

ALFRED VINCENT KIDDER, 1885–1963

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ALFRED VINCENT KIDDER, generally regarded during most of his professional life as the foremost American archaeologist, died at his home in Cambridge, Massachusetts, on June 11, 1963, at the age of seventy-eight. Renowned as a master of field archaeology, scholar of prehistory in two major fields, writer, and administrator, he was also one of the most beloved men of his profession. Kidder was a pioneer in the use of stratigraphy and typological analysis in this country, the first great synthesizer of American archaeology, and designer of Carnegie Institution's famed program of multidiscipline research in Middle America.

He was born in Marquette, Michigan, on October 29, 1885. His father, Alfred Kidder, a mining engineer, and his mother, Kate Dalliba Kidder, moved east when he was still a boy. He attended Brown and Nichols School in Cambridge until 1901, when the family went to Europe and placed him in school at La Villa, Ouchy, Switzerland. The fluent French that Ted acquired there later proved most useful — especially during World War I; he also learned to play soccer in Switzerland, and he was to be a member of the first soccer team at Harvard. On his return to the United States he attended Noble and Greenough School, and in 1904 entered Harvard College. As a youth he had a strong interest in birds; his first published paper was in ornithology, written at the age of fifteen. He was a member of the Nuttall Club, an ornithological society in Cambridge, and this interest continued throughout his life. Extracts from a diary of his first trip West in 1907 (published in *The Kiva* in 1960) are sprinkled with observations of the birds he spotted — black and white Magpies, Canyon Wrens, Western Meadow Larks, Mourning Doves (“I have never seen birds that flew with darting plunges of such terrible swiftness”), Road Runners, and “the far-off mewing of a flock of Pinyon Jays.” The hobby certainly influenced the policy he urged many years later, that lengthy archaeological data be

presented in the more concise, condensed form used in ornithological descriptions.

Although he first came in contact with anthropology through books in his father's library — he specifically mentioned the Smithsonian and other government reports, Catlin, and Stephens' *Incidents of Travel* — and as a boy had hunted arrowheads with his father, it was not until 1907, his junior year at Harvard, that it became a serious interest destined to cancel his plans for a career in medicine. The year before (1905–06) he had enrolled in Roland B. Dixon's Anthropology 5, on the American Indian; he always maintained that he selected this course solely because it did not meet until nine in the morning and was so scheduled as to leave his Saturdays free. To judge from this and other anecdotes that he told on himself (see his *Kiva* reminiscences), Ted's undergraduate days were as typically light-hearted as those of the average Harvard undergraduate.

He also took A. M. Tozzer's Anthropology 9, Maya Archaeology and Hieroglyphics, the second year that it was offered. Tozzer once wrote that Sylvanus Morley, who had taken it the year before, received an A-minus grade, whereas Kidder got an A, something the great Mayanist and epigrapher was apparently never allowed to forget in later years. Kidder said that it was in the spring of 1907 that he “found and for the first time really talked with [these] two life-long friends and close associates: Alfred Tozzer and Sylvanus Morley” — in the former's room in Thayer Hall, where he and Vay had gone in response to a notice in the *Crimson* that three anthropologically trained volunteers would be considered for an archaeological expedition to the Southwest under E. L. Hewett of the Archaeological Institute of America. A third undergraduate, John Gould Fletcher, later well known as a poet and author, also applied, and all three were accepted.

Kidder had never been West. Through his *Kiva* reminiscences we can share with him the



A. V. Kidder at Point of Pines, Arizona, 1952.

wonder and excitement of his first great adventure in archaeology there, also summarized in a tribute to Morley that he wrote for *El Palacio* in 1948:

Tenderer tenderfeet never followed Horace Greeley's advice. We met Dr. Hewett, after a sixty-mile wagon ride from Mancos, Colorado, at "Moke Jim" Holly's ranch in McElmo canyon close to the Utah line. It was a three-room adobe in a little patch of alfalfa, the ultimate outpost on the long desert road to the little Mormon town of Bluff City on the San Juan. We slept in the lee of the Hollys' haystack. Next morning Dr. Hewett — and what a foot-traveller he was in those days — tramped us miles down the blazing hot canyon. We panted after him up the mesa at the McElmo's junction with the Yellow Jacket. From its towering prow we could see Mesa Verde and Ute Peak in Colorado; the Abajos and the distant Henry Mountains in Utah; the tall, red buttes of Monument Valley and the blue line of the Lukachukais in Arizona. None of us had ever viewed so much of the world all at one time, nor so wild and barren, and broken a country as lay about us.

Dr. Hewett waved an arm. "I want you boys," he said, "to make an archaeological survey of this region. I'll be back in six weeks. You'd better get some horses." We thought, looking it over, that maybe we had.

Dr. Hewett, in one of his books, has said that he set us this appalling task to try us out. And it was a trial. I wish that Morley could have written the story of those next weeks, for there never was a better raconteur and the tale

is worth telling: of our struggles to hitch the team of horses we hired and to keep the ancient wagon from falling apart; of our abandonment of that vehicle and our purchase of three mares, each with a shambling colt; of our amateur efforts to survey the maze of canyons with a small pocket compass and to map and describe the many ruins we found.

... I could go on and on about that season: our weeks on the Mesa Verde, our later excavations at Puyé. It brought, for both Morley and me, some valued and lasting friendships: Jesse Nusbaum, who joined us on the Mesa; Kenneth Chapman, whom we met at Santa Fe; Santiago Naranjo, Juan González, Julián and Crecencio Martínez of our Indian workers on the Pajarito.

Even as a college boy, Kidder was impressive in appearance and bearing. Neil M. Judd, who also wrote an article of reminiscences for *The Kiva*, describes him: "Following our return to Bluff City we camped for a few days at the mouth of Cottonwood Wash, a short distance below town. It was there I first met A. V. Kidder, my first real archaeologist. Fresh from Harvard, erudite, and of magnificent physique, Kidder impressed all of us."

Kidder went to New Mexico thoroughly intending to become a doctor, but returned in the autumn a confirmed archaeologist. He concentrated in anthropology his senior year under Pro-

fessors Dixon, Farabee, and Tozzer. In a conversation with John H. Rowe in 1955, the notes on which Rowe has generously made available to me, Kidder said that he also took half a course with F. W. Putnam, and Putnam commissioned him to dig a shell heap near Carpinteria, California, for the other half of the course. He graduated in 1908 and returned to the Southwest the following summer, to work at the Rito de los Frijoles site in New Mexico, and then to dig on Alkali Ridge for the University of Utah. Judd's *Kiva* article tells of this work, and of how Kidder "cultivated a beard — the most luxuriant beard it is possible to imagine — a magnificent beard curly as a bull buffalo's shoulder and bronzed by the Utah sun."

The following year he accompanied his family to Egypt and Greece, spending the winter on the Nile and visiting various excavations. He told John Rowe that he saw nothing particularly stimulating there in archaeological method, but he did hear a lot about Reisner, and when Reisner's course in field method was offered at Harvard, Kidder took it — "a dandy, well thought out and very logical. I never enjoyed a course so much." Kidder said that Reisner explained the aims of archaeology and how to attack a problem, how to determine a culture's chronological relations to other cultures and its trade contacts with its contemporaries, gave a lot of stratigraphic theory, recommended leaving test columns or sections for later checking, explained the proper disposal of backdirt, taught a classification of various kinds of debris, described details of cataloging, and discussed "the organization and housekeeping problems of an expedition." Rowe writes: "One of the things that Kidder is important for in the history of archaeology is that he introduced Reisner's standards and Reisner's methods to the New World. The documentation of this fact is probably the most important result of my long and of course very pleasant interview with him in 1955."

The year 1910 was memorable for Kidder. He was married that year to Madeleine Appleton, thereafter his lifelong companion at home and in the field, collaborator with him in much of his research (they co-authored an article in 1917), and mother of their five children, Alfred Kidder II, Randolph, Barbara, Faith, and James. This was also the year of Kidder's first published article in archaeology, his report on the 1908 explorations in southeastern Utah, which was followed by another in 1913 and, with a few exceptions,

at least one a year — often several — thereafter for over forty years. In 1910, at F. W. Putnam's request, he also went to Newfoundland and Labrador, he told Rowe, to check Merritt Fernald's theory regarding the location of Vineland. A site he dug in Labrador turned out to be a sixteenth-century Breton settlement. In 1910, he was named Austin Teaching Fellow at Harvard, and he began some research that same year for the New Mexico Territorial Museum. He received his A.M. degree in 1912, his Ph.D. in 1914.

During the summer of 1914, Kidder, with Samuel J. Guernsey and assisted by Charles Amsden, then a student of archaeology, carried on investigations for the Peabody Museum of Harvard in the Kayenta district of northeastern Arizona. This archaeological field had been opened by Byron Cummings in 1908. The Kidder-Guernsey expedition reached Kayenta by wagon via Shiprock, New Mexico, and the trading post of Teecnuzpos. Another trading post at Kayenta served as expedition headquarters, and the men returned to it periodically from their camp in the Monument Valley district to the north. In the middle of the season the water supply in this parched land failed, and the expedition moved to the Skeleton Mesa and Marsh Pass regions.

From their excavations and the great quantity of artifacts recovered (including the famed sunflower cache), Kidder and Guernsey recognized two distinct cultures, the "Cliff-house" or "Kiva" culture, and the "Basket Maker." There was evidence also of a third, the "Slabhouse culture." All the cliff dwellings and pueblos examined were enough alike in architecture, kiva construction, and pottery to persuade Kidder and Guernsey that these should be assigned to a single "culture period," but they saw variations in pottery between certain groups of ruins. In the conclusions to *Archeological Explorations in Northeastern Arizona* (1909), the authors distinguished between the San Juan district of the great northeastern "Kiva-culture," with Chaco Canyon and Mesa Verde-McElmo subdivisions; the Montezuma Creek, possibly to be classed with Mesa Verde; the Aztec-Bloomfield, allied architecturally to Chaco Canyon and ceramically to Mesa Verde; and the Kayenta. They noted that this very general classification of San Juan sites "leaves unaccounted for the great and important mass of remains in the Can-

yon de Chelly and the lower Chinlee," with which they were as yet largely unfamiliar.

Kidder and Guernsey described the differences that distinguished Kayenta pottery and kiva construction and discussed comparative data. They suggested that the Slabhouse remains were intermediate in time between the developed Cliff-houses and the Basket Maker habitations, and noted similarity between Slabhouse black-on-white pottery decoration and that of some of the Chaco Canyon sites. They stated that the remains from the Kinboko caves and Sayodneechee were "products of a culture different from that of the cliff-dwellings and pueblos of the region" and linked them with the Utah Basket Maker objects so named by the Wetherill brothers and, later, by Pepper. They confirmed Pepper's description of Basket Maker culture, amplified it with a table contrasting Cliff Dweller and Basket Maker traits, and offered additional evidence to support their view that Basket Maker culture was the older of the two. Finally, they discussed the relationship between these cultures and the geographical range and somatology of the Basket Maker people. "The influence of Kidder and Guernsey's *Archaeological Explorations in Northeastern Arizona*," W. W. Taylor wrote in 1948, "has, I believe, been second to none in the development of Southwestern archeology."

Although Kidder did not list his *Pottery of the Pajarito Plateau and of Some Adjacent Regions of New Mexico* (1917) among his major publications in *Who's Who in America*, this chapter from his Ph.D. dissertation was a significant contribution of the time. Bandelier and Hewett had recorded the location and general appearance of Pajarito Plateau ruins west of the Rio Grande in Sandoval and Santa Fe counties but had given the pottery only summary treatment. Kidder's paper first distinguished between the small scattered dwellings and the large pueblos and described the pottery separately for each. The ceramics were fully described, but the report is outstanding for its attention to decorative patterns and motifs, probably thanks to the feeling for style that he told John Rowe he had obtained from George Chase's course in Greek vase painting at Harvard. Kidder concluded that black-on-white ware was the oldest of the four ceramic groups from the plateau, although there was as yet no stratigraphic proof of this; that "Agua Fria Schoolhouse" was (tentatively) second in point of age, and a forerun-

ner of Pajaritan or possibly contemporaneous with it; that "Frijolito" pottery was more closely allied to standard Pajaritan than was Schoolhouse (he enumerated the similarities and differences), but could not have been contemporaneous with it. He discussed the range and foreign relationships of all these wares through time and space. Kidder was intrigued with the fact that the very unlike redware and biscuitware existed side by side along the Rio Grande and that one or the other predominated at certain sites. He offered the following possible explanations: (1) There was a mixing of two peoples or groups of clans who possessed independently developed systems and who held to them after amalgamation. (2) There were different centers of production, and the pottery was distributed in trade. (3) One of the wares played some special role in the ceremonial or mortuary customs of the people and therefore held to a more archaic style or developed along different lines. (4) Dissimilarities are due largely to technique and divergent evolution from a common earlier form. Although Kidder is best known during this early period for his stratigraphic and typological techniques and space-time reconstructions, here we see what may be even more remarkable for the time: hypotheses regarding the sociocultural significance of ancient pottery.

Until now, Kidder told Rowe, he had not been able to use stratigraphic principles because the only two sites he had dug in the Pajarito were each of a single period. Thus he was forced to obtain chronological results through typology. As Richard B. Woodbury has noted, he had visited N. C. Nelson in the field during his Galisteo work, where Nelson demonstrated so effectively the value of the stratigraphic method that had been only occasionally employed in America. Later in life, Kidder spoke and wrote frequently about the use of this approach by Nelson, Richard Wetherill, and Manuel Gamio. Thus, when Kidder visited Pecos for the first time and realized that "the broken pottery that lay scattered about the mounds indicated that Pecos had been inhabited for a great many years before the coming of Europeans" and, indeed, that there was "no known ruin in the Southwest which seems to have been lived in continuously for so long a period . . . it gave rise to the hope that remains would there be found so stratified as to make clear the development of the various Pueblo arts, and thus enable us to place in their

proper chronological order many other Southwestern ruins whose culture had long been known, but whose time-relations one to another were still problematical." Furthermore, Kidder noted, Pecos had been a large pueblo, occupying a commercially strategic position near the edge of the buffalo plains, and thus should provide chronological evidence for cultures well outside the Pueblo region. "From the point of view of specimens also the site was a favorable one, because its large cemeteries had never been despoiled, and the graves promised a rich harvest of skeletal material and mortuary offerings. Lastly, the survivors of Pecos had taken refuge at the pueblo of Jemez, where their immediate descendants were still living; and investigations among these people could hardly fail to reveal much of value as to the language, customs, and ceremonies of the old town." Here we see another facet of Kidder's early interests: the ethnohistorical approach to archaeological interpretation that Duncan Strong was to employ so successfully many years later in Nebraska.

In 1915, when the Trustees of Phillips Academy, Andover, Massachusetts, decided to undertake excavations in the Pueblo area and invited Kidder to carry out the investigation, he recommended Pecos as the site to dig. Work was begun in June, 1915.

Pecos ruins lie in the valley of the Pecos River in northeastern New Mexico, about 7000 ft. above sea level, and occupy the top of a long, rocky ridge, at the southern end of which are the ruins of a church and monastery. They are so extensive that, as Kidder pointed out, the first six field-seasons spent there sufficed to clear only about 12 to 15% of its total area. At places the ruins lay three and four deep. The bulk of the rubbish had been "thrown down-wind over the abrupt eastern edge of the mesa, where it gradually formed an enormous sloping midden nearly a quarter of a mile long, and, where the original bluff was highest, over twenty feet deep." In the course of excavations, nearly two thousand graves were encountered and "literally millions" of sherds were taken from the rubbish.

Particular attention was devoted to pottery. Kidder recognized eight chronologically sequent groups, which he named for the most characteristic decorated ware in each: Black-on-white, Glazes I-VI, and Modern. This classification, with minor changes and additions, is still in use. Pecos was founded toward the close of the period when Black-on-white was standard; the dis-

covery by Coronado in 1540 took place early in Glaze V times. One could write at great length on the masterly techniques of excavation that Kidder devised and experimented with at this enormous and complex ruin. He was careful to sample it at many different points, realizing that its habitation patterns had shifted frequently, and he confronted and solved innumerable stratigraphic problems produced by such factors as height, slope, wind, and settling, as they complicated the arrangement of rubbish the ancients had tossed in small amounts over the edge of the *mesilla* and its defense wall.

Excavations were carried on in 1915 and 1916, suspended from 1917 to 1919 (World War I), resumed in 1920, and continued to 1929. Digging was also done at the Forked Lightning Ruin (a pre-Pecos Black-on-white site), at a ruin near Rowe, at Dick's Ruin (the last two are later Black-on-white villages), and at a Glaze I pueblo on Loma Lothrop. Carl Guthe was on the Pecos staff from 1916 to 1920; at one time or another S. K. Lothrop, E. A. Hooton, G. C. Vaillant, S. P. Moorehead, C. A. Amsden, Isabel Kelly, Jesse L. Nusbaum, Monro and Theodore Amsden, and I worked there. Mrs. Kidder cleaned and sorted the enormous quantities of potsherds, in addition, of course, to supervising the camp domestic staff and keeping track of her five children, or perhaps I should say six, for they made me a member of the family. For us kids, there was a corral with four horses; there was also Old Blue, an ancient T-model Ford, and, the year I was there, a brand new black 1927 model, which, the day it arrived, everyone circled suspiciously and regarded with great misgivings — it was far too low and racy, it would surely never negotiate deep sand and ruts.

Kidder was in Santa Barbara, California, in 1916, working on Basket Maker materials, and there he enlisted in the Army. He went through Officers' Training School at the Presidio in San Francisco, was commissioned a first lieutenant of Infantry in 1917, assigned to the 91st Division, A.E.F., and went overseas with the northwestern unit. He engaged in divisional intelligence work; his fluent French, learned during his school days at La Villa, stood him in good stead in his liaison with the French. He participated in the actions at St. Mihiel, Argonne-Meuse, and Ypres-Lys, was promoted to captain in October, 1918, and was awarded the degree of Chevalier, Légion d'Honneur. Kidder was typically reticent about his war experiences. The only anecdote

I was ever able to extract from him was his brief comment that he was once up in the attic of an old French chateau, peering through a window and trying to direct artillery fire without the help of a map, when he happened to find an old French geography book lying in a corner with other books and papers; he paged through it and found an ancient map of this very area which was still so accurate on landmarks that he successfully used it to pinpoint targets. One other story, which Kidder did not tell me but which I learned quite by accident from a friend, was that he found Louis Douglas, later our Ambassador in London, lying sick in a dugout. Since it was very damp there, Kidder moved him to an adjoining dugout, and about five minutes later a German shell completely destroyed the one they had just abandoned.

The first of the famous Pecos conferences was held there in 1927. Like most affairs that Kidder presided over, this was most informally run. I still treasure a snapshot I took — with an old box camera — of A. E. Douglass, his tree-ring chart propped on the ground, talking to the group seated on the ground in a clear space between the piñons. Here the first systematic classification for Southwestern prehistory was worked out, a construct still widely followed, with some modifications, for the northern part of the area.

Besides the progress reports and a preliminary description of the ceramics (M. A. and A. V. Kidder, "Notes on the Pottery of Pecos," 1917), eight major publications came out of the Pecos project. The first was the work for which Kidder will be longest remembered, his *Introduction to the Study of Southwestern Archaeology, with a Preliminary Account of the Excavations at Pecos* (1924). This was followed by Guthe's *Pueblo Pottery Making* (1925), Parsons' *The Pueblo of Jemez* (1925), Hooton's *The Indians of Pecos* (1930), Kidder and Amsden's *The Pottery of Pecos: Vol. I, The Dull-Paint Wares* (1931), Kidder's *The Artifacts of Pecos* (1932), Kidder and Shepard's *The Pottery of Pecos: Vol. II, The Glaze-Paint, Culinary, and Other Wares* (1936), and finally, over twenty years later, Kidder's *Pecos, New Mexico: Archaeological Notes* (1958).

Kidder's *Introduction to the Study of Southwestern Archaeology* was a pioneer work of synthesis, making order of the chaos of scattered Southwestern archaeological data. It was designed to give a brief description of Pecos and its history, to outline the work so far accomplished,

and to provide a background for the more specialized monographs to follow. The bulk of the work, however, was devoted to a general account of Southwestern archaeology. Kidder's Viking Fund Award citation in 1947 called this book "the first comprehensive analytical and interpretative study of the archaeology of the Southwest, or of any region in America." As W. W. Taylor commented, subsequent work "has but filled out, and in some spots corrected or sharpened, those outlines which he drew with such remarkable insight." Emil W. Haury's opinion is that "By and large, Ted's constructs of Anasazi prehistory still hold. Elaborations, of course, have been made." Erik K. Reed says, "*Introduction to Southwestern Archaeology*, especially the edition with Rouse's introduction, is still the only work that can be recommended for general reading covering most of the Southwest." These statements from authorities in Southwestern archaeology are truly remarkable, for they refer to a work now over 40 years old. This is by no means to belittle the great contributions made since the 1920's, or to imply that Kidder's constructs have been added to only in minor details. Major new cultures, such as Hohokam and Mogollon, have been recognized, even though their precise genetic relationships with Anasazi and their relative stature as cultures continue to be debated.

Long after he had left the Southwestern field as an active investigator, Kidder kept abreast of developments there and ventured his own opinions of them, usually in prefaces or introductions to publications by others or in book reviews. In his 1932 introduction to *The Swarts Ruin* by C. B. and H. S. Cosgrove, he noted that certain students, himself included, had thought of the culture of the Gila as "a sort of outlying and bastard blend of Southwestern and Mexican elements," but he called attention to the work of Cummings, Gladwin, and Woodworth, which seemed to show "that the Gila, and perhaps also Chihuahua, must be reckoned with as the theater of a development perhaps quite independent, and one which, in Pueblo IV at least, may have exercised no little influence upon its northern contemporaries." Again, in a 1954 review of *Mogollon Cultural Continuity and Change* (by Paul Martin and others), he wrote, in typically mock-rueful vein: "In the concluding section one senses a sort of protective attitude toward the [Mogollon] people in whose former homes Martin has labored so long and so profitably.

This I can easily understand, for as Haury, Gladwin, Martin, and Rinaldo have knocked the props one by one from under my beloved San Juan Basketmakers as the originators and disseminators of all higher Southwestern cultures, I have found myself acquiring a not dissimilar attitude in regard to them." He was not completely convinced that Mogollon, even during its early phases, was "as sharply and fundamentally differentiated from Anasazi or Hohokam as its proponents have held." He felt that to some extent the difference of opinion was really only one of nomenclature, but "if a taxonomic arrangement be insisted upon, I incline to follow Erik Reed and class Mogollon and Anasazi as subgroups of Pueblo." Martin and Rinaldo did not feel that such a classification was possible for remains prior to A.D. 700; they believed that it could be quantitatively demonstrated that a closer relationship existed between Mogollon and Hohokam before this date than between Mogollon and Anasazi.

Although his later career with the Maya program of Carnegie Institution of Washington occupied most of his time from 1929 on, Kidder never lost interest in the Southwest. The introduction to *The Swarts Ruin* suggested, among other things, that the efflorescence of Mimbres pottery should be attributed "to the influence of a single potter, whose work was so outstanding that during her lifetime she was able to overcome the conservatism of her contemporaries and to found a school of brilliant decorators. . . . Mimbres ware has to me all the earmarks of an abrupt and profound artistic mutation. And how otherwise than by the creative effort of an individual can any artistic mutation be brought about?" In 1939, he wrote "Notes on the Archaeology of the Babicora District, Chihuahua." In 1933, Kidder and the Cosgroves spent five weeks at the Pendleton Ruin in extreme southwestern New Mexico, a site which appeared to be a meeting point of several prehistoric groups, and promised to be a northern extension of the Casas Grandes culture which centered across the Mexican border directly to the south in Chihuahua. Cosgrove's untimely death and other events forced the abandonment of the planned early publication, but in 1947 Kidder and Mrs. Cosgrove resumed work on the data, which were published in 1949 under the joint authorship of these three old friends. The same year Kidder wrote a preface to C. A. Amsden's *Prehistoric Southwesterners from Basket Maker to Pueblo*.

In 1951, he brought out an article on whistles from Arizona, and in 1954, the review of *Mogollon Cultural Continuity and Change*, which I would not ordinarily mention here save for the fact that it contains a lengthy discussion of Kidder's own views on the origin, development, and relationships of Anasazi, Hohokam, and Mogollon, and demonstrates his emphasis on environment and foreign contacts as shapers of these cultures. Anasazi became accommodated to a very special arid, mesa-canyon environment and was "singularly resistant to exterior stimuli; it diffused rather than received." Hohokam, with similar origins, "was shaped by its game-poor, desert environment which, because of increasing population made possible by the introduction of maize, demanded the development of elaborate systems of irrigation. It received powerful stimuli from Mexico; it radiated relatively little to its northern and eastern neighbors." Mogollon's response to a less challenging environment was relatively weak but may have been instrumental in transmitting Formative traits from Mexico to its neighbors. Finally, in 1958, appeared a substantial volume, *Pecos, New Mexico: Archaeological Notes*, which reported on the excavations at the Forked Lightning, Dick's, and Arrowhead ruins, and various matters about Pecos Pueblo, especially its kivas, together with comparative data from modern Southwestern kivas.

To judge from the frequent nostalgic references to the Southwest in his writings, from the annual Thanksgiving dinners in Massachusetts, to which he invited all available members of his beloved "Sagebrush Clan," from the delight he took in visits to Haury's camp at Point of Pines and other short visits to the Southwest, Kidder never abandoned his first professional love. He became, of course, tremendously interested and involved in Mesoamerican archaeology, and he enjoyed the years that he spent in Guatemala — particularly so since his daughter Barbara made her home there — but he never wrote more movingly than when describing a Southwestern landscape. His pleasure and excitement could not be concealed in his preface to *The Pendleton Ruin*, where he told of a brief return to Southwestern research:

The present paper, as a matter of fact, would probably never have been finished had it not been for a busman's holiday in the Southwest that my wife and I took in the summer of '47. We visited E. W. Haury's University of Arizona Point of Pines field school on the San Carlos Indian Reservation in Arizona and later were driven by J. L. Nusbaum up into the San Juan country. That was a

memorable month. At Point of Pines we watched the gray curtains of the summer storms trailing far off across the Big Prairie, savored the unforgettable smell of the first rains on the parched Arizona soil. In the north we saw again the brown walls of Pueblo Bonito, Cliff Palace brooding under its vast sandstone arch, the strange rock-perched towers of Hovenweep. It was all most upsetting, because for years I had been struggling to get the Southwestern virus out of my system — and here it was back again worse than ever. And at Haury's diggings good honest Southwestern pottery — not fancy Maya stuff — was coming out of the ground. I had a wonderful time with those sherds, particularly the corrugated ones. Some of them seemed to be like pieces I dimly remembered from the Pendleton site. So, back in Cambridge, I asked Mrs. Cosgrove to get out our old collections. That started us off. We went over the field notes, the plans and photographs. The first thing I knew I had temporarily ditched the Maya and was happily at work with Mrs. Cosgrove in her room in the Peabody basement.

And this from his preface to *Pecos, New Mexico: Archaeological Notes*:

The botanically meticulous will notice, I fear with displeasure, that throughout I have spoken of cedar rather than juniper. Cedar or its Spanish equivalent was always used by us and our workmen, and by the staff in all field notes. As a matter of fact, there grow in the valley at least two species of whose proper names, either English or Latin, I was and still am light-heartedly ignorant. A probably even greater taxonomic sin has been my mulish balking at *ponderosa*. Yellow pine it has been to me since first I came to know it on the Pajarito, just a half-century ago, to revel in its beauty, its fragrance, the souging of the wind in its branches. To change would seem like treachery to an old friend.

Pecos, New Mexico closes with these words:

... I was not nearly as wrong as was he who advised me, just 50 years ago, to take up work in another field because, he said, "The Southwest is a sucked orange."

I only wish I could return to that wonderful country and wet my aged lips once again in the rich juice of a fruit which a half-century of research has little more than begun to tap.

In the late 1920's, Kidder began to accept more and more professional affiliations and administrative posts. He became a research associate of the Southwest Museum, Los Angeles, and later served on its advisory committee. He was elected chairman of the board of the Laboratory of Anthropology in Santa Fe. In 1925–26, he was vice-chairman of the Division of Anthropology and Psychology of the National Research Council and became chairman for 1926–27. This was purely a public service, but it broadened his administrative experience and kept him in close touch with research centers and affiliated societies of NRC throughout the country. Among the anthropological members of his committee were

Hrdlička, Judd, Lowie, Nelson, Spinden, Laufer, and MacCurdy. Kidder also enjoyed his Cosmos Club membership; on returns to Washington for many years thereafter he was a frequent visitor at the club's old headquarters on Lafayette Square.

During his chairmanship, the Division continued its investigations of human migration, mental and physical development of young children, research on the American Negro, college psychological tests, and national intelligence tests. Kidder prepared a subject-and-author index of the then 40 volumes of the *American Anthropologist* and the Division's annual summary of archaeological field work in the United States and Canada. Plans were made for a ceramic repository at the University of Michigan. I used to tell AVK, though, that by far the most important decision he made during this assignment in Washington was in response to a letter from my father, who had written a friend, Vernon Kellogg, then permanent secretary of the Council, that his seventeen-year-old son was anxious to get on an archaeological expedition — or any expedition. Kellogg showed the letter to Kidder, who, to our astonishment, took me on, sight unseen, to go with him to Pecos the following summer. No one could have had a more wonderful introduction to archaeology, and for me it was the beginning of a lifelong friendship with the Kidder family.

The vigorous persuasion of S. G. Morley had led the Carnegie Institution of Washington to enter the field of Maya archaeological research, and for several years Carnegie had been expanding its program there, with major excavations in Yucatan, Guatemala, and Honduras. As the program became increasingly complex, the Institution felt the need to give it a more formal administrative direction. Kidder was the obvious choice to head the program, even though it meant a drastic switch of subject fields. He had finished 15 years of excavation at Pecos; gradual entry into a new enterprise would leave him time to work up the unpublished data. Nevertheless, in spite of its challenge, the decision must have been a difficult one; at the least it meant beginning a new career at the age of forty-two. Kidder accepted, on the condition that he could continue work on the Pecos materials as a joint project of Carnegie Institution and Phillips Academy; to judge from his *Carnegie Year Book* report for 1929–30, he still hoped to remain active in the Southwestern field, "to continue

study of Pueblo remains of the upper prehistoric horizons, paying particular attention to the interrelation of the specialized subcultures." He expected his work to interlock with that of Earl Morris, also on the Carnegie staff, and they hoped "ultimately to undertake a series of technological comparisons which are much needed for accurate delineation both of the chronological periods and the culture areas of the Southwest." He thus meant to continue the trend toward synthesis that, along with stratigraphic methodology, had been his greatest contribution to Southwestern archaeology.

From 1927 to 1929, he was "Associate of the Carnegie Institution of Washington in charge of archaeological investigations." In 1929, the Board of Trustees created the Division of Historical Research, consisting of three sections: (1) The Section of Aboriginal American History, which concerned itself "with studies relating to the rise of native civilization in the New World, its two principal fields being the Maya area in Mexico and Central America, and the Pueblo area of the southwestern United States"; (2) the Section of Post-Columbian American History, which conducted research on the growth of Western European institutions in the Americas; and (3) the Section of the History of Science, which brought "together and made available for interpretation the at present widely scattered and uncoordinated data which bear upon the acquisition and transmission of ordered knowledge." One of Kidder's first administrative acts was to call a conference at Chichen Itza, Yucatan, to take stock of Maya research and to plan for the future. It seems apparent that from the beginning of his administration he had two main goals. First, although he realized that restoration and repair of the spectacular ruins Carnegie excavated should continue — indeed it was required in contracts with the Mexican and Honduran governments — the whole emphasis of the program should, as J. E. S. Thompson expressed it, be shifted to the concept of stratigraphy as the key to history. He also advocated comparative studies to establish relative chronology throughout the area as a whole. "Through his work and that of his pupil, Vaillant," says Thompson, "the new methods took hold in Mexico, and the whole framework of Mesoamerican history, as we now have it, is the result. This is a mighty monument to the work and influence of one man." Second, he wanted to amass a great corpus of information on the

Maya, ancient and modern, and on their habitat, by enlisting the research services of scholars not only from the disciplines devoted to the study of man but also from many biological and physical sciences as well:

It was taken for granted that the findings of such non-archeological specialists would not only be of intrinsic value to the sciences represented by them, but that they would gain cumulative importance because geographical concentration would permit pooling of data, interchange of ideas, as well as formulation of combined attack upon problems of common interest.

And, again:

The archeologist is thus abruptly hailed from the comforting shade of his trenches to the glare of the existing world, and there he stands bewildered. He surveys the environment of today to find that its complexities are utterly beyond his power to comprehend. He looks at the very simplest of modern peoples; their life proves unbelievably involved. But if he be willing to face the situation and not pop mole-like back into his burrow, he will find that other sciences are grappling with the problems of plants and animals, of weather and rocks, of living men and existing social orders; collecting, classifying, winnowing detail, and gradually formulating the basic laws which render this perplexing universe understandable. Beside and with them the archeologist must work if his results are to be more than the putterings of the antiquary.

Kidder stressed many times the potentialities of the Maya area as a laboratory for studying man and culture, what he called "the genetics of civilization":

Their history involves the rise, spread, efflorescence and decline of an agricultural civilization. It gives splendid opportunities for evaluating the influence of those racial and environmental factors which have been so potent in shaping the destinies of all peoples, but whose action has been so little understood.

And, later:

Few more favorable fields than the Maya country could be found upon which to come to grips with the fundamentals of history and of anthropology, nor for bringing about the much-needed recognition of the essential unity of their aims. Study of the Maya from the earliest times to the present involves consideration of age-long and world-wide problems: the relation of man to his habitat, the spread and interaction of nascent cultures, the origin of higher civilization, the decay and fall of social orders, the clash of native and European races, the adjustments between conquerors and conquered, the impact of Twentieth Century ideas upon backward peoples.

Kidder envisaged his program as in keeping with a general trend toward unification in the intellectual world:

In the past many facts appeared to be mutually contradictory. Modern learning, however, shows that all truths are interrelated. Chemistry and physics are striking

downward to common fundamentals; zoology and botany are rapidly merging and the resultant newer biology is joining hands with the physical sciences. Similar tendencies are becoming manifest in the human field, where geography, ethnography, sociology, and psychology are constantly drawing more closely together.

To launch the "pan-scientific attack," as he once called it, Kidder drew not only on the staff and resources of the Institution but also on outside agencies and individuals as well. From Carnegie's Department of Genetics came Morris Steggerda to study the physical anthropology, genetics, and agronomy of the modern Maya; the Geophysical Laboratory assisted in Anna Shepard's technological work in ceramics; the Nutrition Laboratory assigned Benedict to study Maya diet and basal metabolism. The University of Michigan cooperated through its zoological and botanical experts, Bartlett, Lundell, Van Tyne, and Stuart; Duke University, in its studies of the fauna of inland waters of Yucatan, by Pearse and Hall; the United States Bureau of Plant Industry, through its experts on plant biology, Collins, Kempton, and Emerson; the United States Geological Survey, through C. W. Cooke; and the United Fruit Company, through Wilson Popenoe. The University of Chicago joined in with the ethnological program of Redfield, Tax, Hansen, and Villa, and in linguistic studies by Andrade and McQuown; G. C. Shattuck of the Harvard School of Public Health made a medical survey of the Peninsula of Yucatan; Lawrence Roys investigated the engineering knowledge of the ancient Maya; the Blue Hill Observatory of Harvard cooperated in studies of climate; and Clark University and the University of Florida participated in geographical research through the Atwoods.

This is only a partial list, but it indicates the wide scientific net that Kidder cast into the Maya waters. The catch in useful information bearing on a single area was enormous. It is debatable whether, at this stage of knowledge of the region, when even basic data were so lacking, the multidiscipline approach could or should have been more specifically problem-oriented — that is, concentrated on testing specific hypotheses and exploring more limited themes. Even if it had been so sharpened, Kidder would not have been able to entice many specialists to take part in so rigidly directed a research; those of us who have experimented with this sort of program, even on a very small scale and within the staff of only one institution, know what a com-

plex, difficult, and traumatic undertaking it can become. Even if he had wanted such a program, Kidder could surely foresee the rebellion he would confront from scholars who traditionally recoil from any "regimented" research. Imagine telling Morley to pursue only such glyph studies as would bear on evidences of Maya nativism under the Toltecs, or Thompson to concentrate exclusively on some particular process of prehistoric acculturation! Of the Chichen Itza conference Kidder reported: "It was felt by the majority of those present that precise statement of objectives and rigid organization would be unwise; that research should be allowed to flow in such channels as the shifting contours of individual investigations might throw open. . . ." In his preface to *Maya Hieroglyphic Writing* Thompson wrote, "From his initiation of staff meetings to discuss general strategy and specific problems I, together with every participant, have derived great benefit. One could hardly ask more than a free hand to follow one's own investigations when that is coupled with the advantages and profitable obligations of team work."

With his growing maturity and professional stature, Kidder increasingly expressed in writing his thoughts on the methods, shortcomings, and future of archaeology. In his early works one finds little of this; one can guess what he must have thought of the slipshod methods of most of his predecessors, but he never, throughout his life, adversely criticized them by name or in terms that would suggest reference to specific individuals. The 1924 *Introduction to Southwestern Archaeology* ventured only one admonition: that anthropological theorists on such things as culture growth, *traic* transmission, divergent and convergent evolution, tendencies of primitive art, and influence of environment on culture, had been "prone to draw inferences from fragmentary data, to evolve theories which fit well with preconceived ideas. In no science is the need for empirical study more keenly felt."

But by 1932, in *The Artifacts of Pecos*, Kidder expressed himself at some length on museums that were still "collecting agencies rather than scientific laboratories," and fixed on them the responsibility for "looting cemeteries and shrines to the neglect or even the destruction of immensely more significant but, for the museum, unprofitable deposits." Sites, he charged, were being "at best only partly examined, at worst ruinously mangled." Foreign countries were being despoiled "by wanton excavation and by

smuggling," thus forcing them into prohibitory legislation and "thereby hamstringing institutions whose interest, being primarily in scientific facts, would be willing to leave collections in the land of their origin, or to accept an equitable division." (By now Kidder was directing the Carnegie Institution's Maya program and was acutely aware of this particular problem.) He further lamented the failure to study and classify material properly and the failure to publish, which he laid to the difficulty of raising publication funds, the "sheer lack of time" available to the average archaeologist, and the indifferent attitude of many institutions toward "the issuance of reports as an essential and immediate sequel to field work." He advocated shorter digging seasons, fewer workmen, annual publishing or at least completely up-to-date manuscripts ready for the printer before one took to the field again, new methods of presenting data (shorter papers, condensed tables, and concise descriptions), and a more comprehensive system of taxonomy for artifacts. He expressed some of these opinions in his preface to *The Swarts Ruin*, published that same year, and called attention to the fact that the Cosgroves had achieved such a condensation in their report.

Kidder also began to reveal more frequently his view of the aims of archaeology. Prior to 1924, we can only infer it — for the most part from his own research and the aspects of archaeology that interested him. In 1924 (*Introduction to Southwestern Archaeology*), he said that we "must use our results for the solution of those general problems of anthropological science without a true understanding of which we can never hope to arrive at valid conclusions as to the history of mankind as a whole." In 1926 (unpublished manuscript quoted by W. W. Taylor), he wrote: "Anthropology seeks to reconstruct the history of man, of his works, of his bodily structure, and of his mind, in order that there may be deduced from a study of that history the basic laws which have governed in the past, and which therefore may be expected to control in the future, the destinies of the human race." In 1930 (*Carnegie Year Book*), he evinced an interest in the effect of habitat on Southwestern economy and sociopolitical organization: "The far from fertile Pueblo country permitted no great accumulation of surplus wealth; hence life remained democratic and there never arose the aristocracies and priesthoods which so crushingly burdened the southern nations." He noted

that the Southwesterners rose from nomadic savagery to a relatively high degree of civilization, and in an arid region. In 1932 (*Artifacts of Pecos*), he spoke of the time when we shall "be in a position to approach the problems of cultural evolution, the solving of which is, I take it, our ultimate goal." In the same year (introduction to *The Swarts Ruin*), he noted the importance of studying the periphery of the Southwest, such as the Gila, which would provide "extremely interesting and fundamentally significant data regarding the diffusion and interaction of cultures."

Aside from his direction of the Carnegie program, Kidder's self-appointed role in Maya research was at first a modest one, involving no excavations of his own. He knew that nonceramic artifacts had been neglected in this field and, having just finished describing the Pecos artifacts, he probably felt that such a study of the Maya material could be his most useful contribution. *The Artifacts of Uaxactun* was one result; it was the first systematic description of a large quantity of artifacts from a single site and is still of great value, particularly for the comparative data.

Kidder's first trip into Maya country was made with Eric Thompson and two friends, to Coba, Quintana Roo, in 1926; it is described in Thompson's *Maya Archaeologist*. The party took off from Valladolid, Yucatan, thence by muleback deep into the forests. In 1928, Kidder visited Uaxactun with Vaillant and the Morleys, a five-day trip from Belize — two by riverboat and three by mule through the jungle. He spoke of this initiation to the rain forest in his introduction to A. L. Smith's *Uaxactun, Guatemala: Excavations of 1931–1937*:

I learned what a grim country the Peten is on my first trip to Uaxactun. That was in 1928. Morley, Mrs. Morley, George Vaillant, and I left the seaport of Belize, British Honduras, in a light-draft launch towing a dugout canoe and manned by amphibious black boatmen. The water being low, we were two days and two nights getting up the Belize River to Benque Ceiba, where mules, sent out from camp, awaited us. Then three days on the trail — my first initiation to the jungle. To one brought up in the clean, open pinewoods of northern Michigan, whose field work had all been in the semi-deserts of Arizona and New Mexico, it was strangely awesome. Hour after hour in single file on the narrow, twisting trail, the dense forest pushing in on either side with an almost physical pressure, the healthy sunlight never reaching the ground save where the fall of some great tree had brought down others with it, opening a crack of sky and starting a new round of crowding young growth in the endless upward struggle for light. Hour after hour of moist, green gloom. Then,

we would break out into a *bajo*, a low area, in the rainy season a bottomless swamp, the trail now parched and cracked. The sour soil of these always dreaded stretches supports only a tangled, scrubby bush. At first it was a relief to see the sky, but travel through bajos is painfully slow because the hard-baked mud is deeply pitted with old mule tracks, a breath of wind is unknown, the sun beats relentlessly down, clouds of stinging flies envelope everyone. Across, it was good to be again in the cool, quiet forest. I was not prepared for its uncanny silence. There would be an occasional rustle of some little animal in the thick undergrowth, but all vocal life was far above on the sunny, billowing floor of the treetops. Once night came on, however, the jungle burst into sound: shrilling insects, harsh-crying night birds, strange unidentifiable calls; at dusk and dawn, the deep, resonant roaring of the howler monkeys.

As we rode, I kept wondering how the ancient Maya had so thoroughly conquered this vast and hostile land; how, without benefit of metal ax or the machete, they could have made it habitable. The bush once down, though, and the sun let in, even the Peten can be pleasant, as I learned when, tired and sweat-soaked and itching, we reached the expedition's camp.

Kidder soon had an opportunity to view this same country from the air. Early in 1929, Colonel Charles A. Lindbergh, his famous Paris flight less than two years old, was exploring routes over Central America for Pan American Airways and observed some of the Maya ruins in Yucatan. When he offered his services to Carnegie Institution for air reconnaissance of archaeological areas, President Merriam suggested that he fly out to Kidder's camp at Pecos to consult with him. One of Kidder's favorite stories was about how he was called to the telephone several miles away at Rowe for a long-distance call. The voice at the other end of the line said, "This is Charles Lindbergh calling from San Francisco." Kidder, thinking that someone was pulling his leg, promptly replied, "Yes, and this is Calvin Coolidge, speaking from the Black Hills of South Dakota!" Colonel and Mrs. Lindbergh made flights over the Canyon de Chelly, the Hopi country, Chaco Canyon, and the Pajarito Plateau. Their photographs (some of them published in the *New York Times* rotogravure picture section of December 1, 1929) demonstrated the utility of the airplane for finding sites and for locating them relative to water supply and arable land. The Lindberghs demonstrated, too, that they could land at sites and archaeological camps otherwise difficult to reach. In October, the experiment was tried again in five days of flying over British Honduras, Yucatan, and the Peten. Kidder and Ricketson alternated as observers on the flights, testing the feasibility of finding and identifying ruins from the air, and of

plotting geographical features, including forest-type distribution. In the almost 40 years that have elapsed since these flights, the airplane has not yet been used extensively or successfully for site reconnaissance in the lowland Maya area, but Kidder did foresee its possible future usefulness in "transporting small parties to the interior where they could be landed on lakes or the larger rivers." I am sure that he could not in wildest imagination have predicted that a huge operation at Tikal would some day be supplied entirely by air, including the transport of jeeps and drilling equipment. He wrote of one of the Lindbergh flights:

We paid our respects to the governor [at Lake Flores, where they landed in their amphibian plane], who came out in a launch to greet us, surrounded by the entire population in dug-out canoes; and then rose to fly southward over a vast flat stretch of alternating savanna and woodland, toward the northern tributaries of the Pasion River.

These streams traverse a terrible and (to fly over) a most terrifying country, a confused welter of gorges and limestone pinnacles smothered in a jungle so dense, so intertwined, so utterly hopeless to penetrate, save through the foot-by-foot hacking of trails, that I think no one of us failed to give a sigh of relief when we soared over an outlying spur of the Cockscomb Mountains, dodged between two rain-squalls, and saw to the eastward the silver shine of the sea.

In 1935, Kidder and Ricketson made a ten-day reconnaissance of San Agustín Acasaguastlan, on a northern tributary of the Motagua River in the Department of Progreso, Guatemala. He reported on it briefly in the Division's *Contributions* series, and later co-authored a report on the San Agustín site with Ledyard Smith. In 1940, he wrote up pottery and other materials from Champerico and Guaytán, Guatemala. Increasingly, as the years passed, Kidder encouraged specialization in the division of labor among his staff. The task of synthesis, interpretation, and discussion often fell to Kidder in these collaborations. This may have reflected to a certain extent the personal wishes of his associates, for Thompson did not specialize, nor did Kidder himself at Kaminaljuyu. In my opinion it was this quality of Kidder's own work, both in the Southwest and at Kaminaljuyu, that made him and his reports outstanding; it is the quality that has made Thompson our foremost Mesoamericanist. But the large projects at Uaxactun and in northern Yucatan required a certain amount of specialization, and it also resulted in more prompt publication. A careful and gifted writer himself, Kidder insisted on high standards of presenta-

tion, grammar, and format in the Division's sixty-odd major volumes of reports that were published during his chairmanship. For several years he minutely edited every manuscript himself, but when his other duties became too heavy and the monographs began to multiply, he brought in Margaret A. L. Harrison as editor. The Carnegie books became models that few other institutions have since been able to imitate successfully.

As Pecos was Kidder's magnum opus in the Southwest, so was Kaminaljuyu in the Maya area. He excavated there from 1935 to 1937, and the Institution carried on subsequent investigations when Guatemala City building development exposed important features of that extensive highland site. It should be remembered that Guatemala highland archaeology was still in its infancy. Lothrop had dug at a few sites on Lake Atitlan and was about to publish descriptions of collections reportedly from Zacualpa, Utatlan, San Andres Sajcabaja, and elsewhere. Before him there were mostly only the scattered notices by Seler between 1901 and 1908. The first outstanding synthesis of highland archaeology, Butler's excellent analysis of grave contents from the Alta Verapaz, was not to appear until 1940. My own work at Zacualpa was begun the same year as Kidder's Kaminaljuyu excavations (1935). None of us really had much to go on except what we ourselves dug up.

The Kaminaljuyu pyramids and their adjacent shaft tombs presented, in their own way, as complex a set of excavation problems as Kidder had ever confronted at Pecos. No one really knew what the inside of a highland mound looked like. At Kaminaljuyu there was no masonry, no fine, thickly plastered floors; there were dramatic contrasts of fill materials from one superimposed structure to another, but one had trouble discerning walls and stairways and terraces because the hard adobe-like earth showed few changes in color or texture from top to bottom. As Kidder noted, it was only when his trench walls dried out thoroughly that hair-line fissures developed between a structure's clay-veneered surface and the subsequent fill lying against it. Always interested in techniques of excavation, Kidder recorded in some detail the methods used at Kaminaljuyu. He recommended that exploration of a mound begin by cutting a narrow trench or tunnel at ground level from well outside the mound all the way to its center, entering from the side to avoid

damaging a frontal stairway. This should reveal the nature, depth, and junction lines of architectural features, humus, fallen debris, and other deposits. As at Pecos, he recommended leaving control sections for back-checking, then clearing the superimposed buildings one by one, thus simplifying photographic and other recording. (Some of these techniques had already been used in the lowlands, for example, by Ledyard Smith at Uxactun.)

Kidder also devised, after some experimentation, a method of excavating shaft tombs from the side, rather than from above. He determined the size of the grave from its horizontal outline, then sank a pit as wide as the shaft just outside the tomb, on the north side where mortuary furniture seldom lay close to the wall. After recording the composition and stratification of the entire face of the tomb fill thus exposed, he removed the latter in vertical slices divided into columns, but stopping about 50 cm. above the tomb floor, which could then be cleared with trowel and brush.

Kaminaljuyu, co-authored with Jesse D. Jennings and Edwin M. Shook, will remain one of the monuments of Mesoamerican archaeology. The material culture of each phase, with inferences regarding agriculture, sociopolitical organization, and general patterns of life, is described in systematic detail, plus comparative data; evidences are marshaled for the probable language and physical type of the people of Kaminaljuyu and their overlords. The nature of large Maya centers and the function of major structures are discussed, as well as of dwellings. Commerce and cultural contacts with other areas are deduced, especially with Teotihuacan, Tajin, and La Venta. The nature of the strong Teotihuacan influence at Kaminaljuyu is explored extensively, and archaeological evidence for the arrival of Pipil Indians is considered.

During World War II, when most of the Carnegie staff was in service, the Institution's program was sharply curtailed. Although he was too old for active service again in the armed forces, Kidder loved to serve coffee and doughnuts to the young GI's entraining to new assignments. They invariably called him "Pop." He told me once, with obvious pleasure, of an occasion when he stopped in a train aisle to converse in French with a boy from the French army, and an American soldier in the next seat exclaimed to his buddies, "Cheez, waddya know? Pop speaks Frog!" Old World War I warhorse that

he was, he chafed at what he considered his inactivity in the war effort. In his office he posted snapshots of his younger friends overseas, and he wrote me complaining wryly that he "was surrounded by females."

In 1942, Kidder was president of the American Anthropological Association; he had been president of the Society for American Archaeology in 1937. As director of a 1941–42 survey of eastern El Salvador being made by John M. Longyear III for the Institute of Andean Research—which, incidentally, Kidder was instrumental in founding—he visited Longyear several times in the field. In 1943, he attended a round-table conference in Mexico, and visited museums and private collections in Morelia and Jalisco. He plunged into whatever Maya collections he could locate, and he wrote voluminously.

After the war, the Institution's work in the Guatemalan highlands was renewed, but Kidder, possibly sensing the uncertainty of the program's future, insisted more and more on completing publications and undertaking no new projects except emergency ones, preparatory to moving out of the highlands. His annual reports reflect a growing urgency to justify continuation of the Division's work, but at the same time to put affairs in order for the next stage of the program. He spent the winter of 1947–48 working on the potsherds Ledyard Smith had obtained during a highland survey, and wrote up the pottery and artifacts from the Nebaj tombs for a joint monograph with Smith. For the latter work he made the first chronological classification of Classic and Postclassic Mesoamerican jades, a difficult task based largely on typology, for few proveniences of the known jade artifacts were certain. During these years, he also acted as archaeological advisor to the United Fruit Company, which was engaged in the excavation and restoration of Zaculeu in the northwest highlands. One of his last major monographs, in collaboration with Shook, was on Mound E-III-3 at Kaminaljuyu. Kidder and Shook had suspected that some of the large structures at this site were of Preclassic date, but it was not until 1947 that the accidental discovery of a rich tomb in E-III-3 settled the matter. Shook excavated the tomb, already greatly disturbed by building operations, and AVK, as he put it, "studied the specimens in great comfort at the Department's laboratory in Guatemala City." During all these years in the highlands, Kidder

also inspected, photographed, and recorded a number of private collections; these resulted in twelve useful papers which he authored or co-authored for the Institution's *Notes* series. In 1950, the year of his retirement, he, Gordon F. Ekholm, and Gustav Stromsvik cruised the Bay Islands and northwest coast of Honduras and the southern coast and cays of British Honduras, examining sites and private collections.

I shall not attempt here an appraisal of Kidder's concepts of archaeology or of his contributions in general. For one thing, all great men have been interpreted and reinterpreted by lesser lights until history has them saying things and standing for things that they never said or meant to say. Even if it were not presumptuous of me to try to interpret Kidder, I am not sure that doing so would serve any useful purpose. His writings speak for themselves, and his contributions will be forever valuable, whatever I might say about them. Besides, a great deal has already been written on the conceptual framework of American archaeological studies, which certainly cannot be adequately discussed in an article of this length. A few matters are fairly clear from Kidder's own repeated statements, and these can be mentioned.

He definitely linked archaeology with history. In 1926, he wrote, "Archaeology, rightly conceived, is a historical discipline." In designating Carnegie's new department in 1929 the "Division of Historical Research," he said:

. . . the term history has been used as symbolic of a desire to do away with the somewhat rigid distinction which has generally been drawn between archeology and history. The work of the Section is, of course, archeological in that it deals with material remains rather than with written records. But archeologists seek to gather from ruined buildings and potsherds the same sort of knowledge that historians derive from books and manuscripts, and while the subject matter and, therefore, the primary methods of the two disciplines are naturally unlike, their ultimate aims are identical, for both archeology and history strive to recover and to interpret the story of man's past.

In 1937, he wrote:

It seems certain that both disciplines could profit from rapprochement, not only in the matter of mental attitudes and methodologies, but also through better acquaintance with each other's results. Archaeology can not interpret its findings save in the light of what history shows that men have done under given conditions; history must turn to archaeology for understanding of the prehistoric peoples who laid the foundations of the cultures with which they are concerned. . . . If the Division can, by bringing history and archaeology together in a single field, serve to

further in any way the development of a better integrated science of man, it will most amply have justified its existence.

Yet there is no indication that in aligning archaeology so closely with history Kidder felt that this distinguished it from anthropology and its traditional interests. We have seen, from his Southwestern writings and his Kaminaljuyu report, his conviction of the strong influence of habitat on culture, his speculations on sociopolitical organization, language, somatology, commerce, culture contacts, and diffusion. It is true, as Taylor pointed out in his critique of American archaeology, he posed many more questions than he attempted to answer. One gets the impression that Kidder was well aware of the ultimate uses to which archaeological data could eventually be put, but he was not yet ready or inclined to go into such matters in depth himself. And, like Boas, he thought the data were insufficient.

The same year that Kluckhohn's strongly critical *The Conceptual Structure of Middle American Studies* appeared (1940), Kidder published *Looking Backward*, which posed a number of broad cultural questions that archaeology could illuminate: Are all races of man possessed of equal ability? What conditions the growth rate of civilizations? What are the relationships between race, language, and culture? When he wrote *Kaminaljuyu*, he had of course read Kluckhohn's article, and he referred to it toward the close of the book: ". . . we shall be thought by some anthropologists to have concerned ourselves so exclusively with bare facts as to lose sight of fundamental problems of human behavior. It is our firm belief in the extreme importance of just such problems, however, that causes us to fear too great haste in reaching conclusions." He then presented what he called an *apologia pro opere nostro* by posing some of the same questions he had mentioned in 1940, and to which he felt the Kaminaljuyu data were relevant. But again he did not attempt to answer these questions or to marshal his data bearing on them. He merely stated the need for more fact-finding.

By the time of the Nebaj report, Taylor's 1948 memoir had appeared. It is therefore interesting to compare the cultural subjects discussed in the Nebaj report with those in the Kaminaljuyu book. They are almost identical. He discussed contacts between Nebaj and other areas, the homogeneity of Classic Maya culture, the prob-

able sociopolitical organization of the Maya (theocracy and control of mass labor), man's motives for remodeling and rebuilding (he noted that this is one of Kluckhohn's "recurring regularities of human behavior"), the function of Maya structures, inferences on prosperity and peace, and factors affecting the growth of Mesoamerican civilization. The discussions, however, are again brief, even superficial, and end, as before, with a statement of the continuing importance of chronology and the recovery of pottery and artifacts.

Both *Kaminaljuyu* and *Nebaj*, like the *Introduction to Southwestern Archaeology*, the book review of *Mogollon Cultural Continuity and Change*, the *Pendleton Ruin*, the final 1954 report on Pecos, and others of his works, contain masterly syntheses of culture history, of broad developmental trends, of general life patterns, of foreign relationships — Kidder at his best with his forte. He was certainly aware of anthropology's concern with specific culture processes and specific cultural dynamics, but he was obviously not interested in them sufficiently to investigate them empirically and in depth himself.

Although he was a member of the faculty of the Peabody Museum of Harvard, he never taught in a university, except one summer term at Berkeley after his retirement. Nevertheless, he profoundly influenced the professional careers of a great many young people from many universities. I intended to list here the professional anthropologists and ethnohistorians who took part in the Carnegie program at one time or another. When the list of names rapidly reached 54, I changed my mind. It includes most of the leading contemporary scholars in Middle American ethnology, archaeology and epigraphy, art history, ethnohistory, physical anthropology, and linguistics.

AVK retired on November 1, 1950. His friends all over the country had joined in creating the Alfred Vincent Kidder Award for achievement in American archaeology, particularly in the Southwestern United States and Middle America, the two fields to which he had contributed so much. One hundred bronze medals were deposited in the Peabody Museum, one of them to be awarded every third year for 300 years to a recipient designated by the American Anthropological Association. On the day of Kidder's retirement, a small committee visited his office at 10 Frisbie Place in Cambridge to tell him of the plan and to present him with the

original die, which had been designed by Tatiana Proskouriakoff. The first award, made that same day, was to A. M. Tozzer; subsequent medalists have been Earl H. Morris, Samuel K. Lothrop, Charles C. Di Peso, and Tatiana Proskouriakoff.

Other honors in these years included honorary degrees from the University of Michigan (D. Sc., 1949), the National University of Mexico (Doctor Honoris Causa, 1951), and the University of San Carlos, Guatemala (Doctor Honoris Causa, 1955), and Commander, Order of the Quetzal, from the Republic of Guatemala (1955). He had previously received the degree of LL.D. from the University of New Mexico (1934). He was the first Viking Fund Medalist in Archaeology (1946); he also received the Lucy Wharton Drexel Medal from the University Museum, University of Pennsylvania, and a medal conferred by the University of Arizona on the occasion of its seventy-fifth anniversary. As mentioned, he had served as president of both the leading associations of his profession. He was an officer and active member of the American Philosophical Society. He also held membership in the National Academy of Sciences, the American Academy of Arts and Sciences, and the Tavern Club of Boston. In 1963, the Seminario de Cultura Maya, of which he had been an honorary advisor, announced that an *homenaje* would be dedicated to his memory.

An account of this kind usually closes with a few words on its subject's personality. One cannot measure in words the dimensions or quality of Kidder's warmth and innate kindness, any more than one can express the depth of one's emotions on remembering him, but for younger readers who did not know him I should mention a few things, however inadequately they portray him as a man. First of all, as Judd noted when they first met, he was a large, strikingly handsome man — in its story of archaeology some years ago, *Fortune* magazine remarked that in a profession notorious for its homely practitioners, Kidder was a notable exception — who carried himself with an easy, relaxed erectness. One afternoon, when my wife and I were driving home up crowded St. Charles Avenue in New Orleans, with no notion that Kidder was within a thousand miles, I saw in the rear-view mirror, fully half a block away, a familiar figure walking in the opposite direction. When I saw that erect back, the iron-gray hair, the slow easy gait, I told Betty, "I would swear that I see Dr. Kidder back there, but it couldn't be." Sure

enough, when we got home the telephone rang and it was he. Kidder spoke rather deliberately, at times even haltingly, but always concisely, to the point, and in elegant English, albeit peppered with his favorite slang. Even when gravely concerned with a problem, he seemed perpetually unruffled; in all the years I knew him I never saw him in a show of temper. In social conversation his manner was courteous and cheerful, but I understand that on rare occasions, in the privacy of his office, he could give someone a pretty bad time if he strongly disapproved of his attitude. Be that as it may, Kidder is known to have generously tolerated undesirable situations among certain members of his staff long after many a chairman would have lost patience, and he was well aware that he exposed himself to adverse criticism in doing so. Perhaps because he admitted to a close brush with failure on his own Ph.D. orals at Harvard — he told John Rowe that after Professor Ripley had voted to fail him, Farabee came to his rescue with questions Kidder knew the answers to — he seemed anxious to bolster those of us who did suffer this indignity on our first try. The reassurance more often took the form of deeds than words; in my case he proceeded enthusiastically with field plans for my Indian house-type study, giving every indication that his confidence in me was undiminished, and shrugging off the late misfortune as inconsequential. No administrator ever took a more personal interest in the individual members of his staff and the many young people with whom he came in contact.

He was an easy man to live and work with; he wore well. He never blackened another's name, either personally or professionally; he did not gossip; and he never climbed a step higher in his own career by stepping on another's back. He rarely shared personal or professional problems with his friends or associates. He gave the impression that life amused him immensely; even recalling his most disagreeable experiences, he told of them with a chuckle or with mock disapproval. He loved to recount anecdotes, most of them about his friends, and when he told a story on himself it was almost invariably one in which he was the butt of a joke or mishap. All of us who worked with him in the field remember the messages he would send over to us, brightened with clever little stick-figure illustrations; his "signature" on these was usually identified by a battered army hat and pipe. But however dust-caked and baggy his work

clothes, however clumsy he tried to paint himself in his stories, however many "dandy's" and "swell's" and "bully's" he sprinkled on his conversation, however many stick-figures enlivened his messages, he simply could not take leave of a very great innate dignity. AVK was no prude and no Puritan, still moderation seemed a high value with him — excepting his attachment to his tobacco pipe, which was ever with him.

It would be inaccurate to say that Kidder was universally beloved by one and all, although he came close to it. No one with a mind, a will, a personality, and standards as strong as his could escape disapproval from some quarters, nor could anyone in an administrative position as long as he was making only popular decisions. There were a few petty grievances against him, which he never mentioned either in public or in private so far as I know. He was acutely aware that some of his major policy statements would be resented. He realized, for example, that Carnegie Institution has no museum, and that it could therefore be said that he could easily afford his admonitions to museums on their "lust for specimens." He knew, too, that there must be some resentment of the fact that he and his staff had no teaching or curatorial duties, and he pointed out in writing their consequently greater obligation to produce more and publish promptly. In a generation of anthropologists that tended to sneer at popular writing and lecture tours, he stressed the importance of making scientific findings available to the public through these channels.

Kidder once wrote that no one reads archaeological reports for the grace of their English, but his own style was original and impeccable. To my mind, he is the author of some of the most forceful, most beautiful and moving prose to be found in "scientific" writing, much of it relatively hidden in prefaces to others' volumes, or in book reviews, obituaries, and administrative reports. Although reticent to display strong feelings in conversation, in his writing he was not ashamed to express emotion — the beauty of a Southwestern landscape, the awesomeness of the jungle, his nostalgia for the West, or his love for a departed friend.

The one great disappointment that he was fairly frank about expressing in his later years was in the Institution's termination of the Maya program. Although some have tended to blame this unfortunate decision at least partly on Kidder for a so-called lack of administrative aggres-

siveness, it was probably beyond his or any social scientist's ability to prevent. As early as 1940, he made a special plea in his annual report that "the study of man must at all costs be continued, even though it be long and slow," and his justification of this was followed by the significant statement: "The foregoing is, of course, the special pleading of anthropologists and historians for opportunity, in these rapidly changing and materially minded times, to continue their researches." The entire anthropological profession expressed dismay when the news became public, but so far as I know it made no concerted effort to help change President Bush's mind. In 1939, Kidder had written of his hope that eventually the Division could "attempt synthesis of the whole program by producing a history of the Maya based upon a correlation of all branches of the investigation." That dream was never realized. When the Institution's decision became irrevocable, Kidder's concern was typically greatest for the future of his younger staff members. Arrangements were made to carry out the five-year Mayapan program under the direction of his successor, Harry Pollock; meanwhile, affairs could be put in order for the final close-out.

It is not necessary to document fully Kidder's personal courage, which was impressive. His war record, his flights with the Lindberghs in the days when airplane travel over rugged terrain and pathless jungles was extremely risky — things like these speak for themselves. My first view of his instant reaction to emergency was at Pecos in 1927. He, Alfred, Randy, and I — and I believe Jim, the camp caretaker — were returning from a swim in the Pecos River when we noticed that a nearby little piñon "factory" (which extracted oil from piñon boughs) was about to be engulfed in flames raging through the huge stacks of piñon piled outside. Kidder rushed to the spot, quickly organized our little group into a bucket brigade, placing us boys in safe escape positions to the rear, while he became front man next to the fire. The situation looked hopeless to me, but we did stem the fire after perhaps an hour's work — Kidder, I remember, red-faced and perspiring, covered with ash, half-choked on smoke, and with a trickle of blood running down his forehead and cheek from a close brush with his adversary, methodically hurling puny bucketfuls of water and beating back the flames with anything he could lay hands on. I am tempted to say that he never

removed the ever-present pipe from his mouth, but I cannot swear to it.

The one thing that all of Kidder's friends remark on, if you ask them about him, was his humility. This word should by no means imply false modesty; AVK radiated quiet assurance and dignity. Margaret Harrison once wrote of Lila O'Neale some things that I wish I could express as well of Kidder: "Self-assurance escaped self-importance through balance and humor. . . . All her writings are marked by incisiveness and integration. They unite to a singular degree powers to penetrate and to organize an intricate mass of detail. They show, too, that particular kind of honesty which is freedom from pretentiousness in either word or attitude." I am sure no one ever heard Kidder boast, however subtly. Emil Haury believes that it was Kidder's humility in his own accomplishments that, as well as any other factor, made the first Pecos conferences so productive. And as Peggy Harrison told me, he always gave the impression you were working *with* him, not *for* him. There was unfortunately another side to the coin. Eric Thompson has remarked, "Doc was at the disadvantage of being every inch a gentleman in a world in which few were even every other inch a gentleman. He suffered grievous intransquillity whenever that imbalance was brought home to him." Almost everyone also mentions his constant sharing of authorship with his assistants, often taking the junior listing himself. He went out of his way to give more than due credit to the work of others. His reports to Carnegie almost invariably began with praise of Morley's driving enthusiasm that resulted in the Institution's entrance into the Maya field, and of his leadership in the excavations at Chichen Itza. He credited Gamio with the first stratigraphic work in Middle America, and his pupil Vaillant, with the first in the Maya area. He dedicated his last Pecos report to Nels C. Nelson, "Old and valued friend, whose stratigraphic excavations in New Mexico laid firm foundation, not only for the study of Southwestern archaeology, but for all subsequent field investigations of New World prehistory."

He was completely unostentatious. The address 10 Frisbie Place, to which the Division moved about 1936, was an old private residence conveniently near the Peabody Museum of Harvard with its excellent library and collections. Kidder's office was a plain little room upstairs with, as well as I can recall, not even a rug on

the floor or curtains at the windows. One did not have to get an appointment through his secretary. The rule was simple: if his door was open, you could go in and see him; if it was closed, he was unavailable. His camp at Pecos was comfortable but severely plain; his residences (first at Andover and later the fine old house at 41 Holden Street in Cambridge) were comfortably and warmly unpretentious. During his last years, it was at Holden Street that so many of his old friends called on him for an hour or two of reminiscence or discussion of current research. Kidder had given most of them their first opportunity for field work and had guided them proudly to full professional status. Here, after several years of gradually declining health, he died suddenly—as he surely would have wanted, while at work on his latest project. Some men strive for professional achievement and recognition above all else. Some are concerned more with the hope that they can be of help to their fellow men. Many want primarily to live a full, rich life and enjoy it to the hilt. I suppose we really hope most of all to be remembered with affection by family and friends. A. V. Kidder attained all these in full measure.

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