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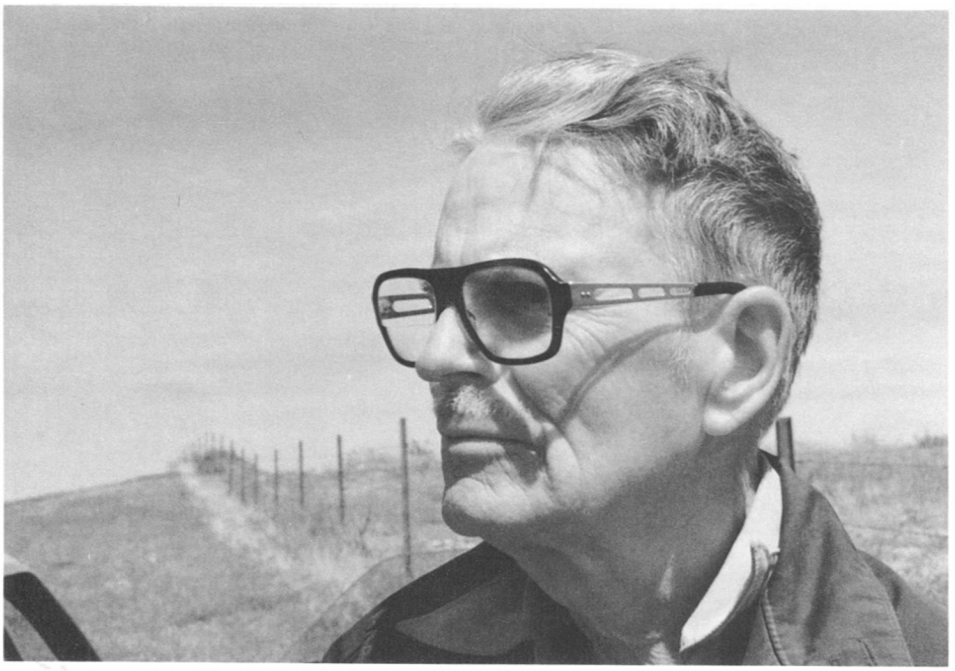
MEMORIAL

BOTH SIDES OF THE MEDAL: A MEMORIAL E. C. OLSON (1910–1993)

Everett Claire Olson, leading scholar, teacher, and enthusiastic supporter of vertebrate paleontology, died November 26, 1993, at the age of 84, in Los Angeles, California. For nearly sixty years, first at The University of Chicago and later at the University of California at Los Angeles, he pursued the study of extinct terrestrial vertebrates with unflagging energy and inspiring intensity. Olson's work was pervaded by his profound appreciation of the eclectic nature of vertebrate paleontology, an appreciation that provided his students with an education of extraordinary breadth, depth, and freedom. Despite the passionate intensity that he brought to his study, his style was always informal, ranging from breezy and irreverent to downright combative, and never did he let his fundamental seriousness obtrude on his personal informality. He was known as "Shorty" by friends and colleagues who had known him before WW II, and as "Ole" by those (including most of his students) who had not made his acquaintance until after the war. How he came to enter his chosen field, and what its consequences were, are set forth in lively detail in his memoirs (Olson, 1990), and his own words, set off by quotes, appear in the following where appropriate.

Ole was born November 6, 1910, in Waupaca, Wisconsin, to Claire Myron Olson and Aimee Hicks Olson, and grew up in Hinsdale, Illinois, a small outer suburb of Chicago where his father practiced dentistry. The family was comfortably situated, and Ole's first interest in science was biological, beginning at about the age of 5 with the collection of beetles, plants, and "anything else that piqued by my curiosity," but especially butterflies and their larvae. His interest in butterflies grew with the years before college, pursued no doubt with the same intensity that he later devoted to Permian tetrapods. It culminated in a trip with a boyhood friend to Florida in a Model T at the age of 16—as he says (in a different context but equally applicable to this one), "The older people were tolerant." In his youthful exposure to sports and music he was more successful in the former than the latter, to hear him tell it, but the jazz piano, e.g., "Little Coquette," of his more mature years was well worth hearing.

Ole came rather late to the field of vertebrate paleontology. He entered The University of Chicago (on an athletic scholarship) with the intent of studying chemistry, but was soon seduced into geology by the dynamic teaching of J Harlen Bretz. From there he moved on to a master's in invertebrate paleo, perhaps because of his early love for matters biological, but it was not until his third graduate year that he settled down to study Permian tetrapods under the tutelage of A. S. Romer. During those



Everett Claire Olson
(1910–1993)

years the country was beginning to suffer hard times, and Ole has reflected that ". . . any thoughts . . . about the commercial value of a degree vanished. This esoteric study of extinct animals . . . was education for education's sake, which is not all bad." Although he sounds a little apologetic about it in this passage, Ole was always a firm believer in the value of education for its own sake, tempered by the knowledge that for most of us as for himself it was accompanied by the need to make a living.

With the departure of Romer for Harvard, Ole moved from student to faculty status at Chicago. This shift was not without its lumps, but within his first few years on the faculty (before WW II) he had laid the groundwork for the major accomplishments of his later career. South African fossils collected by Romer and Paul Miller, curator of Walker Museum, focused his thinking on the relationships between the New World Lower Permian and the Old World Upper Permian. In consequence he embarked on an ambitious program of field research which he pursued until after he retired. His emphasis on field work stemmed from his conviction that, although the Texas Permian had been hunted over for fifty years, there were still new biological data to be recovered. This conviction was vindicated in the years immediately after WW II, when in the supposedly barren upper parts of the Permian section he found fossils that were more nearly similar to those of the Old World than to those of the New. He also pushed field work to accumulate physical data on the circumstances of burial of the fossils, for at the time there was little consensus on the environments in which Permian tetrapods had lived. Besides, field work was fun.

In these and subsequent adventures, Ole was frequently accompanied by his wife, Lila Baker Olson, to whom he was married in 1939 and with whom he raised three children, Claire, George, and Mary Ellen. The presence of small children frequently complicates a couple's ability to get on with field work, but there is a story of a unique way that the Olsons once resolved this problem. They used a nearby dried-up buffalo wallow as an oversize playpen, where their toddler could be watched and be entertained by the local flora and fauna (shades of the Old Man), and yet could not wander away. Many years later, after Ole's formal retirement, he took up once again his early love of Lepidoptera, and he and Lila spent a long and happy footloose period chasing butterflies both at home and abroad. It was during this period that they also found time to visit the field activities of such of Ole's ex-graduate students as were still carrying on the tradition.

Based on his studies of the anatomy, systematics, and distribution of Permian tetrapods, which resulted in the publication of more than 100 scientific papers and books, Olson pioneered in three lines of inquiry of a more general nature. First, he applied innovative statistical procedures to the analysis of changes in structure and function during growth in poikilothermal tetrapods. This was a bold step, for these animals, which make up the great bulk of the tetrapod fossil record, do not stop growing at a certain point as mammals and birds do, and for most of them the initial stages are unknown. "Morphological Integration" (1957), written with Ole's former graduate student the late Robert L. Miller, is functional morphometrics before computers were available to do the spadework. Second, in a long series of papers Ole reported on the origins of terrestrial communities in which tetrapods played a significant part, and how such communities evolved during the transition from Early to Late Permian. The term "chronofauna," which Ole says was coined by his friend and colleague Bryan Patterson, passed into common use from this period of Ole's work. Third, he opened up details of the later Permian tetrapod faunas of Russia, together with the philosophical approaches to their study by Russian experts, to the western scientific community. This includes I. A. Efremov's concept of taphonomy, the study of the circumstances that affect the body of an organism from its death until it is collected as a fossil, a study which in the last 20 years has become a subdiscipline in its own right.

In his memoirs Ole cites people who had profound influence on his thinking and research, notably Texas cowboys and a Russian colleague, Ivan Antonovich Efremov. The cowboys, two in particular, Ernest Crewthirds and Wade Barker, of the Waggoner Ranch of Vernon, Texas, he got acquainted with very early in his career. In addition to teaching Ole how things were done in that part of Texas, these men earned his admiration for their modest style, their total competence in a wide variety of circumstances, and their wise restraint in the utilization of their environment. Efremov influenced Ole's thinking through their common interest in early Late Permian tetrapods, taphonomy and field work, and philosophical bases of the study of paleontology. Efremov and Ole also shared a serious interest in science fiction, which Efremov wrote. Olson's memoirs express clearly his belief that Efremov was a true original, someone from whom Western scientific thought can gain a great deal.

Ole also credits graduate students (some 37 of them in the course of his career) with continuing from beginning to end to bring new intellectual approaches to his attention. In the late 1940's when the department was full of ex-GI's aspiring to higher things, Ole told a group of his graduates, "When I get rid of you guys I'm going to have one graduate student every four years." It never happened—he could not resist the challenge and stimulation of a fresh mind.

In addition to his lifelong commitment to research and teaching, Ole also found time to support his profession in administrative and other ancillary activities. At The University of Chicago he served as associate dean of Physical Sciences from 1954 to 1959 and as chairman of the Department of Geology from 1957 to 1961, and at UCLA as chairman of the Department of Zoology from 1970 to 1972. During his tenure at Chicago Ole was instrumental in resolving the historic administrative problem posed by the interrelationships of paleontology, biology, and geology (Rainger, 1993). He was a charter member of the Society of Vertebrate Paleontology, and its president in 1949–1950. As a member of the Development Committee of SVP from 1986 until his death, his contribution was vital to the enormous success enjoyed by that body in establishing a permanent endowment fund. He served as editor of *Evolution* from 1953 to 1958, and of the *Journal of Geology* from 1962 to 1967. Other learned societies in which he was active include American Geological Institute (board of directors 1948–1949), National Academy of Sciences, Ecological Society of America, AAAS, American Society of Systematic Zoology (president 1979), Geological Society of America, Society for the Study of Evolution (president 1964), American Society of Zoologists, Phi Beta Kappa, and Sigma Xi. He belonged to the social fraternity Phi Kappa Psi.

Ole started with a global view of terrestrial environments of the Permian, and pursued it tirelessly in his field demonstration of faunal similarities between the later part of the American Permian and the Russian Permian. This work tied American, Russian, and South African faunas into a worldwide picture of Permian terrestrial biotas, and its success confirmed Olson's high stature as a vertebrate paleontologist and whole-animal biologist. He will be sorely missed, but the memory of his enthusiasm and accomplishment will continue to inspire paleontologists and other students of evolution on both sides of the water.

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REPLY

COMMENT ON: LACK OF A HIGH BODY COUNT AT THE K–T BOUNDARY

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THIS IS in response to the Comment by Hunter on page 1158 of this issue. Hunter has obviously given considerable thought to the matter and presents several interesting possibilities I had not considered, including a short-term, climate-induced drop in reproductive rates. I agree that such a scenario would result in decreased abundance, but if it played out over a paleontological scale, it would presumably result in a gradual dribbling out above the impact layer.

The accumulation of bone in fluvial channels by lateral migration across the floodplain results in time-averaged deposits which summarize sparsely distributed attritional remains accumulated in the floodplain through time. Such deposits are obvious information destroyers in the sense that they destroy upsection trends, and Hunter is no doubt right in suggesting that such deposits could mask a mass kill. On the other hand, time-averaged deposits are information providers when it comes to census taking. Had there been no change in diversity and a mass kill at the boundary, the stratigraphically higher streams cutting down into the Cretaceous should encounter the same fauna lower ones do.

By and large, I believe that turnover rates are likely to have been rather low in dinosaurs, at least the large ones, and that the taphonomic filter was rather broadly open in both the Judith River and Hell Creek Formations. Since neither is in fact known, I will have to concede another of Hunter's main points. If turnover rates were high, if we had a very tight filter—passing very few remains—or a combination of both, the increase in mortality rates caused by a catastrophic mass extinction might not be enough to produce a readily seen increase at the outcrop.

The difficulty in testing Alvarez's argument is not in framing an adequate test, but in framing a reasonable test that it passes. If an increased body count is not the test of a catastrophic mass extinction, what would be? At the very least one would expect the normal distribution to carry through to the bitter end. It is important to note that while the 2–3-m barren zone is virtually ubiquitous, the normal distribution does not everywhere carry to it (as suggested by the two quotes from Archibald, cited on pages 187 and 189 of my original article).

Hunter is right in suggesting yet another reason why the predicted increase in remains might not occur, but I think this leaves us where we were before. If Alvarez's argument is not falsifiable, upon what other basis are we to judge it, other than the burden of proof?

ANNOUNCEMENT

Dear Colleague:

You are invited to attend the North American Paleontological Convention-VI at the Smithsonian Institution in Washington, D.C., on 9–12 June 1996. The organizing committee welcomes constructive comments on previous NAPC's as well as suggestions for symposia topics and/or format for 1996.

Sincerely yours,
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