NATIONAL ACADEMY OF SCIENCES

GEORGE GAMOW
1904—1968

A Biographical Memoir by
KARL HUFBAUER

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BY KARL HUFBAUER

MOST REMEMBERED NOW FOR his early advocacy of the big-bang theory, George Gamow was only 24 when he led the way in using the new wave mechanics to interpret nuclear phenomena. On a study tour from Leningrad then, he quickly converted this initiative into goodwill from Copenhagen's Niels Bohr, Cambridge's Ernest Rutherford, and their talented entourages. Despite these and an impressive array of other contributions to science, he was not elected to the National Academy of Sciences until reaching 49 in 1953. His achievements were encumbered by such unconventionality that it was a credit to the Academy that he was elected at all.

GROWING UP IN ODESSA

Georgii Gamov came into the world by caesarian birth in Odessa, a cosmopolitan port city of half a million inhabitants on the northwestern shore of the Black Sea. His father, Anton, scion of a prominent military family, taught literature at Odessa's Real School for boys and his mother, Aleksandra, from a prominent clerical family, taught geography and history at a school for girls. Not only well connected but also wealthy, his parents oversaw his initial schooling in their flat in the city and in a dacha in the nearby countryside. Young Gamov was early to show a talent for reciting stories and to acquire a curiosity about natural phenomena. At age six he had the thrill of viewing Halley's comet from the rooftop of the building where his family lived. A few years later he used a microscope from his father to investigate transubstantiation. The skepticism underlying this query may have been triggered by his mother's premature death in 1913.

Later that year Gamov enrolled in the school where his father taught. His rise through the grades went smoothly until 1917, when Russia's revolutions and civil war brought more than four years of political turmoil and shortages to Odessa. During these troubles, which frequently closed the city's schools, Gamov devoted what time he could spare from scrounging for water, food, and other essentials of daily life to studying differential equations and acquainting himself with relativity. Graduating in spring 1920, he soon matriculated in the Physical-Mathematical Institute of Odessa's Novorussia University. His intention was to study physics, but the institute's professor of physics, lacking the funds needed for demonstration experiments, refused to lecture. So he continued his mathematical studies. Hearing in 1922 that physics instruction was available at Petrograd State University, he persuaded his father to help him go there. Late that spring, using tickets funded by one of many sales of family silver, he journeyed north from Odessa.

LEARNING PHYSICS IN PETROGRAD/LENINGRAD

When he arrived in Petrograd in July 1922, Gamov's chief assets were his robust health, his exceptional talent, and his prior work experience as a mathematical computer for Odessa's astronomical observatory. Crucially, he had as well an introduction from his father to a former colleague who was teaching physics and meteorology at the city's Forestry Institute. He soon parlayed this connection into the job of making thrice-daily observations at the school's small meteorological station, a sinecure that covered his room and board. He also matriculated in the university's physical-mathematical faculty. He immediately tested out of the math courses that he had taken in Odessa, making it possible for him to concentrate on the preset physics curriculum when courses began that fall.

Gamov flourished during his first two years in Petrograd. In 1923 he moved from his first job there to a better position at the First Artillery School that combined running the field meteorological station with substitute teaching of physics and meteorology. The following year he gave a paper—his first—at the IV Congress of the Russian Physical Society, which was held in Leningrad (Petrograd's new name following Lenin's death in January 1924). He also earned high marks on the physics exams required by the university for promotion to the standing of "aspirant" [for an academic position]. His success in the intermediate-diploma program enabled Gamov, whose position at the artillery school was soon to end, to land a job in experimental research at the State Optical Institute while he awaited his turn to become an aspirant.

Gamov's third academic year in Leningrad was discouraging. Bored by his experimental work and, possibly as a consequence of this boredom, inept at it, he only lasted six months at the optical institute. Meanwhile, he was obliged to meet the commissariat of education's new requirement that would-be aspirants pass mandatory courses on dialectical materialism and the history of the world revolution. He later opined that he barely cleared this hurdle. By contrast, Gamov eagerly attended a set of elective lectures on relativity offered by Alexander Friedmann. There he learned of the applied mathematician's conclusion that the Universe is not stationary but expanding or contracting. He came away from the lectures hoping to pursue research in relativistic cosmology under Friedmann's direction. Gamov's hopes were dashed, however, by the relativist's untimely death in September 1925.

After these disappointments, Gamov must have been relieved that his fourth year began the following month with his approval as an aspirant and appointment to two computational assistantships. Over the next two years he and other Leningrad students interested in theoretical physics—most notably Dmitri Ivanenko, Lev Landau, and Matvei Bronstein—formed the "Jazz Band," a spirited group that avidly followed quantum mechanics' brisk development in Western Europe. In September 1926 Gamov and Ivanenko sent the *Zeitschrift für Physik* one of the circle's earliest contributions to this literature. The only lasting thing to come from this article on wave theory was Gamov's decision to romanize the spelling of his family name as "Gamow" (the spelling that will be used henceforth in this memoir). An enthusiastic participant in the Jazz Band's debates and hijinks, he could not get excited by the outdated problem in the old quantum physics that his supervisor Yuri Krutkov had assigned him for his diploma research. In late 1926 aware of Gamow's demoralization and yet impressed by his promise, one of his former teachers persuaded Krutkov to endorse the idea of sending him to Germany for studies the following summer. This proposal went nowhere. In fact, a faculty commission complained early in 1928 about Gamow's lack of academic progress. His backers at the university must have redoubled their efforts. That May he was awarded funds for a four-month sojourn in Germany.

APPLYING WAVE MECHANICS TO THE NUCLEUS

Gamow went to Göttingen where Max Born presided over the Institute of Theoretical Physics. The rough and tumble of the Jazz Band had led him to realize that he lacked the determination and subtlety needed to distinguish himself in any of the highly competitive domains where theorists were currently following up on the quantum-mechanics revolution. So, once settled there in mid-June, he started to canvass the latest literature for a problem area where he might break fresh ground. The very first day of his search Gamow found what he wanted in an article by Ernest Rutherford on the structure of radioactive nuclei. The Cambridge experimentalist had proposed a cumbersome model to explain why α-particles of relatively low energy escape from such nuclei but bombarding α's of much higher energy could not enter them. As he was reading, Gamow had a flash of insight—radioactive nuclei are potential wells out of which α's can tunnel wave mechanically. Six weeks later he submitted a succinct paper "on the quantum theory of the atomic nucleus" to the *Zeitschrift für Physik* (1928). There he provided strong support for his theory's promise by deriving from it the well-known Geiger-Nuttall relationship between a radioisotope's decay constant and the energy of its emitted α's. Confident in his model's promise, Gamow devoted his remaining time in Göttingen to writing up a more robust treatment of the α-decay problem with his friend Fritz Houtermans. When his money ran short toward the end of August, he embarked on his return trip to Leningrad.

En route Gamow stopped in Copenhagen to meet Niels Bohr, and ended up basing himself there for the next eight months. He spoke so ably about his unpublished papers on α-decay that the Dane arranged a Rask-Ørsted fellowship for him. Bohr's judgment was sound. During the fall, Gamow had two further ideas for interpreting nuclear phenomena with modern quantum theory. The first, was that the cross-sections of the α-induced nuclear reactions so assiduously studied by Rutherford and colleagues could be accounted for by regarding the α's as leaking wave mechanically *into* nuclear potential wells. The second, which he began exploring in December, was that Frederick Aston's mass-defect curve for the isotopes might be explained by thinking of the nuclei in question as quantized drops of nuclear fluid. Besides monetary and intellectual encouragement, Gamow received other kinds of help from Bohr—appreciation for his irreverent sense of humor, counsel on ways to respond to rivals and critics, and aid with arranging an invitation to Cambridge in January 1929.

Gamow's month-long visit there could not have gone better. He had lively discussions with the theory-wary Rutherford, the mathematical physicist Ralph Howard Fowler, and the low-temperature physicist Piotr Kapitza (a long-term visitor from the Soviet Union) about the usefulness of his theoretical ideas for interpreting the Cavendish Laboratory's results and for designing α and proton accelerators. He made such a positive impression that he was asked to join into a special discussion on nuclear structure to be held at the Royal Society in early February. In this session Rutherford spoke favorably of Gamow's work on α-decay, Fowler gave a semipopular exposition of it, and Gamow himself was given the opportunity to report about his ongoing efforts to account for measurements at the Cavendish of reaction cross-sections and isotopic mass-defects. Rarely has the Royal Society given such a reception to a scientist less than 25 years old.

Upon his return to Copenhagen, Gamow was so fired up that he outlined a book on nuclear theory and secured Bohr's tentative agreement to write its preface. In March he journeyed down to Berlin to consult with his friend Houtermans and Robert d'Escourt Atkinson regarding a paper on stellar theory. Their idea was that the high thermal velocities of protons in stellar cores would enable them to penetrate light nuclei, thereby engendering element-building nuclear reactions capable of providing the observed power output of stars for billions of years. Gamow, who appreciated how their scenario depended on his wave-mechanical approach to nuclear phenomena, happily helped them calculate the cross-sections they needed; and then the three of them were off to the Alps for skiing.

Gamow made sure to get back to Copenhagen by April so that he could take part in a new kind of international conference that Bohr was hosting. The participants were expected to devote themselves to unfettered discussion of theoretical physics' current problems rather than give prepared papers. Gamow was a spirited contributor to the resulting free for all. Indeed, two weeks later when the International Education Board's Wilbur Earle Tisdale visited Copenhagen on a talent search for the Rockefeller Foundation's next round of European fellowships, Bohr told him that Gamow was "another Heisenberg."[1]

MAKING DO WITH A MONOGRAPH

In early May 1929 Gamow reluctantly returned to the Soviet Union. Of course, he looked forward to seeing his friends in Leningrad, visiting his father in Odessa, and unwinding after his strenuous year abroad. But having been welcomed as an outstanding young scientist in Western Europe, he was discomfited by his status as a mere aspirant who was not yet qualified to hold an academic post. He also doubted that he could maintain his lead in theorizing about nuclei if he remained in the Soviet Union. It must have been reassuring to get a celebratory reception from his friends and the press. Still, before the month was out, Gamow responded to Tisdale's prior urgings by filling out an application for a Rockefeller fellowship. In it he requested funding for "further investigation on the theory of nuclear constitution, especially the theory of β-disintegration and the origin of γ-rays" in Cambridge and Copenhagen.[2] Then he enjoyed a relaxing interlude visiting with his father and swimming and hiking in the Crimea. Back in Leningrad by July and confident that he would receive a fellowship, he secured the Leningrad faculty commission's approval for him to make a yearlong visit to Cambridge's Cavendish Laboratory. Two months later, having been awarded the Rockefeller, he was happily leaving Leningrad's port behind.

This time Gamow ended up spending 22 months in Western Europe. He passed most of the time in Cambridge, where he had ready access to the latest nuclear research, and Copenhagen, where he enjoyed the stimulation of Bohr, Bohr's ever-changing circle of young theorists, and Bohr's informal conferences on theoretical physics. Always on the lookout for ways to mix play with physics, he used some of his Rockefeller money to buy a BSA motorcycle while in England. He later had great fun teaching Bohr how to ride it and taking good friends like Landau and Edward Teller on excursions. He often put his sense of humor on display. In July 1930, for instance, he signed a research letter to *Nature* from the summit of Switzerland's Piz da Daint, thanking his climbing companions Rudolf Peierls and Leon Rosenfeld for the chance to work there.[3] And in February 1931, he sent posters out to Werner Heisenberg and other theorists announcing a shadow pantomime about current debates that he would put on at the forthcoming conference in Copenhagen.

When it came to his research, Gamow must have been disappointed not to find any significant way during this period to advance understanding of the origin of β-particles or γ-rays. Instead, he settled on writing the book about nuclear theory that he had first outlined during the winter of 1929 after his exceptional welcome in England. Its initial incarnation was as a set of articles in a Russian physics journal during 1930 and its second as a short book published there toward the end of that year. Meanwhile, Fowler and Kapitza, who were editing a series of monographs on physics for Oxford's Clarendon Press, had recruited Gamow to write a much more ambitious version in English. His manuscript was probably off to the press by late winter 1931. However, as Gamow never was able to write orthodox English, it needed very thorough copy editing (this was expertly done by mathematical physicist Bertha Swirles). In his *Constitution of Atomic Nuclei and Radioactivity*, which finally appeared in the fall of 1931, he did not seek to provide an evenhanded appraisal of all the theoretical research on nuclear structure since his initiative of 1928. Rather, Gamow offered his own, sometimes idiosyncratic, take on the new field. His pioneering monograph served as a benchmark in the emerging specialty of nuclear physics. Presciently, it also expressed doubts about the then common assumption that electrons are constituents in all nuclei except hydrogen's proton. Gamow had warned in his manuscript that this idea was suspect by marking its every appearance with a skull and crossbones. Alas, the press softened his expressions of doubt by substituting a simple ***S*** (for speculative) for each of these dramatic warnings.

March 11, 1931 Cartoon by Gamow from a letter to Kapitza. Reproduced in Frenkel (1994). Used with permission.

DECIDING TO DEFECT—AND DEFECTING

In early August 1931 acting on the firm advice of the Soviet ambassador to Denmark, Gamow again returned to Russia. His first destination there was Moscow where he immediately sought permission to return to Western Europe in September so that he could give invited talks at conferences in Germany and then Italy. While his request was being considered, he journeyed by boat down the Volga River to the Kama and then up this huge eastern tributary to the western slopes of the Ural Mountains. The day before the Urals came into sight Gamow sent Bohr a description of the wild terrain along the Kama. He promised to return to Moscow in time to get his passport for the German conference in mid-September unless, of course, the local bears ate him for breakfast!

Back in Moscow, Gamow learned that his passport application was still under consideration. He cannot have been completely surprised because he had failed to return to the Soviet Union by the dates he had agreed upon at the beginnings of his first two trips abroad. Moreover, rumor had it that the authorities had become increasingly stingy with passports in the last year or so. Giving up hope of attending the German conference, he went to Leningrad to look into his academic status. Technically, he was still an aspirant for he had not formally satisfied the requirements for his advanced diploma in physics (at that time doctorates were not being awarded in the Soviet Union). He may well have wondered whether he would be allotted an academic position that matched his much acclaimed accomplishments. The issue of his status was quickly set to rest. Gamow was appointed as a senior expert in Leningrad's State Radium Institute and given auxiliary jobs as docent in Leningrad University's physics program and as a scientific researcher in the Academy of Sciences' Physico-Mathematical Institute. However, he still did not have his passport for the prestigious nuclear physics conference to be held in Rome in mid-October. Back he went to Moscow, and waited, and waited. During his interminable hours at the Passport Office he met, the attractive physicist Lyubov ("Rho") Vokhminzeva and soon they were socializing. He only gave up his wait when the conference had almost ended. He consoled himself by marrying Rho at the beginning of November.

Over the next half year or so, Gamow's prospects in the Soviet Union trended downwards. Gamow, Landau, and Bronstein stirred up a storm when they wired derisive congratulations to Boris Gesen on his recent article on the "ether" in the *Great Soviet Encyclopedia.* Gessen demanded that the trio be condemned for counterrevolutionary activity. In turn, Gamow—emboldened by the Radium Institute's presidium's nomination of him in December 1931 for corresponding membership in the Academy—penned a letter to Comrade Stalin in defense of the group's efforts to thwart Gessen's distortion of the party's stance on theoretical physics. Meanwhile, egged on by his friends, Gamow was spearheading a campaign to divide the Academy's Physico-Mathematical Institute into separate Institutes for Theoretical Physics and for Mathematics. The main thrust of his initiative was deflected by the Academy's senior experimental physicists, who prized their traditional primacy within physics. At the Academy's meeting in late February 1932, while supporting Gamow's election as Corresponding Member and the Physico-Mathemtical Institute's dismantling, they arranged for their discipline to be represented by a new Physics Institute. Two months later, while responding favorably to Gamow's inaugural talk on thermonuclear reactions as the source of stellar energy, they overruled his proposal that theoretical physics play the lead role in the new institute by securing the appointment of an experimentalist as its head.

The dismay engendered by these reversals was reinforced by the denial of a passport that would have enabled him to lecture at the University of Michigan's acclaimed summer school on theoretical physics.

Late that spring Gamow and Rho decided to leave the Soviet Union without passports. Their first attempt at defecting was in July when they tried paddling across the Black Sea from the Crimea to Turkey. They were thwarted by a storm the day after setting out. Their second was in January 1933 when they looked into sleighing from the Khibini Mountains to Finland. They gave this attempt up when they heard that any sleigh driver they hired would surely turn them over to the border patrol. Their third was the following August when they investigated motor-boating from a marine station near Murmansk to Norway. They gave up this idea when they saw that the Soviet navy was rapidly expanding its presence in those waters.

Shortly after returning to Leningrad, Gamow learned he would receive a passport to give an invited talk in October at the elite Solvay Conference in Brussels on "The Structure and Properties of Atomic Nuclei." Emboldened by this surprising news, he went to Moscow in hopes of arranging permission for Rho to accompany him. According to his autobiography (which is not entirely reliable), he pled his case first to Nikolai Bukharin, who had been impressed by his talk at the academy on thermonuclear reactions in stars. Next, thanks to Bukharin's arranging, he had an interview in the Kremlin with Comrade Molotov. The premier asked why he wanted to take his wife on a trip that would only last two weeks. Gamow replied that while he could say her presence was essential because of her secretarial skills, the real reason was that he wanted to take her shopping and to the Folies Bèrgere. Amused, Molotov indicated that the passport would be forthcoming. This tacit assurance gave Gamow the nerve to tell the passport office that he would only participate in the Solvay if his wife were allowed to accompany him. After five days, he emerged from the ensuing standoff victorious. In mid-October the Gamows left Russia by train for Helsinki, and beyond.

SEARCHING FOR A POSITION

Gamow's role in the Solvay Conference turned out to be rather modest. Of the six invited presentations, his paper about γ-rays—sandwiched in as it was between Dirac's on the positron and Heisenberg's on nuclear structure—had the least éclat. Moreover, his remarks in the discussions were neither profound nor humorous. One reason for his lackluster performance was that he had missed out on much of the action in nuclear physics during the preceding two years. Another was that he was still in wonder that both he and his wife had escaped the Soviet Union. And coupled with this wonder he was already worried about finding a suitable position.

Gamow's search for such a position took nine months, and even then was not entirely successful. His problem was that in an era when academic budgets were still seriously depressed, he wanted a salary that would be commensurate with his achievements and enable him to enjoy the good life. Although hosted in Paris for two months, Cambridge for one, and Copenhagen for four, Gamow soon saw that he had no viable long-range prospects in Europe. So his gaze shifted to America where he had already arranged a lectureship at the Ann Arbor summer school. In the early months of 1934 Gamow had hoped that he would be able to segue smoothly from Ann Arbor to a good job at Berkeley or its equal. These hopes had been crushed by the early summer when the Gamows reached Ann Arbor. During his eight weeks there, the possibility of a fellowship year at Berkeley surfaced, then vanished. Lacking any alternatives, Gamow agreed to join theoretical physicist Gregory Breit in giving a five-day seminar on nuclear theory at the Carnegie Institution of Washington's Department of Terrestrial Magnetism (DTM) in early September. Perhaps he accepted the invitation so that he and Rho could see Washington before returning to Europe at the end of the month. In any case, the seminar turned out to be the opportunity he needed.

It had been arranged with Breit's help by experimental physicist Merle Tuve, who wanted to assess Gamow's ability to serve as a consultant to the small group assisting him with the development of DTM's van de Graaf accelerator. Knowing that DTM would not employ a theorist, Tuve had also persuaded C. H. Marvin, president of nearby George Washington University (GWU), that the most cost-effective way to put the school's subpar physics program on the map would be to recruit Gamow—if the Russian proved his worth in the seminar. All went as Tuve hoped. Gamow was soon at the GWU campus telling Marvin that he would be happy to consider a position. However, there were obstacles to be surmounted—he had already made various commitments in Europe for October; he did not yet have a U.S. residence visa; and if he were to accomplish anything of value, other theorists would need to be appointed and a conference series modeled after Bohr's would need to be established. Marvin decided to proceed cautiously. He offered Gamow a visiting position with an adequate salary, promised a full professorship, and agreed to the appointment of one more theorist and the establishment of a conference series. Gamow accepted.

TURNING POINT

At first Gamow was somewhat embarrassed at having had to accept an appointment at a university without a reputation in physics. He did not feel that way for long. Before May 1935 he had orchestrated GWU's recruitment of his talented friend Edward Teller, secured his own professorship with the then handsome salary of $5,000 a year, and pulled together the first Washington Conference on Theoretical Physics.

Yet, though he was only 31 years of age, Gamow was on the verge of making a major decision about what he later called his "worldline." Would he seek to remain a leading participant in the increasingly sophisticated and robust field of nuclear theory? Or would he get involved in fresh lines of work when he perceived promising opportunities elsewhere? Gamow chose the second path. His center of attention stayed on nuclear theory for about two and a half years after he signed up with GWU. He did make the field the focal point of his theory conference in April 1935. In collaboration with Teller he did add a significant refinement to Enrico Fermi's reigning theory of β-decay in early 1936. And he did publish an expanded and renamed edition of his groundbreaking monograph by April 1937. To judge from this edition's preface, however, Gamow came away from the revision unenthusiastic about trying to remain within nuclear theory's front ranks.

During his remaining two decades of affiliation with GWU, he went on to become a major player in three other research areas: stellar theory from 1938 to 1945, relativistic cosmogony from 1945 to 1952, and protein coding from 1953 to 1955. Simultaneously, he emerged as one of that era's most versatile and widely read popularizers of science. In the interests of brevity, Gamow's endeavors in these various arenas are discussed thematically.

ENHANCING STELLAR THEORY

Gamow had little trouble shifting his focus from nuclear to stellar theory because he had been thinking from time to time about the role of atomic nuclei in stars for nearly a decade. In 1929 he had helped Houtermans and Atkinson explore the possibility that element-building nuclear reactions powered the stars. Three years later and on various occasions thereafter he had followed up on Landau's unorthodox proposal that all stars possess nucleonic cores with his own speculations about how elements might be generated by thermonuclear reactions near such cores. These incidental excursions into stellar theory came to mind when, in 1937, he found himself getting impatient with nuclear theory's incremental advance. He sensed that he, and other physicists, might more fruitfully use their time by drawing on the nuclear theory that was already at hand to enlarge understanding of the stars.

In particular, having recently theorized that some nuclear reactions are particularly likely to occur much more when the incident protons have a certain speed, Gamow was ready to consider whether some such resonance reaction might be the chief source of stellar energy. He failed to find any plausible candidate in the nuclear-reaction data then available. But charmed by the idea, he went on to look into its consequences, if true, for stellar structure. He concluded that it would vitiate the regnant point-source model, according to which most energy generation takes place very near a star's center because the temperature, and hence proton speeds are highest there. Instead, if a resonance were to come into play at a lower temperature prevailing above the center, the reactions occurring in the spherical shell girdling the Sun's core at this height would dominate power output. Gamow was so pleased with this model that in December 1937 he sent accounts of it off not only to the *Physical Review* but also to the *Astrophysical Journal.*

Meanwhile, Gamow had begun planning the fourth Washington Conference on Theoretical Physics, which he wanted to focus on the stubborn problem of identifying the nuclear reactions that power the stars. Pulling together regulars from his preceding conferences (notably his colleague Teller and Hans Bethe) and mixing in two leading theoretical astrophysicists (Subrahmanyan Chandrasekhar and Bengt Strömgren), Gamow organized an exemplary interdisciplinary gathering at the Department of Terrestrial Magnetism in March 1938. He had hoped, it seems, that the conferees would embrace his shell-burning model and identify the resonance reaction that he supposed was responsible for stellar-energy generation. No such luck. Nor did they respond favorably to Landau's very recent attempt to solve the stellar-energy problem. But the conference discussions did lead Bethe to collaborate with Teller's doctoral student Charles Critchfield in assessing the possibility that stellar energy is generated by an element-building chain beginning with proton-proton reactions and culminating with helium formation. The two soon concluded that this reaction chain might well do the trick. However, during their collaboration, Bethe caught glimpse of another thermonuclear scenario that struck him as a more promising energy source for the Sun, which was then thought, wrongly so, to have a central temperature of around 20 million degrees. In just two months Bethe concluded that a carbon-nitrogen-oxygen cycle that converts protons into helium nuclei is the primary source of energy for main-sequence stars with central temperatures over about 15 million degrees.

Gamow, who had been eagerly following these endeavors, led the way in hailing Bethe's proposed solution as a breakthrough on the stellar-energy problem. So confident was he in Bethe's success that he shifted the focus of his own stellar theorizing from the thermonuclear reactions that power main-sequence stars to stellar evolution. Time and again over the next seven years he tried his hand at advancing theoretical understanding of red giants, Cepheid variables, Wolf-Rayet stars, white dwarfs, novae, and supernovae and their respective roles in the overall evolution of stars. Teller, who helped out until getting fully involved in war work, later recalled that Gamow awoke him many a morning to try out his latest brainchild. Most of Gamow's ideas failed to survive their joint scrutiny. Those that did get into print were often driven from the field by withering critiques. But a rare few eventually ended up being incorporated, by others, into robust physical theories about stellar structure and evolution.

Gamow's most important contributions were to theorizing about supernovae and red giants. In the fall-winter of 1940-1941 after some two years of wondering about the process that initiated the spectacular stellar collapses that Fritz Zwicky and Walter Baade called supernovae, he teamed up with the young Brazilian theorist Mario Schönberg to explore a promising possibility. Their idea was that a very large neutrino flux would be released when the contracting core of an evolved massive star reached sufficiently high densities and temperatures that atomic nuclei rapidly captured and then emitted electrons. Unimpeded by the surrounding stellar material the neutrinos would flood out of the core, thereby triggering the star's catastrophic gravitational collapse as a supernova. They called this the Urca process because its efficiency in removing energy from a star's core reminded them of their meeting place in mid-1939—Rio de Janeiro's Urca Casino, which so effectively emptied the pockets of its patrons. In addition to this fruitful, yet still far from robust, explanation of supernovae, Gamow also enriched theorizing about the relatively large stars known as red giants. In 1943 he was the first American (he had become a citizen in summer 1940) to make a case that these giants are highly evolved stars that undergo shell burning around hydrogen-depleted cores. And the following year he was the very first to appreciate that Baade's recent division of galactic stars into two stellar populations might well enable researchers to construct evolutionary tracks for the massive stars that become red giants. Although these ideas remained controversial for years, they played a role during the 1950s in the emergence of a consensus theory of red giants.

ENHANCING RELATIVISTIC COSMOLOGY

By the mid-1940s when Gamow shifted the focus of his research from stellar theory to relativistic cosmogony, he was well used to thinking that the Universe is expanding. In 1925 the Leningrad relativist Friedmann had introduced him to the idea. Some five years later his friend Bronstein had become an enthusiastic proponent of the relativistic interpretation of Edwin Hubble's discovery of the recession of the extragalactic galaxies. In the fall of 1937, having abandoned mainstream nuclear physics, he had deepened his familiarity with general relativity by giving a graduate course at GWU on the theory and its connections with cosmology. The following year, after learning of Carl Friedrich von Weizsäcker's independent discovery of Bethe's carbon cycle, Gamow embraced the German physicist's conclusion that all of the elements must have originated sometime after our Universe began to expand but before the stars were formed. And in the spring of 1942 much of the discussion at Gamow's eighth Washington Conference on Theoretical Physics had dealt with the Universe's age and the prestellar formation of the elements. But in neither these nor his many other pre-1945 engagements with the expanding Universe idea did Gamow attempt to integrate nuclear theory into relativistic cosmogony.

Gamow evidently began such work shortly after World War II. In a congratulatory letter of October 1945 on Bohr's 60th birthday he reported that he was taking a fresh look at the origin of the elements in the early Universe. His preliminary calculations indicated that during the first millisecond of its expansion the dense fluid making up the Universe would begin to sunder and that by the end of one-tenth of a second the formation of the lighter elements would be complete. These tantalizing results led him to think that focusing "on the borderline between nuclear physics and cosmology" might be an interesting way to revive the Washington Conferences on Theoretical Physics in the year ahead.[5]

It took Gamow almost a year, in part because of his increasing involvement in science popularization, to follow up this initial enthusiasm with a two-page paper on the "expanding Universe and the origin of the elements" in the *Physical Review.* By then he had moved away from the idea that the fissioning of the Universe was the first step in the brief process that gave rise to the elements. Instead, Gamow now supposed that rapid expansion cooled the dense neutron gas constituting the early Universe, leading in turn to a short-lived nonequilibrium process of neutron clumping, β-decay, and the formation of stable elements.

About this time Gamow's talented doctoral student Ralph Alpher, then 25 and working at Johns Hopkins University's Applied Physics Laboratory (APL), admitted that his assigned subject of galaxy formation had stymied him. Gamow suggested that Alpher work instead on the origin of elements. Using newly available data on neutron capture rates, Alpher assessed Gamow's line of attack on the problem and concluded that he could do something fresh with this subject. He dove into the investigation with crucial help from Robert Herman, an APL colleague who had acquired a good grounding in relativity theory at Princeton before the war. By early 1948 Gamow was so satisfied with Alpher's progress that he and Alpher wrote up a summary for the *Physical Review* to be submitted under the names of Alpher, Bethe (in absentia), and Gamow just in time to appear in the issue dated April 1,

1948. Bethe, who was then on the journal's editorial board, gave his assent by crossing out "in absentia." In addition to serving as a vehicle for Gamow's April Fool's play on the $\alpha\beta\gamma$ of the Greek alphabet, their report offered a clearer version of his 1946 scenario and indicated the promise of Alpher's endeavor to employ recent nuclear data to match observed elemental abundances. This and other publicity arranged by Gamow gave Alpher an audience of more than 300 for his dissertation defense later in the month.

Soon afterward Gamow was off to the South Pacific to observe atomic tests at Eniwetok. On his return he visited Mt. Wilson and Palomar observatories, then boarded the Superchief for his trip east. En route Gamow had what he was quick to characterize as his best idea since his 1928 theory of α-decay. He realized that at the high temperatures required by Alpher's theory of element building, the mass density of the radiation in the early Universe would be many orders of magnitude greater than the mass density of the primordial neutron gas. Alpher, it seems, independently came to the same conclusion before him. In any case, Gamow immediately went on from this point to argue that the formation of protogalaxies occurred long after the early and fast element-building era when with the ongoing expansion cooling of the Universe, its radiation mass density approximately equaled its matter mass density. By contrast, at a more deliberate and careful pace Alpher and Herman continued working out the details of Alpher's process, estimating, among other things, that the relict radiation from the Universe's initial explosion now has a temperature of about 5 degrees Kelvin. In the ensuing decade they could not get this prescient prediction to be taken seriously. Even Gamow, despite his respect for Alpher and Herman, did not do so primarily because he thought the ambient radiation from our galaxy's stars would obscure the relict radiation.

Gamow's direct involvement in research on the Universe's early evolution decreased rapidly after 1948. He encouraged Alpher and Herman to refine their scenario for element building and persuaded Enrico Fermi, Eugene Wigner, and others to seek reaction chains that would circumvent the theory's troubling isotopic mass gaps at 5, 8, and 11. But it was increasingly as a keynote speaker and popularizer that he devoted time to what Fred Hoyle, an ardent proponent of the alternative steady-state model of the Universe, derided as the "big-bang" theory.

Ultimately Gamow witnessed but did not contribute directly to the triumph of relativistic cosmogony in the mid-1960s. Well before then, however, he had conceded that the heavy elements originated in supernovae, not the early Universe. What clinched the case for the necessarily modified view of the big-bang model was the detection of relict microwave radiation by Arno Penzias and Robert Wilson in 1965. Its temperature turned out, by great good luck for Alpher and Herman, to be fairly close to their 1948 prediction. Upon learning of this prediction and related theoretical work by Gamow's group, Penzias sent apologies for not having acknowledged their priority. In replying Gamow made little attempt to cloak his bitterness that his group's work had already fallen into obscurity.

INITIATING PROTEIN-CODING RESEARCH

Even as he was starting research in relativistic cosmogony, Gamow came to think that the time was nearly ripe for physics to help biology move beyond its descriptive stage. This perception probably derived from Erwin Schrödinger's *What Is Life? The Physical Aspect of the Living Cell* (1945) and his longtime friend Max Delbrück's successful migration from theoretical physics to experimental genetics. In any case, Gamow got so caught up with the idea that rejecting his initial plans to revive the Washington conferences with one focused on cosmogony, he instead devoted the first postwar gathering to "the physics of living matter." His preparations for the conference held in the fall of 1946, and his subsequent endeavors to promote the infusion of more physics into biology, led Gamow to believe by the early 1950s that the central "riddle of life" is how each species' genes shape its distinctive proteins. But lacking any notion about the molecular structure of genes, he could not imagine how to formulate this enigma in a tangible way.

In June 1953 Gamow got an idea for doing so from reading James Watson and Francis Crick's soon-to-be-famous *Nature* paper on DNA's structure. Confident that they were on the right track, he impulsively introduced himself to them by letter, praising them for their success in moving biology into the "exact' sciences" and expressing his hope that he could meet with them in England at the end of the summer to talk about the possibility of using combinatorics to tackle genetic problems.[4] As both were planning to be away then, Watson discussed Gamow's letter briefly with Crick, then filed it away. In late October undeterred by their failure to respond, Gamow sent a short note off to *Nature* on a "Possible Relation between Deoxyribonucleic Acid and Protein Structures" (1954). He opened by crediting Watson and Crick with having established that the basic hereditary materials are DNA molecules . Then he daringly outlined what soon evolved into the protein-coding research program. He proposed that each organism's DNA "could be characterized by a long number written in a four-digital system" that "completely determined" the composition of its unique complement of proteins, which in turn "are long peptide chains formed by about 20 different amino-acids [that] can be considered as 'long' words based on a 20-letter alphabet." The problem to be solved was how these "four-digital numbers [are] translated into such 'words.'" Gamow closed by suggesting how this might be done and promising that a fuller account would be published elsewhere.[6]

During the next few months, Gamow plunged into work on the protein-coding problem. He wrote up an expanded version of his note in *Nature* for the National Academy of Sciences' *Proceedings* and, when it was not accepted there—possibly because Gamow jokingly listed his fictional character Tompkins as coauthor—submitted it successfully (without Tompkins as coauthor) to the Royal Danish Society of Sciences' biological series. He also spurred first Crick, then Watson, and then many other researchers—especially those associated with Caltech's Delbrück and Berkeley's Gunther Stent—to join the enterprise of identifying how DNA coded proteins. As this growing research circle reviewed prior and ongoing experimental work of relevance, a consensus soon emerged that DNA did not serve as a simple template in protein synthesis. It appeared instead that the coding might be a two-step process in which DNA first coded RNA and then RNA coded proteins. Although initially resisting this view, Gamow ended up as the "synthesizer" in the "RNA Tie Club," founded in mid-1954 to foster the circle's informal communications and camaraderie.

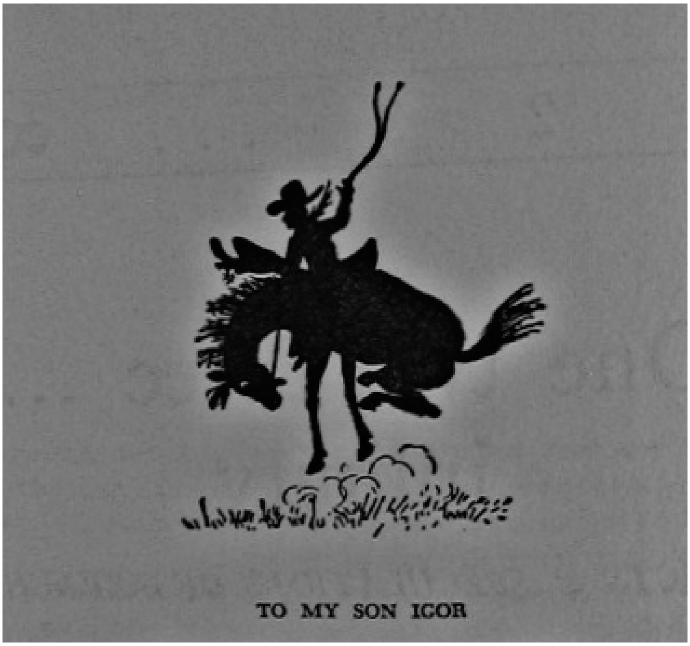
Gamow's involvement in the expanding circle of coding researchers remained intense for another year and a half. He found it stimulating to be once again on the wave crest of an exciting new specialty. Just as important if not more so, he enjoyed being at the center of the ambitious circle's partying and joking. But starting in late 1955, years before a consensus emerged about the coding of proteins, Gamow's engagement with the problem wilted. One reason was that his marriage of 23 years had just fallen apart. A second, and more compelling reason was that, as he had experienced toward the end of his active participation in nuclear, stellar, and cosmogonical researches, he was getting bored with coding research because the opportunities for someone with his freewheeling style were ever more limited in this increasingly competitive and empirically constrained field.

POPULARIZING SCIENCE

Back in 1937 just as Gamow was moving from nuclear to stellar theory, he drafted six whimsical stories about a toy universe in which the values of c, G, and h differed immensely from their values in our own. His submissions to *Harper's* and other American magazines resulted in a pile of rejection slips. While in Warsaw for an international conference during May 1938, he mentioned his disappointment to C. G. Darwin, an acquaintance from his Cambridge days. Darwin advised sending the first story to C. P. Snow, who had recently taken over the editorship of Cambridge University Press's monthly *Discovery.* That fall Gamow gave it a try. To his delight he soon received news that the story would appear in the December issue along with a request for the remaining stories. The early response to the series was so favorable that Cambridge University Press commissioned Gamow to do a book-length version. Dedicated to Lewis Carroll and Niels Bohr, *Mr Tompkins in Wonderland* appeared in early 1940. A quarter century later Gamow proudly reported that it had been reprinted 16 times and translated into many languages.

Gamow directed his second science book for the layman to a comparatively highbrow audience—i.e., those who might be curious about the origins and implications of Bethe's breakthrough solution of the stellar-energy problem. His initial plan was to have a university press publish this book as an advanced text similar to his 1931 monograph on nuclear theory. However, his inquiries at Chicago and Oxford indicated that such a work was not likely to yield royalties from their university presses. So Gamow arranged instead to do a semipopular version with Viking Press entitled *The Birth and Death of the Sun: Stellar Evolution and Subatomic Energy* (it also appeared in 1940). The many drawings that Gamow created to illustrate the points he was making were a special feature of this entertaining narrative of physics' recent interpretive contributions to stellar theory. Such drawings, which had long before begun appearing in his handwritten correspondence, became one of the trademarks of his science writing.

These two books inaugurated what became a stream of popular and semipopular books to flow from Gamow's pen. The most successful of the later ones was *One two three …infinity: Facts & Speculations of Science* (1947) (which he dedicated to his "son Igor who would rather be a cowboy.")



One two three …infinity: Facts & Speculations of Science (1947), New York:Viking, dedication page. Used with permission..

Within the cohort of research scientists that reached maturity during the 1920s he was unique for both the time he dedicated to popularizing science and the range of subjects that he addressed. One motivator for these books was the supplemental income they provided. Another evidently was a desire for a larger readership and greater name recognition than his relatively esoteric researches were ever likely to bring him. Indeed, these books enjoyed a good market, garnered many favorable reviews, and in 1956 earned him UNESCO's Kalinga Prize for science writing. However, what appears to have most strongly inspired Gamow's popularizing was a love for sharing his own enthusiasm about the fresh and often startling insights emerging from contemporary science.

WINDING DOWN

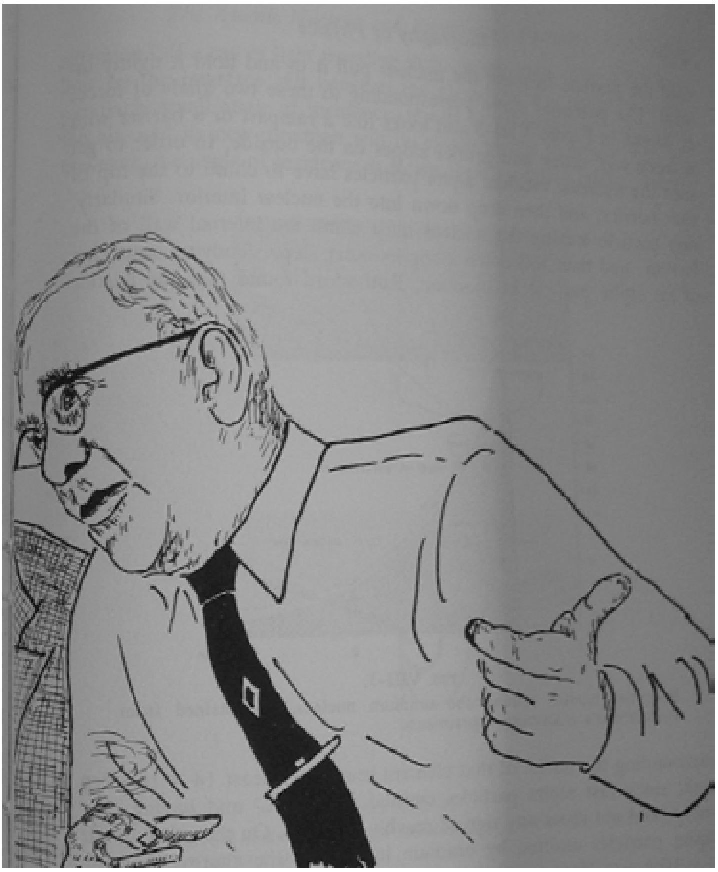
Gamow turned 50 on March 4, 1954. That day he was probably on California's Highway 1 en route from Pasadena to Berkeley in his new Mercury convertible. Wherever he was, he might well have taken stock of his first two decades in America. There was much to be proud of—his hosting of 11 Washington Conferences on Theoretical Physics, his perception of fresh opportunities on three research fronts and his agility in seizing them, and his success in conveying science's excitement to broad audiences. However, as one with an especially strong sense of self-importance Gamow would not have been completely content. In particular, until spring 1953 he had been passed over for election to the National Academy of Sciences. Moreover, his first submission to the Academy's *Proceedings* had been returned for revision just months before this significant birthday. He was not the sort to have considered the underlying reason for these perceived slights. But had he done so, Gamow would surely have suspected that his unconventionality—his opportunistic approach to research, his unreliable handling of mathematical calculations, his substantial commitment to popularization, his relentless mockery of science's solemnity, and his unrestrained consumption of alcohol—had stood in the way of the Academy's acknowledgement of his achievements.

The next five years were mixed for Gamow. He enjoyed the camaraderie of the protein-coding circle and the attention engendered by the Kalinga Prize. But the collapse of his marriage in 1955 made him desperate to get away from Rho and Washington, D.C. The following year he relocated to a fine position at the University of Colorado in Boulder. However, he lacked the clout and follow-through to pull off a coding conference sponsored by the National Science Foundation there in the summer of 1957. Worse yet, he was not invited a year later—probably because he could not push away from the bottle and was no longer active in cosmogonic research—to the 11th Solvay Conference, which dealt with the structure and evolution of the Universe. A second marriage in October 1958 to Barbara ("Perky") Perkins, a poet of about his age who had done the publicity for his third Mr Tompkins book, restored his *joie de vivre.* After she moved to Boulder from New York City and settled in with Gamow, they enjoyed a grand time together on a lecture tour that he had arranged to India, Japan, and Australia.

Gamow's routine during the early 1960s was one of graduate teaching, science writing, and traveling (including a last visit to Bohr in 1961). Two shocks in 1962—his friend Landau's incapacitating automobile accident in January and especially his teacher and friend Bohr's death in November—were sad reminders of his own mortality. Not long afterward he began a semipopular book about the revolutions in physics during the early decades of the 20th century. He dedicated his *Thirty Years that Shook Physics* (1966) "to the friends of my youth." After its publication, spurred on partly by the discovery of the big bang's relict radiation, Gamow began contributing once again to the research literature on cosmological questions. He evidently wanted to be remembered not only for his popularizing but also for his originality. Health problems in the summer of 1967—detox and, after he got out, surgery to clean his carotids—made him more introspective. He worked with Barbara on his autobiography (it appeared after his death as *My World Line*). And in April 1968 he granted historian Charles Weiner of the American Institute of Physics a two-day interview. Circulation problems, or possibly liver failure, carried him away less than four months later.

Now, some four decades after Gamow's passing, the number of scientists with personal memories of him is small, and getting smaller. By contrast, his virtual presence on the Web is large, and getting larger. At present (Oct. 15, 2008) there are some 360,000 Google hits for "Gamow" and 140,000 for "George Gamow." The names with which his name currently has the most Web associations are Niels Bohr (his chief mentor and a dear friend); Hans Bethe (his competitor in working on the stellar-energy problem and co-opted signer of the αβγ paper); Mr Tompkins (a dreamy bank clerk in four of his popular books); and Fred Hoyle, Robert Herman, and Ralph Alpher (respectively the chief rival of his big-bang Universe and his friends who were its most dedicated proponents). He is also known eponymously by nuclear physicists who think about "Gamow-Teller strengths (or transitions, or resonances, or rules, etc.)," by nuclear astrophysicists who think about "Gamow peaks ([or windows)," by theoretical physicists who think about "Gamow vectors,"…and by selenologists who think about "Gamow Crater" on the Moon's far side. I doubt that Gamow would have complained about his virtual life on the Web. But he surely would have found several joking ways to point out that this virtual life could never hold a candle to the remarkable life that he had experienced.

Gamow's self-portrait. Gamow, *Biography of Physics*, New York, Harper, 1961. Used with permission.



NOTES

1. Bohr's opinion of Gamow as paraphrased by Tisdale in his log for Apr. 25, 1929—see Gamow's International Education Board file at the Rockefeller Archive Center.
2. Gamow's International Education Board application (dated Kharkov, May 22, 1929), Rockefeller Archive Center.
3. Gamow's party ascended either the Piz Daint or the Piz Plavna da daint. In renaming the summit "Piz da Daint," he was playing on the vulgar Russian word "pizda," the meaning of which curious readers will need to learn from their Russian friends.
4. Gamow to Bohr (October 24, 1945) as quoted by Kragh, *Cosmology*...p. 106. In his popular book *Atomic Energy*...(1946), pp.75-88, Gamow expanded on the theory that he sketched out for Bohr.
5. Gamow to Watson and Crick (July 8, 1953), facsimile in Watson (2002), letter 1 (in facsimile section toward end). That Gamow should have made time to write this letter in the midst of his 12 lectures on the "Evolution of Stars and Galaxies" at the important Michigan Summer School of Astrophysics suggests how disengaged he had become by that time with research in stellar physics and relativistic cosmogony. For the significance of the Summer School, see Gingerich.
6. Gamow, *Nature* (1954), p.318.

SOURCES

As a scholarly book-length biography of Gamow has yet to be published, this memoir is necessarily based on a wide variety of sources: archived letters and manuscripts, contributions to memorial symposia, biographical articles, historical studies dealing with one or another context in which he lived or worked, and his immense (and widely scattered) array of publications. The archival collections and publications listed below were especially useful. Finn Aaserud, David DeVorkin, Genady Gorelik, Jens Gregersen, Alexei Kojevnikov, and George Trilling gve me helpful comments on this memoir's first draft.

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American Institute of Physics: Center for History of Physics, College Circle, Maryland. http://www.aip.org/history/ (Gamow interview [1968]; Goudsmit papers; Struve papers).

Cornell University Library: Division of Rare and Manuscript Collections, Ithaca, New York. http://rmc.library.cornell.edu/ (Bethe papers).

Library of Congress: Manuscript Division, Washington, D.C. http://www.loc.gov/rr/ (Gamow papers [mainly incoming 1950-1968]; Tuve papers).

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Rockefeller Archive Center, North Tarreytown, New York. http://www.rockarch.org/ (Gamow I. E. B fellowship).

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