JOHN BEACH RINALDO: QUINTESSENTIAL CULTURE HISTORIAN

Stephen E. Nash

During an archaeological career that spanned four decades, John Beach Rinaldo (1912–1999) made substantive contributions to the delineation and definition of the Mogollon Culture, the culture history of west-central New Mexico and east-central Arizona, and the identification of material relationships between precolumbian cultures and modern-day Zuni. For a variety of reasons, Rinaldo is overshadowed by his Field Museum collaborator Paul Sidney Martin. As a result, historians of archaeology have failed to critically evaluate Rinaldo's career and contributions. This paper offers a controlled analysis and comparison of data in unpublished archives, artifact collections at the Field Museum, and the published record to illuminate previously unrecognized but important aspects of Rinaldo's many contributions to archaeological knowledge, method, and theory.

Durante una carrera arqueológica que abarcó cuatro décadas, John Beach Rinaldo (1912–1999) hizo aportaciones sustantivas a la delineación y definición de la Cultura Mogollón, la historia de la cultura del centro-oeste de Nuevo México y Arizona centro-este y la identificación de relaciones materiales entre las culturas precolombinas y Zuni actual. Para una variedad de razones, Rinaldo es eclipsada por su colaborador Paul Sidney Martin del Museo Field. Como resultado, los historiadores de la arqueología no han podido evaluar críticamente la carrera y las contribuciones de Rinaldo. Este documento presenta un análisis controlado y comparación de datos en archivos inéditos, colecciones de artefactos en el Museo Field, y las actas publicadas previamente no reconocidos para iluminar aspectos importantes de las muchas contribuciones de Rinaldo a la disciplina de la arqueología.

John Beach Rinaldo (Figure 1) played an important if underappreciated role in the development of North American archaeological knowledge, method, and theory. During a career that spanned a quarter of a century at the Field Museum of Natural History in Chicago, Rinaldo made substantive empirical contributions to the archaeological definition of the Mogollon culture, the elucidation of the culture history of west-central New Mexico and east-central Arizona, and in positing connections between the precolumbian Mogollon and modern-day Zunis (Martin 1943; Martin and Rinaldo 1940, 1947, 1950a, 1950b; Martin et al. 1949; Martin et al. 1952; Martin et al. 1954; Martin et al. 1956; Martin et al. 1957; Rinaldo 1941a, 1959, 1964). After leaving the Field Museum in April 1963, Rinaldo faded from the archaeological scene, but not until after helping Charles Di Peso of the Amerind Foundation in Dragoon, Arizona, publish the massive and monumental Casas Grandes site report (Di Peso et al. 1974).

Rinaldo excavated 52 archaeological sites for the Field Museum, usually serving as field director, as his colleague and supervisor Paul Sidney Martin preferred to remain in the laboratory at the luxurious Pine Lawn Tourist Camp near Reserve, New Mexico (Table 1; Eloise Barter, personal communication 2014). Rinaldo conducted major surveys in the areas around Reserve and Vernon, Arizona. He authored or coauthored 14 monographs, two book chapters, five peer-reviewed journal articles, a book review, a comment on a book review, a popular article, and three data compilations. He amassed and took responsibility for cataloging, at least partially, one of the world's great collections of Mogollon material culture (Nash 2005, 2006).

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Table 1. Sites Excavated by Rinaldo and Martin 1939–1962

Site Name	Excavation Year(s)	Artifacts (Rounded to nearest 10)	Site Report
SU Site, NM	1939, 1941	34,860	Martin 1943; Martin and Rinaldo 1940, 1946, 1947
Wet Leggett Site, NM	1947, 1948	80	Martin et al. 1949
Promontory Site, NM	1947	450	Martin et al. 1949
Twin Bridges Site, NM	1947	980	Martin et al. 1949
Oak Springs Pueblo, NM	1947	920	Martin et al. 1949
Turkey Foot Ridge, NM	1947, 1948	8,220	Martin et al. 1949; Martin and Rinaldo 1950a
Wet Leggett Pueblo, NM	1949	2,350	Martin and Rinaldo 1950b
South Leggett Pueblo, NM	1949	1,360	Martin and Rinaldo 1950b
Three Pines Pueblo, NM	1949	1,410	Martin and Rinaldo 1950b
Pine Lawn Camp, NM	1949	180	Unpublished
Tularosa Cave, NM	1950	51,670	Martin et al. 1952
Apache Cave, NM	1950	330	Unpublished
Bull Basin Cave, NM	1950	20	Unpublished
Balke Site, NM	1950	80	Unpublished
Cordova Cave, NM	1951	15,060	Martin et al. 1952
Negrito Cliff Dwelling, NM	1951	300	Unpublished
Kiehne Pueblo, NM	1951	170	Unpublished
Sawmill Site, NM	1951, 1952, 1954	1,200	Bluhm 1957
O Block Cave, NM	1952	6,780	Martin et al. 1954
Y Canyon Cave, NM	1952	650	Martin et al. 1954
Hinkle Park Cliff Dwelling, NM	1952	3,790	Martin et al. 1954
Cosper Cliff Dwelling, AZ	1952	830	Martin et al. 1954
Higgins Flat Pueblo, NM	1953, 1954	9,970	Martin et al. 1956; Martin et al. 1957
Apache Creek Pueblo, NM	1954	990	Martin et al. 1957
Valley View Pueblo, NM	1954	350	Martin et al. 1957
Foote Canyon Pueblo, AZ	1955	5540	Rinaldo 1959
Saddle Mountain Pueblo, NM	1955	280	Unpublished
Powerline Site, NM	1955	260	Unpublished
Delgar Site, NM	1955	510	Unpublished
Perry Lawson Site, NM	1955	40	Unpublished
BO Site, NM	1955	30	Unpublished
Frisco Pueblo, NM	1955	40	Unpublished
Phipps Site, AZ	1957	3,440	Unpublished
Laguna Salada Site, AZ	1957	770	Martin and Rinaldo 1960a
Little Ortega Lake Site, AZ	1957	230	Martin and Rinaldo 1960a
Vernon Survey Site 30, AZ	1957	8,300	Martin and Rinaldo 1960a
Vernon Survey Site 31, AZ	1957	1,600	Martin and Rinaldo 1960a
Curtis Site, AZ	1957	860	Unpublished
Table Rock Pueblo, AZ	1958	19,610	Martin and Rinaldo 1960b
Mineral Creek Site, AZ	1958, 1959	8,190	Martin et al. 1961
Swinburn Cave, AZ	1959	6,120	Unpublished
Hooper Ranch Pueblo, AZ	1959, 1960	26,030	Martin et al. 1961
Tumbleweed Canyon Site, AZ	1960	650	Martin et al. 1961, 1962
Thode Ranch Site, AZ	1960	1,020	Martin et al. 1961, 1962
Rim Valley Pueblo, AZ	1960	1,050	Martin et al. 1961, 1962
Chilcott Ranch Site 1, AZ	1960	1,268	Martin et al. 1961, 1962
Chilcott Ranch Site 2, AZ	1960	153	Martin et al. 1961, 1962
Chilcott Ranch Site 3, AZ	1960	81	Martin et al. 1961, 1962
Goesling Site, AZ	1960	2,490	Martin et al. 1961, 1962
Rhoton Site, AZ	1960	220	Unpublished
Longacre Survey Site 211, AZ	1961	1,700	Unpublished
Carter Ranch Pueblo, AZ	1961, 1962	58,260	Martin et al. 1964
Longacre Survey Site 199, AZ	1962	1,480	Martin et al. 1964
Broken K Pueblo, AZ	1962, 1963, 1964	31,510	Martin et al. 1967
Vernon Site, AZ (a.k.a. Mineer Site)	1962, 1963, 1964	14,370	Longacre and Graves 1976

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Figure 1. John Beach Rinaldo in a 1950 staff photograph. Courtesy of Field Museum. ID# A93274_B.

Rinaldo was the quintessential culture historian and systematist, more of a technician than an innovator, theoretician, or early adopter of new methods and techniques. Whereas Martin became an early and ardent advocate of the New Archaeology, Rinaldo never adopted its central tenets, which included rigorous sampling techniques, deductive reasoning, explicit hypothesis testing, and an emphasis on culture process rather than culture history (Binford 1962; Lucas 2001; Watson 2008). In active demonstration of his resistance to change, Rinaldo resigned his curatorial position at the Field Museum at age 50, at the peak of his career, in the midst of Martin's (1971) very visible and public conversion to the New Archaeology. Rinaldo, in contrast, made only cautious and tentative forays into archaeological method and theory.

Published analyses of Rinaldo's career do not exist. Three brief obituaries offer disturbingly few details (Ramsey 2000; Society for American Archaeology 2000; University of Chicago Magazine 2000). Relevant contributions to the history of archaeology either fail to mention him at all or do so merely as Martin's coauthor (e.g., Fenner 1977; Haury 1985; Lekson 2009; Lyman et al. 1997; O'Brien et al. 2005; Reid and Whittlesey 2010; Woodbury 1993; cf. Gregory 2008:134–136; Reid 1986). Even Martin (1959) fails to mention his long-time collaborator and colleague except in citations. Rinaldo's career has therefore never received the critical analysis it deserves; this paper reddresses that situation.

I (Nash 2010:105) have argued that museum-based archaeologists are, generally speaking, responsible for making professional contributions in five realms: (1) research (typically, though not exclusively, in the field); (2) exhibition and outreach; (3) collections acquisition and curation; (4) administration; and (5) service, the latter to both discipline and home institution. The time, effort, and resources that a museum-based archaeologist invests in these realms will change in response to shifting institutional priorities, idiosyncratic historical moments and opportunities, and new personal and professional trajectories. Nevertheless, this structure offers a framework on which to analyze a scholar's career. Rinaldo's contributions lie primarily in two of these five domains: research and collections. He did very little exhibition development or outreach, and he had no formal administrative or service responsibilities, at least that are recorded in the archives consulted.

A controlled analysis and comparison of data from unpublished archives, Field Museum collections, and the published record illuminates facets of Rinaldo's career and contributions that are unavailable from an analysis of the published record alone. This approach helps avoid falling into a presentist trap, in which a historical figure is unfairly evaluated against modern professional standards (Stocking 1965:211). Three questions come to the fore: What were Rinaldo's primary contributions to North American archaeological knowledge, method, and theory? Why did he resign from a secure position at the Field Museum at the peak of his career? Why is Rinaldo, undeniably one of the most productive and prolific archaeologists of the mid-twentieth century, still underappreciated?

The Data

Given his ancillary, if not subservient, status with respect to Martin, the Field Museum archives contain no Rinaldo-specific archives; a similar situation obtains at the Amerind Foundation, where DiPeso was in charge of Rinaldo's work (Eric

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Kaldahl, personnal communication 2014). Archival data are available only if one digs deep, for Rinaldo-specific correspondence, artifact tabulations, unpublished manuscripts, and other documents can indeed be found in archives generated by others.

Five primary archives were consulted for this paper. The Field Museum Director's Office Expedition Files (FMEF) contain correspondence between expedition leaders, in this case Martin, and museum administrators. The files contain important details regarding expedition funding, logistics, personnel, public relations, and scientific discoveries. Content emphasizes what Martin wanted his Field Museum supervisors to know and glosses over problematic concerns; Rinaldo is occassionally mentioned.

The Field Museum Anthropology Department Correspondence Files (DACF) contain extensive scientific and professional correspondence between Martin and his archaeological colleagues, including the departmental secretary, again with occassional mention of Rinaldo. They contain information on scientific problems and academic concerns, but also personnel data. Given that Rinaldo and Martin were usually working together in person, either in Chicago or in the American Southwest, direct correspondence between them is virtually nonexistent.

The Field Museum Anthropology Expedtion Files (DAEF) contain excavation records, artifact tablulations, and some correspondence about specific artifacts, discoveries, and sites (Coleman 2004). Rinaldo was largely responsible for the creation of the DAEF between 1946 and 1963; these documents may be read as a de facto record of his approach to archaeological research during that period.

The Arizona State Museum/Field Museum Martin Archives (ASM/FMMA) contain documents that Martin took with him to Tucson when he moved there from Chicago in 1972. The ASM/FMMA contains correspondence, documents, grant proposals, site data, and other personal and professional documents that he deemed important enough to have within arm's reach during the final years of his life. All of these documents were transferred and fully incorporated into the Field Museum archives in 2000. Rinaldo is occassionally mentioned.

Finally, Rinaldo's nephew, Peter "Mick" Ramsey, generously supplied relevant portions of the Rinaldo Family Files (RFF) that deal directly with his uncle. The family files contain important and previously unpublished details about Rinaldo's early education, his military service in World War II, and his service to the family business.

Although I never met Rinaldo face-to-face, I maintained a professional correspondence with him from late 1997 until his death in late 1999. At that time, I was cataloging the Martin Collection at the Field Museum under a National Science Foundation Grant (SBR-9710181) awarded to curator of North American anthropology Jonathan Haas. Rinaldo sent me his small personal collection of archives, slides, data sheets, and other resources, all of which are now fully intetgrated into the Field Museum archives. One of the biggest regrets of my professional career is that I did not make the five-hour drive from Chicago to Des Moines, Iowa, to record oral histories with him. Nevertheless, over the last two decades I have discussed Rinaldo, his career, and his impact on the discipline with many scholars who knew and worked with him, including Eloise and James Barter, Lewis Binford, Gloria Fenner, John Fritz, Elaine Bluhm Herold, and William A. Longacre, among others. All of these people, particularly the Barters, Fenner, Frtiz, and Longacre, offered insights on Rinaldo as a scholar, as an archaeologist, and as a person; their insights informed this paper. As is often the case in historical research, however, unanimity on certain points remains elusive, hence the need to use multiple and complementary datasets.

Academic and Biographic Chronicle

Rinaldo was born in Wheaton, Illinois, just west of Chicago, on November 29, 1912, the fourth child of Philip Sidney Rinaldo of Chicago and Harriet Huntington Beach of East Orange, Iowa. He spent many summers at "Camp," the family's summer vacation home on White Earth Lake in northern Minnesota, where as a youth he built from scratch a replica Navajo hogan (RFF:111). After spending his elementary and junior high school years in Wheaton, Rinaldo attended high school at The Principia, a Christian Science boarding school near St. Louis, Missouri.

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Rinaldo enrolled at Carleton College in Northfield, Minnesota, in 1930, obtaining his B.A. in Romance Languages with honors in 1934. He then enrolled in Harvard Business School at a satellite campus in Princeton, New Jersey (Ramsey 2000; University of Chicago Magazine 2000). One semester proved enough to demonstrate that he did not want to be a full-time businessman, but business ultimately became an important part of his life. His grandfather and father cofounded the Flexible Steel Lacing Company in 1907. Today doing business as Flexco, the company makes fasteners for conveyor belts used in mining and manufacturing applications (Ramsey 2007). Most members of the extended Rinaldo family work for the company at some point in their lives; John was no exception. He began serving on the company's board of directors in 1950 and was the longest serving board member in the company's history, with a tenure of nearly half a century at the time of his death.

After dropping out of business school in the spring of 1935, Rinaldo enrolled in graduate school in anthropology at the University of Chicago. As Fay-Cooper Cole's student, he concentrated on archaeology while becoming immersed in the four-field approach then emphasized at Chicago (Stocking 1979). He secured a position on the famous Rainbow Bridge-Monument Valley Expedition in the summer of 1935; this led to additional but poorly documented fieldwork in northeastern Arizona in the summer of 1936 (Beals et al. 1945).

In late 1935, the Civilian Conservation Corps (CCC) exposed archaeological deposits during construction of a hotel at Pere Marquette State Park near St. Louis. The CCC contacted Cole for assistance; he sent Rinaldo to do salvage excavation. Rinaldo's methods fell squarely into the culture history paradigm promulgated by Cole and his many students, which was so prevalent in Americanist archaeology at the time (Farnsworth 2008; Rinaldo 1937:1; see also Browman 2002; Nash 2010). The project served as the focus of Rinaldo's Master's thesis; the degree was awarded December 21, 1937. Rinaldo continued to take classes at Chicago in the winter quarter, but withdrew before the spring quarter, in early 1938.

At some point, possibly in the spring of 1938, Rinaldo first met Martin, who had been Cole's star student during the late 1920s (Nash 2003). After being hired by the Field Museum in 1929, Martin performed a swift administrative ascent to become chair of the Department of Anthropology in 1935 (Nash 2010). During the summer of 1938, Rinaldo served as a crew member on Martin's Archaeological Expedition to the Southwest (hereafter the Expedition), under the auspices of which he excavated Basketmaker sites in southwestern Colorado (Martin et al. 1938).

Rinaldo demonstrated his value to the Expedition in the field, in the lab, and back at the Museum. Martin lavished praise in print, presciently describing Rinaldo's role for much of the next quarter century:

> John Rinaldo was of invaluable aid in washing, classifying, counting and packing sherds, mending pottery, and cataloguing and classifying stone and bone artifacts. Throughout the summer he kept abreast of the thousands of sherds and artifacts which came in daily. Mr. Rinaldo continued his services as volunteer assistant after we returned to the Museum and has prepared the report on the stone and bone artifacts included herein [Martin et al. 1938:318; see also Martin and Rinaldo 1940:7].

Rinaldo's personnel file at the Field Museum indicates that he formally became a volunteer on October 1, 1938. On April 1, 1939, he parlayed that service into a paid position as an Associate in Southwestern Archaeology. Remarkably, and for reasons not recorded, his salary was $1 per year, suggesting that the family business, existing family wealth, or another job provided him with funds to cover his living expenses.

Rinaldo sought to build on his 1938 Colorado fieldwork experience. He wrote to John C. McGregor, another former Cole student and a curator of archaeology at the Museum of Northern Arizona in Flagstaff, seeking insights into how diachronic variability in stone and bone tools might be used to test the tenets of the Pecos Classification, which had been criticized for an overreliance on pottery (letter November 16, 1938, Museum of Northern Arizona Archives, MS 304, Folder 3; Kidder 1927; Woodbury 1993). This correspondence led directly to his dissertation research (Rinaldo 1941b, 1945).

Rinaldo was not a registered student from early

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1938 until the spring of 1941; his research at and for the Field Museum apparently continued unabated. He collaborated with Martin on seminal excavations at the SU Site in west-central New Mexico in 1939, conducting and publishing the pottery and artifact analysis sections of the site report during the fall and winter (Martin and Rinaldo 1940:34–84).

The Expedition did not return to the field in 1940 because of budget problems residual from the Great Depression (Nash 2013). Circumstantial evidence suggests that Rinaldo worked on his dissertation after the SU Site report was finished, however.

In 1941, Rinaldo (1941b) defended a collections-based dissertation entitled "An Analysis of Prehistoric Anasazi [hereafter Ancestral Pueblo] Culture Change." He analyzed the artifacts Martin collected during six years of fieldwork at Lowry Pueblo and other sites in southwestern Colorado, using trait lists to examine the transition from the pithouse-dwelling Basketmaker to masonry room-block-dwelling Ancestral Pueblo. He argued that the changes were too dramatic to be explained by in situ cultural evolution and were best explained as the result of an influx of new populations to the area.Whence they probably came, he did not say. An 18-page addendum to the dissertation, inexplicably submitted and published four years later while he was at war, contains surprisingly modern prose: Rinaldo believed he was "testing the hypothesis of independent Anasazi development " (Rinaldo 1945:1).

Martin promised Rinaldo a position on the SU Site field crew for the second excavation season in the summer of 1941, but he had a high draft number (70) and was inducted into the Army on July 1, 1941 (letter Martin to Mott Davis, DACF 1936-1958, Box 2: Coo–Fz, Folder 10: J.O. Brew). He completed basic training at Camp Roberts, near Monterey, California, and was then released from active duty. He was recalled in January 1942, and assigned to a miltary police unit at Fort Custer Training Center in Battle Creek, Michigan. Stationed stateside for well over a year, his unit helped quell the Detroit race riots of June 1943 (letter Martin to Private Ben Rosenbaum, Station Hospital, Camp McCory, WI, July 17, 1942, DACF 1936-1958, Box 6: Ro - Sm, Folder 1; see also RFF: 112).

Rinaldo shipped out to the European theater sometime after June 1943. He saw combat in Belgium, the Netherlands, Germany, and France, serving in an anti-tank platoon at the Battle of the Bulge in late 1944 and the Battle of the Rhineland in March 1945 (RFF: 113). He was honorably discharged as a Staff Sergeant on January 2, 1946 (Letter John O'Hanlon, Archives Technician, National Personnel Records Center, to Denver Museum of Nature & Science volunteer Ed Martin, 16 October 2014, responding to request number 2-11779800037). Unfortunately, no other records of Rinaldo's military service exist, apparently because a massive fire at the National Personnel Records Center in St. Louis, Missouri, on July 12, 1973, destroyed up to 18 million original and unique U.S. Army discharge files dating from 1912 to 1960 (Stender and Walker 1974).

According to Field Museum personnel files, Rinaldo returned to the Museum as an assistant in archaeology on February 1, 1946. He (re-)joined a department still adjusting to the negative social, demographic, and psychological effects of the Great Depression and World War II, set against the positive economic impact of enhanced Works Progress Administration-funded staffing rosters (Nash 2013). He was promoted to assistant curator of archaeology in 1950, with a salary equivalent to just over $22,000 in 2013 (Figure 2).

On March 4, 1951, Rinaldo married his college sweetheart, Ruth Bauer of Colfax, Iowa. Prior to then, he lived with his mother at his boyhood home in Wheaton. The newlyweds bought a home in Elmhurst, Illinois, slightly closer to his work in Chicago, and remained there for more than a decade. They never had children.

On January 1, 1961, Rinaldo was promoted to curator of archaeology at a salary equivalent to $34,500 in 2013. On April 30, 1963, he resigned that position. His personnel card states that he left the Field Museum because he "accepted other employment," but this bland bureaucratic statement ignores the radically changing scholarly context in which his departure occurred.

Lewis R. Binford joined the anthropology faculty at the University of Chicago in 1961. Although he would not complete his dissertation at the University of Michigan for another three years and would ultimately be denied tenure at Chicago, Binford was a charismatic and dynamic scholarly force

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Figure 2. Martin and Rinaldo examining Southwestern pottery in a Field Museum laboratory, 1952. Courtesy of Field Museum. ID# A93707.

who led Chicago students and others, including Martin, in a full-fledged assault against traditional culture history, pursuing instead a New Archaeology (Fagan 2012; Martin 1971; Stocking 1979).

The central tenets of the New Archaeology included rigorous sampling techniques, deductive reasoning, explicit hypothesis testing, and an emphasis on culture process (Binford 1962; Watson 2008). Rinaldo's time-honored methods, however, were quintessentially culture history, seeking to place sites in time and across space through tried-and-true excavation, seriation, and precentage stratigraphy techniques, all of which were firmly grounded in inductive reasoning. The methodological changes implemented during the Expedition's 1962 field season under the mantra of the New Archaeology, particularly the use of random sampling, ultimately proved too much for him to bear. As we shall shee, Rinaldo found no quarter in the midst of these changes.

Charles Di Peso, whom Rinaldo had known since they conducted fieldwork together for Martin in southwestern Colorado in 1938, was now director of the Amerind Foundation in Dragoon, Arizona. In late 1962, DiPeso completed a continuous, three-year excavation program at Casas Grandes in Chihuahua, Mexico. DiPeso sorely needed help with artifact analysis and monograph preparation, both of which were Rinaldo's stengths. After nearly a quarter century at the world-famous Field Museum, in a major metropolis, working on collections he amassed, catalogued, and analyzed, Rinaldo accepted a position at a tiny regional museum, in a tiny town, on artifacts that he did not excavate, and with which he had limited experience. Clearly, Rinaldo was no longer happy in Chicago, but he quickly fell into position at the Amerind and saw the Casas Grandes analysis through to completion. The resulting report is a massive, eight-volume, tour de force in culture history reporting that was exceedingly well-received when finally published a dozen years after the cessation of fieldwork (DiPeso et al. 1974; see also Euler 1976; Heizer 1977).

In 1970, John and Ruth moved back to her family farm in Colfax, Iowa, where they built a modern home. They moved to an assisted living facility in Des Moines sometime in the early 1990s. John died there on November 27, 1999, his eighty-seventh birthday.

Chasing the Mogollon I

David Gregory (2008:133) has aptly described Rinaldo and Martin's work in west-central New Mexico and east-central Arizona as "Chasing the Mogollon through Time and Space." Their fieldwork followed the straightforward path typical of culture historians, focusing first on early sites, then later sites, with an eye towards filling in gaps in the sequence with perishable remains from cave sites, if the latter could be found.

In addition to their excavation program, Rinaldo and Martin variously participated in theoretical debates on topics ranging from the definition, validity, and analytical utility of the Mogollon concept to an attempt to apply Wendell Bennett's "area co-tradition" model to the American Southwest. They made conjectures about precolumbian social organization and addressed the possibility that the ancient Mogollon were related to modern-day Zuni. They also applied for National Science Foundation funding as soon as it became available for archaeological research in 1958. Grant applications required that they be more explicit about research designs, programs, and plans

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Figure 3. Mrs. Crackel's store at the Pine Lawn Tourist Camp office outside of Reserve, New Mexico, in the early 1960s. The Field Museum's Archaeological Expedition to the Southwest was based at Camp from 1939 until 1955. Uncatalogued photo, accession 2013–593, Arizona State Museum, University of Arizona, William A. Longacre, photographer.

before entering the field. They therefore document shifting theoretical and methodological positions. Each of these efforts warrants attention, for the archived documents reveal significant differences in the way Rinaldo and Martin approached archaeological data and interpretation; these differences are not apparent from an analysis of the published record alone.

The Mogollon Concept

In 1939, Martin moved the Expedition to a new base at the Pine Lawn Tourist Camp near Reserve, New Mexico, to excavate the SU Site and help define the precolumbian Mogollon culture that Emil Walter Haury of the University of Arizona had recently described (Haury 1936; Figure 3). In August, after excavating eight pithouses and one surface room, Rinaldo and Martin took the SU Site artifacts to Forestdale Creek, Arizona, where Haury had the University of Arizona archaeological field school. Rinaldo and Martin sought Haury's confirmation that what they were finding was indeed Mogollon (letter Martin to Gregg, August 5, 1939. Box 6, Folder FMEF [1939–1941] Folder 3 of 3). It was.

Rinaldo (1941a) then published "Conjectures on the Independent Development of the Mogollon Culture," in which he focused on the crux of the problem as he understood it:

> Much of the discussion about the separateness of the Mogollon has hinged on the problem of dating the earliest known phases of this culture, namely, the San Francisco, the Georgetown, and the pre-Georgetown. Those who hold grimly to the idea that the Mogollon culture was never anything but a bastard, peripheral variant of the Anasazi do not wish to admit that the Georgetown or pre-Georgetown phases were at least contemporary with or earlier than the Basketmaker and Modified Basketmaker periods. If they did admit this, then they would also have to admit that the Mogollon culture could be rated as a "basic pattern" or "root" [Rinaldo 1941a:5].

Merriam-Webster's dictionary defines conjec-

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ture as "an opinion or idea formed without proof or sufficient evidence" (www.merriam-webster.com; accessed 29 September 2014). Seen in this light, Rinaldo was not at all engaged in conjecture. To make the case for Mogollon independence, he compared published material culture traits, architecture, and available tree-ring dates for Ancestral Pueblo, Hohokam, and Mogollon sites as they were then available. Rinaldo (1941a:9) wrote: "from this evidence ... one may safely state that certain Mogollon traits of the San Francisco phase must have been in existence at least as early as A.D. 700." The Mogollon, therefore, had primacy over Ancestral Pueblo and Hohokam.

A previously unrecognized aspect of Rinaldo's (1941a) exegesis is that it was coauthored with Martin but published singly under Rinaldo's name, probably because Martin feared retaliation from powerful colleagues who did not agree with their conclusions. Martin revealed his fears in a November 13, 1942, letter to McGregor. In so doing, he hinted at the sociopolitical, academic, and historical context in which they were working:

> You might be interested to know—from an historical viewpoint—that I had originally written a fairly lengthy section on comparisons and synthesis for inclusion in [the SU Site] report [(Martin and Rinaldo 1940)]. Many of my statements were based on the work that [Ted] Sayles [of Gila Pueblo Archaeological Foundation] had done in Cochise [Arizona] and his Cave Creek material [from north-central Arizona]. However, when I sent the chapter to Sayles and [his boss Harold] Gladwin for criticisms, they both blew up and refused to let me refer to any of their findings. In fact, Sayles backed out of all the statements that he had made to John Rinaldo and me and would not admit that any of our [SU Site] material-pottery or stone artifacts, was similar to his Cochise and Cave Creek stuff! These objections, which were expressed strenuously and somewhat disagreeably, cut the heart out of my whole section, and so I withdrew it. Later John Rinaldo and I got together and wrote what we could about conjectures on the Mogollon and published that article in [American] Antiquity under Rinaldo's name [Rinaldo 1941a]. Gladwin immediately wrote me and several other people in some anger and said it was a great pity that we had expressed ourselves in print [and] that the whole thing was premature, unwarranted, and unfortunate and he would have nothing of it [DACF 1936–1958 Box Ki–N, Folder 5: Departmental Correspondence 1936–1958: Mc].

Gladwin relished his role as a bombastic iconoclast, but his tirades had real and significantly detrimental effects on archaeological scholarship in the American Southwest in the 1940s (Nash 1999:93–142). In this instance, he denied access to comparative collections that might help confirm or deny the independence, and indeed existence, of the Mogollon. Ironically, Haury (1936) had excavated and published the Mogollon Village type site while working for Gladwin.

Rinaldo and Martin returned to the SU Site for a second season in 1941 and a third in 1946, but they considered the data recovered to be effectively redundant (Martin 1943; Martin and Rinaldo 1947). They spent five months in the field in 1947, a formative year in which they laid the groundwork for the Expedition's next decade in and around Reserve. Rinaldo surveyed the region to find excavation-worthy sites spanning the regional occupation through time and across space; he documented at least 68 (letter Martin to Sayles, March 8, 1947. DAEF Box SW 3. Folder J.B. Rinaldo, SW Expedition 1947, Survey of Wet Leggett Site; see Martin et al. 1949).

The Expedition was firing on all cylinders. Assisted variously by Eloise and James Barter, Elaine Bluhm, Tod Egan, and others, Rinaldo and Martin excavated 31sites, ranging from preceramic (Wet Leggett Site) to early (SU Site, Promontory Site) and late pithouse sites (Turkey Foot Ridge Site, Twin Bridges Site), from small pueblos (Oak Springs Pueblo, South Leggett Pueblo) to later, larger pueblos (Apache Creek Pueblo, Higgins Flat Pueblo), and including sites with protohistoric components (Apache Cave).

Each year, for up to one month after the completion of excavations, professional staff remained at the Pine Lawn Tourist Camp to write up the season's findings:

> The Pine Lawn report statistics were done on slide rules and electric adding machines, the manuscripts were written on manual typewriters ..., photographs were developed in a rudi-

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> mentary dark room, and still a sleek, illustrated final report came out of Chicago before the next season started. I can't think of anyone else in the Southwest who achieved that [Eloise Barter, personal communication 2014].

Although 12 sites excavated between 1947 and 1955 were never published, one can argue that these sites were merely tested, not fully excavated (Nash 2006). Rinaldo (1956a) wrote, but never published, a short descriptive paper on six of these "minor excavations."

By the time they left the region in 1955, Rinaldo and Martin had documented regional culture history from ca. 1500 B.C. to A.D. 1300. They documented a possible intrusion of Ancestral Puebloan groups in the region, represented by small masonry pueblo sites and the appearance of Reserve black-on-white pottery. And they posited a relationship between the precolumbian Mogollon and the existing Zuni.

The Southwest Co-Tradition

Martin and Rinaldo (1951) published an uncharacteristically synthetic and theoretical contribution entitled "The Southwestern Co-Tradition," in which they attempted to use Wendell Bennett's notion of an "area co-tradition" to explain the broad arc of Southwestern prehistory (Bennett 1948). Although they never mentioned the Mogollon by name, it is clear that this article was yet another salvo in their attempt to establish the Mogollon as an independent entity:

> The groupings and listings given in this paper are not new; but we happen now to be in a better position to document our previous conjectures—namely, that there were uniformities and fairly even levels of cultural development throughout the Southwest. At about the beginning of the Christian era there were three cultures on the horizon, each readily distinguishable on stylistic grounds [Martin and Rinaldo 1951:227].

Hiroshi Daifuku (1952) published "A New Conceptual Scheme for Prehistoric Cultures in the Southwestern United States," which Reid (1986:7) describes as a "thinly disguised critique of the Mogollon distinctiveness question." Rinaldo (1952) published a single-authored, point-by-point response to Daifuku less than six months later. In so doing, he demonstrated the importance of the Mogollon debate in Americanist archaeology and the rapid publication pipeline of the time. Rinaldo "found himself essentially in accord" with Daifuku but felt compelled to critique and clarify the latter's presentation because his "outline is essentially similar to that outlined in [our] Co-Tradition paper [Martin and Rinaldo (1951)]" (Rinaldo 1952:580, 583).

The area co-tradition argument in print can be read as a proxy for the entire Mogollon debate. Archaeologists took sides in a special issue of *American Anthropologist* (Brew and Smith 1954; Wheat 1954). Irving Rouse (1954, 1957) took Martin and Rinaldo (1951) to task for what he viewed as their inappropriate application of Bennett's (1948) original co-tradition concept. Martin (1954) penned a single-authored rebuttal to Rouse in which he expressed "mild astonishment" at Rouse's critique, emphasized the pedagogical utility of the co-tradition concept, appealed to authority derived from fieldwork, and acknowledged that both inductive reasoning and hypothesis testing would remain useful techniques in future research.

It is worth emphasizing the extreme stylistic differences in the single-authored contributions Rinaldo (1952) and Martin (1954) published in the co-tradition debate. Rinaldo (1952; see also 1941a, 1950, 1964) made systematic point-by-point arguments focused on trait lists and archaeological data. He wrote as a sophisticated archaeological technician and expert culture historian (Lyman et al. 1997:128). Martin, on the other hand, was reflective and philosophical, if not metaphysical and emotional, and actively considered how different research methods would affect his ability to teach, to develop exhibits, and to apply archaeological discoveries to the solution of the world's problems (Martin 1954, 1971; see also Chazin and Nash 2013; Martin and Gregory 1973; Nash 2010). Where Rinaldo saw details in trees, Martin saw an existential forest.

Although it is possible that Rinaldo and Martin consciously took a divide-and-conquer, single-authored approach in order to strengthen their combined position, it seems more likely that their epistemologies were so different that they operated on separate cognitive, rhetorical, and scholarly paths, if not different realms entirely. Their divergent approaches proved complementary for more

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than two decades, but ultimately contained the seeds of destruction for their professional relationship.

Conjectures

Rinaldo and Martin published their first co-authored "conjectures" paper in 1950, presaging the New Archaeology by a decade. (Curiously, Rinaldo's [1941a] conjectures paper goes unmentioned except in the citations.) They wrote:

> For some time we have been interested in making historical reconstructions and in achieving a deeper understanding of a culture by inferring the type of social organization that was present in archaeological horizons for which no written records exist. We recognized that, lacking a quantity of materials with which to work, we had to assess the value of every tool of bone and stone and every potsherd. We were convinced that these poor remains of a culture were important and that each object represented a problem that had been solved by men in terms of their cultural pattern.
>
> But we wanted to go farther. Could we analyze our data in such a way that we could perceive trends, if any existed, in culture growth? Could we interpret these trends and raw data—such as location of house sites, kind and number of houses per time phase in a given area—to see if they would lead to probable inferences concerning social organization of the long dead Mogollon Indians? Would it be possible to utilize the analyses of fully described recent social systems to make guesses concerning the social structures of dead cultures? [Martin and Rinaldo 1950b:556–557].

In giving primacy to the data, the first paragraph embodies Rinaldo's approach and predilections; in seeking to go beyond the data, the second paragraph embodies Martin's growing displeasure with culture history as an end in itself. It also more closely fulfills the dictionary definition of "conjecture." A footnote indicates that the conjectures section was written with the help of George Quimby and Don Collier, both of whom were curators of archaeology at the Field Museum, and with whom Martin had recently published a best-selling textbook on North American archaeology (Martin and Rinaldo 1950b:556, Note 1; Martin et al. 1947). In the late 1940s, the trio taught a museum anthropology course at the University of Chicago, and Martin at least had audited Fred Eggan's seminar on Western Pueblo Social Organization (William Longacre, personal communication 2014). In these efforts, we can see Martin returning to his four-field anthropological roots at Chicago. Rinaldo was apparently not involved in these activities, either because he had recently returned from the war, because he was still formally only an "assistant in archaeology," because he was becoming more involved in the family business, or because he simply was not interested in that aspect of their work. The archives are unclear. Nevertheless, whereas Martin's prose and rhetoric slowly shifts towards the New Archaeology, Rinaldo remains methodological and rhetorically consistent over time.

Perishables and Stratigraphy

By the late 1940s, Rinaldo and Martin came to recognize that, in the absence of multiple tree-ring dates or a deeply stratified site, their reconstruction and interpretation of the regional Mogollon sequence in New Mexico could not be independently tested. Equally important, Mogollon perishable technologies were effectively unknown because only open-air sites had been excavated up to that time.

In 1950, they excavated Tularosa Cave, the largest of a series of caves in Tularosa Canyon east of Aragon, New Mexico (Martin et al. 1952). The 2.8-m-thick deposit was characterized by a remarkable degree of preservation and an occupation spanning at least 1,300 years from late Archaic to late prehistoric times. Excavations yielded more than 33,000 maize cobs, 1,700 pieces of string, netting, basketry, and wood, and 2,600 well-preserved animal bone fragments.

The Tularosa Cave site report is a classic in its detailed analysis and presentation of the artifacts; Rinaldo played a central role in its production (James Barter, personal communication 2014; Martin et al. 1952). Together, Rinaldo, Martin, and colleagues discerned three major shifts in material culture. The first occurred at the beginning of the Pine Lawn Phase (ca. 150 B.C.). The second occurred at the beginning of the San Francisco Phase (ca. A.D. 700). The third occurred at the beginning of the Reserve Phase (ca. A.D. 1000;

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Martin et al. 1952:496). While reluctant to identify a single cause for these changes, they pointed to diffusion as a causal factor in the introduction of pottery, which is not surprising given their emphasis at the time on culture history. They inferred an increased reliance on wild plants during the Georgetown Phase (A.D. 500–700), evinced by a 50-percent drop in the amount of maize present, as well as a diminished reliance on hunting after A.D. 700. The excavation of Tularosa Cave thus allowed a shift in analytical emphasis from purely descriptive proclamations towards a nascent cultural ecology. Also notable in the site report is their use for the first time of the term "culture process" (Martin et al. 1952:496).

Rinaldo and Martin received a boost in confidence when the chronology they established through superposition and seriation at Tularosa Cave confirmed the regional seriation they had derived over the previous decade. Unfortunately, they analyzed and published only the two deepest 2-x-2-m excavation units from the cave; more than a dozen more units remain only partially analyzed and unpublished.

Chasing the Mogollon II

Zuni Origins

In 1956, Rinaldo and Martin moved the Expedition headquarters to Vernon, Arizona, 60 miles west-northwest of Reserve. Just as he had done in the Reserve region a decade earlier, Rinaldo led the survey of the Little Colorado River valley to garner an understanding of regional culture history and to identify sites for excavation; he documented 95 of them. In his report, Rinaldo summarized his understanding of their recent research:

> The Mogollon sequence of the Reserve area was examined in detail over the course of more than a dozen seasons to provide an adequate sampling of this [region's] cultural development. Having reached a point of diminishing returns we turned our attention to a related area.
>
> We choose an archaeologically unexplored area which might yield data that would help in answering [what happened to the Reserve area population.] The area enclosed by the highway between Springerville, St. Johns and Vernon, [Arizona], seems to fit these specifications [Rinaldo 1956b:3].

He went on to describe their intent:

> In this search for the descendants of the prehistoric peoples of the Reserve area, the relationships between certain styles of pottery design made by these people and those made by the historic Zuni seem particularly significant.
>
> Pottery is not the only clue we have to this relationship. The resemblance between stone artifacts form the Reserve area such as the painted stone animal figures and painted stone disks [found at Higgins Flat Pueblo in 1953 and 1954] corroborate this sequence.
>
> Until we have excavated some of these Little Colorado sites, we cannot be at all sure of this relationship. *Until then the question will remain: to which of the historic western Pueblo cultures was the Reserve and Vernon area culture related?* [Rinaldo 1956b:3; emphasis in original].

These are clearly Rinaldo's words, not Martin's. The emphasis is on culture history. The prose is devoid of conjecture, particularly with respect to social organization, and there is no mention of issues germane to cultural ecology. While emphasizing artifactual data, Rinaldo used the phrase "point of diminishing returns," belying his dual life in business, for he had by then been serving on the board of directors of the family business for six years (Ramsey 2007).

In 1957, Rinaldo and Martin focused on excavations covering the full chronometric spectrum of regional sites, beginning with early pithouse sites (Vernon Survey Site 30, Vernon Survey Site 31) and lithic scatters (Little Ortega Lake Site, Laguna Salada Site), moving on to later pueblo villages (Phipps Site, Curtis Site), the latter of which remain unpublished (Martin and Rinaldo 1960a; Nash 2006).

They tackled even later sites in 1958 and 1959, focusing on Table Rock Pueblo, followed by Hooper Ranch Pueblo and the Mineral Creek Site (Martin and Rinaldo 1960b; Martin et al. 1961). They wrote:

> The evidence that is accumulating suggests the strong possibility that the remarkable flowering of the Hopi and Zuni cultures in the fourteenth and fifteenth centuries may have been due, in no small degree, to the influx of the vigorous, distinct, and desirable Mogollon

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ideas [Martin et al. 1961:168].

But they reversed causal mechanisms from push to pull:

> Commonly, we have assumed that the Mogollon peoples were forced, by some as yet unspecified reason or causes, to migrate from the Pine Lawn-Reserve area to the Little Colorado River Drainage and later to Zuni country. Perhaps they were asked by a disturbed community to join with it to create a more stable, purposeful society [Martin and Rinaldo 1961:169].

On the face of it, this is a curious statement, given the traditional emphasis in culture history on diffusion, migration, and emigration, not (necessarily) immigration (cf. Haury 1958). However, Martin spent a great deal of time during his career thinking about whether or not archaeological data could be applied to the solution of problems in modern society (Martin and Gregory 1973). He was convinced that they could and often published popular contributions to this effect (Nash 2010:112–115). Given the social ills and Cold War tensions prevalent in modern American society ca. 1960, the suggestion that a "disturbed community" might actively seek stabilizing forces elsewhere does not necessarily seem out of place in an archaeological monograph (cf. Patterson 1995). But it is clearly Martin's prose, not Rinaldo's.

A year after leaving the Field Museum, Rinaldo (1964) published "Notes on the Origins of Zuni Culture," thereby closing a scholarly loop. He examined ceramic, architectural, lithic, and settlement pattern data to make inferences regarding historical relationships between groups that he believed ultimately became the Zuni. The paper, summarized in the abstract, fits squarely within culture history:

> Surveys and excavations in the Zuni area have indicated that the culture of the earlier periods [i.e., Pueblo I and Pueblo II] in that district was broadly Puerco-Chacoan in character, and that it was strongly influenced during the late periods [i.e., Pueblo III] by a Mogollon culture that probably stemmed directly from the "budding off" of late Mogollon pueblos of the northern branches. Evidence for this relationship is outlined as having developed in three stages beginning with trade in pottery and axes of a Puerco-Chacoan character to the Mogollon villages and culminating in a period when the Zuni were making black-on-white and black-on-red polychrome pottery in the Tularosa style and building a characteristic type of rectangular kiva [Rinaldo 1964:86].

Gone is any attempt to hint at topics of importance to Martin, from conjectures on social organization to cultural ecology or the application of archaeological data to modern social ills. Instead, Rinaldo remained true to his data-driven style, constructing point-by-point arguments on the basis of trait lists and artifact frequencies.

The acknowledgments section of the "Zuni Origins" paper is fascinating. Rinaldo chose not to mention the Field Museum at all, despite the fact that all the reported research had been conducted under its auspices (Rinaldo 1964:94). He noted instead that he had presented the paper at a December 1959 meeting of the Wisconsin Historical Society in Madison and a December 1963 meeting of the Arizona Archaeological and Historical Society in Tucson. He noted that students in Haury's Seminar in Archaeology course at the University of Arizona considered the paper, but he failed to record the date; that probably occurred in late 1963. He tersely acknowledged that Martin and Di Peso had read early and late drafts of the paper, respectively, but this is the only time that Martin is mentioned in the article, aside from citations. Their relationship was indeed strained.

Grantsmanship

The National Science Foundation (NSF) began offering social science research grants in 1958 (Mazuzan 1988). Rinaldo and Martin received their first grant (G-13039) in early 1960 in support of James Schoenwetter's pollen analysis, Bill Longacre's archaeological survey, and the excavation of a small pithouse village, the Tumbleweed Canyon Site (Martin et al. 1962:3).

Rinaldo and Martin received their second grant (G-16006) in 1961 (continued by grant G-22028 in 1962). Rinaldo was primary author, so the proposed project lay squarely within culture history and the search for Hopi and Zuni origins. He included the New Archaeology's ecological predilictions almost as an afterthought:

> The proposed research is to be conducted in the

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> Upper Little Colorado River Drainage, in east-central Arizona, and will be concentrated on ruins assigned to an unknown culture, the Snowflake. Its origin, development, and dispersal may hold some of the keys to the development of Hopi and Zuni cultures, which apparently arose from a common ancestor in this area. Study of the Snowflake may also determine the prehistoric boundary between these two. Furthermore, the environment of the Snowflake was far different from that of neighboring areas, and a study of this culture may yield information about the adaptation of a culture to its ecological situation [DACF 1950?–1970? Box 5: NSF, Folder 1: NSF 1961 (G-16006), p. 2].

Grant proposals submitted after Rinaldo's departure in 1963 are more explicit in their focus on the tenets of the New Archaeology (Chazin and Nash 2013). The prose in Martin's successful 1965 NSF grant application (GS-245, continued by GS-984) is nothing if not a manifesto:

> The [Undergraduate Research Participation Program student] participants will see that they may make contributions (1) to anthropological methods by devising and testing hypotheses and techniques for obtaining adequate processual information concerning the extinct cultural systems under study; and (2) to anthropological theory by providing data on the dynamics of culture process and culture change. Thus, the students will realize that Archaeology *is* Anthropology and that they are conducting a scholarly, ethnographic study of extinct cultures. The emphasis will be on interpretation of prehistoric cultures and social organization rather than on mere description [ASM/FMMA, Folder A-1127: Undergraduate Research Participation Program; emphasis in original].

The rhetorical contrast with Rinaldo's 1961 application is extraordinary: students in the Undergraduate Research Participation Program in the late 1960s were not just digging rooms, they were testing hypotheses (Chazin and Nash 2013).

The New Archaeology

The seeds of change and the shift to the New Archaeology at the Field Museum were finally, truly sown in 1959 with the arrival of William A. Longacre. Longacre had been Elaine Bluhm's student at the University of Illinois at Champaign-Urbana; she introduced him to Martin. In 1959 and 1960, Longacre, not Rinaldo, conducted the Expedition's survey, focusing on areas in the Little Colorado River Valley and around Vernon that Rinaldo had previously avoided. Longacre's survey then moved north to the area around Snowflake, Arizona, and the Hay Hollow Valley (William Longacre, personal communication 2014).

When Binford arrived at the University of Chicago in 1961, Longacre quickly became a convert to, and acolyte of, the New Archaeology. Longacre assumed a new role as liaison, serving as a social, methodological, and theoretical linchpin between Martin and Binford. Martin explained the transition to Haury several years later:

> About ten years ago [ca. 1957], I felt that my theoretical approach to archaeology was deficient. I was in a rut and was going nowhere fast.... But when I reviewed my professional work of the last 30 years, I found that I had no basic guiding philosophy beyond that of seeking historical ends—events, dates, and the like. Good—but not good enough, because that is not Anthropology. *Culture history is important as a base on which to build; but it ought not to be an end in itself.*

He continued:

> About that time, several students of Binford [i.e., Longacre, John Fritz, James Hill, etc.] were working for me and they began to pry open my closed mental faculties. Bless them! I am forever grateful. I talked with them, listened, invited criticisms, [and] asked to be shown why I had been following a dead end. Their replies enraged me. I had a vested interest to protect—30 years of it—and I was afraid to admit I had not developed any epistemological approach worthy of the name [Letter 15 November 1967, ASM/FMMA Folder A-824: Personal: F, G, H; emphasis added].

Looking back, 1962 proved to be a watershed moment in the history of North American archaeology (Chazan and Nash 2013). The Expedition focused on three now famous sites that year—Broken K Pueblo, Carter Ranch Pueblo, and the Mineer Site. The shift in excavation strategies

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from those of traditional culture history to the radical New Archaeology are evident in the archives and in cataloging data.

In 1961, Rinaldo directed excavations at Carter Ranch Pueblo following a time-honored strategy. They inductively chose which rooms to excavate, and, without explicit consideration of sampling issues, they excavated by arbitrary levels, differentiating only between "fill" and "floor" contexts. In 1962, Longacre's influence, and by extension Binford's, is discernable. The crew excavated by natural levels, differentiating between contexts such as "ashy fill level" and "clay and sand level." The analytical implications of this shift in excavation strategy at Carter Ranch Pueblo have never been explored. The site report ignores the change, which is only visible when examining catalog data and archives on file at the Field Museum (Martin et al. 1964).

At Broken K Pueblo, the Expedition went further, implementing random sampling strategies and bond-and-abutment analyses. Longacre (personal communication 2014) remembers well a meeting of the minds at Broken K. He, Fritz, and Rinaldo stood next to the Expedition's brown van. While Fritz served as recording secretary, Rinaldo read and called out random numbers from Longacre's personal copy of *Tables for Statisticians* to decide which rooms to excavate. Presciently, Longacre recorded the auspicious and arguably iconic moment (Figure 4). For Longacre and Fritz, this was an exciting moment, a point of inflection in the history of North American archaeological method and theory (William Longacre, personal communication 2014). For Rinaldo, it was excruciating, and his bemused expression indicates that he participated only under duress (John Fritz, personnal communcation 2014). Rinaldo had more than two decades of archaeological experience and had never been concerned with sampling issues. Why on earth would he need a random number table, much less two precocious graduate students, to tell him which rooms to dig? But he still reported to Martin, who was deep in the throes of his public conversion to the New Archaeology. Rinaldo simply had no choice.

The Mineer Site (a.k.a. Longacre Survey Site 187 or the Vernon Site) is an extremely large lithic scatter with Folsom and Concho (i.e., Archaic) components near St. John's, Arizona. Exploratory

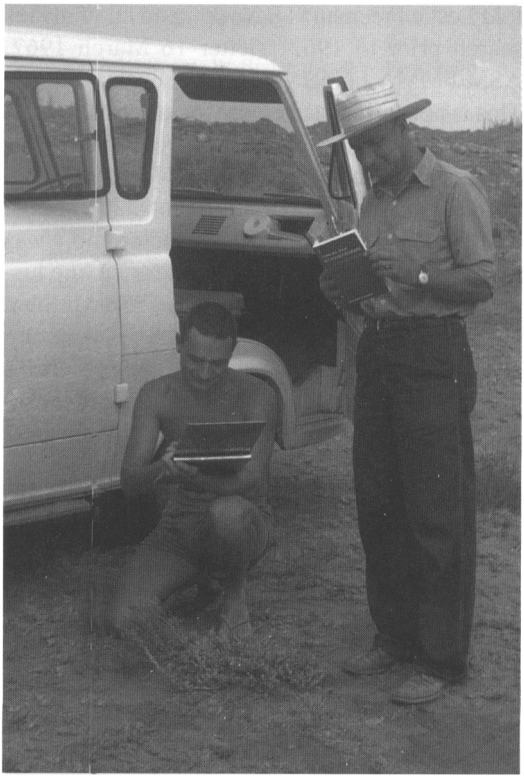


Figure 4. John Fritz and John Rinaldo using a random number table in Bill Longacre's brand-new copy of *Tables for Statisticians*, Broken K Pueblo, east-central Arizona in June, 1962. Rinaldo's bemused expression belies his distaste for the entire exercise, which was firmly grounded in the tenets of the New Archaeology, not the culture history that he preferred and that had served him so well over the previous quarter century. Uncatalogued photo, accession 2013–593, Arizona State Museum, University of Arizona, William A. Longacre, photographer.

test excavations revealed that the site was not stratified, but it was too large to surface collect in its entirety. Longacre, Fritz, and Roland Strassburger laid out a 4-x-4-m grid over the site and assigned 1,637 sequential numbers to the mapped squares. They used Longacre's random number table to selected 102 squares (6 percent of the total) for surface collection. The center point of each of the selected squares was located with a pin flag, the team then used a "dog leash" technique to create a radius and collect all artifacts from within each circle/square (Longacre and Graves 1976). Rinaldo would have none of it.

Looking back, culture history stopped being an acceptable goal for Martin by the late 1950s. By 1967, his conversion was so complete that he

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told Leslie Freeman "I have dumped all of my research prior to 1962" (letter 16 March 1967, ASM/FMMA Folder A-824, Personal: F, G, H). This is not a trivial statement for an archaeologist with four decades of experience. For Rinaldo, culture history remained an acceptable pursuit until the day he retired and, indeed, probably until the day he died. By the early1960s, there simply was no social or intellectual space left for culture history, and therefore for Rinaldo, in Field Museum archaeology. The New Archaeology's momentum was self-reinforcing and self-fulfilling; Martin's conversion was epiphanic and complete. Rinaldo fled, and Field Museum archaeology was never the same (Martin 1971; Nash 2003).

Conclusion

Returning to the question posed at the outset of this article: What were Rinaldo's primary contributions to North American archaeological knowledge, method, and theory? The archaeological understanding of the American Southwest, and particularly the empirical demonstration of the Mogollon culture as an independent entity, would not be as refined and complete as it is today in the absence of Rinaldo's systematic attention to detail and willingness to sacrifice the limelight for the laboratory. No other archaeologist had the range, breadth, and depth of Mogollon research experience that Rinaldo had between 1939 and 1962. Seen in this light, he is the quintessential culture historian.

Less obviously, Rinaldo's willingness to direct field research and oversee collections-based activity provided Martin with the freedom to embark on extensive exhibition and outreach activities, including writing for the Field Museum's popular audiences (see references in Nash 2010).

With respect to collections, the artifacts returned to Chicago each summer between 1939 and 1962 were more completely catalogued, analyzed, and published than those the Expedition collected after Rinaldo left. Given that archaeological excavation destroys the context in which artifacts were recovered, this is important. I have argued elsewhere that the research value of the artifact collections created under the auspices of the New Archaeology have been compromised by the use of non-intuitive provienence designations, inconsistent record-keeping, and a failure to fully catalog and publish the amassed collections (Chazin and Nash 2013; Nash 2014). Seen in this light, Rinaldo stands out as a curator's curator.

Why did Rinaldo resign a secure and productive position at the Field Museum at the peak of his career, when he was only 50 years old? Although Rinaldo and Martin's skillsets and work styles proved complementary for more than two decades, the juxtaposition of their divergent philosophies and epistomologies became increasingly awkward after Martin's public and emphatic conversion to the New Archaeology. Their relationship was doomed to fail, for there was little professional room for Rinaldo's traditional approach in the burgeoning, dynamic field actively being negotiated by progenitors of the New Archaeology. Rather than fight these changes, Rinaldo chose to flee, riding out his professional years at the Amerind Foundation, quietly making one last substantive contribution to the culture history of the greater American Southwest.

Why is Rinaldo, undeniably one of the most productive archaeologists of the mid-twentieth century, still underappreciated? Like Robin to Martin's Batman, the reserved and socially awkward Rinaldo remained in the shadow of his urbane and gregarious mentor and colleague (Nash 2001). As with Martin, part of the reason he is underappreciated is that they worked at a museum that did not have a formal university affiliation. They did not have access to a steady stream of graduate students with whom to build a school of thought, especially during Rinaldo's time there. Rinaldo published his research as monographs in the Field Museum's *Fieldiana* series, not in nationally distributed, peer-reviewed journals, which therefore reduced his visibility. Indeed, Rinaldo (1941a, 1952) published in *American Antiquity* and *American Anthropologist* only once each, early in his career. Finally, the bulk of Rinaldo's career is overshadowed by Martin and his colleagues' contributions to the New Archaeology, which involved large numbers of highly productive, prolific, skilled, and visible students and scholars who went on to productive careers of their own (Chazin and Nash 2013; Nash 2003, 2006).

More than two decades afte Rinaldo left the Field Museum, Haury failed to recognize Rinaldo's contributions:

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> Paul S. Martin's promptly reported findings in the Pine Lawn area of west-central New Mexico provided significant support for the maturing Mogollon concept by demonstrating that the remains of the group in question had territorial breadth and that once again a local chronology could be established [1985:viii].

Haury knew through personal experience dating back to 1939 that Rinaldo was a critical contributor to the production of those "promptly reported findings," that Rinaldo was the field crew leader in excavations that demonstrated the "territorial breadth" of the Mogollon, and that Rinaldo helped establish and confirm the Mogollon "local chronology." Haury's failure to acknowledge Rinaldo may perhaps be explained as a result of the latter's ancillary status with respect to Martin, by the fact that Rinaldo dropped out of scholarly visibility after his retirement from the Amerind in 1969, and by the fact that Martin and Haury were close friends and colleagues for nearly half a century. Haury's omission is nevertheless emblematic of Rinaldo's historical treatment by the discipline.

Rinaldo was a force to be reckoned with in the production of archaeological knowledge. Solid Christian Science and Midwestern values coupled with a Protestant work ethic, not to mention the regimentation of military service, imbued in him a sense of loyalty, duty, heirarchy, and solicitude that served Martin and the Field Museum well. It is debatable whether those qualities served Rinaldo well, for the asymmetric nature of his relationship with Martin meant that he failed to garner the recognition, much less accolades, that he deserves.

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