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A Biographical Memoir by

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ANTHONY LEONID ("TONY") TURKEVICH WAS born in New York City in 1916, one of three children of a Russian Orthodox clergyman, Leonid Turkevich, who became head of the entire Russian Orthodox Church in North America and Japan. Tony went to Dartmouth College for his undergraduate studies, completing his B.A. in 1937. From there he went to Princeton, working with J. Y. Beach on structures of small molecules for his Ph.D., which he received in 1940. Turkevich then went to Robert Mulliken in the Department of Physics at the University of Chicago as a research assistant, studying molecular spectroscopy. He also worked on the radiochemistry of fission products.

Soon after the outbreak of World War II Tony joined the Manhattan Project as one of its youngest scientists. In 1942 he worked at Columbia and the next year went to the Metallurgical Laboratory at the University of Chicago where he stayed until 1945, when he moved on to Los Alamos. During that period Turkevich worked closely with Charles P. Smyth and Enrico Fermi. At Chicago he studied the separation of uranium isotopes by gaseous diffusion of the volatile uranium hexafluoride. He also studied the radiochemistry of reactor products, including plutonium produced by neutron capture in uranium. At Los Alamos he participated in the first test of a nuclear bomb, at Alamogordo in July 1945. Turkevich made accurate estimates of the energy released in that explosion—rather more accurate than the estimate Fermi made by measuring how far the blast wave carried bits of torn paper.

After the Alamogordo test, Turkevich moved to the theory group under Edward Teller to investigate thermonuclear reactions and the potential of fusion. In particular, he worked with Nicholas Metropolis and Stanley Frankel, using the ENIAC computer to demonstrate the feasibility of a fusion bomb. This was one of several stimuli that led to the development of the Monte Carlo random-selection statistical computational method (so named by Metropolis, but actually invented independently and much used in the 1930s by Enrico Fermi but not published then). Tony worked closely with Nick Metropolis to develop and apply the Monte Carlo method. This method, in turn, was the one Tony used in the next-generation computer, the MANIAC, at Los Alamos in 1952 to study cascades of nuclear processes.

In 1946 he became an academic scientist, officially a chemist, when he returned to the University of Chicago's Department of Chemistry as an assistant professor. One of Tony's distinctive characteristics as a scientist was the way physicists thought of him as a nuclear physicist, and the chemists considered him a chemist. The boundaries were irrelevant to him; the challenges of the problems were what mattered. At Los Alamos he worked closely with many people who also had associations with the University of Chicago: Enrico Fermi, Herbert Anderson, Richard Garwin, Willard Libby, Harold Agnew, Murray Gell-Mann, and others. He collaborated frequently with George Cowan; to quote George:

He introduced a kind of nuclear science at Los Alamos that seamlessly merged chemistry and physics. It was a happy marriage. Tony was an invaluable asset who returned whenever he chose, particularly during those summers when he wasn't diverted by some other overriding commitment. I quickly learned to go to him with my most intractable problems.

It was Tony Turkevich who, with S. Katcoff, proposed using atmospheric sampling as a way to monitor nuclear explosions. An early application was the determination of the atmospheric concentration of the radioactive isotope krypton-85 as a means to measure the number of fissions produced in reactors and atmospheric nuclear bomb tests. This proposal was written in a letter from Tony to Philip Morrison in July 1946, but that letter and the subsequent history of the monitoring of atmospheric radioactive krypton came into the literature only in an article by Tony, Lester Winsberg, Howard Flotow, and Richard Adams, published in 1997 in the *Proceedings of the National Academy of Sciences*, after it was finally declassified. There had been two internal reports in 1949 and 1951 on which the article was based. This article demonstrates how the concentration of atmospheric krypton-85 increased in proportion to the number of fissions in reprocessed fuel elements and nuclear tests. Krypton-85 is the fingerprint gaseous fission product that is released when fuel rods are dissolved and processed. Air samples from 1934 showed no krypton-85 with its (roughly) 10-year half-life. Samples taken in the 1945-1950 period show significant levels of this isotope, which could only be related to nuclear reactors and explosions. The research also showed the effectiveness of global circulation and mixing in the atmosphere.

In 1948 Tony was promoted to associate professor, and in September of the same year he married Ireene ("Renee"), his lifetime partner. In 1954 he became professor. Throughout the 1950s he carried on his research at the university and worked steadily at Los Alamos as well. A significant part of the latter activity involved classified studies. Much of that work was in response to Soviet nuclear technology. The first Soviet atomic bomb test, in 1949, brought Los Alamos back to a near wartime state with six- and seven-day work weeks to develop and test a thermonuclear weapon. The first crash tests were in Nevada, and the next stage moved quickly to Eniwetok atoll. Tony and George Cowan worked on that project. Then George and Tony went off to see the crucial Greenhouse George test. George was the first thermonuclear fusion device, designed largely by Richard Garwin in the spring of 1952. Tony was also continuing to implement what had become a growing intelligence program to monitor Russian production of plutonium by observing the changes of krypton-85 concentrations in the northern hemisphere. The Chicago campus became a center for this activity.

Los Alamos was involved in measuring other Soviet emissions and evaluating reports of atmospheric and seismic signals, mostly but not exclusively from nuclear tests. Tony consulted with George Cowan and Rod Spence, members of the Bethe Panel that regularly met, examined data, and reported to intelligence authorities in Washington. The U.S. tests provided unique opportunities for scientific discovery, including the production of new heavy elements and other multiple neutron capture products in the Ivy Mike device. Discoveries made this way were not announced until tight secrecy requirements were relaxed. The group of collaborators included scientists at Chicago and Berkeley.

That "nuclear weapons club" of scientists involved with weapons intelligence expanded to include the British. However, there were limitations on what information could be exchanged. Tony's official role in this activity was formally as consultant, but of course his involvement was very deep.

The activities were heavily compartmentalized. The group that met to evaluate Soviet plutonium production from the isotope measurements had regular meetings at Patrick Air Force Base in Florida for several years. George Cowan, who chaired the meetings in the early seventies, recalls that they ate well at the officer's club across the street from their meeting center, and would regularly visit the best local citrus orchards to collect loads of tree-ripe fruit to carry home.

Tony spent the academic year 1958-1959 at CERN in Geneva and was at Orsay, France, the following year. During that year at CERN he was a member and a very important expert adviser of the U.S. delegation to the Conferences on Nuclear Test Suspension in Geneva.

While he was an assistant professor he and Enrico Fermi wrote a seminal but unpublished paper, "Thermonuclear Reactions in the First Hour of an Expanding Universe." Although officially unpublished, it was quoted in detail by R. A. Alpher and Robert C. Herman in their paper in *Reviews of Modern Physics* in 1950 and cited by D. Ter Haar in the same issue. This was the first detailed analysis of how elements were produced in the Big Bang, an analysis that took into account all the reactions that involve neutrons, protons, deuterons, tritons, and both helium isotopes but require less energy than the breakup of the deuteron. There were 28 reactions in all, a remarkable task at that time; fortunately, several of them could be neglected. A principal result was the evolution of the composition of the early Universe: Following the first 500 seconds, when deuterium, tritium, and the helium isotopes were forming rapidly, the composition they found was almost 75 percent hydrogen and 25 percent helium after the neutrons decay. The results implied significantly more helium and heavy hydrogen than is found now, presumably because these were consumed in later nucleosynthesis reactions.

During the 1960s Tony began another collaboration with George Cowan, who had become interested in time-of-flight neutron spectrometry studies at Columbia in John Dunning's group. Specifically, the idea was to use the single huge pulse of neutrons from a nuclear explosion in a weapons test to detect and measure epithermal neutron fission resonances in uranium-235. A long vacuum pipe led a fraction of the pulse to a collecting wheel of metallic uranium-235 that spun past a thin slit in a heavy neutron shield. From the recovered wheel they were able to isolate fission products from individual epithermal resonances and demonstrate that fission symmetry varied with spin at each resolved resonance. Tony was a major coauthor of a series of papers in *Physical Review* that reported these results. The time-of-flight technique later became widely applied by physicists and chemists who used photographic and then electronic detectors with good time resolution to take the place of those early radiochemical analyses of exposed metal.

Tony kept his association with Los Alamos and with his friends there. He came regularly in summers, and bought a house near the Cowans. They would go swimming on weekend mornings in the neighborhood pool. When he stopped to rest, Tony would sometimes softly sing Russian liturgical music. The discussions covered almost everything including science and politics, but never religion. Tony always enjoyed food and eating. The Cowan garden grew beets; Renee, Tony's wife, was skilled at making them into a proper Russian borscht that they all enjoyed together under crystalline blue New Mexico evening skies.

In keeping with the spirit of the Monte Carlo method, Los Alamos was also a site for a regular poker game for a striking group of scientists. In addition to Tony himself, regular participants were Nick Metropolis, Stan Ulam, Carson Mark, Berndt Matthias, James Tuck, Paul Stein, Foster Evans, Rod Spence, Roger Lazarus, and occasionally Edward Teller. Teller writes of those games in his memoirs. It was a group that came for the company and the process of playing; like most of the group, Tony was neither a big loser nor a big winner. A quotation from George Cowan is appropriate here: "Fortunately for the majority, there was also a variable sprinkling of those who liked to bet to inside straights and three card flushes."

In the late 1970s Tony became a visiting laboratory senior fellow, a position that gave him complete freedom to work on whatever he liked at Los Alamos. He was a close associate and friend of the resident senior fellows. That group began a series of conversations about forming a new kind of science center that would work at the boundaries between traditional sciences, particularly at the boundaries of natural and social sciences. Among that group were Herbert Anderson and Nick Metropolis, both of whom had moved to Los Alamos from the University of Chicago. That concept crystallized in a short time into the Santa Fe Institute. Tony, as one of its founders, presented a paper titled "Reconstructing the Past Through Chemistry" at its organizing meeting in 1984.

He was elected to the National Academy of Sciences in 1967. Consistent with his faculty appointment in the Chemistry Department at Chicago, he joined Section 14.

Tony was made James Franck Distinguished Service Professor in 1970. He retired in 1986 but remained active in research and in departmental activities. In a tradition long sustained at Chicago Tony was both an experimentalist and a theorist. Because his experimental research studied or used nuclear processes, he chose to publish in the *Physical Review*, rather than the more "chemical" journals, but this was irrelevant to his important role and influence in the Chemistry Department (to which we shall return).

One of us referred to Tony as the University of Chicago's analytical chemist after he accomplished one of his most widely known achievements. (The Chemistry Department had no courses in analytical chemistry and nobody on the faculty who was officially classed as an analytical chemist.) In 1961 Tony proposed to NASA that it would be possible to do a remote analysis of the elemental composition of lunar soil and rock on the first missions to the Moon. The unmanned Surveyor project came into being formally in 1960. It was a very daring project, using the first hydrogen-fueled rocket as its upper stage, to follow the launch with an Atlas. Then the 2-ton Surveyor itself was made to land softly on the lunar surface, with controllable retrorockets to achieve that landing. The scientific payload was only 100 pounds. *Surveyor I* sent to Earth the second set of pictures taken on the Moon; the Soviet *Luna 9* had sent back the first pictures four months earlier. However the Surveyor pictures had much better resolution. The second Surveyor mission was lost in a midcourse tumble. *Surveyor III* landed safely with a controllable digging device that it used to make trenches. *Surveyor IV* failed just before touchdown. But then *Surveyors V, VI*, and *VII* were successful, and all three carried Tony's analytical devices: foremost, an alpha-particle backscattering instrument that not only gave us the first complete elemental composition of the lunar surface but also showed that the Moon is a differentiated body, not a homogeneous conglomerate such as a giant chondrite, which was the prevailing view at that time. *Surveyor V*, landing on September 11, 1967, at Mare Tranquillitatis, showed that there are basalt-like materials—essentially cold lava on the lunar surface. The results stirred considerable skepticism, but the results from later expeditions, notably *Apollo 11*, gave undeniable confirmation. Then *Surveyor VI*, which had landed on November 10, 1967, at Sinus Medii site, gave very similar results. *Surveyor VII*, which landed January 10, 1968, this time at highlands near crater Tycho, was perhaps the most remarkable insofar as it was successfully repaired remotely when the backscatter instrument became stuck during deployment and was freed by commands from Earth 250,000 miles away. The other remarkable thing of the Surveyor analyses was the finding of a very high titanium level in the lunar material, comparable to the basalts on Earth.

The Surveyor missions and their findings laid the necessary groundwork for the subsequent Apollo manned missions. These were the first that returned lunar material to Earth for study here. In fact, in 1970 the astronauts of *Apollo 12* found *Surveyor III* undisturbed, with a small pile of lunar dirt that had deliberately been dropped on one of its footpads.

An unexpected byproduct of this work was Tony's development of an analytical device to detect lead in paint, at the time its poisonous character was being recognized.

That set of lunar studies was the beginning of Tony's growing interest and activity in planetary and space studies. He became more and more involved in unmanned spacecraft missions, always with deep scientific questions as his motivation. There were sometimes problems, as when U.S. scientists were not allowed to collaborate with Soviets on Soviet projects. However, Tony was able to find a way to do one of his most dramatic—but unsuccessful—experiments by participating with a group of Germans in experiments that would be carried on Soviet spacecraft. It was a very ingenious extension of the methods he had used to analyze the surface of the Moon.

The alpha backscattering technique was incorporated in the Soviet missions to the Martian moon Phobos on spacecraft *Phobos 1* and *Phobos 2* in 1988, and then on the Russian Mars-96 mission in 1996 and on NASA's *Pathfinder* in 1998. The Phobos missions and *Mars-96* all failed, a series of experiences that only someone with Tony's optimism and resilience could accept with any equanimity. One of the Phobos missions failed, it seems, because the command system did not have a "check before sending commands" structure, and a mistyped command reoriented the spacecraft so that it could no longer receive commands from Earth.

NASA's Pathfinder mission was a fine success. The spacecraft carrying the Alpha Proton X-ray Spectrometer (APXS)—a miniaturized version of his lunar instrument and in addition containing now an X-ray spectrometer—landed safely on the surface of Mars and sent back the first elemental analyses of the Martian rocks.

During the 1970s and 1980s Tony moved to studying exotic particles and the processes that would produce them. He looked for polyneutron systems, for example, in research that involved one of us (T.E.). He also looked for delta particles and for superheavy particles within nuclei. These were some of the most exotic and difficult experiments to carry out. His reasoning was that "everyone can do the easy ones."

In the 1980s and early 1990s Turkevich, Cowan, and Economou collaborated on a difficult series of experiments to measure the half-life of double beta decay in uranium-238. Despite early failures Tony persisted until they finally separated a countable sample of plutonium-238 from some old uranyl nitrate Tony found stored for many years at Chicago. The indicated half-life was $2x10^{21}$ years, an order of magnitude shorter than theory predicted. This result would suggest the existence of Majorana neutrinos, neutrinos with very small but nonzero masses. This was very much in conflict with the then-current view. When T. D. Lee and C. N. Yang's predicted parity violations were confirmed by the experiments of C. S. Wu and Leon Lederman and reported at a meeting in New York of the American Physical Society, Eugene Wigner gave an elegant talk that explained how these experiments “proved” that the neutrino could not have a finite rest mass. That specific experiment on double beta decay has never been repeated although it was attempted unsuccessfully (due to contaminating problems) by a French group. It would require a major new effort on a much larger, demonstrably uncontaminated sample of uranium to produce a generally accepted result, but Tony ran out of time in an attempt to reconfirm it. Other methods, however, have made the idea of neutrinos with small finite masses acceptable. The field has become very much more sophisticated, and finite-mass neutrinos are now considered quite reasonable objects.

Tony lunched regularly at the Quadrangle Club, which serves as the University of Chicago’s faculty club. There were several *stammtischen* of which one was that of the physicists, where he normally sat. One could count on lively conversation with all the senior members of that department, with other regulars and occasionals from astronomy and chemistry. Visitors to the university, particularly to physics and astronomy, joined in these lunches. They were characterized by lively reviews of the latest results and new puzzles, some scientific and some just for fun.

Another daily routine was tea, otherwise the “coffee *klatsch*,” in the afternoons in Nathan Sugarman’s rooms in the Research Institutes’ building. These were very popular and well attended not only by the institute faculty but by many visitors as well. Nathan would persuade one person or another to go to the blackboard to present their new results.

During the first few years after it had been announced, there were still people who considered cold fusion plausible. An experiment came from Los Alamos that claimed to have detected small amounts of tritium that were presumed to be produced by the cold fusion process. When one of us (R.S.B.) asked Tony about that work, his typically insightful reply was, "Never believe a low-level tritium result from Los Alamos. Everything there is so contaminated that it always appears." He had superb taste in distinguishing the unbelievable (and wrong) from something very surprising but probably real. His own experimental results clearly showed that the cold fusion was not occurring.

After a few years in the university faculty housing development on 57th Street, the Turkevich family moved to the suburbs in Oak Brook. Tony would commute to the university or Fermilab by train and bus. If he and his colleagues worked late, they would retire to a soul food restaurant in Hyde Park and rejuvenate. Even these late night suppers of tired scientists were filled with lively discussion.

Tony and Renee had two children: a son, Leonid Turkevich, and a daughter, Darya Carney, both scientists, and three grandchildren, Elizabeth, Paul, and Julia Turkevich. Tony had two brothers, John, now deceased, who was on the chemistry faculty at Princeton, and Nicholas.

Eventually Tony was not able to live and work at the high altitude of Los Alamos and retired from his position there. He and Renee moved from Chicago to a retirement center in Virginia, where they lived until his death in 2002.

Tony carried out regular teaching of undergraduates and graduates until his retirement in 1986, at what was then the mandatory retirement age—not an absolutely strict condition, but Tony was quite ready at that time to devote his efforts entirely to research. It was at that time that he worked on the double-beta decay and its implications.

Tony received many awards, including the E. O. Lawrence Memorial Award from the Atomic Energy Commission in 1962, the Atoms for Peace Award of the Ford Foundation in 1969, an honorary doctorate of science from his alma mater Dartmouth College in 1971, the Award for Nuclear Applications of the American Chemical Society in 1972 and the Boris Pregel Award from the New York Academy of Sciences in 1988. He was a fellow of the American Academy of Arts and Sciences and of the American Physical Society.

Within his department Tony was one of the people who always insisted on maintaining traditional standards of excellence, especially when new appointments were being considered. He would ask the most penetrating questions, and look hardest at the qualities of originality and deep insight that he wanted to find in anyone appointed to a faculty position at Chicago. Praise from Tony was as strong a recommendation as a faculty candidate could receive.

Always gentle in manner and ready to listen to any well-thought arguments, he personified the Chicago style of a place where criticism is considered the best way to make things better.

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