THE LUCKY LIFE OF AN ARCHEOLOGIST

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When I think about my trajectory, I tend to think about it as an archaeologist would. Accordingly, I will describe a series of projects carried out in the Near East; tell why I did them, what the results were, and how one led to another. In all, the projects I have worked on encompass perhaps 50,000 years of human history, although most of my research has focused on the last 9000 years. In this brief discussion I will focus only on my research in Iran and Syria.

A few years ago, when I was awarded the lifetime achievement award by the Society for American Archaeology, I was asked to say a few words at the Annual Meeting. I said there were five good things that define my career in archaeology. I had good timing, good luck, good mentors, good colleagues and good health. While none of these is more important than another, in combination they made my successes possible. I have always had a very positive outlook on life and I prefer to think about what I'm going to do next rather than dwell on my own past. Usually when I discuss the past it's the ancient past and not my own, so this talk is a little different from what I normally do. How do I characterize my research interests in a few words? If I were to put key words by my name I would say environment, ecology, adaptation, technological change, climate change and sustainability. There are all things that have to do with the way humans occupy the land and the way the land and humans have changed over the millennia.

I didn't start out with the idea that I'd become an archeologist or even a scholar of any sort. I wasn't too keen on formal study in school or even college for that matter, but I read widely, particularly about foreign places, adventure, travel and discovery. After graduation from Cornell College, I became a journalist and worked as a reporter in a Chicago suburb. This was during the Korean War, and in September of 1953 I was inducted into the Army. I spent sixteen weeks at Fort Riley, Kansas in the bitter cold of winter learning how to carry and fire heavy mortars, machine guns and bazookas. Fortunately, at the end of my basic training a cease fire ensued, so the Army did not need to send me to Korea. For my first airplane ride, the Army sent me on a DC3 to a relocation camp at Fort Jackson, South Carolina. When it was discovered that I could type, I was relocated to Third Army Headquarters at Fort McPherson in Atlanta, Georgia, where I was assigned to office work in G2, the intelligence section. In my barracks I met men who had been in graduate school and we talked about my possibly going to graduate school. I was particularly attracted to the idea that, if I was accepted to graduate school, I could get out of the Army early, an appealing prospect, especially as I was getting a little bored with Army routine. As for a field of study, I thought that anthropology would be particularly interesting and offer the promise of travel to exotic places. I asked one of my professors at Cornell whether he could

recommend a graduate school in anthropology, and he suggested the University of Chicago. I applied and was accepted, largely I suspect, because of the GI Bill, which paid all expenses. The GI Bill was a wonderful, wonderful program, which allowed an awful lot of us to get an education that we would not have gained otherwise.

Before going further with my trajectory, I would like to acknowledge a few of the mentors and colleagues who were important influences in developing my career. I think first of Robert Braidwood, my professor at the University of Chicago, who was one of the pre-eminent prehistorians of the ancient Near East. He took me under his wing and introduced me to Near Eastern archaeology, but more importantly he taught me how one goes about doing archaeology. He involved me in his projects right from the beginning, from planning to execution, which was of enormous help to me as I went onto my own research.

Another great influence was Emil Haury, a professor at the University of Arizona, who had devoted a lifetime to archaeology in Arizona and the Southwest. At an archaeological field camp on the Apache Indian Reservation, he introduced me to the beauty and challenges of arid lands, and the ways prehistoric people adapted to aridity. The experience of working with him in an environment so different from that of the Midwest gave me a solid foundation for my own fieldwork in the Near East, also an arid land where abundance and predictability of water are ever-present concerns.

Edward Norbeck, who hired me at Rice University when I finished my Ph.D., was another major influence in my life and career. After I was hired in 1961, he encouraged me to follow my own interests. In other words, I decided what I wanted to teach and what kind of research I was going to do, and he never interfered. Ed was extraordinarily efficient, an excellent editor, and a generous colleague. His help during my formative years as an independent researcher was critical.

I would be remiss if I didn't also acknowledge the collegiality of the faculty at Rice University. I began there at a time when jobs were relatively plentiful in all fields and Rice was expanding its faculty. At this relatively small but elite University, it was easy to get to know people in all fields, engage in lively discussions, and get answers to any esoteric question. I had friendly relations with the president, provost, deans, faculty and students. We played squash and softball and partied together. The university provided a stimulating environment for a young archaeologist.

I also have to mention a man from Iran, Sekandar Amanolahi, whom I met in 1965 when I was excavating a Paleolithic site about 25,000 years old. One day Sekandar came peddling up to the site on a bicycle and said something to the effect, "Hello meester," which was the normal greeting for people who were not yet fluent in English. He explained that he was a secondary school teacher and that he wanted to get further education. He had done some work with foreign oil company personnel and had learned a little English in the process. He had also studied on his own from an elementary English textbook. Fortunately for him, a group of Americans – I've forgotten which group it was – were looking for bright young Iranian students to bring

to the United States. He came to their attention and impressed them with his enthusiasm and potential. They offered to take him to the United States and enroll him in college. He was overjoyed, of course, but the Iranian government would not let him go because he had a commitment to teach (in lieu of military service) and he still had a couple of years left to serve. Eventually they did bring him to Washington, D.C. and enrolled him in Howard University, which they thought would be a suitable place for him to improve his English and gain experience in the American way of life and educational system. Over the next several years, I stayed in touch with Sekandar, and when he finished his undergraduate courses he said he wanted to attend graduate school. I invited him to come to Rice University where I was teaching and we developed a collaboration in Iran, which resulted in my discovery of the ancient camp of nomadic people.

While I was at the University of Chicago, I became a student of Robert Braidwood, the man I mentioned earlier. He was something of a maverick in archaeology at the time in that he was practicing what he called "problem-oriented research." Today this sounds strange, but the fact is that much archaeology of that time was being practiced by dilettantes, people affiliated with museums whose principal motivation was to secure objects for display. These people were well-funded and they specialized in excavating the largest sites where they might find interesting material, their principal justification for digging. Often this work was based on historic sources or cuneiform texts.

Braidwood departed from this approach and asked, "If you could go anywhere in the world to find out how people developed agriculture, where would you go? How would you do it?" He created an interdisciplinary team consisting of a zoologist, a botanist, and a geologist who put their heads together and began to think about these questions. His team reasoned that you would have to go to where the species are present. You have to go where the animals and plants live in the wild, and where the climate and landscape provide the right conditions.

Braidwood had already had long experience in the Near East and was well aware that the oldest evidence of the foods that fuel our civilization – wheat, barley, lentils and peas, as well as sheep, goats, cattle and pigs – occurred in archaeological sites and were recorded in the ancient texts from the Near East. So he reckoned that these species must have been domesticated somewhere in the region. His team identified an arc of land across the northern part of the Near East – the Fertile Crescent – as being the most likely place to seek origins. On that basis he selected an area of Northeastern Iraq where he carried out excavations that made a convincing story of agricultural origins.

I was invited to join the excavations in 1959, but the Iraqi revolution of 1958, in which the monarchy was overthrown, intervened. This is the revolution that ultimately precipitated the change that led to Sadam Hussein. The revolution prevented Braidwood from returning to Iraq and he was unable to secure a permit to excavate in either Syria or Turkey. But as luck would have it, he had had an Iranian student,

Ezat Negahban, who, since graduation from Chicago, had become a Professor at the University of Tehran and Technical Director of the Iranian Center for Archaeological Services at the National Museum. At an international meeting in Hamburg in 1958, Braidwood told Negahban of his plight. Braidwood reasoned that since Iraq is right next to Iran, the conditions that he found suitable in Iraq should be also present in Iran. He asked Negahban to support his application for a permit to carry out exploration and excavation. This was granted and I joined the Iranian Prehistoric Project in 1959.

While the Iranian project was developing, I took the opportunity to attend Harvard as a special student to take classes with a couple of professors whose work was particularly salient to my developing interests. At the time I was interested in learning about the cultures that preceded the advent of agriculture, as well as to learn the techniques of analyzing flint tools, both of which I could do at Harvard. This led to my being invited by Professor Hallam Movius, to join his excavation in France the summer before going to Iran. The site, Abri Pataud, is in the little town of Les Eyzies, home of Cro Magnon. There I learned how to dig a Paleolithic site some 20,000 years old, an experience that served me well in coming years in Iran when I also excavated sites of similar age and analyzed flint tools.

After the dig in France, I met up with Braidwood's team in Mittersill, a picturesque mountain village in Austria where we purchased the supplies that we were going to need, including three German Jeeps and some trailers before we headed east toward Iran.

In 1959 Europe and particularly the eastern part of Europe had not yet recovered from the Second World War. To me it seemed as if it had not changed much in hundreds of years. This was my first experience abroad and it was utterly fascinating to see the countryside, the people and primitive conditions of life. The drive itself was rugged because roads were not paved and were often nearly impassible, and facilities along the way were rudimentary to say the least. Nevertheless, the trip was extraordinarily interesting because I discovered while slowly driving along these bumpy roads that the people in each valley had their own habits, customs, dialect, culture, and clothing. People stacked their hay in different ways, they built their barns differently and so on. I was fascinated to see how you could go 20 miles and see entirely different customs and costumes. Our trip took us through Yugoslavia, then Greece, across northern Turkey along the Black Sea, and eventually to Iran. I am sure this long, slow trip had a formative importance in the way I think about space and geography, human adaptation and cultures.

In Iran we were going for a nine-month field season and I had already been in Europe for two months. Such a long stretch of fieldwork is unheard of today where archaeologists go for six or eight weeks between academic terms, but it seemed perfectly normal at the time to devote the whole year to fieldwork. Of course I took "normal" to be what Braidwood did. Since Iran at that time was virtually unexplored archaeologically except in a few key places, our first task was to discover sites. This

entailed fanning out over the landscape looking for *tepes*, hills that contain the remains of ancient settlements. People, in the past as today, built houses of mud bricks, and over time, if they lived on the same place, houses were built on top of the remains of previous houses and so a mound built up. When we found a *tepe* we examined its surface for pieces of pottery and other artifacts whose age we could tell by their styles and other characteristics. Our focus was on the earliest settlements so we ignored a great many of the hundreds of *tepes* that we found. We spent about three months going out every day, until the weather became too cold and snowy.

On the basis of these surveys we selected some sites to excavate in the spring. The site that I was most involved with turned out to be rather interesting and set the stage for much of what I did in subsequent years. The site, Tepe Sarab, had pottery and flint tools on the surface, similar to what Braidwood had found in Iraq, so it met his expectations that it was an early agricultural site. But there were some curious aspects to it. First of all, it was not a *tepe* and when we excavated the site, we found none of the expected building foundations. Instead we unearthed a series of layers of dirt and ash, filled with figurines and pottery, animal bones and so on, but no houses. Moreover, it didn't have any of the utensils that one normally associates with agriculture like grinding stones and sickles and bread ovens. Thus, the site was an anomaly, difficult to understand.

However – another case of good timing and luck – it was possible at that time in Iran to observe tribal people who migrated seasonally with their herds from the lowland areas of Iraq or Iran up into the mountains during the summer, and then reversed the trip before the onset of winter. These people lived in tents and moved with their sheep and goats following the pastures. It occurred to me that the site we were excavating might be a camp of the nomads like these. That surmise was effectively supported when I was out doing survey one day and came across a camp of people who had just come up into the mountains. They had their black tents of woven goat hair, but they were building shelters out of tall *typhus* reeds. When bound together, these reeds, which are about ten feet tall, were used to build simple arched structures. As soon as the people got those built, they gave up their black tents, which are vermin-laden and very hot in the summer and they moved into these straw structures. At the end of the season they burned the straw and left what effectively looked like the kind of thing we had been excavating. So I was pretty intrigued with that, although Braidwood was not persuaded.

Following the field season I returned to the University of Chicago, where I wrote my dissertation, in part on the site of Sarab. When I finished my Ph.D. dissertation in 1961, I accepted a job at Rice University in Houston, Texas

With a job in hand, I went back to Chicago and Braidwood asked me what I wanted to do next. I told him that I had just taken a job at Rice. He said, "Yes, but what archaeological project would you like to do next?" It so happened that I had been reading geological literature about climate change and an article by Wally Broeker

particularly intrigued me. He had new evidence that climate after the end of the Pleistocene had changed significantly. The significance of climate change was that Braidwood believed that the first experiments with agriculture occurred in the lower parts of the mountains on the edge of the Fertile Crescent. In his scenario, people learned to grow crops in the zone of their present native habitat and later moved down onto the Mesopotamian plain of Iraq where the first cities and civilizations emerged. The image he used was of people "fingering down the hilly flanks of the fertile crescent." In other words, they moved slowly down from the mountains as they learned to adapt to the lowland area.

With Broeker's findings in mind, I reasoned that if there had been climate change agriculture must have gone the other way, from lowland to upland. When I told Braidwood that I was going to show that his scenario was wrong, he told me to go out and prove it. Since I had just been hired at Rice I did not imagine that I could start any project immediately; however, Braidwood suggested that I ask Rice if they would let me take a semester off to carry out my project. I called Norbeck and told him what I wanted to do. He agreed that I could delay my appointment and start in January. When I told Braidwood that I had the leave, he said that the Oriental Institute at Chicago would put up half of the money if Rice would contribute the other half. I called Norbeck again and told him that I appreciated the leave, but asked whether Rice would fund half of the project. He said, "Yes".

I skipped my August graduation at Chicago to start the project. With Kent Flannery, a graduate school friend of mine, who had joined Braidwood's project as an assistant to the project zoologist, we set off by boat for Iran via Europe.

Given my hypothesis that agriculture had started in the lowlands and moved up into the mountains, the question was, how do you test that? Geography plays a really important role here. The Zagros Mountains of Western Iran consist of a series of parallel ridges with valleys in between. As you travel from west to east they get consecutively higher, like a staircase. You start down on the Tigris River which is low and then you go to the first ridge which is a little higher, and you go across another ridge and it is a little higher, another ridge a little higher. Eventually you get up to the top. With Braidwood we had surveyed in the high part where there seemed to be endless numbers of archaeological sites. My plan, then, was to start at the top and work down to the bottom. If my idea was right, the sites would get older the farther down we went. It seemed easy, a perfect research project. The only problem was we found no sites as we descended valley by valley. Clearly we had to figure out what was wrong.

Before enrolling in archaeology, Kent had studied zoology and had a keen appreciation for ecology and ways the various parts of an eco-system interrelate. Our dilemma presented a challenge to Kent and he and I spent long hours debating a variety of hypotheses. Every ridge and valley told a story that we were trying to decipher. As we accumulated knowledge our ideas became more refined and we began to get a better appreciation for how humans adapt to the varied topography and climates of

Western Iran. For our immediate problem, to find sites of early agriculture, it turns out that the critical variable was whether or not water was accessible at the surface. The valleys and ridges were excellent pasture seasonally, but they were not suitable if you wanted to live there year round. Even long after people had learned to dig wells, the absence of settlements was remarkable. By the time we reached the lowest valley we were discouraged about the fate of our project and wondered what we would do with all the extra time. Fortunately, when we got to the bottom, we did find what we were looking for and it was really interesting.

We found a site, Ali Kosh, which was much older than Sarab, the site we had excavated with Braidwood's team, so I applied for a permit to do a quick test excavation.

The excavation turned out to be important, but for reasons that I didn't expect. Let me digress before continuing the story of the excavation. Another instance of good timing and good luck was that after the National Science Foundation was founded Braidwood got one of the first grants to do the Iran project. Braidwood also had a colleague at the University of Chicago, Willard Libby, who invented radiocarbon dating for which he received the Nobel Prize. Braidwood provided material for Libby to test his method and was one of the pioneers in making use of radiocarbon dating. As a result, I was attuned to the idea of getting charred material that we could date. During the excavation, Kent and I carefully collected bits of charred wood from the site and I brought them back to Houston. Through the good offices of Kenneth Pitzer, President of Rice, I was introduced to the Shell Development Company, which had a radiocarbon lab at their research center in Houston. I took my bag of charred material to the lab and asked if they could date it. They looked at it rather dubiously and remarked dismissively, "It's a bag of dust and we can't do much with that." I was a little disconsolate, but they told me that if I could pick out the good stuff they would date it. I asked how to pick out the good stuff. The lab technician said, "Wash it." So I went home, filled a bowl of water in the sink and I poured the stuff into the bowl. Almost immediately the loose dust fell to the bottom and seeds floated to the top. This was my Eureka moment, a major breakthrough in understanding the history of agriculture, and it changed the way we do archaeology in the Near East.

To know the history of agriculture you had have some tangible evidence of it. You could not just infer it from the presence of grinding stones, houses, sickle blades or storage bins. So, this discovery was very exciting. Again good luck intervened. With Braidwood's team there had been a Danish botanist, Hans Helbaek, the world's expert on ancient seeds. He had spent his career painstakingly picking a seed or two out of a site, but except for historic caches, he never had enough evidence to work with. More importantly, he had never had enough seeds to help him reconstruct the early stages of agricultural development. I sent him a telegram telling what I had found and then sent the seeds. He replied immediately, saying that I must return to Ali Kosh and he was going to come with me to analyze the seeds on the spot. What I had done in my kitchen sink was apply a technique known as flotation, which is

now used routinely in archeological sites to separate seeds and other charred material from the soil matrix. As luck would have it, the technique was being pioneered on the Illinois River by another fellow graduate student, Stuart Streuver, who gave us suggestions for setting up a system in Iran.

With an NSF grant in hand, Flannery and I went back to Ali Kosh and, using flotation, found thousands of seeds, where before we had no idea they were present in the site. Our field camp at Ali Kosh was literally in the middle of an arid steppe where we had no water. Fortunately, I was able to find a man with a tank truck who carried water once a week to the settlement of Deh Luran. On his way to Deh Luran he stopped at our camp and filled 55-gallon drums with water, enough for cooking, drinking, washing, and for floating seeds. With the primitive flotation process that Flannery was able to set up, and despite the little water we could spare, we came out with what was the best and most unusual collection of early agricultural material anywhere in the world. Helbaek's analysis of the seeds became a landmark study and formed an important basis of the reports we wrote. I attribute much of our success to good luck and good timing. I had met the right people, they were available at the right time, I had serendipitously found seeds in the residues of the radiocarbon material, and NSF was making grants to support such research.

Although Ali Kosh was occupied near the beginnings of agriculture, we also tried to excavate a long sequence. I didn't just want to stop where the first domestic seeds and lives appeared, but I wanted to see what happened afterwards. For this excavation I enlisted the aid of James Neely, whom I had met while excavating sites in Mexico. He was in charge of a later site, Tepe Sabz, which allowed Helbaek to expand his study from the early stages to advanced agriculture. During analysis of artifacts from the two sites, I realized that a gap of some centuries existed between them, which needed to be filled. I wrote another proposal and we got a permit to dig a third site, Chagha Sefid, which would attempt to fill this gap. The gap was important because Helbaek had found that during the early part of the sequence, agriculture was completely indigenous and rain-fed. People had domestic sheep and goats, but they were still doing a lot of hunting of gazelle and onager, the local wild ass. But after this gap they had irrigation, hybrid grain, and were using mostly domesticated animals. With the excavation of Chagha Sefid, we filled the gap. We were able for the first time to track irrigation from its origin in Iraq to its subsequent development in Iran.

It seems to be the case that each completed project leads to another. In the sites in Deh Luran we had found large quantities of bones of domestic livestock. It occurred to me to ask what happened to the local people who had been displaced by the irrigation folks, and what they were doing with their livestock. Now Deh Luran is a very remote place in Western Iran, located on the Iraqi border and the temperature rises to about 130F in the summer. While it is not a pleasant place to stay in the summer, the mountains are easily accessible and just a few miles away. The modern day people migrate into the mountains in the summer, so I thought maybe that is what they were doing 8,000 years ago. This time I organized a project in collaboration with Sekandar

Amanolahi, whom I described earlier. Sekandar was about to embark on his dissertation research on tribal people of Iran and he was a member of a tribe that still migrated. I told him that I would help him on some aspects of his dissertation if he would help me organize a trip with one of the tribes that wintered on the Deh Luran Plain. I told him where I wanted to go and which people I wanted to go with. He agreed and went to Iran to make the arrangements. A month later when I got to Iran I asked him if we were ready to go. He said, "There may be a problem, I don't think we ought to go with the people you want to go with." He suggested another group. I was a little bit unhappy about that because I had my heart set on one particular tribe. We talked about it for some time, and then he said, "If we go with the other one, they'll kill me."

Following his advice, we joined a group with whom he had relations and we carried out our migration study. My object was to learn how people manage the herds, where they camp, what routes they use, what problems they face, what technology they use, what kind of remains they leave, and so on. My basic questions as an archeologist were how can I find sites of nomadic people, and when did nomadic pastoralism begin? We joined a camp of Baharvad Lurs who lived in five black, goat-hair tents. After breaking their camp in their winter pasture, we headed up the staircase mountain ridges and valleys to their summer pastures. Along the way, I searched for evidence of ancient campsites, but spent most of my time asking questions and observing nomadic life. Following the migration, Sekandar and I spent some weeks traveling through tribal territories visiting other camps of transhumant herders. Normally the tribal people in Iran are very hospitable and always invite strangers in for tea or a meal. Late one afternoon we were invited for dinner at the house of a local sheik. We had a pleasant meal with good conversation, but afterwards as we were leaving, the man said to Sekandar, "A number of years ago I buried two of your kin alive." It was a sobering experience for Sekandar as well as for me, and a reminder that hostilities among tribes still run deep.

The only time I had a problem was when I was detained by well-armed tribesmen in Kurdistan. I had inadvertently lost my way along the Iraqi border and entered into a zone controlled by Kurdish militia men. Normally when people stopped us they would make tea and socialize a little bit while they decided whether we were a danger or not. The local tribes' people often had never seen a foreigner and they were as intrigued with us as I was with them. In the Kurdish incident Sekandar and I were taken to a small base on the Iraqi border where a very young, apprehensive Army officer was in charge. A recent graduate of Tehran University, he had been assigned to command Kurdish tribesmen, whose language he did not speak and of whom he appeared to be deathly afraid. After prolonged pleasantries with his tea and our supply of fresh oranges, he ordered the men to take us to the right road, while he returned to looking down the barrel of a gun aimed at his position from across the border.

After traveling with the nomads, I wanted to find an archaeological site of these people who leave few remains. Again I was lucky. A colleague of mine, Henry Wright,

who was looking at late historic irrigation canals, told me that he had found a canal bank that had some early pottery on it. Intrigued, I went and looked at it and found that the pottery was about the same age as at Ali Kosh. I applied for a permit for a two-week excavation.

Before I was able to begin an excavation, the land was leveled in preparation for a large-scale irrigation project. Large earth graders were lined up side-by-side and driven across the landscape removing all irregularities. When I went back to dig the site I noticed a lot of places in the leveled fields that had lines or clusters of rocks. This seemed unusual because the land was essentially flat and consisted of stoneless soil. My first reaction was that the rocks looked like the remains of nomad camps, of which I had seen dozens. When they set up their camps, nomads lay a bed of rocks to keep their bedding and other belongings off the ground. At first I thought, this must be a modern campsite, but I wanted to be sure, particularly as there was early pottery here. As we dug, we discovered that the campsite was 8,000 years old. We had hired workers from a nearby settlement who had been nomadic until settled recently by the government. The men thought the site unremarkable, despite its age. They said, "That's the way it's always been." This was the first and probably the best example of an ancient pastoral camp that we have anywhere in the Middle East. Again, this was a case of luck, my being in the right place at the right time and knowing the right people.

Chance intervened again in my career when the Iranian Revolution took place. With no further possibilities of doing fieldwork in Iran, and after a five-year term as Editor of *American Antiquity* and a move to Yale, I shifted focus to Syria. In the early 1980s, the Syrian government was building dams on some of the rivers and they wanted excavations of sites that would be inundated by the new reservoirs. In 1984 I was invited to select a site on the Khabur River in northeastern Syria, a tributary of the Euphrates. Among the possible sites a small mound, Umm Qseir, particularly interested me. This was a site about 7500 years old in a semi-arid landscape, which was not suited to agriculture without irrigation. Although the site was on the bank of the Khabur River, it was a long distance from any known contemporary site and these facts made me wonder whether it might have been a seasonal camp, perhaps of nomadic people. The excavation revealed that it had been a small, short-lived settlement, but did not inform on the possibility of seasonal use. By this time I was once again intrigued with the idea of devoting time to finding sites of ancient nomads.

I organized a survey project to explore a landscape that was largely devoid of modern settlements, but had recently been territory of nomadic tribes. The Syrian government assigns a government representative to each project both to help and to make sure that the archaeologists adhere to the terms of their permits. Survey during the summer, bumping around in an ancient Land Rover, is not a preferred job for a government representative who would rather stay at an excavation site, drink tea, and talk with the locals and rest, unless specifically needed. The head of the local Antiquities office assigned us a woman, knowing full well that as a woman she could not go

with us. Although for administrative purposes she was technically responsible, in her place they sent a man, Slimu, who worked in the office as a houseboy. It was our good fortune to have him take the woman's place, for he was a former nomad who knew the country backwards and forwards. We told him what we wanted to find, and since he knew from first-hand experience where nomad camps should be, he could take us to them. This was amazing because there were no roads, no fences, nobody living on the land. We would drive along and he would point this way or that way, and sure enough we would wind up at possible campsites. The way he could navigate was uncanny. With his help we traversed an enormous amount of territory and were able to find the evidence of nomad camps we were looking for.

But more importantly, we found much more than camps of nomads. We discovered many ancient settlements on this largely unoccupied featureless steppe. The area we explored is barren today, yet it contains many large mounds where people had apparently lived and thrived in the past. However, we found considerable discontinuity in the settlement sequence. Based on pottery on the surfaces of the sites, we estimated that there was something like a 1000 to 1500 year gap between the times when people were living there. My immediate reaction was that the climate must have fluctuated. To me this was obvious, because people could not grow crops and live there unless it had been wetter than today. My archaeological colleagues were not impressed: "This can't be. We don't believe in environmental determinism." Nevertheless, to me it seemed obvious and I published my ideas, admittedly based only on the indirect evidence of discontinuous settlement in a very dry region. Eventually, however, other supporting evidence has accumulated and periodic fluctuations in climate are now well established, and I think they can be tied directly to those episodes of settlement and abandonment that we saw.

After the survey I did a project that other people had never attempted and many thought was quite irregular: traditionally one digs a site, not a dozen sites. What I did was to carry out an opportunistic sampling of previously excavated sites to obtain material for radiocarbon dating, as well as recover seeds and animal bones to use for reconstructing agricultural practices and the environment. Because of the imminent destruction of sites when the new reservoir was finished, a dozen to fifteen international groups had done some digging in this area to salvage what information they could. My project at Umm Qseir was one of these. In many cases the archaeologists had failed to make timely publications of their results or even to recover botanical and faunal evidence. However, when they had finished digging the archaeologists left openings in the sites that I could examine and from which I could extract the material that I wanted. Seeing this potential, I visited as many of these sites as possible and recovered the material before the sites were inundated and no longer accessible.

The project was relatively inexpensive, it was easy to do, and it allowed me to build up a nice chronological sequence of the different periods represented. In fact, I was able, for the first time, to produce a local chronology based on radiocarbon dates.

The botanical material that we recovered has also become extremely important in understanding changes in agriculture and it has been incorporated into a global database for these materials at the University of Tübingen in Germany. Both the botanical and faunal remains have figured importantly in a developing picture of the varieties of agriculture and animal husbandry through the millennia.

The next lucky thing that happened is that Ron Smith of the Yale University Department of Geology started the Center for Earth Observation to make use of satellite imagery that had been obtained by the United States over the last 30 years. NASA was making large grants for investigators to figure out what you can do with satellite data. We decided to join this pioneering effort and also develop a course to teach students the techniques of satellite analysis, but first we had to learn the technology ourselves.

As our first application at Yale we tried to look at the relationship between precipitation and the way plants grow. At the outset that seemed to be a pretty simple kind of relationship, but it turns out that using the satellites available at the time, you could not see much by looking at the forests of New England, because seasonal differences in precipitation are not very great. I suggested that we should look at the Near East where there is a stark contrast between the land and the plants. Moreover, winter rain-fed agriculture, which ripens in the spring, shows up as bright images. By contrast, the rest of the year is dry, and only irrigated agriculture would be green. By combining rainfall records with changes observed in the satellite images we were able to develop an understanding of how crops and other vegetation responded to rainfall.

As satellite imagery and our skills improved we were able to chart changes in the ways people had used the land over some 30 years of imagery. That is, we developed time-series studies, which eventually led to my most recent research. By focusing on the Khabur River Basin where I had carried out my archaeological research, I could extend the time series of land use back in time some 9000 years. This made use of the information that we had already collected on episodic settlement, as well as historic records of government policies, economic factors, agricultural techniques and population changes. In combination these allowed me to see how the modern situation developed and to address issues of future sustainability. Agriculture is imperiled as a consequence of social, political and economic factors that have developed over more than half a century. The land has been degraded by intensive cultivation and irrigation, the water table has fallen catastrophically, and the river no longer has a natural flow. In the past several years I have given presentations at conferences in Europe and Asia on sustainability, as well as published a number of papers that describe how government policies and interventions have alternately encouraged exploitation of the land for economic benefit, and attempted to mitigate the consequences.

Inevitably considerable unpublished residue remains from some of my past projects and a great many new techniques have been developed that can be applied to the artifacts I have collected. Fortunately I was able to bring most of the excavated material from Iran and Syria to Yale where it can continue to be analyzed. Today neither

country allows the export of material and the collections that I have given to the Peabody Museum are among the most complete and important anywhere in the world.

I think this brief reflection on my research in the Near East illustrates how lucky I have been over the course of some 50 years to be able to capitalize on opportunities and to follow my own inclinations. I could not have done this without the support of friends, teachers, students, colleagues and my family. To all those who have stood with me and encouraged me, I owe a profound debt of gratitude. I should say in conclusion that I am not finished while there are still good ideas to pursue and data that I have not yet analyzed.