Terrell L. Hill

1917–2014

BIOGRAPHICAL *Memoirs*

A Biographical Memoir by
Ralph V. Chamberlin
with a personal recollection
by William A. Eaton

NATIONAL ACADEMY OF SCIENCES

TERRELL LESLIE **HILL**

December 19, 1917–January 23, 2014

Elected to the NAS, 1965

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Terrell Leslie Hill, physical chemist and molecular biologist, passed away at his home in Eugene, Oregon, on January 23, 2014, at the age of ninety-six. Hill spent roughly equal parts of his career at the University of Oregon, University of California, Santa Cruz, and the National Institutes of Health (NIH). Many researchers recognize Hill as a pioneer in several areas of science, especially at the interface between physical chemistry and theoretical biology. He was among the first to emphasize the need for interdisciplinary research across chemistry, biology, and physics. At the University of Oregon he initiated the world's first Institute of Molecular Biology and was instrumental in its early success. He was the first to apply statistical mechanics to physical adsorption, providing a foundation for the Brunauer-Emmett-Teller (BET) theory. He originated the field of small-system thermodynamics, "the most extensive addition to equilibrium thermodynamics since Gibbs," and used it to model molecular aggregates and polymers. He introduced a general diagram method for steady-state kinetics and used it to develop the principles of free-energy transduction in biological systems. He developed the theory of steady-state and kinetic cycles with free energy transduction, a forerunner of the modern theory of stochastic thermodynamics. He combined statistical mechanics and biochemical kinetics for a theory of muscle contraction and ciliary motion. He was elected to the National Academy of Sciences in 1965.

The Hill family is of English descent, having immigrated to the Boston area in 1727. In 1848, Terrell's grandfather (a carpenter and cabinet maker) moved from Suffolk County, Massachusetts, to the San Francisco area. Thereafter, the family lived at various times in Northern California and Oregon. On his mother's side, the Morelands are of Irish and German heritage, having immigrated to Pennsylvania around 1840. Subsequently, the family moved west, spending several years in Minnesota and Kansas, before finally settling in Northern California.

Terrell Hill was born on December 19, 1917, in Oakland, California. He was the second of three sons born to George Leslie Hill and Ollie Isis (Moreland) Hill. George and Ollie were married on April 22, 1914, in San Rafael, California. George Hill was an electrical engineer who worked in a research laboratory for the Pacific Gas and Electric Company. His originality is reflected in several patents that he obtained, and despite being mostly self-taught, he and his wife always emphasized education for their boys. Books and classical music played an important role in their home, as they did for Terrell Hill throughout his life. Terrell's older brother, Kenneth Hill, was a chemical engineer and an investment analyst who specialized in the petroleum industry. He also collected rare books as a serious hobby. In 2001, Kenneth and his wife Dorothy donated to the University of California, Berkeley, one of the few remaining original (1869) two-volume texts by Mendeleev, which was the first publication to show the periodic table of elements. Terrell's younger brother, Ernest Hill, was a nuclear engineer who worked at Lawrence Livermore Radiation Laboratory for more than thirty years.

Terrell's excellence in mathematics was initiated in high school. There, he was guided by E. J. Albrecht, whom he called his "early and best teacher" and who "made learning and mathematics fun." Terrell remained in contact with Albrecht for many years.

In 1934, at age 16, Terrell began studies at the University of California, Berkeley. He first majored in mathematics, then psychology, then biology with an eye towards medical school, before finishing with an A.B. degree in biochemistry. Because there were few opportunities for theorists in biochemistry at the time, he chose theoretical chemistry for graduate school at Berkeley. Although motivated by the Lewis School of Thermodynamics, most of his graduate work was done with G. E. K. Branch. During fall semester of 1940, Terrell switched briefly to study mathematics and physics at Harvard, but the uncertainty of war made it favorable for him to return to chemistry at Berkeley. His PhD thesis from Berkeley (1942) is titled "I. The reaction of diphenylamine green with bases. II. Theory of the isoelectric point." The first part includes an experimental problem proposed by Branch, while the second part contains his own theory on the isoelectric point of polymers.

Terrell's early work was done at several institutions. His post-doctoral appointments were as instructor in chemistry at Western Reserve University and research chemist at the Radiation Laboratory in Berkeley, working on isotope separation for the Manhattan

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project. In 1945 he moved to the Department of Chemistry at the University of Rochester. Later, he learned that his recommendation letter came with the warning that he was a better basketball player than chemist. Nevertheless, after one year as a post-doctoral fellow he was hired as an assistant professor at Rochester. Although quite adequate as an instructor of introductory courses, he realized that it was not the best use of his talents, so in 1949 he moved to the Naval Medical Research Institute (NMRI) across from the NIH in Bethesda, Maryland. This switch allowed Terrell to work full time on basic research and to interact with experts in diverse areas of science, as the NMRI was becoming a world leader in biophysical research.

In 1956, as research at the NMRI was becoming more applied, Terrell accepted a position as professor of chemistry at the University of Oregon. At that time, scientific research at the University of Oregon was struggling to recover from a decade-long hiatus after being eliminated by a State Board decision in 1932. Terrell worked with Frank Reithel (the department chair in chemistry) and Robert Clark (the arts and sciences dean) to strengthen the sciences. For the chemistry department they first attracted John Schellman and Richard Noyes, then Virgil Boekelheide and William Simpson. Other scientists that Terrell sought to hire for the biology department were not so quick to join. He needed to enhance the attraction.

The inspiration came to him one day as he walked by the campus gym when, as he later explained, "The solution suddenly just popped into my head." His idea was to form the Institute of Molecular Biology, combining biology, chemistry, and physics into the first interdisciplinary group of its kind in the United States. It is also believed to be the first time that molecular biology was used for the name of any group. His efforts and influence were crucial in attracting Aaron Novick to Eugene as director of the institute. Others soon followed, including George Streisinger, Frank Stahl, Sidney Bernhard, and Peter von Hippel, who provided the foundation for the institute as it grew into a world-class center. Terrell himself chose not to join the institute, insisting that it should be made entirely from new appointments.

About the same time, Terrell proposed the Institute of Theoretical Science to bolster both chemistry and physics at the University of Oregon. Terrell invited Marshall Fixman to become its director; Fixman was soon joined by Robert Mazo and others. In 1962, Virgil Boekelheide became the first person from the State of Oregon to be elected to the National Academy of Sciences. In 1965, Terrell was the second. Subsequently, John Schellman, Richard Noyes, George Streisinger, Frank Stahl, and Peter von Hippel were also elected to the National Academy of Sciences.

While at the University of Oregon, Terrell wrote the book *An Introduction to Statistical Thermodynamics*. He also developed the theory of small-system thermodynamics and the general diagram method for treating nonequilibrium steady-state kinetics, which he and others have recognized as his most important contributions. A list of Terrell's PhD students while at the University of Oregon includes Richard Gordon, Joel Keizer, and Donald McQuarrie, who became well known in their own right.

In 1967, Terrell's love of Northern California induced him to move to the recently opened University of California, Santa Cruz. Soon after arriving, he served one year as vice chancellor, becoming involved in the landscape design of the university. Other activities at this time included applying the diagram method to the sliding filament theory of muscle contraction, working on equilibrium thermodynamics for steady states, and writing poetry. It was at UC Santa Cruz where Terrell met his long-time friend and collaborator Yi-der Chen.

In 1971, dissatisfied with the state of higher education and politics in California, Terrell made his last career move to the NIH, where he was chief of the Section on Theoretical Molecular Biology at the National Institute of Diabetes and Digestive and Kidney Diseases. This appointment came with an agreement that he would return to UC Santa Cruz each summer. During this time he worked on nerve conduction, muscle contraction, cooperativity in steady-state systems, aggregation of microtubules and actin, fast axonal transport, and application of a modified diagram method for mean first-passage times and random walks. In 1988, Terrell formally retired from science to live quietly in Santa Cruz. In 1996, he came out of retirement to add some new ideas to the field of small-system thermodynamics, which he aptly renamed "nanothermodynamics." Some additional details about Terrell's many contributions to chemistry, biochemistry, and physics are as follows. Near the start of 1946, while at the University of Rochester, Terrell attended a lecture by Paul Emmett on the theory of physical adsorption of gases on solids, now associated with the names Brunauer-Emmett-Teller (BET). For the next several years, Terrell focused on developing the first comprehensive statistical-mechanical and thermodynamic derivation of the BET theory. He then extended the ideas to include the surface tension and adsorption of liquids on diverse interfaces. In 1959, this work culminated in an exact definition of quasi-thermodynamic point functions applicable to non-uniform systems.

Terrell's theory of small-system thermodynamics has been called "the most extensive contribution to equilibrium thermodynamics since Gibbs." Indeed, it can be said that this theory is so fundamental and innovative that it is how Gibbs himself would have developed finite-size thermodynamics had he not died prematurely. Terrell began his work on small-system thermodynamics in 1961 while on sabbatical in Cambridge, England, a place his family thought of as their favorite "second home." A brief introduction to his work was first presented at the American Chemical Society meeting on March 23, 1962. It then took another two years to fully develop the theory, which was published in a journal article and two books. Because there were no length restrictions for the books, Terrell was able to explore many of the significant effects that arise when thermodynamics is adapted to treat finite-size systems. A key result is that the Gibbs-Duhem equation must be modified to include non-extensive size effects if energy is to be conserved.

The main idea of small-system thermodynamics can be understood from Terrell's 2001 publication, "A Different Approach to Nanothermodynamics," in which he emphasizes the subdivision potential, E. This E facilitates conservation of energy in finite-size systems and can be understood by comparison to Gibbs' chemical potential, μ, which is the change in energy to take a single particle from a bath of particles into the system. In contrast, E is the change in energy to take a cluster of interacting particles from a bath of clusters into the system. Generally, a cluster of N interacting particles does not have the same energy as N isolated particles due to surface terms, length-scale effects, etc. Now, for the first time, these non-extensive contributions to energy could be treated in a systematic and complete way. Most important are the non-extensive contributions to energy that come from thermal fluctuations, which are difficult to treat using other approaches.

After completing the theory of small-system thermodynamics, Terrell shifted his main interest so that his subsequent use of the theory was limited to the infrequent times that he needed a general description of the statistical and thermal properties of small systems. Furthermore, with few exceptions, no one else used this theory for more than thirty years. Rediscovery of Terrell's theory was initiated in 1994 when Dover republished Terrell's article and two books into a single volume, *Thermodynamics of Small Systems (Parts I and II.)* Around this time, Terrell privately suggested "nanothermodynamics" as a shortened and more current name for small-system thermodynamics. Terrell noted that the term nanothermodynamics did not appear on the World Wide Web until it first appeared in print, in the year 2000, so it is clear that Terrell was the originator of the theory and the name. Again, a comparison to Gibbs is in order. It is legendary how Gibbs introduced the chemical potential in 1876, but it took fifteen to twenty years before his work was translated into German and widely adopted by early physical chemists. Terrell's small-system thermodynamics, which includes a subdivision potential that is mathematically similar to Gibbs' chemical potential, was introduced more than fifty years ago and is still not widely used. Part of the explanation is that Terrell was not inclined to overtly advertise his own ideas. Another explanation was best stated by Joel Keizer in 1987: "It may be, as with much of Terrell's work, that it was simply ahead of its time and that in future years much will be made of it."

In 1964, Terrell began to focus on systems exhibiting non-equilibrium steady-state behavior. At first he collaborated with Ora Kedem to develop several models for transport across membranes. Then Terrell developed a general diagram method for steady-state cyclic kinetics, which allowed him to compute various quantities in a non-equilibrium steady state where probabilities do not satisfy detailed balance. This work extended and generalized the earlier work of L. Onsager (reciprocal relations) and E. L. King and C. Altman (thermodynamic fluxes and forces). He spent several years elaborating and applying these ideas, which he summarized in two books: *Free Energy Transduction in Biology* (1977) and *Free Energy Transduction and Biochemical Cycle Kinetics* (1989 and 2005). Indeed, some of Terrell's results on cycle kinetics were early examples of modern fluctuation theorems that now occupy a central stage in nonequilibrium statistical physics. In 1968 Terrell combined statistical mechanics and biochemical kinetics for a proper treatment of muscle contraction, based on the sliding filament model of A. F. Huxley and H. E. Huxley. More detailed versions of the theory, published in 1974 and 1975, were adopted by workers in the field of muscle contraction and ciliary motion.

In 1977, Terrell began a comprehensive program to extend the two-state Ising model from equilibrium behavior to steady states far from equilibrium. In this problem, enzyme molecules cycling at steady state influence the rate constants of other molecules on neighboring sites. The standard critical behavior of the Ising model becomes much more complex in these steady-state systems. Applications are made to enzyme molecules in membranes, in lattices, or moving freely in two dimensions. A common link in much of Terrell's work at this time is that it involves small biochemical systems that are non equilibrium in nature, with chemical energy being dissipated into heat and/or converted into useful electrical and mechanical forms.

Terrell's work on nonequilibrium kinetic processes was also well ahead of its time, so that later researchers often had to re-discover things that he had done much earlier. He studied cross-bridge theories of sliding filaments before the notion of motor proteins even existed. In the 1980s, when the motor protein kinesin was discovered, Terrell was able to immediately apply his theory of nonequilibrium steady state transport. Also in the early 1980s, in collaboration with Marie France Carlier, Dominique Pantaloni, and Marc Kirschner, Terrell investigated various features of polymer assembly for both actin and microtubules. These relatively simple noncovalent polymers had the unusual property of binding and hydrolyzing nucleotide triphosphates during assembly. Several times Kirschner urged Terrell to examine the role of treadmilling in microtubules, a process where subunits are preferentially assembled from one end and disassemble from the other. At first Terrell showed little interest in the process of nonequilibrium GTP hydrolysis, because detailed balance is violated during the assembly process in polymers at equilibrium. However, when Kirschner proposed a simple model of microtubules suspended between two rigid barriers, Terrell quickly applied many of his theories coupling kinetic and thermodynamic properties, and he suggested a number of interesting properties for these polymers. Theoretical work continued on various models of polymers with nucleoside triphosphate caps and binding and capping proteins, published in the *International Review of Cytology.* Terrell managed to persuade the publisher to accept two nearly book-length articles of one hundred twenty-six and one hundred fifty pages. In these collaborative studies, Terrell would typically start by interrogating Kirschner about the important features of microtubules, go off for a month to calculate intensely, and then proceed to write a first draft of the paper. Someone once said that these papers were "equations by Terrell Hill and words by Marc Kirschner," but Kirschner insists that Terrell also provided most of the words.

Terrell's work on heterogeneity in the polymer lattice concerning the role of ATP hydrolysis in actin assembly led to further theoretical collaborations with Carlier and Pantaloni. Terrell's calculations of energy conservation in the kinetics of assembling polymers became important fifteen years later for understanding its role in actin assembly. Terrell was electrified when Kirschner and Mitchison discovered the striking property of dynamic instability in GTP assembly. He then used Monte Carlo simulations to rationalize how the GTP cap at the end of the polymer could cause this unique behavior. The "sleeve model," which explains how microtubule disassembly can do significant work in moving chromosomes, is one of the most important models that Terrell generated during this period. This model was experimentally verified by Koshland, Mitchison, and Kirschner, and it has been very influential in the field of mitosis. Terrell's seminal work on single-molecule motor proteins and the role of ATP and GTP hydrolysis in polymerization remains highly respected by researchers in cell biology as well as in theoretical biophysics and mathematical biology. This work was beautifully summarized in his 1987 book, *Linear Aggregation Theory in Cell Biology.*

In his theoretical work on the mechanism of muscle contraction and relaxation, and more generally on the mechanism of free energy transduction in biological systems, Terrell made three major contributions. The framework of this theoretical work came from structural, physiological, and biochemical studies conducted in many different laboratories. Muscle contraction is driven by two sets of filaments: the thick myosin filaments and the thin actin filaments sliding past each other. This sliding motion is driven by myosin cross-bridges extending from the myosin filaments repeatedly interacting with the actin filaments as the myosin cross-bridges hydrolyze ATP. Biochemical studies have shown that the myosin cross-bridges pass through a number of different conformational states as they interact with actin and ATP. However, prior to Terrell's model, it was difficult to relate biochemical measurements to physiological measurements of force, velocity, and stiffness made in intact and skinned muscle fibers. Terrell's contribution was to develop a method to relate the minimum free energy of each of the myosin cross-bridge states as measured in solution to the change in the free energy of the cross-bridges as the myosin and actin filaments slide past each other. He did this by plotting the free energy of the cross-bridge states on the ordinate and the relative positions of the actin and myosin filaments as they slide past each other on the abscissa. The slope of the free energy curve for a particular state at a particular position then provides a measure of the force exerted by this cross-bridge state at this position, while the change in force versus the change in distance provides a measure of the stiffness. This method for modeling muscle contraction, developed by Terrell, marked the first time that the stiffness and force exerted by the cross-bridges could be related to the free energy of the cross-bridges as they hydrolyze ATP.

Terrell also developed a theoretical basis for understanding muscle relaxation by again developing a model that combined biochemical and physiological parameters. Muscle relaxation occurs when calcium is removed from the troponin-tropomyosin complex that lies along the thin actin filaments. Because a single tropomyosin molecule covers seven actin monomers, and the tropomyosin molecules, in turn, are in contact with their neighbors, the binding of the myosin cross-bridges to the thin actin filaments is highly cooperative. Terrell incorporated this cooperativity into his model, as well as the idea that relaxation occurs because the cross-bridges are prevented from transitioning from a weak-binding cross-bridge state to a strong-binding cross-bridge state, the latter being the major force-producing cross-bridge state in the muscle. Although there is still controversy as to whether relaxation also involves small changes in the binding strength of the weak-binding cross-bridge state to the thin filament, Terrell's model of muscle relaxation, like his model of contraction, has again provided a method for directly relating biochemical to physiological measurements.

Finally, Terrell's work on muscle contraction led him to an understanding of a much broader problem, the general mechanism of free-energy transduction when ATP is either hydrolyzed or synthesized. He clearly showed that the free energy of ATP hydrolysis is not localized in the ATP molecule, nor is this energy released when ATP is hydrolyzed at the surface of a protein. Rather, this free energy is due to a difference in the free energy of ATP and ADP plus inorganic phosphate (Pi) in solution. In other words, ATP does not energize a protein in the way that light energizes the protein rhodopsin. Terrell showed that the free energy of ATP hydrolysis arises simply from the fact that following the binding and hydrolysis of ATP to ADP and Pi, these products can be released from the protein at no cost in energy because ADP and Pi together are more stable and/or present at a lower concentration than ATP in solution. Therefore, the change in protein conformation caused by its interaction with ATP does not have to be reversed in order to release the bound nucleotide. The protein can do work as it returns to its original conformation and can then rebind ATP to undergo repeated cycles of free energy transduction. The binding of an ATP analog could cause a protein to undergo the same conformational change as the binding of ATP but then the free energy made available by reversal of this conformational change could not do useful work because it would be linked to dissociation of the bound analog. Terrell recognized that this mechanism of free-energy transduction was applicable to many biological processes, such as the kinetic cycle that drives active transport as well as a proton gradient driving ATP synthesis from bound ADP and Pi, which was, in effect, simply the reverse of ATP hydrolysis driving active transport.

Various sabbaticals and visiting positions played an important role in Terrell's career, and they also greatly influenced scientists where he visited. He was awarded a Guggenheim

Fellowship to visit Yale University in 1952 and 1953, where he interacted with J. G. Kirkwood and wrote his first book. In 1955 and 1956, and again in 1964, he visited the Weizmann Institute as a guest of Aharon Katzir and Shneior Lifson. Others from the Weizmann Institute whose careers were impacted by Terrell's work include Michael Sela, Ora Kedem, and Arieh Warshel. Terrell spent sabbaticals in Cambridge, England, from 1960 to 1961, again in 1964, and in 1977. He often found time to travel, visiting many places in Europe, Israel, and Japan, which broadened the horizons for him and his family and helped them to become true citizens of the world.

Terrell was a member of the American Chemical Society and the Biophysical Society, and as a student he was elected to Phi Beta Kappa. In 1965 he was elected to the National Academy of Sciences. Some additional honors and recognitions include: Arthur S. Flemming Award from the United States Government in 1954; Distinguished Civilian Service Award from the United States Navy in 1955; Washington Academy of Sciences Award in 1956; Sloan Foundation Fellowship from 1958 until 1962; Kendall Award from the American Chemical Society in 1969; Superior Service Award from the Unites States Public Health Service in 1981; and Distinguished Service Award from the University of Oregon in 1983.

Terrell published two hundred fifty-six scientific articles and ten books. He is the primary author on about ninety percent of this work, including the sole author on sixty-five percent of the publications. These statistics are not from selfishness—in my experience, Terrell generously gave credit for even the slightest contribution—but they arose from his general preference to work alone. Terrell collaborated most frequently with Yi-der Chen, but also had multiple publications with others including R. M. White and E. Eisenberg. I have the honor to have my name on Terrell's last publication, "Fluctuations in Energy of Completely Open Small Systems," *Nano Letters* (2002). Terrell then abruptly stopped all scientific work, stating that he was afraid he might make a mistake.

Throughout his career, Terrell wrote a total of ten scientific books. It started in 1952 when he was awarded a Guggenheim Fellowship, with the plan to write a book with J. G. Kirkwood at Yale. Due to administrative duties, Kirkwood found no time to help, so *Statistical Mechanics: Principles and Selected Application* (1956) became Terrell's first book. The book summarizes several important advances in statistical mechanics over the previous twenty years, primarily from Kirkwood, J. E. Mayer, L. Onsager, and Terrell himself. In 1960, the book was translated into Russian at the urging of N. N.

Bogoliubov, to be used by him and his associates in theoretical physics and mathematics. Indeed, it has been said that Terrell's books were preferred in the former Soviet Union, where instructors and students chose books based on the quality of the presentation, not on appearances or pressure from the publisher. Terrell's second book, *An Introduction to Statistical Thermodynamics*, has been translated into many languages including Bulgarian, Chinese, Spanish, and Italian. This book started the road to research for a generation of physical chemists and molecular biologists, and is still used in courses where mathematical clarity and completeness are emphasized.

Dover has republished four of Terrell's books—two textbooks and two monographs—more than from any other author. In fact, *Thermodynamics of Small Systems* (which was originally published as two books) has been reprinted by Dover in three different versions, as it becomes increasing relevant fifty years after first publication. Terrell's work can be summarized by adapting a sentence from the preface to *Statistical Mechanics Made Simple*, by D. C. Mattis. In English there is a saying that things of negligible value are not worth a hill of beans; in statistical mechanics there is a saying that some books are not worth a bean of Hill's.

No biography of Terrell is complete without some mention of his activities outside of science. Tall and agile, with quick feet and soft hands, he was an exceptional athlete. For many years he was a playground director for the Oakland Recreation Department. He joined the basketball team at Fremont High School for the first time as a senior, helping the team to an undefeated season and victory in the Oakland City Championship. He played basketball collegiately at UC Berkeley, where as a freshman his team upset the archrival team from Stanford University. The Stanford team featured Angelo (Hank) Luisetti, who is often considered the greatest basketball player prior to the 1950s. Luisetti was the first person to successfully use the jump shot, and he still holds the single-game record for the most points scored by a Stanford basketball player. From the time Terrell was a junior until he finished graduate school, he played as a member of the Athens Athletic Club, a team based in Oakland that belonged to the Amateur Athletic Union (AAU), which was the highest level of basketball at the time. During the 1940–41 season they "surprised" the rival team, named Golden State (before they became the NBA Warriors), to win the Oakland Tribune Championship in a two-game playoff, earning a trip to Denver for the AAU Championship. At the Championship, Terrell and his team made it to the semifinal game, where they played the Olympic Athletic Club from San Francisco, whose star was again Hank Luisetti. The headline reads, "Athens Cagers Stop Hank," holding Luisetti to eleven points, but in the end Terrell's team lost by a score of

42-36. Still the article states, "Defensively, Athens can credit Terry Hill, Lew Goldenson and Bill Taretson for keeping them in the game." Terrell continued to play basketball until the age of thirty-three, when injuries forced him to stop.

At the age of forty-one, Terrell took up tennis and quickly developed a powerful serve-and-volley game. He was a founding member of the Eugene Swim & Tennis Club, "the only club he ever joined." From the age of forty-five until nearly eighty, he often beat players half his age. One year when he had a shoulder injury, he taught himself to play left handed, using his quiet determination to still find a way to win. Perhaps this was from practice playing ping pong with his wife, who once wrote: "When Terrell plays left handed and gives me an eight-point lead, we are pretty even." Around 1988, Terrell prepared a manuscript entitled "Tennis Probabilities," for which he sought comments from the great American tennis player Arthur Ashe. Although the manuscript was never published, it manifested Terrell's love of sports and statistics. Starting in high school, and continuing throughout his life, Terrell enjoyed inventing board games. One such game, which also manifested Terrell's many interests, was "boson chess," where multiple pieces may simultaneously occupy the same square.

In 1941, Terrell met Laura Etta Gano. Their first encounter was in an introductory chemistry class at Berkeley, when Laura was a nursing student and Terrell the graduate teaching assistant. They were married on September 23, 1942, in Berkeley. They honeymooned by driving a serviceman's car across the country to Terrell's new job in Cleveland. The trip included some car repairs in Jackson Hole, Wyoming, where Laura's grandmother had homesteaded and they still had relatives. Jackson Hole is also where Terrell and Laura took their last vacation together, sixty years later. Their marriage lasted seventy-one years, until Laura preceded Terrell in death by about two months. Together they had four children: Julie Eden, Lynn Lineburg, Terry Hill, and Ernie Hill. At the time of Terrell's death, there were five grandchildren and eight great grandchildren. Throughout their lives, Terrell and Laura had a mutual love of classical music, reading, traveling, and philanthropy. The world has lost a giant of a man who was a deep and creative thinker, a gifted athlete, and a kind and compassionate person. It has been an honor and a privilege to have known him.

Here are the final lines of "Some Perspectives of a Scientist," written by Terrell Hill circa 2005.

> *We are indeed fortunate that, at the present time, enough science has been done already (though science, especially biology, is really in its*

infancy) to have available at least a sketch…of the universe and of the sun and earth. We each have something like 50 to 100 years to do our witnessing. On the universe's time scale this amounts to practically an instantaneous glimpse. We knew nothing of this story before our lives began and we will again know nothing after our lives end (this is our fateful symmetry). But the universe will go on indefinitely doing more of what it has been doing, including the possible creation of additional planets that eventually produce living and thinking beings.

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Recollections of Terrell Hill, by William A. Eaton

I met Terrell Hill for the first time when he arrived at the NIH in 1971. To me, he was the legendary theorist who wrote the famous textbook, *Introduction to Statistical Thermodynamics*, which I had used as a graduate student at the University of Pennsylvania in the middle 1960s to learn statistical mechanics. We became friends, initially, because of morning tennis at the Linden Hill indoor tennis club near the NIH campus. It was very kind of him to play with me because he was so much better than I, with what was for me an overpowering serve and volley game by a tall and agile athlete. I guess he liked my enthusiasm for the game and did not seem to mind not having much of a challenge. I was not that bad for a scientist, but still I recall winning only one set from Terrell over the ten years or so that we played together. The tennis connection sparked a close social relationship, together with our wives Laura and Gertrude, and we took turns hosting frequent dinner parties. Gertrude and I also had great affection for Laura, who was always incredibly gracious, kind, and an interesting and beautiful person. Terrell had many interests in addition to science and tennis, especially poetry, classical music, history, and basketball, and we had almost all of these in common. Gertrude, a devotee of poetry, described Terrell's poetry as Wordsworthian in its lyricism. Terrell also liked the fact that, coming from Philadelphia, I was quite knowledgeable about basketball, including the history of professional basketball, as my father took me to Convention Hall to watch the Warriors in the late 1940s, just after the NBA first came into existence.

Even though Terrell's interests at NIH were in theoretical biophysics, our specific research interests never overlapped, so we did not really have much of a scientific relationship, even though we were in the same small research building, Building 2. One reason is that Terrell was a highly focused and disciplined scientist who wanted to spend virtually every minute of his afternoons at the NIH (he worked at home every morning) making progress on whatever problem he was thinking about at the time. These characteristics contributed to his impressive productivity. Terrell did, however, give me theoretical help on several occasions, but in a rather unusual way. I would show up in his office without any warning, and ask him a question for which I needed theoretical help. His response was invariably the same: "Billy, tell me one more time so I am absolutely clear on what you are asking me." I would repeat the question, often scribbling something on his blackboard to try to explain it in more detail. He would then respond: "OK, Billy, let me think about it. Come back tomorrow." When I returned the next day, Terrell would hand me a reprint and point to the section in which he had solved exactly the problem that I was concerned with or something very close to it, usually in a paper where it was a special case of a general class of problems. This did not happen just once, but almost every one of the ten times or so that I came to him with a serious theoretical question. The reason his answers were invariably a reprint is that Terrell solved an enormous number of problems in his more than fifty years of research. Martin Karplus, another great theorist and Nobel Laureate, once remarked to me that Terrell had solved such a large number of problems that rediscovering his work could produce a respectable career for at least two theorists. In fact, Terrell only infrequently discussed science at length with colleagues while at NIH, other than with his close associate Yi-der Chen, his rare post-doctoral fellow, and just a few NIH collaborators, most notably Evan Eisenberg of the Heart Institute. No one ever held this against Terrell, because he was so well liked and everyone in his Laboratory of Molecular Biology was so pleased with the fact that one of the most famous theorists of the twentieth century was a member of their laboratory, and was there if they needed the help of a brilliant theorist.

Terrell hated to give seminars, and he had a reputation of not being a good seminar speaker. He also did not attend many seminars and hardly ever went to scientific meetings. When I asked him about this, he told me that if he wanted to know what scientists have to say about a subject, he would read their papers. What they say in a seminar may not be their considered opinion. A paper is different. The author writes and rewrites, and has yet another chance after putting the paper aside for a few months when the proofs arrive. So, explained Terrell, the final printed paper represents their considered opinion on the subject.

I was very curious to hear Terrell give a seminar and after many attempts finally was able to convince him to give one to my laboratory in Building 2, the Laboratory of Chemical Physics. By most ordinary measures, it was not a very good seminar because it really was difficult to follow. However, I had decided beforehand to listen and concentrate as hard as I could. It turned out that every sentence was in fact clear. The problem was that Terrell assumed that the audience instantaneously understood every sentence, and that they would retain the idea for the remainder of the seminar. He simply did not repeat any points. If one missed a single one, the listener could easily be lost and not understand anything that followed. My mind usually drifts, at least for a short period, during any seminar, no matter how good, but that day it did not and the seminar really was excellent.

Terrell was an incredibly well organized individual. Not very long after he arrived at NIH, at age fifty-three, he surprised me by telling me that he would definitely retire at age seventy. Sure enough, a few months before his seventieth birthday, he came to me and said, "Billy, I am going to retire, and I don't want you to make any fuss about it. If you insist on doing something, just have a dinner party for Laura and me." The "fuss" Terrell was referring to is that, normally, a retiring scientist of his prominence at NIH would have a symposium in his honor and a large reception and formal dinner for dozens of people. In addition to the dinner, I did organize a farewell luncheon for Terrell with his tennis buddies. At the luncheon, I presented him with a plaque showing the winner of the "NIH Building 2 Tennis Championship Sponsored by D. R. Davies, W. A. Eaton, H. Eisenberg, I. H. Pastan, A. Szabo Tennis Association" for every year from 1972 to 1988 (see picture, right). Terrell's name was engraved as the champion for every year, except 1982 and 1983, when there was no name because he had a serious foot problem during those two years and did not play; Terrell was so much better than the other members of the "Association" that it would have been in bad taste to have one of our names on the plaque.

There was another interesting aspect to his retirement from NIH. He told me that he was not going to do any more science and prepared two manuscripts that he was holding in the event that he found himself in a situation where he felt he had to contribute a paper to a friend's Festschrift. I believe a paper published in *Proceedings of the National Academy of Sciences U.S.A.* in 1996 was one of them. He did, moreover, come out of retirement during the period 1998–2002 and published several papers on the thermodynamics of small systems, described in detail in the preceding beautiful biography by Ralph Chamberlin.

Terrell rarely talked about any of his accomplishments, even at dinner parties, where after sufficient wine he might have said something. There were two exceptions. He was very proud of the fact that two of his nine books were reprinted by Dover. Classic books are reprinted by Dover, and it is rare for any scientist to have more than one. Dover has reprinted two more since Terrell left NIH; I suspect that no other individual in any field of science has ever had four. Terrell was also, it seemed, equally proud of his basketball career and actually bragged to me on several occasions about his basketball accomplishments. For the first few years after he left the NIH he would occasionally send me original newspaper clippings in the mail with the understanding that I would return them, knowing that as a Philadelphia basketball aficionado I would appreciate them. One of Terrell's newspaper clipping's had a headline: "Terry Hill holds Luisetti to 10 points."

I had one experience, which revealed how proud Terrell really was of his basketball prowess as a young man. After leaving the NIH, Terrell became an adjunct faculty member in the chemistry department at the University of California, Santa Cruz. He told me that he did this primarily to have library privileges, as he really was not going to continue doing science, but that he was going to continue with mathematics and tutor high school students. (I always thought that one reason he solved so many problems in statistical mechanics was that applied mathematics might have been his true love.) In 1997 I was invited to give a seminar in the chemistry department at Santa Cruz. Terrell came to the seminar and sat in the first row, but in a corner out of sight of most of the audience. I learned from my host that this was the first time that Terrell had ever come to a seminar in the chemistry department. So, I began my seminar by telling the audience that the famous theorist and member of their department, Terrell Hill, was present at the seminar. Terrell had this terribly annoyed look on his face and whispered at me, "Billy, I will get you for this." I then went on to tell the audience that what they probably did not know about Terrell Hill was that he was a great basketball player and played in the basketball league that preceded the NBA. The look on Terrell's face changed dramatically to one with a gigantic grin that expressed, "Yup, I'm that guy!"

Terrell was a gentle man, but extremely self-confident. I think I understood the origin of this quality after a conversation many years ago with one of his former University of Oregon PhD students, Joel Keizer. Joel told me that once he had dinner with Terrell and Terrell's mother. Joel was also quite impressed with Terrell's self assuredness, so he thought he might learn something about this by asking Terrell's mother what he was like as a boy. Her reply was, "Oh, Terrell was always so incredibly smart. Why, I don't think he ever said anything that was wrong."

I enormously enjoyed my friendship with Terrell during his years at NIH. We had many good times together. I am especially proud of the inscription in the Dover reprint version of his textbook that I used as a graduate student: "May 1987, For Bill, kindred spirit, Terrell." I visited Terrell only a few times when he and Laura were still living in their beautiful townhouse in his beloved Santa Cruz (with the tennis plaque prominently displayed in his study), but, unfortunately, not after they moved into assisted living. Our only contact during this period of his life was by email. Terrell's last email to me, at age ninety-five, said, "Billy, I still have all my marbles."

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