

PIONEERS

*This is the second installment of *Pioneers*, a section of the Journal of California and Great Basin Anthropology devoted to personal reflections by students and colleagues on major figures in the study of the indigenous cultures of the region. These are not obituaries or memorials, but candid recollections that convey insight into the personalities of the pioneers, as well as the cultural context of anthropology during their lives. If you have suggestions for a pioneer, and names and contact information for those who may wish to prepare a recollection, please contact the editor.*

MEMORIES OF ISABEL T. KELLY (1906–1982) AND JULIAN H. STEWARD (1902–1972)

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In this issue, we step back far enough in time to outstrip living memory, but do so to symbolize a period when Great Basin anthropology took the form that would make the region internationally known for both ethnographic richness and for theoretical contributions to ethnology. The works of Isabel T. Kelly and Julian H. Steward are known to all who work in the region, and here we remember them as examples of an earlier time in Great Basin anthropology. We have the good fortune to have contributions by two scholars with deep insights into Kelly and Steward gained from personal connections, from reading their correspondence, from interviews, and from intimacy with their scholarly works. Catherine S. Fowler has explored elements of Kelly's biography before (Fowler 2012; Fowler and Van Kemper 2008), and is perhaps the scholar most familiar with Kelly's original works, since she is in the final stages of publishing Kelly's Southern Paiute ethnographic notes for the Las Vegas area (Fowler and Garey-Sage 2016). Virginia Kerns has written two outstanding books that show different views of Steward, based on his notes, interviews with those who knew him, and the perspectives of the indigenous people he interviewed (Kerns 2003, 2010).

Like many who work in the region, my own encounters with Kelly and Steward were transformational. Reading Kelly's Southern Paiute ethnography in 1979 for

perhaps the fifth time, I found something new with each read. For instance, passages such as "Metate, mano.... When possible obtained from prehistoric site," or "The mano invariably was picked up at an archaeological site" (Kelly 1964:152), were so archaeologically prescient that the research problem became self-evident—if reuse is an inexorable process in which grinding stones are moved to later sites, thus biasing the sample from earlier times, are there then statistically significant differences in the frequency of grinding stones between Late Prehistoric and earlier archaeological sites in the Great Basin? My consequent study ended up as an article in a 1983 issue of this journal.

Steward's monograph (with the title many of us abbreviated to "*Basin-Plateau*," or "the 1938 BAE volume") became a fixture on the seat of every vehicle I wore out in the deserts of the American West. Steward's descriptive work was a font for models for a science that lives by the test of analogies, and for a profession where younger students are challenged to control the literature about living hunters and gatherers, not just the technical analyses of material culture. Steward and Kelly were and are my heroes.

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ISABEL T. KELLY: PIONEER GREAT BASIN ETHNOGRAPHER

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Isabel Truesdell Kelly (1906–1982) was an indefatigable field worker, often in rigorous situations that would challenge even the most seasoned outdoors person with all of the modern gear of today. She worked in the western United States, many rugged areas of Mexico, and also in Central and South America, as well as Pakistan. Archaeology was her "first love," although she did major ethnographic studies and made many contributions to applied anthropology. Theory was not her strong interest, but deep description was, in whatever she was pursuing. Her employment career was outside of academe, owing in part to the period in which she took her graduate training (late 1920s-early 1930s), as well as



Departure: Isabel Kelly and her 1932 Ford.

Courtesy of DeGolyer Library, Southern Methodist University, Isabel T. Kelly ethnographic archive.

to circumstances that led her in her early years to spend most of her life in Mexico. But she remains a seminal figure in anthropology, as a pioneer in several geographic areas (including the Great Basin), and—in spite of what was then a non-traditional career path—as a role model for women.

From the beginning of my ethnographic work, I have felt a kinship with Kelly, having worked a generation behind her with the same Great Basin peoples. Our areas of interest—basic ethnography, ethnoecology, ethnogeography, relationships of the ethnographic and archaeological records—solidified those bonds. And more recently, having worked with her extensive Southern Paiute field notes, material culture collections, and her biography, those bonds have grown stronger (Fowler 2012; Fowler and Garey-Sage 2016; Fowler and Van Kemper 2008). Kelly is someone to not only be admired, but to be emulated in her dedication to the rigorous pursuit of one's goals.

I had the pleasure of meeting Isabel Kelly only once, when Don and I visited her beautiful compound in Tepépan outside of Mexico City in 1978. Kelly was a most gracious host, showing us her delightful home, complete with arched windows framed by stones she salvaged from one of Mexico City's colonial *acequias* (she found them in a dump), her spacious gardens, archaeological lab, antique furniture, and much more. We talked about her U.C. Berkeley undergraduate (B.A. 1926) and graduate (1927–30) days, with mentors Alfred Kroeber, Robert Lowie, and Edward Gifford in anthropology and Carl Sauer in geography. She was very fond of Gifford (Giff) and Lowie, but especially of Sauer (and his wife), with whom she developed a particularly close friendship, especially maintained during her many years in Mexico. Fellow grad students then were Anna Gayton, Lila O'Neale, Dorothy Lee, Cora Du Bois, Julian Steward, Charles Voegelin, Ralph Beals, Forrest Clements, and Lloyd Warner. No one taught

archaeology, her “first love,” but Sauer’s on-the-ground approach to geography and subsistence, especially as taught in his classes in field geography, was the next best thing.¹ She said that she learned to look for and try to understand human/land relationships from Sauer’s approaches. She was able to attend the Laboratory of Anthropology’s first archaeological field school in 1929 in New Mexico on the basis of a good but not glowing recommendation from Kroeber to A. V. Kidder.² But upon returning to Berkeley, Kelly was not able to pursue further archaeological training; she did complete three studies of museum collections, one involving Peruvian ceramics (Buzaljko 1993). Although Kroeber continued to support Kelly and thought highly of her, he apparently felt that women were better suited to ethnography and museum work than archaeology. George Foster, who was a student at Berkeley a decade later, saw Kroeber as “basically antagonistic to women, thinking that they had no place in a graduate program,” while Lowie felt genuinely positive about them and was more responsible for their success at Berkeley (Buzaljko 1991:xiii). Foster’s comments are perhaps a little harsh, but suggestive of a less-than-embracing atmosphere.

Kelly also spoke the day we visited about her first field work with Northern Paiute people at Fort Bidwell in Surprise Valley. It was Anthropology Department policy that all graduate students do ethnographic field work as part of their requirements—preferably with indigenous Californians—following Kroeber’s orientation toward “salvage” ethnography. Kelly said that she hadn’t a clue as to where to go, what to do, and even less direction from Kroeber as to how to do it. But a friend of her mother’s ran a boarding house in Cedarville, California, where she could stay, and she was sure that there were Indian people nearby. So Kelly decided to give that a try in the summer of 1930. She told us that field relationships were pretty tough. People sicced their dogs on her, closed the door in her face, and pretended not to be home, but she persevered, thinking that was likely “just normal” in field work. She arrived back in Berkeley with material for a full ethnographic sketch, plus a large body of traditional tales, and several specimens for the museum. Kroeber was surprised, and perhaps a little pleased (although he never told her so), especially given that he considered Great Basin peoples particularly difficult to work with

(Kroeber 1935:9). But she had apparently “passed the test” as far as Kroeber was concerned. She published her Surprise Valley monograph in 1932, a year ahead of Julian Steward’s equally important Owens Valley monograph and during the same year she finished her dissertation. She published the traditional tales in 1938.

Times were tough for women hoping for academic careers in the depression years, and Kelly was caught in that situation. Jobs were few, and what there were went to men—who were assumed to have families to support. There was also a strong undercurrent in those years that women weren’t worth all that work, as they would “just go off and get married” and leave the profession (Parezo 1993). But Kelly stayed. She was very committed to her chosen profession. Her Coast Miwok work in 1931–32 (see Collier and Thalman 1991) was based on a small grant from U.C. Berkeley, and her Southern Paiute work on a National Research Council fellowship—the only funds available—that covered minimal subsistence, travel, and consultants’ fees. Kroeber helped her secure both, and she was able to get a small supplement for her Southern Paiute work by making material culture collections for museums. In the latter case, Kelly was in the field for 15 months (July, 1932 to December 1934, minus three trips to Berkeley) on a budget of \$3,050. She furnished her own transportation (see photo) and usually camped out, rain or shine. She was affiliated with the Laboratory of Anthropology in Santa Fe, with Jess Nusbaum, the Lab’s director, as a co-sponsor.

I have always wondered about the relationship between Kelly and Steward, given that he does not cite her work in any meaningful way in his Great Basin publications (but see Kerns on Steward’s relationships with women colleagues). He did remark in his diary that “Kelly’s ‘Las Vegas’...covers half of the U.S.” (Kerns 2010:87), implying that he felt she had somehow missed the (his) mark. If there was animosity, it was apparently not on Kelly’s part. Her contact with him was renewed immediately after she began her Southern Paiute field work in July, 1932. Steward was then teaching at the University of Utah, and had apparently laid out a plan of work within the state, including both archaeology and ethnology. He had recently told Kroeber he specifically planned to work with the Shivwits Paiute. Wanting to avert any difficulty, Kroeber alerted Nusbaum who in turn alerted Kelly. She immediately contacted “Stew”

and agreed to meet him when he finished his July archaeological reconnaissance by boat through Glen Canyon. Kelly reported to Nusbaum that they met in the field, and that Steward looked “worn, not to mention heavily bearded. ...It was awfully nice to see him again; and I think that we shall not come to blows over the Southern Paiute. ...Under the circumstances, I think that the Shivwits will languish neglected for a long time to come” (Kelly to Nusbaum, July 27, 1932; LAA). In a later letter, she defended him against comments as to his conduct on the just-completed river trip, saying “that you have heard only one side of the story—and the wrong side, I am morally certain...he is such a good sport that otherwise he probably would not mention the situation” (Kelly to Nusbaum, Aug. 6, 1932; LAA). She would, however, write a month later that she would not work exhaustively at Shivwits, as Steward still planned to work there that fall and winter—which he never did. They would have additional contacts in later years, again seemingly amicable on Kelly’s part (Kerns, this issue; Fowler and Van Kemper 2008).

Fortunately for all of us, Kelly was a prolific letter writer with a marvelous wit and a unique flare for language. Her correspondence stemming from her Southern Paiute work (and later) describes vividly her travels and travails, as well as her successes. A few examples will suffice: “This past week I have been camped in the wilds at Indian Peak [Utah], on exceedingly chummy terms with 7 Paiute, 9 horses, and countless canines resident here. This does not include the amiable bobcat which wandered into my tent one moonlit night” (Fowler 2012:50). On another occasion, after working for a few weeks with the Chemehuevi in the summer heat at Parker, Arizona, “in complexion I am coming to look like a cross between a Rhode Island Red and a Plymouth Rock” (Fowler 2012:53). She corresponded with her mentors at Berkeley, A. V. Kidder and Jesse Nusbaum, and over the years with scores of others (especially Sauer), at a time when the discipline was much smaller and seemingly much friendlier—and people wrote letters. Throughout her Southern Paiute field work, she continued to look as well into the local archaeology, always informing her mentors of her finds. But in a note to Nusbaum she did admit that “I never thought I could become an ethnologist, but I manage to find it pretty interesting.

Even so, archaeology was my first love” (Kelly to Nusbaum, June 10, 1933; LAA).

Kelly was forced to take two “breaks” in her Southern Paiute field work because of back troubles, one worse than the other. On the first occasion, she wrote in her characteristic manner:

“I have been obliged to take an enforced vacation which Harold Hitchcock, the famous bone specialist, claims has been brought on by too much Ford-driving. He insists that everyone who drives a Ford must pay a penalty sooner or later and that account is being taken on me because of my having averaged something like 2,000 miles monthly since June [it was December]. At any rate, I must confess that although I have always been a rugged and hearty soul, the last few weeks have been made miserable with a stiff back—all of which involves sleeping on planks, sporting 20 inches of adhesive tape, and desisting from driving. All of this is a perfect nuisance” (Kelly to Nusbaum, Dec. 20 1932; LAA).

Kelly’s work was interrupted a second time for four months when she was hospitalized with a broken back after her car was “totaled” in a roll-over on a dirt road in northern Arizona while she was giving two fellow students a lift back to Berkeley. She quipped, “Just as I suspected; it *isn’t* safe to travel about with a Rockefeller student” (a “Harvard man” was driving; Fowler and Van Kemper 2008:152–153). But Kelly, still undeterred from her goal of visiting and working with descendants of all 15 Southern Paiute groups, and with a another car, was back in the field for more, ultimately covering an estimated 7,000 miles, mostly over dirt roads in Arizona, Utah, Nevada, and California.

Unfortunately, Kelly was deterred from publishing most of the extensive data she gathered from her Southern Paiute ethnographic work when the chance for employment (this time archaeological) sent her to Mexico in 1939 on grant funds secured by Sauer, Kroeber, and Herbert Bolton (joint founders of *Ibero-Americana*). She had found her true home there, doing what she loved—but mixed with ethnography and several projects in applied anthropology. Her highly successful career and life in Mexico has been summarized in a *Homenaje* (González 1989).

In the mid-1950s, Kelly and Kroeber planned to write a joint monograph on Chemehuevi culture based on both their field work and Kroeber’s ethnohistoric

research, but his unexpected death in 1960 ended that plan. Fortunately, she did contribute the sketch of Southern Paiute/Chemehuevi culture to the *Great Basin* volume of the *Handbook of North American Indians* that supplemented her earlier articles on her Southern Paiute/Chemehuevi work. Her professional vita is lengthy, including her Great Basin ethnographic work, Mexican archaeology and ethnology, and other studies elsewhere. Although she did not take the usual academic route, she remained professionally employed throughout her lifetime. She made her own way, very successfully, in a time when the situation for women in the field was far different than it is today.

NOTES

¹The anthropology and geography departments under Kroeber and Sauer were particularly close, with students in each benefitting from courses in the other, as well as from jointly taught seminars (see also Kerns 2003).

²A. L. Kroeber to A. V. Kidder, January 9, 1929; Laboratory of Anthropology Archives, Museum of New Mexico, Santa Fe [hereafter LAA]. He also states that he was in favor of an all-male crew that first year.

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JULIAN STEWARD IN THE FIELD: ON THE ROAD TO CULTURAL ECOLOGY

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I never met Julian Steward, but over a span of many years I did come to know him through his own words and through the personal memories of other people. He died in Urbana, Illinois, in 1972, at the age of seventy, just months before I entered the doctoral program in anthropology at the University of Illinois. During the next few years I encountered faculty members and a student or two who had met him—but very few who had known him. He had spent his last ten years living a reclusive life, avoiding the university and only rarely meeting with students at his house. His position as a research professor, without routine teaching duties, allowed that self-chosen isolation even before he retired. So did the help of his wife—but more on that below.



**Julian and Jane Steward, eastern California, 1935.
Courtesy of Deep Springs College.**

In the halls of the anthropology department, Steward retained a shadowy presence after his death. Older students mentioned him in a respectful way, passing on graduate-student lore about a man they regarded as a prominent theorist, a luminary. They had heard that he was cordial enough to graduate students, if not a willing teacher and advisor for many. Few were aware of Steward's importance as one of the founding

figures in Great Basin anthropology. His editorship of the *Handbook of South American Indians* and his connections with Latin American and Caribbean anthropology remained more salient at that point to the department's program. Even fewer students, if any, knew about his controversial work as an expert witness in the Indian Claims Commission trials (Pinkoski 2008; Ronaasen et al. 1999).

A year or so after Steward died, I happened to meet his widow in the hallway of the anthropology department. For years, Jane Steward had visited campus regularly to collect his mail, do errands, and (not least) visit with colleagues and others at the university whom she regarded as their friends. She was not only a wife but also something of a goodwill ambassador.

More years had passed when, as a new faculty member at another university, I was asked to teach a graduate seminar that included readings on cultural ecology. I wanted to tell the students something about Steward's life, in an effort to illuminate his distinctive ideas, but my search of published sources turned up only a *festschrift* essay and obituaries. Even after reading those, he still seemed a shadowy figure. I finally decided to contact Jane Steward and some of his former students, including Sidney W. Mintz and Robert F. Murphy, who had worked with him at Columbia University in the late 1940s.

To understand the ideas of theorists and other creative thinkers, it is common to search for intellectual influences: from their teachers, for example, or even from fellow students. (Sid Mintz emphasized the latter when he spoke to me, citing his intellectual ties with other Columbia students.) Steward's fellow students at the University of California, Berkeley, in the 1920s included Isabel T. Kelly, Ralph L. Beals, Theodora K. Kroeber, and William Duncan Strong, but he largely disavowed the influence of his peers or teachers. Privately, he always claimed to be a "maverick," and the sole architect of the theoretical perspective he came to call cultural ecology.

Steward's well-known teachers at Berkeley were anthropologists Alfred L. Kroeber and Robert H. Lowie, and geographer Carl O. Sauer. Lowie, his dissertation advisor, was something of a friend. Kroeber was not so much his intellectual mentor as a patron who provided professional support. As Jane Steward put it, paraphrasing her husband's words, which alluded to

patrilineal kinship systems, "Lowie was his mother's brother, and Kroeber, his father's brother." The warm feelings between Lowie and her husband contrasted with the formality of his relationship with Kroeber. His relations with Sauer were chilly.

If Steward was reluctant to credit other people with influencing his ideas, he did give some credit to a school and place in eastern California: an unusual boarding school located on a working ranch in Deep Springs Valley (see Newell 2015), near the western edge of the Great Basin. Steward arrived in 1918 at the age of sixteen. His three years there provided an immersive experience of Great Basin landscapes, and his first contact with Native people—along with a college preparatory education (Kerns 1999). In 1927, he returned briefly to the area to do ethnographic research with Owens Valley Paiutes. Eight years later, after a short stint as a faculty member and archaeologist at the University of Utah (see Janetski 1999), he returned again to the Great Basin for fieldwork. It resulted in a landmark monograph, *Basin-Plateau Aboriginal Sociopolitical Groups* (Steward 1938), which is still in print.

Steward was unemployed at the time he began his ambitious research project. The country was then entering the fifth year of the Great Depression. But he had some crucial advantages, even in those hard times: financial support from Kroeber, familiarity with Great Basin landscapes, and contacts with Owens Valley Paiutes. He also had a sense of confidence about his professional prospects, which led him to turn down non-academic employment in favor of fieldwork, and to wait for an offer he judged better. By 1935, Steward had already held two full-time academic positions—at the University of Michigan and the University of Utah—for five years. Despite the scandal that led him to leave the University of Utah, he had reason to think that his academic career had not ended. The hiring practices of the time gave him priority (Kerns 2003).

In contrast, Isabel Kelly, who also carried out fieldwork in the Great Basin during the 1930s, eventually made a career outside the United States and academe (Fowler and Van Kemper 2008, and this issue). In the early 1940s, at Kroeber's urging, Steward hired Kelly as a staff member in Mexico for the Institute of Social Anthropology, a program he directed at the Smithsonian Institution's Bureau of American Ethnology. "She may

have a tiny remnant of her last Guggenheim [fellowship] left,” Kroeber had told him, “but essentially she must be at loose ends.” When I later read Steward and Kelly’s correspondence, I was struck by her wry, off-center sense of humor, which often left me laughing out loud. I thought her humor must have helped her cope with years of financial insecurity. I also found that she was one of a very few women Steward corresponded with, but that he almost never cited her work on Southern Paiutes—or the work of other women on anything (Kerns 2003:232–233, 330 n.12).

But back to 1935 and his Great Basin fieldwork. At the outset, Steward had yet another advantage: the assistance of his young new wife, who came from a prominent family in Salt Lake City. Her grandfather was among the first settlers to reach the Great Basin. Jane had slight knowledge of anthropology, but she brought enthusiasm and energy and some family support and contacts to her husband’s research project. She was convinced of the scientific importance of his fieldwork, and embraced her part in it—especially during the first and more exhausting phase.

When I talked to Jane fifty years later, her memories of fieldwork were vivid if limited. Later I read the field journal, much of it written in her hand, and found letters to her family that also documented the daily realities of their fieldwork. The rapidity, intensity, and immense range of that field survey could not have been sustained without her presence and support. She had a disarmingly friendly manner, as I witnessed and others recalled, and the ability to break through social barriers quickly. Her husband did not share those qualities. Robert Murphy, who knew Steward well, remembered him as quiet and often withdrawn—except in intellectual discussions with a few trusted men, his students and colleagues.

In two spells of fieldwork in 1935 and 1936, Julian and Jane Steward covered the length and breadth of the Great Basin. In eastern California, Steward questioned acquaintances from Deep Springs Valley: Tom Stone and Mary Harry, who had worked at the school as a ranch hand and laundress, respectively. Using person-to-person referrals, he located dozens more Indian elders to interview as he and his wife drove across the Great Basin. In a departure from the usual practice of the time, he did not name them in print, identifying them only by initials: TS, MH, and so on.

Steward was searching for ethnographic evidence of the patrilineal band, a foundational concept in his thinking about cultural ecology. He learned from the many Indian elders he interviewed about how they—or their parents and grandparents—had lived on wild lands: the plants they gathered, the animals they hunted, their tools and techniques, the people with whom they lived and worked. These elements—resources, technology, and the organization of work—were central to Steward’s cultural ecology. But to his great disappointment, what elders told him did not provide evidence of the patrilineal band. *Basin-Plateau* became a different book than the one he had envisioned when he set out on his quest (Kerns 2010).

Steward had such a strong sense of being a self-made man and original thinker that it was difficult for him to see how others had helped and taught and guided him. The presence of his wife in fieldwork is not detectable in *Basin-Plateau*. (This is no doubt due also to conventions of the profession and ethnographic writing at the time. Claude Lévi-Strauss reveals the presence of his wife in just one jarring sentence in *Tristes Tropiques*.) Steward likewise said nearly nothing in print about his relations with the anonymous elders, the so-called informants. That term, long conventional in anthropology, simplifies the relationship between ethnographers and the people who act as their teachers in the field. Or call them cultural guides.

Who, then, were these people who served as Steward’s teachers or guides in the Great Basin? By reading the field journal in tandem with *Basin-Plateau*, I found that I could match some names with initials. Later, I searched old records, and identified dozens more elders by name; often by life circumstances; and sometimes by face, in photographs taken by Steward or others. There were a few women, but most were men. They included Owens Valley Paiutes, Northern Paiutes, Southern Paiutes, Western Shoshones and Gosiutes, Northern and Northwestern Shoshones, and Bannocks. Some lived on reservations; others, off reservation in remote valleys or on the outskirts of towns. All were survivors of an ecological crisis, American settlement of their homelands. But that did not engage the interest of a theorist in search of the patrilineal band. And his wife did not then see a connection between her family’s success as settlers and the visible hardships of the elders’ lives.

I want to end here simply by giving credit to a few of the resilient men and women who were patient, perceptive teachers and cultural guides: George Hanson, Indian Ranch, California; Tom Stone and Mary Harry, Big Pine, California; John Shakespeare, Cow Camp, Nevada; Albert Howell, Ash Meadows, Nevada; Barney Hicks, Railroad Valley, Nevada; Jennie Washburn, Ely, Nevada; Bill Gibson, Elko, Nevada; Johnnie Pronto, Duck Valley Reservation, Idaho; Grouse Creek Jack and Silver Ballard, Fort Hall Reservation, Idaho; Ray Diamond and Seth Eagle, Washakie, Utah; and George Moody, Skull Valley, Utah. There were many, many more.

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