My Path to the Quark-Gluon Plasma

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Abstract I describe the peculiar path that took me from my undergraduate education to my role in the very early days planning for the physics program at the Relativistic Heavy Ion Collider (RHIC).

1 Introduction

My path from an undergraduate student to a tenured professor working at RHIC was much more of a random walk than a directed evolution. More so than anything else, I benefited from multiple serendipitous incidents that helped determine that path, so much so that I decided to deploy a counter in these notes to help tally such instances. So you will note an ever-increasing count in the form of "**S**n", where n is an integer. I don't think I am unique in benefitting from chance encounters and circumstances, but I did want to provide some indication for younger students, to emphasize the importance of being open to possibilities rather than trying to pursue a pre-determined trajectory at a point too early in your career.

2 Caltech 1971-75

I entered Caltech in the fall of 1971 when I was not quite 18 years old. Getting into Caltech, and getting my parents to agree for me to enroll there, was by no means serendipitous, rather it required a significant amount of self-study and persuasion, which I will describe in detail in an expanded version of these notes. So I realized at the outset that I was very fortunate, but had little appreciation for how that statement

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would continue to apply to my future career development. Caltech was my absolute dream school - I already knew I wanted to study physics, and was convinced that Caltech was the best place to do so.

While I was already very interested in physics by my second year in high school (when I discovered that the parts of chemistry that I liked were called physics), my goals were strongly reinforced from reading *The Feynman Lectures on Physics* [1], which I has first seen the summer before my senior year in high school, when I attended a National Science Foundation (NSF) program in nuclear physics (**S1**) at MacMurray College [2], a no-longer extant small liberal arts four year college located near Springfield, Illinois. This was a summer program for selected high school students, one of many such programs then supported by the NSF across the U.S. [3][1]. The program was excellent, combining coursework using a college-level text for relativity and particle/nuclear physics with lab work with real sources, electronics (pre-amps, discriminators, logic gates, scalers). It also provided my first exposure to programming, laboriously entered in BASIC via paper(!) tape. I did sufficiently well that the instructors asked me to skip my senior year in high school and enroll at MacMurray, an "opportunity" my parents wisely declined (**S2**).

At Caltech, the most important instance of serendipity was meeting and bonding with Joe Polchinski (**S3**), who would go on to become the most accomplished string theorist of his academic generation [4], literally writing the book(s) on string theory [5, 6]. Joe and I met in our first week at Caltech, and spent the next ten years together as both undergraduate and then graduate students. As described in his poignant memoir [7], we were both inspired by the proximity of such greats as Richard Feynman and Kip Thorne. We were also both guided and mentored by Tom Tombrello [8], an extraordinary person who engaged us in various undergraduate research projects (**S4**). At the time this was considered revolutionary - undergraduates, even at Caltech, were typically relegated to the most mundane tasks as laboratory assistants. Tom, as he recounts in his extensive oral history [9], had us involved in real research projects. I worked on calculating the modes and losses in a spirally-loaded waveguide that eventually led to a real device [10]. I also spent an extended amount of time modeling possible charge-symmetry breaking contributions to the isospin-forbidden reaction $d + d \to \alpha + \pi^0$, an effort that was deconstructed by Steve Koonin in about five minutes when I discussed it with him the next summer.

Our activities were hosted by the Kellogg Radiation Lab [11], an ideal environment that made an enormous impression on me. Professors, postdocs, graduate and undergraduate students all interacted with very little pomp or circumstance. From my undergraduate perspective, the graduate students were already old, and personalities such as the avuncular Willy Fowler [12] (a future Nobel Laureate) were walking ancient legends (he was then in his early 60's). The highlight of the day was tea in the Kellogg library, with free-ranging discussion on a variety of topics. It was there

[1] Sadly, I believe the NSF no longer sponsors such programs, having succumbed to charges of elitism, but a similar effort lives on the form of the privately supported Summer Science Program.

that I met Steven Hawking, then visiting Caltech doing work that led to his discovery of the eponymous radiation properties of black holes.

In our senior year, Joe and I were graders (along with Ken Jancaitis and my very best friend Roland Lee) for Richard Feynman when he taught the junior-level quantum mechanics course (**S5**). I am more than a little reluctant to assign this a serendipity marker, because I was so foolish at the time (and for some extended period afterwards). I attended all the lectures, but did not want to be seen taking notes, since after all I was supposed to be the grader who knew all this stuff. As a result of my youthful dopiness, I have only fragmentary memories of Feynman's approach - it made heavy use of the Fourier transform between continuum representations, and special functions were usually treated by generating functions rather than differential equations[2]. This may be the only course Feynman taught at Caltech that did not result in a polished set of lecture notes, and I have only myself to blame!

3 Berkeley 1975-1982

Joe and I both began graduate school at UC-Berkeley in the fall of 1975. We had visited in the spring of 1974 (prior to riding our bikes from Berkeley back to Pasadena), and were escorted in a grand fashion by Geoffrey Chew [13], who was then chair of the department. In retrospect, his hospitality to two graduate students was probably 90% driven by Joe and 10% by me, but his gracious manner hid this very well. He took us to the LBL cafeteria, which even with its spectacular views shrouded in rain and fog, was very impressive. Knowing my interest in nuclear physics, he also kindly arranged a meeting for me with Wladyslav Swiatecki [14], a senior theorist at LBL interested in heavy ion collisions. I had never heard of heavy ion collisions, and freely admit that for some significant part of Swiatiecki's enthusiastic description I was still thinking of ions in the chemical sense. like the anion SO_4^{2-} from dissolving copper sulfate in water. Eventually I realized he was interested in developing a cascade code to follow all nucleons in a nucleus+nucleus collision. In my youthful arrogance I thought this was all quite mad. As a bit of retroactive justification for my intellectual narrowness, I am not sure if the point of such collisions was obvious then - it was very much at the outset of what eventually became a program in relativistic heavy ion collisions, as detailed in the review article [15] I co-wrote with Wit Busza for this volume. I clearly missed whatever motivating framework Swiatecki[3] described to me.

By the end of my second year at Berkeley I realized I had to find a research advisor. My first summer was spent at Lawrence Livermore Laboratory, working on various

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[2] Very recently, my classmate Jim Weatherall has graciously provided me with copies of his notes from the class(!). I have not digested all of them, but already I see that they confirm what little I recall. Feynman assumed the students had learned QM at the level of Feynman Lectures Vol. III, and began straightaway with the Fourier transform.

[3] For a much more nuanced view of Swiatecki's important role in the field, see Joe Kapusta's contribution *From Fireballs to Finite Temperature QCD: A Personal Recollection* to this volume.

aspects of laser-induced inertial confinement fusion, fascinating stuff but hardly a path towards a Ph.D. thesis at Berkeley. In my second summer I had somehow become fascinated with Weinberg's soft-pion "theorems" [16]. Looking at the Berkeley faculty only Ken Crowe [17] was doing anything remotely related to this topic, so I asked him if he would take me on as a research student. I was "armed" with a graph I had made (by hand, on semi-log paper) systematizing the lifetimes of π^- absorption as a function of atomic number Z, following a trivial calculation I had made of the expected Z-dependence. Ken was nice enough to pretend to be impressed, and took me on as a student. Although far from apparent for a year or two, this was indeed serendipitous (**S6**).

While I wanted to do a thesis that let me engage in phenomenology as well as experimental measurement, I enthusiastically set about acquiring the needed detector expertise with scintillators, photomultiplier tubes, multiwire proportional chambers, low and high voltage electronics, data acquisition systems, etc. I was greatly aided in this by my fellow (and more talented) graduate students Carl Clawson and Jeff Martoff, along with postdoc Jim Miller. I also enjoyed learning "experimental theory" from Segrè's book [18] and "the Sauli bible" [19], as well as months of "on-the-job" training, particularly the summer of 1978, at Los Alamos National Laboratory's (LANL) LAMPF facility [20], There I worked on a radiative pion capture experiment that became the basis for Jeff Martoff's thesis [21].

As much as I enjoyed that summer of intense experimental effort at LANL, there was also some frustration involved, in that I really wanted to be working on what would become my thesis experiment. This is where the serendipity points really begin to add up. I had not been in the Crowe group more than 6 months when I heard a very interesting seminar (**S7**) given in LBL's Nuclear Science division by a young postdoc, Miklos Gyulassy. I was absolutely fascinated by Miklos's proposal to use two-pion correlations to measure the coherence of the pion source, even though at that time I had no idea why a coherent source would have no Hanbury Brown - Twiss (HBT) correlations [22]. What I did know, thanks to my undergraduate quantum mechanics course based on Gordon Baym's textbook [23], was that the HBT effect could be understood both quantum-mechanically and from classical electromagnetic theory[4].

I conveyed my enthusiasm for this idea to Ken Crowe immediately after Miklos's talk. Out of all the serendipitous events in my career, none approached what happened next. Ken's response was "Why don't you design an experiment to measure it?" (**S8**). It is hard to convey how striking this development was. I was a new graduate student, having been in his group less than half a year. Ken had no program in heavy ion physics, and in fact had strong negative views of its "crash and splash" approach (his opinions also extended to several of its practitioners). This is perhaps not surprising from the man who literally wrote the book on precision measurements [24]. Regardless of this, by April of 1978 I had managed to write a Bevalac [25] proposal *Search for Coherent Identical Pions in Multiple Pion Production for Deep Collisions of Heavy*

[4] What was very hard, particularly in a first-quantized framework, was understanding the absence of HBT correlations for a coherent source.

Ions, which was approved in November of the same year. The experiment was built in 1979, largely from used and/or discarded equipment, installed on the Bevalac's Beamline 30 and had first results by 1980. The next two years were dedicated to additional data-taking. Progress was slow - heavy ion beams from the Bevalac were used during the week for medical therapy; experiments took data only on weekends! During this time I was also performing intensive data-analysis, leading to my Ph.D. in August of 1982 [26] and publication of the results in 1984 [27].

The path from proposal to publication was anything but smooth, but here I'll reflect only on the positive and truly serendipitous aspects. Berkeley then, as now, enjoyed a steady stream of distinguished visitors. There were at least three with direct relevance to my thesis topic (broadly interpreted to include the original HBT experiment and its quantum optics underpinnings). Ed Purcell, whose short 1956 *Nature* article [28] both brilliantly deconstructed claims that the HBT effect violated quantum mechanics and provided a clear discussion of its quantum mechanical origins, patiently explained this to me (**S9**), noting that his interest in such matters dated back to trying to understand radar signals from clouds during his wartime activities at the MIT Radiation Lab. Roy Glauber was kind enough to meet (**S10**) and explain in his pithy yet voluble style[5] his seminal papers on quantum optics [29–31], describing in some detail the errors of those who claimed that the HBT photon correlations were impossible. I also met with Abraham Pais, the "P" in the 1960 GGLP paper [32], which presented the first observation of like-particle bosonic correlations in particle physics. Pais at that time had just published an article *Einstein And The Quantum Theory* in the Reviews of Modern Physics [33], which I had carefully worked through. His clear description of the interplay between wave and shot noise in bosonic fluctuations was crucial to my understanding of the HBT effect. Pais was characteristically modest, giving generous credit to Sulamith Goldhaber (the second "G" in GGLP) for her work in performing the necessary calculations. The knowledge I derived from meeting with these giants appeared in one form or another in the introductory material of my Ph.D. thesis.

4 Penn 1982-1986

I began my post-doctoral work at the University of Pennsylvania in August, 1982. How I arrived there was yet another instance of serendipity: I had presented a preliminary version of my thesis results at the 1981 *5th High-Energy Heavy Ion Study* in the LBL auditorium. After the talk, a dapper gentleman, a dead-ringer for the actor Jason Robards, asked me if I would be interested in a postdoctoral position in his group at Penn (**S11**). He apparently had liked how I handled a perhaps snarky question from Richard Weiner [34], citing results from one of Weiner's own papers to rebut his criticism of my own approach. My new fan was Sherman Frankel [35], and the offer was to work on Experiment R807 [36] at the CERN ISR [37]. Sherman's offer simultaneously addressed many issues: I very much wanted to work at CERN, and I

[5] Traits that became only more pronounced as I got to know him better over the ensuing years.

would have a chance to work with Bill Willis, already known to me as a visionary in the field of both high energy detector design and the discovery potential of truly relativistic heavy ion collisions. Most importantly to me (at that time) it provided a way out of heavy ion physics, towards high energy physics.

While I loved working on my thesis experiment, I was disillusioned with heavy ion physics, which seemed not only unable to but also uninterested in making quantitative statements, in particular when testing hypotheses. The concept of a quark-gluon plasma was still very much in development at the time [15], so attention at the Bevalac tended to focus on exotica such as pion condensates. Yet there were neither quantitative calculations nor reliable models, so it was impossible to make exclusion statements such "We show the coupling parameter can be no larger than X at confidence level Y." That of course showed a certain naïveté on my part, but such is youth.

As soon as I arrived at Penn I eagerly embarked on learning a new field, assiduously working through Werner Hoffman's superb monograph *Jets of Hadrons* [38], while also attempting to self-educate on QCD, splitting functions, running coupling constants and the brand-new UA1 and UA2 data. But for reasons that are not completely clear to me to this day, I found myself drawn back to "heavy" ion topics. This was possible because R807, also known as the Axial Field Spectrometer (AFS), had an eclectic program measuring p+p collisions at the ISR top energy of 63 GeV, while also studying p+α, d+d and α+α collisions (hence the quotes around the word "heavy" - these were light ion collisions, but many of the techniques were taken from the much lower energy heavy ion experiments at the Bevalac). Therefore, when I was asked to serve an internal referee on the collaboration's first HBT publication [39], I learned a very valuable lesson about working in what was then thought of as a "large" collaboration ($\sim$ 70 authors). My childish internal review of the paper was borderline savage, needlessly showing off the hard-won expertise I had developed in my thesis analysis. This was a particularly grievous offense in the AFS Collaboration, with its strong Scandinavian groups and associated emphasis on communality, so I was kindly but very firmly educated on proper behavior in an international collaboration.

Sherman was a superb mentor and basically gave me free rein. Before I began at Penn, Sherman and his colleague Bill Frati made a clever extension to the Wounded Nucleon Model [40]. My first contribution at Penn was finding a mistake in the Monte Carlo "Glauber" code they used, and more importantly, extending their approach to perform analytic convolutions of the single-nucleon contributions to the transverse energy in light-ion collisions [41, 42]. I was not successful in first attempts at removing the underlying event from jet production in α+α collisions, but did demonstrate that the event-to-event fluctuations had to be understood and controlled; this led indirectly to a paper on what KNO scaling really means [43]. Sherman also indulged me while I spent endless CPU hours on Penn's IBM mainframe developing simulations of multi-boson effects in heavy ion collisions [44], work inspired by typically oracular remarks from Bill Willis on the utility of speckle interferometry in analyzing high-multiplicity pion states created in heavy ion collisions. Apparently these various efforts sufficed for

Sherman to convince the Penn faculty to promote me to a junior faculty position, beginning in the fall of 1984.

I spent the summer of 1985 as a visitor at BNL, thanks to a generous offer from Peter Bond (**S12**). Peter and I shared an office in the Tandem Group, where his many questions helped sharpen my knowledge on HBT correlations, which I presented as three lectures that summer titled *The Two-Pion Trilogy*. Nominally the purpose of my visit was to help the Tandem Group transition to relativistic heavy ion physics. But Peter, who went on to a distinguished career in senior lab management at Brookhaven, like Sherman also made very few demands on my research direction, so I was able to develop some of the arguments needed to finish my work on the multi-pion states Monte Carlo. I felt it was a productive summer, but also believed it was effectively the end of my time in heavy ion physics. Little did I know that I would return to this very same venue, again as a card-carrying heavy ion physicist, in just two short years.

By fall of 1985 I had made the decision to "officially" become a high energy physicist. Since 1983, Sherman had been working, with little success, to convince CERN to do early runs with ^{16}O beams, using existing facilities such as the Ω' spectrometer [45], rather than waiting for the official SPS heavy ion program to get underway a few years later. While I was involved in many aspects of those proposals, I had grown very disaffected with the basic premise - that one could do fundamental work in relativistic heavy ion physics by re-using apparatus designed for lower multiplicity hadron-hadron collisions. I did not really see a path forward, so I made the difficult decision to tell Sherman I wanted to work on the D∅ [46] experiment at Fermilab.

Sherman graciously accepted my decision, although even at the time I realized he must have been very disappointed. There was an existing modest effort at Penn on D∅, led by senior professor Walter Selove [47]. In retrospect, I assume Walter must have been pleased to increase Penn's presence on D∅ with an additional faculty member. At that time, D∅ was very much in an early formative stage, having achieved official Fermilab status in July, 1983, with its first design report published in late 1984. Serious work was getting underway when I joined the effort through Walter's group.

Working on D∅ in 1985-1986 was very instructive. I enjoyed the Fermilab environment, so much more centralized than Brookhaven's (which of course as a multidisciplinary laboratory has a much more diffuse "center"). I managed to write five or six technical notes in that time, mostly focused on test beam and Monte Carlo studies of the calorimeters. I learned the rudiments of GEANT and applied it to a study of e/π (shorthand for the ratio of the electromagnetic to hadronic response). I saw firsthand the calm effective leadership of spokesperson Paul Grannis [48] in aligning the interests of many groups towards a common cause. Mike Marx [49], the technical coordinator for Paul, was already concerned with the trade-offs necessary to satisfy both physical and financial constraints. At that time, I would have been stunned to be told that in a decade I would be working much more closely with Mike taking a similar role, this time preparing a collider detector for first collisions at RHIC.

5 Columbia 1986 to present

My happiness working on D∅ was not without bounds. Walter Selove was a good and sincere man, but much more of a micro-manager than any of my previous mentors. Inevitably issues arose, at least as much due to me as to Walter. Also, although I had but the faintest glimmer of understanding the tenure process, that feeble knowledge was enough for me to question my path forward at Penn. So it was an extraordinarily serendipitous event when Shoji Nagamiya contacted me in the spring of 1986 to ask if I would be interested in a junior faculty position at Columbia (**S13**). I had just read Bob Crease and Charles Mann's excellent book *The Second Creation* [50], which was (appropriately) rather Columbia-centric, given the many outstanding contributions from Columbia physicists in the 1950's and 60's to high energy physics. Shoji of course was not asking me to come to Columbia to continue my transition to high energy physics. He had just been hired at Columbia to lead a new program in experimental heavy ion physics at Brookhaven, initially in fixed target experiments at the AGS [51], but ultimately focused on RHIC.

It is not completely clear why Shoji selected me. In principle, my hiring needed to be approved by the entire faculty, but for assistant professor positions in high energy physics (and by extension high energy nuclear physics) the department still adhered to the now-abandoned model that assistant professors were "assistants to the professor". Little oversight was provided for such hires, and prospects for an internal promotion from an untenured assistant professor to tenured associate professor were dismal. I certainly was unaware of this fraught system at the time, and it is not clear to me that Shoji realized it either. What I do know is that there was more serendipity at play, dating back to our shared time at Berkeley. Shoji was already a leader in the Bevalac program, first as a postdoc and then research scientist in the Chamberlain group, while I was doing my thesis experiment. His jewel-like apparatus was located just a few meters downstream from my experiment on Beamline 30, so we shared the same experimental "cave" in Bevalac parlance. I believe this close proximity (**S14**) helped make Shoji aware of my results, which I know he referenced in at least of one his many talks at the time.

For my Columbia interview, I arrived at the Ardsley-on-Hudson train station, near Nevis Laboratories, Columbia's off-site research facility. I was picked up at the station by Ann Therrien, the absolutely amazing Nevis Administrator. Ann is a force of nature - she is one of the most intelligent people I have ever met, and has an encyclopedic knowledge of the physics community. Her advice, counsel and admonitions would play a major role in my eventual success at Columbia. This too increments the serendipity register (**S15**), since nothing in Ann's job description mandated the care and attention she lavished on Shoji's program and my career.

I was given a tour of Nevis Labs, then the next day presented my talk on campus, returning to the Nevis area that night for dinner with Shoji at a restaurant on Broadway in Tarrytown, NY. I vividly recall our conversation. In a word, I was idiotic. I was adamant that I wanted to continue on D∅, splitting my time between it and the new experiment at the AGS that Shoji was leading. In the course of the discussion, Shoji explained to me he made his own future plans based on a five-year time scale; one could attempt to plan for longer periods but life happens. He very politely told me my plans for a joint effort on D∅ and a new heavy ion experiment were moronic (my word), but I persisted, to the point where he grudgingly but graciously accepted a dual involvement as a possibility. Shoji must have realized that this fantasy of mine would evaporate once I arrived at Columbia, which is exactly what happened. I don't recall any effort whatsoever on my part to continue my participation on D∅, as I immediately became subsumed with my work in heavy ion physics. I had returned to the field from which I had attempted to escape, and have never looked back.

In fact, I had returned to working with the same Tandem Group at BNL I had visited under the auspices of Peter Bond in 1985. Shoji was co-spokesperson with Ole Hansen, a senior member of the BNL Tandem Group, for E802, slated to be the primary experiment for the new era of ion beams with energy $\sim$ 15 A $\cdot$ GeV at the AGS. "The E802 Collaboration" in fact became a general title for a series of experiments E802, E859 and E866 that made a variety of measurements [52] in Si+Au and Au+Au collisions.

In this series, E859 holds a special place in my heart. In 1990 BNL senior scientist Lou Rembsberg brainstormed with Bob Ledoux (junior faculty at MIT) and me to design an upgrade to E802 to pursue rare physics, in particular, events with two kaons present in the spectrometer, to study both two-kaon interferometry and ϕ-meson production. We converged on a design based on the LeCroy ECLine of programmable logic devices [53], together with new trigger wire chambers. The LeCroy modules were "programmed" by connecting their multitude of two-pin front panel ports to others by a plethora of individual twisted-pair connectors. This was painstaking in the extreme, but after months of all-night efforts, performed by my graduate student Ole Vossnack, and later Matt Moulson, and a cadre of equally talented MIT students working under Bob's charismatic leadership, we made it all work. Our rat's nest of hundreds of twisted-pair cables and downloaded look-up tables calculated the momentum and flight time of each particle in spectrometer, determining if they were pions, kaons or protons in the allowed 40 microsecond time window. I had also developed a real-time event display, presenting this information for each track in an event. The first night this all came together, and we watched event after event flash by, with each track labeled as a pion, kaon or (anti)-proton, was so moving for me that I had to step outside - I didn't want "the gang" to see how emotional I was. I also recall the pride we all felt a few nights later when showing this same display to Lab Director Nick Samios, who had heard about the experiment and stopped by on his way home.

In March of 1992, while completely entrenched in E859 details, I received an urgent request to return to Columbia to present a seminar detailing my thoughts for a RHIC experiment. This could not have come at a less opportune time - by then I was co-spokesperson for E859 (along with Lou Remsberg), the experiment was at a critical phase, and my wife and I were expecting our first child while trying to close on our first house[6]. I resisted, until it was made known to me that this request was really as a directive, at which point even dull me realized this was the first step in developing my tenure case. So I quickly put together a talk organized around three design principles: 1) The QGP will not be "seen" at RHIC - rather it will emerge as a consistent framework for describing the observed phenomena. 2) There are no cross sections at RHIC - except $\sigma_{GEOM} \sim$ a few barns and $\sigma_{CENTRAL} \sim (1\!-\!10\%) \times \sigma_{GEOM}$, but what was unknown was σ_{QGP} - perhaps it was of order $\sigma_{CENTRAL}$ but no one knew, and 3) Expect the unexpected. I took these to imply three corresponding criteria: 1) Avoid single-signal detectors 2) Preserve high-rate and triggering capabilities and 3) Maintain flexibility as long as the $'s allow. Perhaps unsurprisingly, the actual design I proposed was something similar to R807, since the open nature of an axial field magnet maximizes design flexibility over the lifetime of the experiment. I won't claim a causal correlation, but it is pleasing to revisit these ideas from 30 years ago and see they are quite consistent with what became the PHENIX experiment.

More to the point, despite such diversions, E859 did achieve its goals, making quality studies of both two-kaon correlations [54] and ϕ production [55]. And I did receive tenure - eventually - in 1994.

Each time I consult my notebooks or email archive from that time, I am amazed by the number of BNL-related projects ongoing at Nevis Labs in those years. E859 morphed smoothly into E866 (first study of Au+Au collisions), then E917, which was led by former Nevis physicist Richard Seto. In 1992 I hired Brian Cole, the stand-out in the first generation of MIT students on E802, as a postdoc. Shortly after being promoted to assistant professor in 1994 Brian proposed experiment E910 [56], based on his brilliant idea to repurpose the EOS TPC [57,58] to search for the H-dibaryon [59] in p+Au collisions at the AGS. This was a successful effort, providing not only studies of H^0 production, but also new insights into particle production in proton+nucleus collisions and forward production data for the design of neutrino beams. Along the way, Brian and I, together with Matt Moulson, wrote a nice little paper on the proper way (working in phase space, not just momentum space) to calculate coalescence rates [60], in this particular case for two Λ's to coalesce into an H^0.

At the same time, under Shoji Nagamiya's superb leadership, we were deeply engaged in preparing a proposal for a major experiment at RHIC[7]. What followed was a very complicated evolution: starting from the Columbia-led OASIS proposal, a complicated process led to a forced consolidation with two other RHIC proposals (DiMuon and TALES/SPARC, the latter itself an unforced marriage of two proposals). The new effort, with the uninspiring lab-imposed RE2 label (RHIC Experiment 2) was led by BNL physicist Sam Aronson, with the aforementioned Mike Marx serving as technical director. Within a relatively short period of time, RE2 became PHENIX, with Shoji Nagamiya as spokesperson. PHENIX received formal approval from the

[6] A month later, with the assistance of Brian and Melissa Cole, we moved into our new house one day before our first son was born

[7] An important part of that process was the design [61] of a precision time-of-flight system, led by postdoc Tapan Nayak and Nevis scientist Cheng-Yi Chi.

BNL Program Advisory Committee on March 10, 1994, more than a year after the STAR experiment. How PHENIX evolved from that point, and how I became PHENIX spokesperson three years later (again triggering the serendipity counter (**S16**)), is part of a much longer story to be told in a significantly expanded version of these brief autobiographical notes.

6 Final Thoughts

I have emphasized the role of serendipity in my career, not to persuade young scientists that all depends on the luck of the draw, but rather to emphasize the importance of being open to new possibilities as they arise. The standard bromide usually deployed in this context is "Chance favors the prepared mind", which is true but incomplete. The quote is from Louis Pasteur [62], but what he actually said was much more specific. His precise statement, intended to forestall audience objections that Oersted was simply lucky in observing the compass needle motion in response to an electrical current, was "*mais souvenez-vous dans les champs de l'observation le hasard ne favorise que les esprits prepares*" [63], which translates to "but remember in the fields of observation, chance favors only prepared minds". So yes, sure, in some cases my mind was (more or less) prepared, but it would be foolish indeed to assert that preparation led directly to my career path as part of some master plan. Whether you call it prior preparation, good luck, serendipity or privilege[8], I do feel very fortunate to find myself doing precisely what I set out to do some number of decades ago.

There were also many cases where I failed to follow through, particularly as the not-unwelcome demands of RHIC physics became dominant. Perhaps the most egregious example is not publishing all of the impressive results found in Ole Vossnack's thesis [64], where he was way out in front of the field on the "sunny-side" effect seen in realistic cascade models of particle production (work done with Tapan Nayak), and on the possibility of using Coulomb effects in unlike charged particle pairs to measure source sizes. It's the smallest of consolations to note that at this same time I was equally remiss in not publishing some of my own work in HBT phenomenology, on a convenient parameterization of sources with x-p correlations (kindly referred to as the "Zajc model" in the comprehensive review by Heinz and Wiedemann [65]) and on its utility in investigating the unphysical limits of various approximations that violated the uncertainty principle [66]. These missed opportunities are an important lesson to me. I shall endeavor to do better in the future, particularly for my collaborators and students. And should I be so fortunate, I will continue to welcome the role of serendipity in research and in life.

[8] In my view, not so much, as a first-generation student from a (lower) middle-class household raised in a community with decent but not outstanding schools.

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