my most exotic ideas was to replace the fixed divertor plates with rapidly moving divertor plates [29]. This was a radical solution to the almost intractable problem of divertor erosion, and I was very enthusiastic about it for a while about 2009-2010. I gave talks about this at PPPL and MIT, but it was too radical to be taken seriously by the lab managers. Perhaps the idea was impractical, but I still think it is better than liquid metal divertors.

SP18. e-beam in MRX: After the end of C-Mod and during the repair of NSTX (about 2014), I was looking for a side-project at PPPL. I had the idea of using an e-beam to track the magnetic field reconnection in MRX, and Masaaki Yamada agreed to support this. I did a lot of bench-testing of the e-beam and Langumir probe detector, and built and tested a set-up for MRX (with help from an engineer). Later a student followed this up and produced a fairly nice paper [30].

SP19. Plasma separation: Probably my most exhaustive side-project was a collaboration initiated by Nat Fisch on plasma mass separation, which lasted from 2012-2016. Nat pushed for PPPL funding for a post-doc and small test experiment based on an existing linear helicon device at PPPL, and we submitted a few proposals for serious funding, mainly directed at nuclear waste processing. However, none of these proposals was funded. The difficulties were summarized in what I thought was a good tutorial paper [31], which was finished after I retired.

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