Discovery's Ecstasy, Freindship's Reward

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When I received the request from professors Roger Porter and Robert Reynolds to contribute to this volume, two words came to mind: hubris and hegemony. I learned both in Hum 110 during the fall of 1960. My section was led by William Alderson, professor of English, an imposing, theatrical sort of figure with a basso profundo voice. Would it be hubris on my part to accept the invitation? I thought about Reed's accomplished alumni, and I thought about all those paper conferences with Alderson. There was the time he simply said, "That was nice paper, why did you type all over it?" Another time, he returned a paper in tatters inside a large manila envelope. Often, instead of humanities, Alderson wanted to use meeting time to discuss my major—physics—especially cosmology. One day in class, when asked about some point in history, I pronounced *hegemony* with the accent on the first syllable—**hej**-uh-moh-nee instead of hih-**jem**-uh-nee—much to the mirth of my peers. Alderson rumbled away with his hand over his mouth as he sometimes did, his deep voice vibrating through everything in the room. I learned a lot about writing from him. At the end of the course, he informed me that my writing had improved more than that of any other student he had ever taught. With this prior—if wary—approval in mind, I accepted the invitation and have tried to write a Reed-worthy essay.

The repeated influence of my Reed education on my career has been manifest. Experiencing quality teaching and developing independent, critical thinking are two of the virtues of a Reed education. Coupled with my innately anti-authoritarian nature, these provided the foundation for a career in scientific research and education. As I reflect on that career, I will highlight the effects of events that took place early on.

My PhD thesis, which I wrote in 1969 with my mentor and teacher George Uhlenbeck, has continued to be a source of scientific development throughout my career. It contained a foundational theory for physical applications of stochastic processes—processes described in terms of variables that are essentially random—or so-called noise. The subject was thought to have been completed when I made my contributions. I would later apply this theory to fluids and chemical reactions, operator cumulants, chaos, lasers, functional calculus for stochastic processes, quantum chaos and coherent states, and rectified Brownian motion. Throughout these years, I also worked to understand the origin of life on Earth and the origin of the genetic code. In 1988, I wrote about these biological problems in my book, *Energy and the Evolution of Life*. In 2007, I created a website, fefox.com, where I address these problems in a section tantalizingly called "Mysterium Tremendum."

For 36 years I earned my living at Georgia Tech, where I rose through the ranks. From 1999 until 2005, I served as chair of the School of Physics, acting as chief administrator for about 45 faculty members (professors and research scientists) and about 15 staff members serving about 300 undergraduate and graduate students. This was a multi-million-dollar-per-year enterprise. During my term as chair, the School of Physics notably progressed in atomic and optical physics, condensed matter physics, and nonlinear dynamics. Both Georgia Tech and Reed share a respect for teaching as the primary mission of educational institutions. Research is essential, and it facilitates and is facilitated by the teaching mission. These similarities made it easy to work at Georgia Tech after a Reed education.

I taught physics every year from 1971 through 2007, even during my tenure as Chair. I loved teaching, and in 1992 Georgia Tech presented me with an outstanding teacher award. When asked what I thought made for good teaching, I responded, “know your subject, love your subject, and love teaching.” Otherwise one’s students will perceive the teaching as weak. The best academics feel joy in their profession while teaching, as experienced through elegant logical relationships and helping students get “turned on.” Unfortunately, the pressures of funding research and publishing papers often prevent us from feeling that joy. Of course, the deep breakthroughs in research that bring momentary ecstasy are even less frequent.

At Reed, I saw many great instructors in action: Dorothy Christensen, John Leadley, and Byron Youtz to name just three. Christensen was my faculty advisor for all my time at Reed. She also taught me freshman calculus. She was always well prepared and thoroughly in command of the subject and of us (I hesitantly, but with fond memories, record this class chant by a classmate, the late Joe Parnell ’63: “D. O. T . . . T. I. E., Dottie is the girl for me”). I later had Leadley in linear algebra and in group theory. He was a spectacular teacher who instilled in us the dual desires to understand and to solve problems. He would not shy away from problems that also visibly taxed his imagination. Equally impressive were the teaching skill and polish of Youtz, who instructed me in electromagnetism. Later I found it a privilege to teach this deep subject frequently to undergraduates and graduate students alike. In his class, Youtz laid the foundation of my understanding of this sublime and difficult discipline.

In 1964, I graduated from Reed Phi Beta Kappa in mathematics and physics, a dual degree issued by Reed for the first time that year (to Eugene Hirschkoff and to me). I left with an at large NSF Graduate Student Fellowship and a spot in the graduate physics class at Caltech. The Caltech experience was a rude awakening. The atmosphere was tense and competitive, different from the collaborative rivalry of Reed, and physics classes were large and poorly taught. I transferred the next year.

I went to Rockefeller University to work with George Uhlenbeck on random processes in physical systems. Life sciences with a medical emphasis dominated the atmosphere at Rockefeller. My fellow Reedie Barbara Ehrenreich ’63 had gone there two years earlier to study biochemistry, and her move had inspired me to look into that institution. Rockefeller had about 100 graduate fellows, of whom a handful were in mathematics or physics. In those days, the purely graduate-level program of Rockefeller was unique in the United States. Students could live in the posh dormitories at East 63rd and York Avenue, eat by candle light with tablecloths and silver, and play tennis or squash. Abraham (Bram) Pais was a famous particle theorist who became my squash partner. Although there was a great difference in our ages, and although Jack Scrivens at Reed had trained me in racquet sports, Bram got to relish, if rarely, winning a well-constructed point. We also shared an interest in the representation theory of groups.

Part of the first-year program at Rockefeller involved writing a research paper. This exercise gave students a chance to feel out research specialties and various working environments. I chose to research and write about the character representations for the symmetric groups, in which I had developed an interest in John Leadley’s course at Reed. The Reed senior thesis experience served me especially well in my first-year project. I knew how to produce a serious paper quickly. I discovered some new properties about the characters of the symmetric groups and impressed the faculty, especially Gian-Carlo Rota. My results appeared in a 1967 issue of the *Journal of Combinatorial Theory*. This was my first published paper. It was unusual for a first-year research paper to become published; Rota had facilitated publication. From him I also learned about fine wines and cigars!

Leaving Caltech for Rockefeller proved fortunate for me in developing wonderful professional relationships. Among these, my mentor and friend Mark Kac was a very funny man who exemplified excellence in all facets of human life. I remember experiencing the Northeast Blackout of 1965 with him in the Rockefeller University bar, 5:16 p.m., November 9. He wasn't certain whether it was electricity or drink that caused the initial flickering that preceded blackout.

During these early years of my career, two events dominated my experience: the choice of a doctoral thesis topic and a scientific fight with Ilya Prigogine. After my success with the first-year research paper, various faculty members, particularly Bram Pais, offered me a role in their research programs. However, I had chosen to work with Uhlenbeck when I transferred from Caltech. Typically Uhlenbeck's students would do a project he assigned to them, and then they would sit with him while Uhlenbeck wrote the thesis. He gave me off-diagonal long-range order in density matrices as a topic. Although I did work on it for several months, eventually I went back to my own research on irreversible thermodynamics; this was my real interest, and Uhlenbeck had contributed to fundamental papers about these stochastic processes earlier in his career. I knew about this literature when I chose Rockefeller. As it worked out, I found new results on a theory similar to one for which Uhlenbeck was certain everything was already known. During the summer of 1967, I wrote a report concerning my ideas and got Mark Kac to read it. He saw the novelties and spoke to Uhlenbeck on my behalf. With his subsequent generous input, I wrote my thesis myself in 1968, and Uhlenbeck and I also coauthored two papers published in *The Physics of Fluids* in 1970. Uhlenbeck's questions and other contributions strengthened the content enormously. These papers continue to be cited today.

I graduated in 1969 and became a full member of the Rockefeller University chapter of Sigma Xi, a rare honor for a graduate student. My ties to Kac and Uhlenbeck remained strong during the 1970s. Uhlenbeck promoted my thesis work and our joint papers. At the time of my graduation I was chosen to be a Miller Research Postdoctoral Fellow in Physics at the University of California, Berkeley.

It did not take me long to rediscover the work of Ilya Prigogine, which I had encountered at Caltech. Uhlenbeck and Lars Onsager had strong opinions, indeed qualms, about his research, but Kac communicated with him and even exchanged a postdoc. In 1971, I moved from Berkeley to Atlanta to become an assistant professor of physics at Georgia Tech. At the time, the Prigogine group in Brussels was promoting the then-new Glansdorff-Prigogine Criterion for nonequilibrium thermodynamic steady states with applications to biology. At first I couldn't understand their theory, but once I did, I found a flaw in the logic—where a result could be sufficient but not necessary. I coauthored a paper with Joel Keizer '64, and later wrote two more, criticizing Prigogine's idea.

At first Prigogine was livid, and he even telephoned Kac from Europe one morning (the middle of the night for Kac) to protest an editorial decision to publish my paper in the *Proceedings of the National Academy of Sciences* (*PNAS*). Regardless, the critique was published, along with a response from Prigogine; time would tell who was right. Some years later, in 1981, Prigogine was the Hitchcock Lecturer at the University of California, Davis, while I was on sabbatical there as a guest of Joel's in the chemistry department. I was eager to meet him face to face. Once I did, we spent many hours together. By then, he was a controversial figure so that the local organizer for the Hitchcock lecture series had difficulty filling his time slots. At one point in our conversation, Ilya asserted in his thick accent, "When you do as many things as I do, you make a few mistakes"—although he never admitted as much publicly.

Publicly, the Glansdorff-Prigogine principle got restated for a while, and then finally Prigogine retired it from his writings altogether. He was a charming and multi-talented man but a little grandiose in his scientific claims, which is what worried Uhlenbeck and Onsager. After our exchange in *PNAS*, his group waged a campaign to promote his (flawed) principle. He won the Nobel Prize in Chemistry in 1977, the year after Onsager died, and the award cited his principle and its putative importance for questions about the origin of life. It was known that Manfred Eigen and Francis Crick were in favor of the award. In print, Eigen misquoted the principle with regard to precisely the issue Joel and I had criticized, necessity and sufficiency, thereby missing the flaw. I wrote Eigen a letter about it. He did not respond. Years later, the scientific layman's perception was that I had done battle with a Nobel laureate (Prigogine, not Eigen) and won. Alas, that he wasn't yet a laureate when the argument was won was a point people often missed. Indeed, the fight triggered events leading to the awarding of the prize! This story highlights the politics of science at work. To underscore it, the argument that I had won the fight was used, I was told, when I was selected for the title of Regents' Professor of Physics at Georgia Tech in 1991.

My very close friend and research colleague, Joel Keizer had abetted me in my battle with Prigogine. The repercussions for Joel, a member of the chemistry department, were much harsher than for me. Many vocal, well-established physicists were clearly critical of Prigogine so that my colleagues at Georgia Tech and elsewhere were sympathetic to my position. Many chemists, on the other hand, could not admit that a Nobel laureate (in chemistry) could be in error, or unwarrantedly honored. Joel lamented to me about negative effects even several years later.

Joel and I had met as freshmen at Reed in 1960. Over the years, we shared an interest in stochastic processes as well as many other subjects. He and I also shared appreciation of stunning facts about nature. Joel had read my thesis and mastered the subject. Years later a well known physicist, Yuri Klimontovich, translated into Russian a monograph Joel had published in 1987, *Statistical Thermodynamics of Nonequilibrium Processes*. The Russian edition had an added preface that made it seem that Joel had been my first student. This remained a joke between us—one that Joel did not always appreciate. Nevertheless, the content of his book manifests the influence of my thesis. In particular Joel took the idea of *contraction of the description* that I learned from Uhlenbeck and greatly extended in my thesis to new limits. I had shown that the results applied to a Brownian particle of molecular size, i.e. on the nanoscale. Joel showed that they also applied to neutron scattering and worked down at the few-angstrom scale. These stochastic elements are determined from macroscopic principles in this Onsager-like theory. Therefore the connection with nanoscale processes is profound. The equations were known to Landau and Lifshitz much earlier than 1969 but were based on a misapplication of Onsager's theory for irreversible processes. It was this subtle difficulty concerning time symmetry that I corrected in my thesis. Uhlenbeck had renewed his interest in Brownian motion and challenged me to show that a sphere inside a fluctuating fluid executes Brownian motion. I had struggled with this problem for a few months and then found a beautiful identity in the integral calculus that made it so. I experienced that momentary ecstasy that accompanies discovery.

One of the pleasures of being a scientist is the opportunity to make a positive impact on the work of one's colleagues. I am gratified by a reference to my research by my Georgia Tech colleague and fellow Reedie David Dusenbery'64. In 1978, I published a review of my work on stochastic processes. Dave was interested in temperature sensitivity in microscale organisms. He included the following statement in his recent book *Living at Micro Scale: the Unexpected Physics of Being Small*:

> What are the physical constraints limiting sensitivity to temperature? Conveniently, my college classmate in physics at Reed College and colleague in physics at Georgia Tech, Ron Fox, had analyzed related problems using sophisticated statistical mechanics. He calculated the correlation in temperatures at two positions differing in time and space for a substance in thermal equilibrium, and here I make use of this result.

Dave had added explicit information about our relationship, an example of the collegiality that makes the practice of science gratifying.

In the 1970s, chaos entered my life. I am referring not to personal problems but to the subject of chaotic dynamics. Certain parallels exist between the dynamics of thermal fluctuations in mixtures and chaotic dynamics in deterministic systems. At the time, chaos theory was a hot topic. A mathematical object, the Jacobi matrix, governs the dynamics of thermal fluctuation correlations. In the deterministic dynamics (in the absence of fluctuations) used in chaos theory, the Lyapunov exponent that characterizes the existence of chaos is determined from a time-evolving Jacobi matrix. I began seeing this connection in the 1980s and published about it late in that decade. In 1990, Joel and I coauthored a paper explaining the ideas, especially the amplification of fluctuations by chaos. I also published a paper focused on the related topic of quantum chaos. I showed that the classical Lyapunov exponent was a quantum signature of classical chaos. A more entrepreneurial soul would have made a big deal about this insight. Joel appreciated it, but the chaos community as a whole was lukewarm. I, on the other hand, consider this connection to be one of my most important scientific results. Sir Michael Berry has cited in print my application of these quantum chaos ideas to the classically chaotic motion of the Jupiter moon Hyperion. This sort of connection among celestial mechanics, quantum mechanics, and chaos is at the heart of the subject and is one of its wonderful features. It is gratifying to be explicitly cited by other scientists for one's ideas and not just for one's publications.

Berry had previously acknowledged my influence on his work with respect to the eponymous Berry's Phase. He characterized an exchange between us during one of his seminars as the key to the discovery. He said so publicly in Atlanta in 1990 during his acceptance speech for the Lilienfeld Prize given by the American Physical Society. Berry thanked me for "inseminating him with the idea" that led to Berry's Phase. Berry is exemplary in crediting intellectual antecedents. I mention these two examples to illustrate the satisfactions from exchange of ideas we scientists enjoy among ourselves.

My affinity for mathematics and physics had been evident at Reed and continued during my research career. In the 1970s, I worked on the mathematical side of stochastic processes and the rapidly evolving art of numerical computation. Early on, operator calculus in particular fascinated me. I became adept at it and published a long review article in which operator methods were applied to the theory of Gaussian stochastic processes in physics. "Gaussian Stochastic Processes in Physics" continues to be my most cited publication. It discusses many esoteric topics, including Onsager's theory, Ornstein-Uhlenbeck processes, correlation matrices, operator cumulants, hydrodynamic fluctuations, and quantum relaxation. When you write about a subject at its deepest level, you are rewarded by unexpected discoveries. The resultant elation is such that it doesn't matter much whether later it is found that the result is already known.

It is significant that I changed sub-fields several times during my career. Each time, I met resistance (most commonly in the form of anonymous referee reports) from those who already claimed turf in my new chosen area of research. I consider it to be human nature that ideas of turf as well as a genuine necessity to compete for limited funding intrude into the practice of science. Nevertheless, I feel extraordinarily fortunate to have lived during the era in which I did, with NSF funding available to allow me for many years to pursue research on whatever I chose.

My choice of mathematics and physics as fields to study was always based on the desire to obtain a penetrating understanding of biology. When I entered Reed, the study of biology at the molecular level was undergoing revolution as a result of the discovery and elucidation of DNA structure, RNA function, tRNAs, genetic codons, and ribosomes, among other macromolecular components. Much of this became known during the 1960s. I was good at mathematical physics and found my way as a professor of physics, yet during the past 20 years, I have published papers in biophysics and have written two books about the origin of life.

I conducted my biophysics work in two arenas, stochastic neuron dynamics and rectified Brownian motion. Overseeing the work of my last graduate student, William Mather, on rectified Brownian motion and publishing with him about it are among the highlights of my career. Again, because I was moving into a field jealously guarded by those already in it, some resistance to our results did arise. We showed that a cartoon video (produced by a leading research group) on the function of the motor protein kinesin was in clear violation of the dynamics at the nanoscale and at low Reynolds number (a measure of the relative importance of viscosity and inertia). We also showed that thermal energy could be harnessed to do useful work at the expense of metabolic free energy in the form of adenosine triphosphate (ATP). This is a revolutionary perspective. There is no violation of the second law of thermodynamics. At my current age, I am content to know that the facts will push biophysicists to this point of view eventually, even if I am long gone by that time. I am also at peace with the real possibility that someone else will claim, or be given, credit for the idea. Some of us do our science primarily for the love of it, for the joy of discovery and the appreciation of beautiful ideas, not for fame and fortune. I have always found that it is far more satisfying to be appreciated by a few personal friends than to be appreciated by any number of anonymous persons. When Joel Keizer succumbed to lung cancer a decade ago, I lost more than my best friend.

As I mentioned early in this essay, my lifelong interest in the origin of life has never been far from my thoughts. After retirement, it became my central focus. My father spent the majority of his career working on this question in his laboratory and got me interested in it at an early age. His friendship with biochemist Arthur Livermore of Reed College was a major reason that I came to Reed in the first place. I think it all worked out quite well after that.

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