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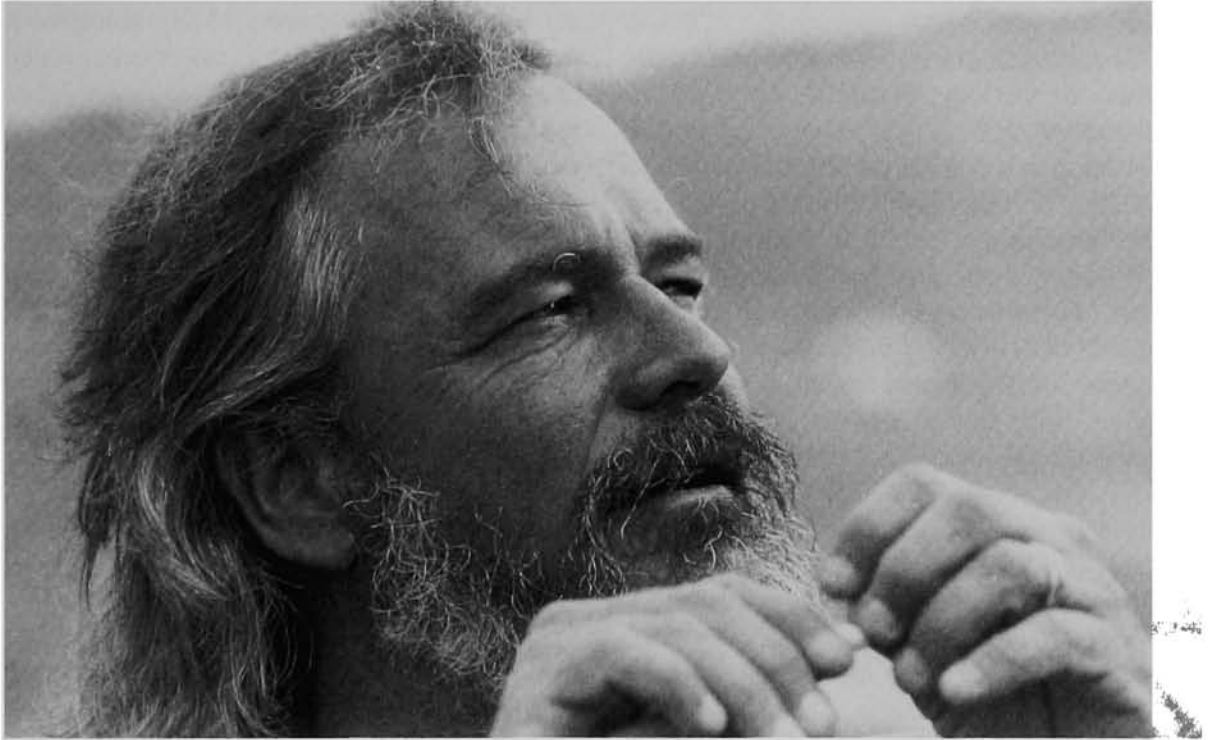
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## Christopher Raven (1943 - 1994): A Personal Appreciation



Christopher Raven, 1993 (courtesy of Intermountain Research).

**CHRISTOPHER RAVEN** died at Lagomarsino Canyon, east of Reno, on March 12, 1994, just five months past his fiftieth birthday. We were nearly the same age and had been good friends and professional associates for more than half our lives. He was an arresting figure: darkly handsome, unusually articulate, knowledgeable in music, art, literature, and film. He loved women, deserts, hot springs, travel, good food, good talk and good writing. Originally trained as a Mesoamericanist, he later made significant contributions to the archaeology of California and the Great Basin. Here I record some of what I know of his life and work.

Raven was born Christopher Robert Corson, the only child of Kenneth and Roberta Corson, in North Hollywood, California, on

October 27, 1943. The family moved to Tahoe when he was about 11. His father's sudden death five years later marked him deeply, making him reluctant to form close relationships but strongly committed to those few he allowed to develop.

He graduated from South Shore (Tahoe) High School in 1962 and went on to undergraduate school at Berkeley. He had originally intended to major in music but dropped the idea when, by his own account, his performance skills proved insufficient. Anthropology was a suitably romantic and exciting substitute. He completed his degree in 1966 with an honor's thesis on Tzeltal love poetry.

That fall, he entered the Berkeley graduate program, joining the mixed cohort of Great

Basin/Mesoamericanists then working with Bob Heizer and John Graham. Richard Ambro, Billy Clewlow, Richard Cowan, and I were already part of it; Lew Napton, Butch Hallinan, Tom Hester, Tom Wilson, Bill Wihr, and Karen Nissen came later.

As a student, Raven read widely in Mesoamerican archaeology, cultural ecology and the history of archaeology. Culture history was the prevailing archaeological paradigm at Berkeley; the "New Archaeology" then burgeoning at places like Chicago, Michigan, Santa Barbara, and UCLA was scarcely mentioned in formal courses, except among the Africanists under Desmond Clark and Glynn Isaac, with whom most New World students had little to do, even socially. Still, anyone who read the journals could see the field was changing: we all immersed ourselves in Lee, Flannery, Sanders, and Price, and even Binford himself. Raven's exposure to the processual approach was further broadened in tutorials with Robert Rodden, a Cambridge-trained specialist in the European Neolithic who introduced him to the work of Eric Higgs, David Clarke, and the British cultural geographers.

Opportunities for fieldwork in Mesoamerica were limited for those in Raven's age set. Heizer and Graham organized two trips in the late 1960s; Raven went along on the second, winter quarter 1968. The main objective was to remap the massive Olmec site of La Venta. Extensive clearing in connection with the construction of a nearby petrochemical complex had shown the site to be much larger than previously thought. As it turned out, Napton handled most of the mapping. The rest of us were teamed with pairs of local workers who dug two by two meter pits through mound fill while the assigned student stood at the top of the backdirt, like Flannery's "Real Mesoamerican Archaeologist," combing the pile with a hand rake, looking for keepers. On returning to Berkeley, we divided the data and wrote descriptive

reports. Raven and Clewlow drew the sculptures; their paper was Raven's first publication.

At the time, Heizer and his students also had an active field program in Basin archaeology. Raven was encouraged to take part and did so, mainly in Surprise Valley along with Ambro, myself, and a number of others, mostly undergraduates from Berkeley and Santa Cruz. He spent three seasons there, 1967-69. They were good years. Heizer gave us about \$800 for each ten-week season, enough to keep the trucks fueled and feed the four to six person crews we usually had with us. Most of the time we lived roughly, high in the Warner Mountains. Nights were cold; water left out in a bucket often froze, even in July. We drank cheap red wine by the gallon in the evenings and regretted it every morning. Apart from that, there was nothing to do but work hard at archaeology and enjoy the country. We did plenty of both.

Around the end of his time in Surprise Valley, Raven took and passed his Ph.D. qualifying exams and began work on his dissertation. Like Clewlow and Ambro before him, he nominated an exercise in stylistic analysis based on existing collections, something he could do largely on his own with a little money for travel. He picked the Jaina material, a corpus of some 500 figurines found in Classic burials at several sites on the Campeche coast, including the island of Jaina itself. Some of these pieces are exquisite: beautifully modeled, dynamic, brightly tinted, their faces executed with portrait-like attention to detail. With the help of Ignacio Bernal, Don Leonard, and Carmen Cook, and financial support from NSF, Raven visited collections in the United States and Mexico, identified significant patterns, and analyzed their connections with other aspects of Maya art and iconography. He enjoyed the project, largely for the opportunity to read more deeply in Maya ethnohistory, but found its results disappointing. The limits of the paradigm in which

he had just been educated had become apparent.

After finishing his degree, he taught for a while at Berkeley and through the UC Extension in San Francisco. He applied everywhere for tenure-track jobs, taking the obligatory trips to AAA meetings, but had no success. Then as now, the market was poor and the search disheartening. Soon he began to find teaching itself unrewarding. In 1976, he was offered the position of Bureau of Land Management District Archaeologist, based in Susanville. It looked perfect: professional employment, outside the academy, close to then wife Karen's family in Cedarville, and, best of all, in a part of the world he had come to love—the arid mountains and great white playas of the northern Basin. He jumped at it.

He spent six years on the job. His c.v. lists 14 surveys and clearances, a monograph on the history of Fort Bidwell, and a few other popular pieces. It makes no mention of his other contributions, including strong support for student research in local archaeology and ethnography (notably Melinda Leach's UCLA Ph.D.), long-running battles with livestock operators over the destruction of archaeological sites near springs and stock tanks (highlighted by a public confrontation with a local-born, "distinguished elder" archaeologist, based at an eastern university but hired by the ranchers to protect their interests in court—Raven won), and his critical role in having the archaeologically important High Rock Canyon region of northern Nevada formally designated an "area of critical environmental concern" and as such protected from disruptive development. He was quietly but quite rightly proud of these accomplishments.

Raven left the BLM in 1982 and worked for a time "on the road," running big projects in the northern Sacramento Valley for INFOTEC and Wirth Environmental, writing the chapter on northeastern California for Mike Moratto's *California Archaeology*, and helping Bob Bet-

tinger with high altitude survey and excavation in the White Mountains. Sometime in 1984-85, he took on the job of Research Director for Emma Lou Davis's Great Basin Foundation. He did several interesting studies in that role, including a technically innovative piece on rock art near Bishop and a research design for the Panamint Valley. A key element of the latter was a plan to map large scale rock alignments from hot-air balloons. Some years later, with the assistance of Ken Juell and Earthwatch, he actually carried it out.

He spent much of 1985-86 on Boigu, in the northern Torres Strait Islands, helping then wife Shelly with her dissertation research on dugong and turtle hunters. It was his first extended experience in ethnography. Though he never published anything of his own on the project, it contributed to a sharp change in his approach to archaeology, strongly reinforcing his concern with the importance of predictive models of human behavior and its archaeological consequences.

On returning to the States early in 1987, he discovered that the Great Basin Foundation—or at least his part of it—had evaporated. He soon found new work with Cashion Callaway and Bob Elston's Intermountain Research (IMR) group, based near Carson City. It was the beginning of the most intellectually productive period of his life. He played major roles in two highly innovative projects: the first, a multi-year investigation of the massive Tosawihi Quarry north of Battle Mountain; the second, an exercise in settlement pattern modeling at Stillwater Marsh, near Fallon. He was engaged in an elaboration of the latter at the time of his death.

Reports of these projects, particularly those on Stillwater, include some of Raven's best work as an archaeologist, and are in some sense the product of one of his most persistent research interests. He was first introduced to settlement pattern studies as a graduate student.

He read Gordon Willey's work on the Viru Valley and Belize, Bill Sanders' on the Valley of Mexico, and Chorley and Haggett's on locational analysis; and actually drafted a proposal to NSF for a regional study of Classic sites in southwestern Yucatan, only to have it rejected, as he read it, because it called for five years of fieldwork to be accomplished in five months.

His work on hunter-gatherer settlement studies had a longer history and a better payoff. His first experience was in Surprise Valley. At the time we began that project, there were very few archaeological studies of hunter-gatherer settlement patterns in print. Julian Steward's 1938 Great Basin ethnography led us to expect variation in site location and assemblage composition relative to resource distribution, but gave no guidance as to the specific patterns we might expect in the northwestern Basin, where the resource base was quite different from any of those he described to the south and east. Binford's 1964 article, "A Consideration of Archaeological Research Design," was much more useful. In it, he outlined a regional sampling scheme that promised to help us identify whatever patterns in site location and assemblage composition were actually present, regardless of how they might be interpreted. It seemed a perfect recipe for Surprise Valley. (We were not the only ones so inspired: David Hurst Thomas put Binford's proposal in practice in Reese River Valley at about the same time; Margaret Lyneis had initiated a similar program a year or two earlier in Warner Valley.)

Taking Binford's lead, Raven and I proposed to spend the 1969 season sampling a 12 km. wide, 30 km. long transect of the southern end of the valley, designed to identify relationships between characteristics of archaeological site location and the composition of associated lithic assemblages. Heizer rejected the plan outright: it was too much work, and besides, as he put it, "A healthy person could walk across that valley in a day." We took this

to mean that local plant communities were so closely spaced that site location and assemblage composition might not be good indicators of resource use.

In the end, I spent most of the summer digging at King's Dog, while Raven and fellow graduate student Jack Pritchett ran a much simplified version of the survey. They recorded about 30 sites and found close relationships between vegetation and lithic assemblage composition. Heizer's skepticism notwithstanding, we still thought these relationships reflected resource use. The questions were: which resources, when, how, and by whom? Many different answers were possible. Unlike Thomas's situation in the central Basin, the local ethnography provided no basis for predictions about resource use and little guidance in interpreting the archaeological evidence thereof. With help from Raven and Rodden, I ultimately developed a settlement model that accounted for the data; but it was neither well-warranted theoretically, the only interpretation imaginable, nor readily testable. We should have been able to do better.

Raven subsequently took two important steps in that direction. The first involved an appeal to "range type" maps, developed by the Department of Agriculture to reconstruct climax vegetation from analysis of soils and other site characteristics. In an unpublished paper presented at the annual meetings of the Society for California Archaeology in 1977, Raven used these maps to develop hypotheses about the character and distribution of subsistence resources potentially available to hunter-gatherers, particularly plant foods. As he indicated, this approach should be especially useful in those parts of the Basin most heavily affected by agricultural or pastoral development.

The second step involved gaining an education in the use of formal optimality models to predict actual patterns of human resource choice. Fieldwork with Shelly on Boigu helped

him appreciate the rewards and difficulties of applying such models ethnographically. Joining IMR brought him into contact with people like Ken Juell, Troy Tucker, Dave Zeanah, Kris Carembalas, and, of course, Elston himself, all of whom were familiar with the models, understood their potential as frameworks for archaeological research, and were actively attempting to apply them.

Raven's contribution to this process came in two parts. At Tosawihi, he helped devise theoretically-driven predictions about season(s) of use, toolstone extraction and processing, residential site location, and subsistence, personally collecting many of the archaeological and other data needed to operationalize and test them. Many of these predictions were matched by patterns in the archaeology; even for those that were not, the nature of the mismatch was often informative. At Stillwater, a project on which he played the leading role, he used range type maps to reconstruct late prehistoric resource distribution, appealed to available experimental and ethnographic data to estimate resource utilities, and on that basis made predictions about land use and its archaeological consequences over a 1,000 km.<sup>2</sup> area. A partial test showed that actual patterns of site distribution and assemblage composition were largely consistent with model predictions.

This work is important because it directly addresses the problem we encountered 20 years earlier in Surprise: it provides a basis for developing well-warranted, archaeologically testable predictions about past human behavior. In doing so, it also helps explain variability in that behavior in general terms. In principle, the approach Raven and his associates pioneered in these two studies can be pursued anywhere. It represents an important contribution to the difficult process of turning archaeology into a true social science.

There was, of course, much more to Raven than his research, even though he made it dif-

ficult to know. He was a very private person; as a close friend once put it, "the man at the edge of the frame, just slightly, but quite deliberately, out of focus." Many people found him arrogant and aloof. They were not completely wrong. His friends preferred to see him as mysteriously distant and charmingly eccentric.

One of his most striking characteristics was his domesticity. From the time he entered undergraduate school until the day of his death, he was never without a housemate for more than a few months. He was married four times. His first wife, Kristin, was a high school sweetheart. They were together about five years. The second was Karen Hironymous, an accomplished organizer, administrator, and editor, whom he met in Surprise. She supported him through graduate school, made him a home, and gave him a son, Matthew Sebastian. The third was Michelle (Shelly) Cross, who worked as an archaeologist, ethnographer, and resource person for various government agencies and Native American communities in Modoc and Lassen counties in the late 1970s. He found her so irresistibly attractive that he left the security of job and home, married her, and adopted the surname Raven to mark the change. When she left, after ten years of marriage, he was devastated. He married again just a few months before he died. The new bride was ethnobotanist/archaeologist Julia Hammett, whom he met when she joined the IMR staff.

Raven's friends will remember many other things about him as well, some wonderful, some not: his knowledge of music and film (there was always a piano in his house, always something by Wagner somewhere in sight); his sensitivity and skill as an artist (the figure studies of Maya women he had met in the field); the precise, mannered rhythm of his speech (many will recall the time he ran long in a presentation at the Basin conference: when the chairman scurried up to cut him off, people shouted out from the dark, "Let him finish!"); his occa-

sionally spectacular social flourishes (like the time he placed a perfect, long-stemmed red rose atop a stack of contract reports he delivered by push cart to an official at Caltrans); his propensity for gently but shamelessly “shining on” the unsuspecting (he often assured beginning graduate students that Herbert Spencer’s greatest contribution to humanity had been the invention of the paper clip); his unwillingness to suffer fools gladly (marked in later years by a propensity to bark sarcastically at utterances he thought particularly stupid).

For me, one of the great treats was reading Raven’s prose. He wrote voluminously, maintaining active, intense, often quite intimate literary relationships with people he cared for. Most of his letters were handwritten, uniformly bled to the margins, with seldom a cross-out or correction. The style was rich, mellifluous, and full of arcania; the topical coverage staggering. My own files include such entries as a discourse on the role of calligraphy in reconstructing Bach’s musical chronology, a report on the rigors of root canal surgery in Merida without benefit of anesthetic, a review of the still unmade film “Hadza Grandmothers in the Mist,” and an insightful essay on the topic “Why pornography’s not what it used to be.”

Many of these pieces are real gems: wry, funny, sharply evocative. An example, dated October 30, 1992:

Shelly gave me a birthday on Boigu; she invited all our Papuan neighbors. In a friend’s hut, she managed to cook a birthday cake, frosted green and contrived in the shape of a green sea turtle. The Papuans, to understate, were amazed. Old Mrs. Wuruki, who had come down the coast from Sigibadura, was going to perform a birthday dance for me. Someone noticed, however, just after the cake and before the dance was to begin, a light moving in the cemetery, a few hundred meters away. Everybody decided it must be a ghost, so they all ran home and closed their doors. So did we. Best birthday ever.

Another, from Boigu itself, dated November 4, 1986:

I could not have believed, those late afternoons in Graham’s seminar (with the sun gone down over the Bay and JG checking his watch from time to time, momentous engagements obviously more on his mind) while Richard [Ambro] and Billy and I (and later, Tom Wilson) competed for approbation for the most perfect recall of the Uaxactun sequence—Mamon, Chicanel, Matzanel, Tzakol, Tepeu—and debated with unfeigned earnestness whether Ledyard Smith or William Coe was the better field technician, and joking afterwards that Graham’s epigraphic myopia blinded him to the manifold excellences of one or another methodological innovation that promised to tell us what we *really* wanted to know about the Maya and in pursuit of which we had decided to study anthropology in the first place, that sometime I should find myself living among displaced Papuans in a corrugated iron shack on a swampy island in Torres Strait, watching the thunderheads conspire toward another, interminable monsoon. Clearly, I had not read enough Joseph Conrad.

The other thing I remember about Raven was his generosity. He spent three seasons with me in Surprise, learning his craft, but also helping to make my dissertation possible. He was a steady hand, good to have around in difficult times, and around whom good things happened. One more story. In 1968, he and I stayed late in the field, cleaning up after the rest of the crew had gone home. The main task was backfilling a five-by-ten-foot wide, eight-foot deep pit, by hand. The day we picked to do it, a freak storm blew up, complete with high wind and a heavy rain that soon turned to driving sleet. The site was in the open on the valley floor. The only shelter, an old, partly collapsed homesteader’s cabin, was too far away to do us much good. We were miserable and the work went very slowly. Two hours into it, a fellow from town fishtailed up through the mud in an old jeep. Karen had sent us a thermos of coffee and a pint of bourbon. We retreated to the

shack and polished off both. By the time we were done, the storm had lifted. I can't recall backfilling the rest of the pit, but I know we both felt a lot better doing it.

I don't know why he killed himself. Twenty years or more of serious drinking, the hard effort over his last two at drying out, the breakup of his marriage to Shelly, his fear of losing his creativity, his regret about leaving Karen and Matthew, the distant but still painful memory of his father's suicide—all these things and maybe some others figured into it in ways that the love of his friends and even that of his new wife, Julia, could not offset. Whatever the reasons, I'll always remember him as a great companion and colleague. I miss him. I know others do, too.

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