NATIONAL ACADEMY OF SCIENCES

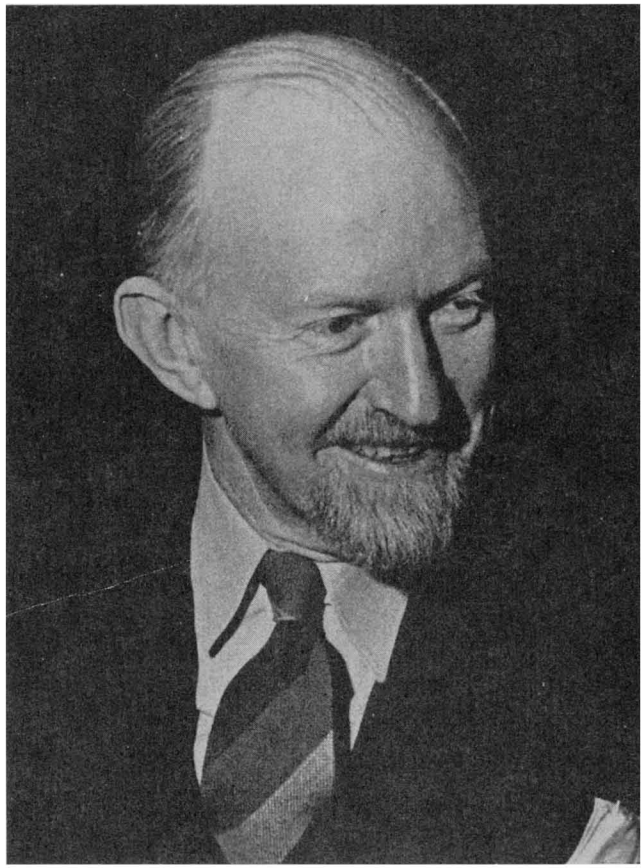
GEORGE GAYLORD SIMPSON

1902—1984

A Biographical Memoir by

EVERETT OLSON

Biographical Memoir



G. G. Simpson

GEORGE GAYLORD SIMPSON

June 16, 1902–October 6, 1984

BY EVERETT C. OLSON[1]

GEORGE GAYLORD SIMPSON'S passing in 1984 brought an era in vertebrate paleontology to an end. Along with Edward Drinker Cope, Henry Fairfield Osborn, and Alfred Sherwood Romer, Simpson ranks among the great paleontologists of our time. The intellects of several generations of students were shaped by either following or rejecting his elegant analyses and interpretations of evolution and the history of life.

Although the "Simpson Era" had its roots in the 1920s and 1930s, it seemed to emerge fully formed and without precedent with the publication of *Tempo and Mode in Evolution* (delayed until 1944 by World War II), following belatedly on the heels of *Quantitative Zoology* (1939), which Simpson had written with Anne Roe. Both books left researchers in a variety of fields pondering and often revising, conceptual bases

[1] Although I had earlier written a memorial to George Gaylord Simpson for the Geological Society of America, I agreed to prepare a more intimate and more personal essay for the National Academy of Sciences *Biographical Memoirs*. The more objective accounts of his life include the essay mentioned above (*Memorial Series*, Geological Society of America, 1985) and the essay by Bobb Schaeffer and Malcolm McKenna (*News Bulletin*, Society of Vertebrate Paleontology, no. 1933, 1985). Simpson's autobiography, *Concession to the Improbable* (New Haven: Yale University Press, 1978) and his book, *This View of Life: The world of an evolutionist* (New York: Harcourt, Brace and Company, 1964) provide a more comprehensive view of his life and thoughts.

of evolutionary analysis. The consistent empiricism of *Tempo and Mode* proved so pervasive that terms such as "tempo" and "mode"—and derivatives "pattern" and "process"—continue today to be key words in evolutionary treatises throughout molecular, organismic, and ecological biology.

After the war, the "Synthetic Theory of Evolution" espoused by Simpson created an excitement throughout paleobiology that waned somewhat in the mid-1950s, only to resurface in the 1960s and persist to the present. The debate among paleobiologists has often approached an intensity reminiscent of that following Darwin's publication of *The Origin of the Species* in 1859, including even a new breed of so-called "scientific" creationists.

AS I KNEW HIM

When I first met George Simpson at the American Museum of Natural History in 1935, he was thirty-three years old and already well established as a scholar and scientist, due in no small part to his two monumental works on Mesozoic mammals of the United States (his Ph.D. thesis at Yale) and of Europe, based on the natural history collections of the British Museum. Between 1925 and 1935 he had published some sixty scientific papers, some quite extensive, and all important.

Even then he was to many of us an enigmatic character whom, rumor had it, Walter Granger and Henry Osborn kept sequestered behind closed doors so that the workings of his genius would not be interrupted by trivia. George, who began as an assistant curator of vertebrate paleontology at the American Museum, did not see it that way at all, for though his was a crucial and demanding task, it was not particularly high on the scientific scale. But if truth in this case proved more prosaic than fancy, fancy more often than not won out.

In those early years, I—who was very young, along with

many others who were not—found myself awed and tongue-tied in his presence. This reticence in turn affected George, who, misunderstanding it, acted withdrawn and taciturn, confirming our expectations. I felt he deemed us not quite up to his advanced level of reason and knowledge—a fact that was certainly true but was not, I believe, a correct assessment of his reactions. Looking back this now seems very mixed, but it certainly seemed real at the time and continued to affect George's relations with others for years to come.

A remedy of sorts—the very dry martini—gradually made its effects felt. This beverage was then the favorite of the vertebrate people, and sufficient martinis did much to dispel our mutual shyness. Martinis and serious scientific discussion did not blend well, but this probably did not matter to George, who felt at all times that informal discourse was not profitable for discussing ideas of any import.

When I met George in 1935 he was just thirty-three, yet he had already formulated the roots of *Tempo and Mode*, his ideas of biogeography, and his views on life and evolution.[2] Today the early to mid-thirties are the normal time of life for attainment of the Ph.D. in geology and biology, and some years as a "post-doc" often follow. With much more to learn and the funds available to support it, graduate study now moves at a leisurely pace in sharp contrast to the poverty-stricken years of the 1930s and the hurry-to-finish psychology of the prewar and war years. But even then real eminence at so young an age was rare in the natural sciences. In 1935, with the ink of my own Ph.D. still wet, George's early prominence did not puzzle me. Later it did, and by talking with George and reading what he had written, I came to realize how the complex of phenomena of his life had created the man I met then and would come to know well thereafter.

[2] These concepts are presented and discussed in *This View of Life*, cited in note 1 above, and in *The Meaning of Evolution* (New Haven: Yale University Press, 1949).

A BIT OF FAMILY HISTORY

It seems to have gone something like this. Until he went to college at the age of sixteen, George spent most of his years with his family in Denver, Colorado. An exceptionally bright youngster but frail and small, he learned to accept being the "eccentric," the "smart kid," and became the unwilling but docile recipient of taunts and jibes from the "tough guys" of the street. His family supported him fully, and their careful handling of his persistent ailments, which were to plague him throughout his life, engendered a love for his father and mother that lasted the full span of their lives.

Strongly Presbyterian, the family attended church three times on Sunday and once mid-week. George became a Church member and was later, with some difficulty, "deconverted." The dogma of formal religion, he explained to me, did not hold up under his questioning, which led in time to personal and social problems. Yet in a contrary way, the same dogma had much to do with developing his ideas regarding the nature of truth and reality—the scientific philosophy that would permeate his scientific work. As he wrote in his autobiography, his discovery of the "rather silly distinction between dogma and reality" was a starting point for intellectual growth.

His school years, interrupted by periods of bad health and intermittent hard times, went by rapidly. The obligatory piano lessons failed, but those on the flute "took." A puckishness and sense of whimsy carried him through the hard times then and throughout his life, bubbling up in his writings when things were good.[3]

[3] This sense of humor came through in many charming ways in his *Letters* (Berkeley: University of California, 1987) and shows up in the offhand comments, vignettes of how he saw himself, and verses which are interspersed throughout his fascinating autobiography. They surfaced now and again as well in his scientific work, as, for

After some time at the University of Colorado (1918–1922), George grasped at an opportunity to attend Yale University. There he studied with Professor Richard Swann Lull, gaining his Ph.D. with the strong support of a superb geology faculty, including such professors as Charles Schuchert and Carl O. Dunbar.

In 1923, while his graduate work was in progress, he married Lydia Pedroja, and from this marriage issued four daughters (Helen, Patricia Gaylord ["Gay," deceased], Joan, and Elizabeth). George loved each dearly, but the marriage ran amuck. During his time of study on the Mesozoic mammals of Europe (1926–1927), Lydia refused even to come to London for a visit. The maintenance of separate residences in southern Europe and London stretched their modest resources, and George worked in London essentially at a poverty level. The whole affair was devastating to him and ended in formal separation in 1930 and divorce in 1938.

The British work completed in 1927, George returned to the United States. As so often seems to have happened to him, frictions (inadvertent or otherwise) had developed at Yale, and he went instead to the American Museum of Natural History. He remained at the Museum, with his star steadily rising, until 1959. It was then to Harvard and the Museum of Comparative Zoology, where, as Alexander Agassiz Professor, he worked with an outstanding group that included Bryan Patterson, Ernst Mayr, and Alfred S. Romer. After this productive but not always happy time (1959–1967) fraught with misunderstandings, he moved to the University of Arizona in Tucson, where he remained contentedly for the rest of his career.

In 1938 George married Anne Roe, whom he had known

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example, in the comedic cartoons he used to illustrate some of his biogeographic essays and in the incomparable, *Attending Marvels: A Patagonian Journal* (New York: MacMillan, 1934).

from childhood. She was a noted psychologist and had a great and good influence on him. They proceeded together through the mazes of academia, lived tranquil summers in their New Mexican summer place, made many contributions to their own fields and—working together—jointly to the fields of biometrics, evolution, and behavior. This gentle little coda to the autobiographical notes George deposited with the National Academy of Sciences in 1975 expresses something of their perfect harmony:

"Now, at 4:30 P.M. on Sunday, this tenth day of August, 1975, I leave my study to go play duets with Anne for half an hour, drink some martinis, discuss plans for a trip to Indonesia, and read a murder mystery before going to bed."[4]

This short passage, which shows so well their quiet love and mutuality, also reveals the George Simpson others too rarely saw.

THE EXPLORATORY SCIENTIST

By this time, in the 1970s, Simpson had been showered with honors, medals, honorary degrees from prestigious universities around the world, and membership in many professional organizations at home and abroad. Among these was the signal honor of election to the National Academy of Sciences in 1941. Though quiet about these honors, I know he was immensely proud of them, for they attested to the general acceptance of his work.

There were—and still are—those who disagree with one or another of his methods or conclusions, or even disparage the results of his work and the "bandwagon" effect engendered by his early successes. From time to time George answered his critics in writing, both published and in letters.

[4] This unpublished autobiographical sketch is now in the archives of the National Academy of Sciences in Washington, D.C., and is available upon request.

Because he was hurt by certain criticisms, feeling a gulf of misunderstanding and animosity, his responses were direct and his comments far from gentle.

George's scientific work formed a pyramid, each field he investigated providing a base for advances toward new horizons. Several short reports and two monographs on Mesozoic mammals, one based on his studies at Yale (1928) and one on those at the British Museum (1929), culminated the first phase of his work. While setting the stage for continuing investigations of primitive mammals, it also led to the evolutionary studies that would occupy the next fifteen years of his life.

The Biogeography of the Americas

Beginning in the late 1920s, Simpson extended his interest in primitive mammals to include early Cenozoic mammals of the western United States and South America. The Crazy Mountain Basin of Montana and the San Juan Basin of New Mexico—with their broad exposures of early Tertiary, mammal-bearing beds—were rich targets for his North American work. Though the collections of the U.S. National Museum (now the National Museum of Natural History) provided a wealth of fossils for study, paleontological tradition demanded that the real heart of the work be in the field.

In the basin regions of Montana and New Mexico, Simpson followed his predecessors—walking endlessly over the dry badlands with their stacked layers of rock, "prospecting" for fossils, collecting specimens, and studying the sediments and depositional patterns of the formations. They all sought to understand the life of those remote times—how the preserving beds had been deposited and the environments in which the ancient plants and animals had lived. These studies continued through much of Simpson's life, and the results are preserved in long series of reports, ranging

from short papers and essays to book-length descriptions of the life of the early Tertiary in the northern hemisphere.

The San Juan Basin charmed George from the time he first visited it in the 1920s. Beginning in 1947 George and Anne spent their summers and one winter in their cabin-home, Los Piñavetes, near Cuba, New Mexico. Only years later, when neither George's nor Anne's health permitted such isolation, did they give up this way of life they both loved. It was at Los Piñavetes that George wrote his contemplative works about the nature of evolution and the meaning of life.

When Simpson started his scientific career, it had long been known that the mammals of South America evolved in "splendid isolation" during the first two thirds of the Cenozoic era. The carnivores were predominantly marsupials; the herbivores were placentals—including some that had remarkable counterparts of northern-hemisphere mammals; and others that were strangely different, such as the great sloths and armadillos, grouped as edentates. Intrigued by the similarities and differences of the evolutionary pathways of mammals of the north and south during this long time of isolation, Simpson undertook field and museum studies in South America parallel to those he had done on the early Tertiary of western North America.

To carry this work out during the financially lean years of the early 1930s he organized and supervised the Scarritt Expeditions, generously supported by Horace Scarritt. From 1930 to 1931 and 1933 to 1934, Simpson and a young colleague, Coleman Williams (with the aid of many Argentinians) spent long hours in museums and collecting in the Patagonian region of Argentina.

There were many problems to be solved. Two overly-enthusiastic Argentinian patriots, Carlos and Florentine Amgeghino, had claimed that material they had found (and mistakenly dated as more ancient than it was) proved that the

ancient beds of South America contained the sources for all mammalian evolution. The evolutionary parallelisms and divergences among mammals evolving separately in South America, North America, and Europe cried out for critical analysis. The biogeographic problems were baffling. The question remained as to why the mixing of the northern and southern faunas began only in the Miocene, first as a trickle and then in successive waves. Then, too, the rodent and primate groups annoyingly did not fit usual concepts of biogeographic arrangement.

Any parallel study of North and South American faunas had to deal with problems of adaptive evolution and differential rates of change in separate regions. It had to address the "mundane" task of taxonomic assignments and more difficult decisions concerning phylogenetic relationships within and between the faunas of the two continents. To this was added the major question of the Americas' biogeographic and continental relationships during the Cenozoic. George attacked these problems with seemingly boundless energy, soon making the leap from the particular to the general to lay the foundation for his most significant theoretical contributions.

The early Tertiary's animal, plant, and land distributions proved particularly puzzling to paleontologists of the early 20th century, who were secure in the "knowledge" that continents were, and always had been, fixed in their positions. The few maverick proponents of continental drift—followers of Alfred Wegener—were generally dismissed as dreamers, and Simpson cast his own biogeographic explanations within a framework of continental fixity. In the 1960s and 1970s, tangible evidence of continental movement was found to support the theory of plate tectonics. This created a revolution in geology and paleontology, and many new, and more meaningful, interpretations emerged.

Yet, within the concept of continental immobility, many

of Simpson's proposals have proved continuously fruitful—such as "island hopping," "rafting," and that Antarctica was a land corridor from South America to Australia. Island biogeography, which has played a seminal role in ecological and evolutionary biology, owes much to his stimulus. When the fixed-continent theory was finally broken, Simpson neither jumped hastily on the bandwagon nor adamantly held to the older view. Studying the emerging evidence, he was able to reject what was inappropriate in his earlier theories and accommodate new information in his explanations.

Theories of Evolution

Simpson's great curiosity about what the fossil record revealed of the ways and means of evolution and his interest in evolutionary ideas formed a backdrop to all his studies. He became a student not only of evolution but of the history of the development of evolutionary ideas as epitomized in his essays on such luminaries as Lamarck, Darwin, and Butler (1941) and his books probing the significance of evolution to humankind: *The Meaning of Evolution* (1949), and *This View of Life* (1964).

When he began his studies, evolution had by no means fully emerged from the anti-Darwinian determinism of the early 1900s or the "Neo-Lamarckism" that espoused the "inheritance of acquired characters." Yet Columbia University's outstanding school of genetics—and, in particular, Theodosius Dobzhansky—had a lasting influence on Simpson, who also greatly admired the studies of J. B. S. Haldane, Ronald Fisher, and Sewall Wright.

Columbia University maintained close ties with the American Museum, and in 1945 Simpson became a professor there. Unlike most of his paleontological colleagues, he recognized the importance of genetics in evolutionary studies. As he had with statistics, probability, and numerous lan-

guages, he set out to master the field. Rumor has it that, abetted by his colleagues at the American Museum, he shut himself up for a year and "learned" genetics—though, as a Texas rancher said to me after I had told him a yarn, "It's a good listening story."

Whatever the truth may be, the results of his studies showed in his *Tempo and Mode,* which, among other things, proved to the satisfaction of many that the data of paleontology and the emerging data from genetics were compatible. Recognizing the strong empirical evidence of the congruency, Simpson took the next crucial step of examining the fossil record for solutions to problems of evolution. He saw there rates of change so slow as to be almost nonexistent and so rapid they produced the appearance of major "gaps" in lineages.

To explain extremely rapid evolutionary change he developed the idea of "Quantum Evolution," combining Wright's theories of genetic drift and passage across "nonadaptive zones" with the general tenets of phyletic evolution. Yet this theory proved not fully satisfactory, and he softened his position in *The Major Features of Evolution* (1953), in which he discussed such problems as the long-term trends in evolution, systematics and classification, equilibrium and disequilibrium, and extinctions.

As an alternative to gradualism, a part of many of Simpson's concepts, Eldridge and Gould suggested their hypothesis of "punctuated equilibria."[5] The ensuing controversy produced various hypotheses regarding selection in populations of organisms, with punctuated equilibria, in particular, developing far beyond its initial formulation. In this way Simpson's provocative work stimulated intense study of the

[5] Niles Eldridge and Stephen Jay Gould, "Punctuated Equilibria: An Alternative to Phyletic Gradualism," in *Models in Paleobiology,* J. M. Thomas, ed. (San Francisco: Schopf, Freeman and Company, 1972).

fossil and living biological record, even down to the molecular and genetic levels, for evidence of ontogenetic events. It is this sort of stimulation that makes so much of his work of signal importance.

Simpson maintained a deep conviction that evolution worked through populations, not individuals, and that quantitative analyses—with probabilistic statements concerning intra-and interpopulations parameters—were indispensable for understanding evolutionary processes. He was, of course, not alone in this, although he was somewhat lonely among paleontologists in the early and mid–1940s. Others working in a variety of fields were espousing similar concepts—among them, Julian Huxley in evolutionary biology, Ronald Fisher in genetics and eugenics, Ernst Mayr in ornithology, C. H. Waddington in genetics and embryology, and G. Ledyard Stebbins in botany—and the Synthetic Theory of Evolution developed rapidly.

Taxonomy

In the course of his empirical work in museums and the field, George became keenly aware that a hierarchical organization of organisms based on evolutionary relationships was necessary to the orderly study of evolution and biogeography. He became a superb systematist, publishing landmark monographs on mammalian classification and theories of systematics. Two of his best known—both the most followed and the most controversial—are *Principles of Classification and the Classification of Mammals* (1945) and *Principles of Animal Classification* (1961). Mammalian classifications have, of course, been revised, by Simpson and by others, and changes are continuing.

It was Simpson's work in taxonomy and systematic theory that gave rise to the greatest controversies. In 1966 an English translation made Willi Hennig's phylogenetic systemat-

ics, originally published in German, more widely known among American and British systematists. Old theories were analyzed, recast, and encoded into what has become known as "cladistics." This new approach to animal and plant distribution, coming of age at a time when the theories of plate tectonics and continental drift were reshaping our ideas of the history of the world, created great conflict among systematic zoologists and vicariance biogeographers.

George's work, as the most lucid exposition of the "old and outmoded" evolutionary systematics, was bitterly attacked in the areas to which he had contributed so much. For the most part he remained silent, although hardly unaware. About a year before his death he casually referred to some members of the institution that had become the U.S. stronghold of cladistics and vicariance biogeography as "the American Museum mafia."

Fieldwork

A crucial aspect of Simpson's career, often overlooked given his extraordinary productivity, was his empiricism: He found his substance in museum collections and in samples he himself gathered in the field.

He was an avid and able field geologist and paleontologist. This time-consuming part of a paleontologist's life is the *sine qua non* of a historian of ancient life depending on the fossil record for his understanding of evolution. Understanding requires walking the rock exposures where fossils come to the surface, digging, collecting and carefully documenting fossils as they appear. Although never completely well, year after year Simpson went where the work was to be done and spent exhausting days and long nights in camps under difficult conditions. Like most true paleontologists, he loved it.

This phase of his work ended only when an unfortunate accident occurred far up the Amazon River in Brazil. Fatal

to anyone less stubborn, it left Simpson with a severely impaired leg and put an end to his field trips. From the journals he kept of his ventures into wild, out-of-the-way places, Simpson produced two fascinating accounts of the life of a field paleontologist: *Attending Marvels: A Patagonian Journal* (1934) and *Splendid Isolation* (1980), an account of the lands, peoples, customs, and development of life in ancient times in South America. His equally extensive studies of other areas of the world, particularly western North America, are mostly found in his technical reports.

HISTORIAN AND EDUCATOR

Many scientists follow narrow pathways of research during their careers, seldom being drawn off into byways by problems that crop up and pique their interest. Others are perennial gadflies, lacking any discernible course. Simpson was neither, but continually sought broader meaning in several main lines of study that remained dominant throughout his life. His restless mind frequently carried him off in other directions—at times into byways that led to productive research.

Penguins, for instance, seem far from primitive mammals, his first love, but their unusual morphologies, behaviors, and distributions posed irresistible evolutionary and social problems that led Simpson to study them intensely over a number of years. Horses, one of the prime keys to evolution in the fossil world (as *Drosophila* has been to neobiology), inevitably became a target of his evolutionary interests. He even went beyond his evolutionary interest to study horses in modern times and as a factor in human history.

George's strong bent for history made him interested in those people whose ideas and deeds had opened new vistas in his own field. One of his first books, *Attending Marvels: A Patagonian Journal* (1934) and one of his last, *Discoverers of the*

Lost World (1984), discuss explorers past and present with a sensitivity and humor that often failed to emerge in personal discussions. In his lucid writing, exemplified in *Discoverers*, he paints pictures in which the founders of South American paleontology come alive in a style found only in the best biographies.

Another direction in Simpson's career that is often overlooked was his role in education. Though more effective in print than in personal interactions, George did have a few graduate students under his direction at Columbia and Harvard and taught classes at this level. His scientific papers and books were used widely in advanced graduate classes and seminars. In 1945–1946, the Department of Anthropology and the Paleozoology Program at The University of Chicago sponsored a year-long seminar centered on his *Tempo and Mode in Evolution.*

Yet his greatest direct influence on education was perhaps his *Life: An Introduction to Biology* (1957), written in conjunction with C. S. Pittendrigh and L. H. Tiffany. Designed for less advanced students, the text was widely used in secondary schools and in beginning college courses.

Aside from Simpson's main interests, little things often stirred his curiosity enough to produce a short essay. While at the American Museum of Natural History, he wrote from time to time for the semi-popular magazine, *Natural History.* A sampling includes such items as "Horses and History" (1936), "How to Misconstruct a Mastodon" (1936), "How dost thou portray the simburh?—Animal Art through the Ages" (1941), and "The Meek Inherit the Earth" (1941). He contributed somewhat more technical pieces on subjects well away from his field to the American Museum's *Novitiates*, including: "Large Pleistocene Felines" (no. 1136, 1936) and "Some Carib Indian Mammal Names" (no. 1114, 1941). These short trips into strange areas, declining somewhat

after the mid-1950s, continued throughout his life, often in a more philosophical vein (as reflected in his collected essays, *This View of Life* [1965], and the many ruminitive digressions sprinkled throughout his autobiography, *A Concession to the Improbable* [1978]).

THE SOCIETY OF VERTEBRATE PALEONTOLOGY

The Society of Vertebrate Paleontology came into being informally in 1940 through the efforts of George Simpson and Alfred S. Romer and was very important to both of them. With some help from others, it was fledged and nurtured in a meeting—technically a session of the Vertebrate Section of the Paleontological Society—at the Museum of Comparative Zoology at Harvard. The first formal meeting was in 1941, at which time permanent officers were nominated and later elected. There has been some confusion about who did what, what is fiction and what fact. Feelings were hurt, but whatever the truth, a small scientific society emerged and has thrived and grown ever since.

For a number of years its original purpose, to serve as an informal forum for the exchange of ideas without formal papers or a scientific journal, was served by annual gatherings of thirty to forty persons. The open meetings, freedom of discussion, and evening "bull sessions" over a toddy or two were grand, and news of activities was carried (as it is today) in a *News Bulletin*. But, as size increased, this could not last. Limited time eventually led to a format of formal papers with restricted discussions. Evenings, however, have remained informal, and after a smoker and a banquet, the Society's sessions generally revert to the older, more boisterous times.

George and Al Romer missed the older days; George began attending fewer meetings, while Al remained to fight change. All of us "old timers" rued the passing and were staunch in our gratitude to the founders who had had the

foresight to see the need for cementing vertebrate paleontologists into a coherent group. Today the Society of Vertebrate Paleontology has spread around the world, the *News Bulletin* carries items from foreign regions, and scientific articles appear in the Society journal.

Field conferences in the summer, usually with a relatively limited participation, were affairs to be remembered, and as usual George was at the heart of many of them. Evenings and sometimes all night "off-hours meetings" have been described as boisterous by those who are inclined to be mild. Sometimes they were, in fact, rather wild (of course in a gentlemanly and gentlewomanly way).

One of these trips was held in the Permian and Paleocene of New Mexico and for a night we stayed at the beloved summer home of Anne and George, Las Piñavetes. The creek was the water supply for a nearby village and George, eyeing the few children in the party, announced sternly over the P.A. system mounted on a truck, "Do not urinate in the creek!" "The pompous ass!" someone near me said in disgust, for George had managed to turn his real concern for the villagers into a reprimand.

As evening came on, the tensions of the days waned. Tents, cots, and blankets came out and bottles appeared like magic. These field conferences brought us together and became the grist for anecdotes for years to come.

FRIENDS AND COLLEAGUES

George Simpson was acquainted with colleagues in the physical and biological sciences around the world. He was royally received wherever he went and consistently acclaimed for his scientific acumen and accomplishments. Too often, as he himself said, he was asked to give lectures—something he disliked and was only moderately good at. For all of his interchanges with scientists in many countries, his work re-

mained very largely his own, influenced by the cultures he visited in only the subtlest ways. Among his some 500 publications (not counting reviews), only a few were coauthored and these mostly books. Notable were his joint publications with Anne Roe, *Quantitative Zoology* (1939) and *Behavior and Evolution* (1958).

His insights into evolutionary processes, of course, benefitted through interchanges with his intellectual associates—among them Walter Granger, W. K. Gregory, W. D. Matthews, Theodosius Dobzhansky, Ernst Mayr, and Glenn Jepsen. But he depended more on books and the fossil record than on conversation, and the written word was his favorite medium of communication.

Theodosius Dobzhansky was not only a respected and inspirational colleague of Simpson but a revered friend, both during the years at Columbia and later, when their paths diverged. In my judgment his two closest friends among paleontologists were Bryan Patterson, at Harvard in his later years, and Paul O. McGrew, of the University of Wyoming. Both, like George, were students of fossil mammals.

Bryan was a free-spirited man who knew well the blythe spirit that underlay Simpson's public surface reserve. The two could tangle over serious matters in their scientific work but disagreed only about trivia in their friendship. Bryan, for example, persisted in calling him "G. G.," which George did not like, though I doubt he ever told "Pat" that it irked him.

Paul McGrew was a hardy extrovert, serious in his science but enthusiastically devoted to having a good time, whether at Jack-Straws or climbing mountains. Magnificently complementary, he and George were very fond of each other.

My relationship to George was somewhat different, for our paths crossed in science only in broader theoretical areas and we never worked together for any extended time. Yet my dealings with him were probably not atypical and are perhaps worth a bit of space.

In the informal autobiographical sketch he submitted to the Academy,[6] George wrote of Will Rogers' famous "I never met a man I didn't like":

> "I do not believe that I have ever truly hated anyone, but I have despised some and disliked more. I have truly loved a few and liked an enormous number."

So he saw himself. My own experiences with him over the years lead me to temper this a bit, for our relationship vacillated between these categories—never quite reaching "despised" nor, perhaps, "loved," but teeter-tottering somewhere in between. Off and on, it went something like this.

Soon after I received an aggrieved, excoriating letter from George about a remark I had made (recorded on a garbled tape of a session on the "Emergence of Synthetic Theory of Evolution," sponsored by Ernst Mayr), I was asked to give a talk about George along with one by Stephen Jay Gould at a medal presentation. I think George had misunderstood something I said about philosophy and soft science. I accepted the invitation but then had the thought that this might be offensive. I called George to ask if he did or did not want me to give the talk. "But Ole," came back the astonished answer, "you are one of my dearest friends!" I gave the talk.

IN CONCLUSION

This was the George Simpson I had come to know since 1935. I admired his quiet wit, his sharp mind and penetrating intelligence, and I was often a bit awed in his presence. Yet at times awe would melt into affection, as when Anne and George, with just a few hours stopover between trains in Chicago, would call and say "How about coming down to the Palmer House bar for a drink or two?" My wife and I often

[6] See note 4 above.

did. At such times with martinis and black coffee, barriers disappeared and there were no closer friends.

In this informal memoir to George Simpson, I have striven to show that, under his greatness and richly deserved position of honor was a very true friend—sometimes shy and sensitive, and sometimes directly cutting. His difficult childhood, bad health, and rapid education broken by long absences perhaps benefitted him later by saving him from the desire to cloak himself in classroom orthodoxy. His on-and-off church experiences, his chance to go to Yale, his first marriage and divorce, his happy marriage to Anne, his four daughters, and the "mishaps" that brought him to the American Museum are all part of what Simpson came to be.

Certainly none of the "greats" of their time are easy to know. Barrages of criticism (too often by critics who have misunderstood a cherished idea) may dim the subject's luster, but they also serve to validate stature—lesser lights do not become "whipping posts." The greats' susceptibility to anecdotes further shapes their public image, for stories repeated and "improved" as they go too often explain much and distort more. I have added my voice to the storytellers, not only to speak of the greatness, but also to show something of the fallible man behind the mask.

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