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Maine Folklife Center

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| Interviewer Pauleena MacDougall and Adam | | | | | |
| /Depositor: Lee Cilli | | | | | |
| | | | | Narrator: David Sanger | |

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Description: **4008 David Sanger**, interviewed by Pauleena MacDougall and Adam Lee Cilli, July 18, 2013, in his office in South Stevens Hall at the University of Maine, Orono. Sanger talks about the beginnings of his career in archaeology; his beginnings at UMaine and the Climate Change Institute; conducting research in Maine and New Brunswick; the relationship between archaeology and climate science; and how, at UMaine, the anthropology department contributed to the CCI and vice versa.

Text: 11 pp. transcript

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Related Collections & Accessions Restrictions

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Notes

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To transcriber

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Narrator: David Sanger

Interviewer: Pauleena MacDougall

Transcriber: Adam Cilli

Date of interview: July 18, 2013

ABSTRACT: This interview took place in David Sanger's office in South Stevens Hall. The interviewer, Pauleena MacDougall, was accompanied by her research assistant, Adam Cilli, who took notes during the interview but did not ask questions. In the beginning of the interview, Sanger discussed how he became interested in archeology. Later, he talked about how he came to the University of Maine and about his experiences with the Quaternary Institute. He also discussed the relationship between archeology and climate science and how, at the University of Maine, the anthropology department contributed to the Institute and vice versa.

Note: This is the transcriber's best effort to convert audio to text, the audio is the primary material.

MacDougall: This is Pauleena MacDougall. I'm in the office with David Sanger, archeologist at the University of Maine and we're going to be talking about the Climate Change Institute history. So, could you tell me a little bit about how you became interested in archeology in the first place?

Sanger: Well, I was an undergraduate at the University of New Brunswick, and not really knowing what I wanted to do. And just by accident I got into an anthropology course. And I discovered that this was something that I really enjoyed because it was a social anthropologist and he's trying to teach us physical anthropology and archeology. And I became fascinated by it, and very soon cleaned out their library, such as it was, on those topics. And Professor MacFeat said to me one day, "You know you can make a living on this." And I said, "Oh, I didn't know that!" And he said, "Yeah, but you have to go to graduate school first." So from there I went to the University of British Columbia; in those days there were only two universities in Canada that were offering graduate degrees with archeology specialty. One was Toronto and the other was British Columbia. And I arrived out there to work with Professor Borden, who had never heard of me when I showed up. And, because I wanted to continue working on the Pacific Northwest, I applied to a number of universities for a Ph.D. And they said, "Oh fine. Come along, come along." But only two, the University of Washington and the University of Oregon, were willing for me to do a dissertation on northwest coast archeology. So I met the archeologist in charge at Oregon; decided there was no way I could work with him. And then went to Washington, and it turned out to be a very good choice.

MacDougall: So, how did you end up at the University of Maine?

Sanger: Well, the Canadian government had supported my research in Canada. And so, when they started to expand in the mid-60s, they began looking at some of the bright young people they had been supporting over the years (king of thing), and they offered me a job as their west-coast archeologist. When I got to Ottawa, I discovered that I'd already been flipped with

someone you may know of, certainly George MacDonald. He did his dissertation on the Debert site in Nova Scotia, as you know. And he decided that the northwest coast was more interesting. So when I arrived in Ottawa, on a brutally cold morning (1966 I guess it was, or 1967), I was told, "you are now our east-coast archeologist." Well, you went to UNB, so you know the landscape. Okay. So I stayed there for five years, but I was taking out student crews on a regular basis, from the University of New Brunswick, University of Montreal; I was lecturing at the University of Montreal. And I decided I like being with students more than I liked being with administrators, and so I started looking around. And Dick Emerick, who as you know, Pauleena, wanted me to replace Dean Snow. But this was a very small department and I really couldn't see myself here. And then I got a call from Hal Borns. He said, we're thinking of starting up this institute and would like you to come down and talk with me and George Denton and see what you think. And I'd always been interested in doing an interdisciplinary approach; I'd worked with geologists on the West Coast. It was an area that interested me, so when I found out that, yes in a couple of years we hope to go down to the administration and get an institute officially blessed, I decided "okay, that sounds like a good spot for me." So I came down here in 1971.

MacDougall: And when you came, it was with the understanding that the Quaternary Institute was going to be established, but hadn't yet. So how did you develop your research at that time, without that? 'Cause I understand that's sort of a joint appointment.

Sanger: Well, when I first came down it was no joint appointment. It was 100% anthropology, and I had done several years of research in New Brunswick. So, it was easy, of course. As you know, there was no order in the old days. So, I came down here and I inherited Bob McKay. You knew Bob and Gene. He ran the lab. And he'd been doing a little bit of introduction to archeology teaching and really found himself out of his depth. He had a BA, and he was not particularly sure of himself in the classroom situation. And I came down here and I found out that he and his students had been down in Hirundo and had a very interesting collection already, which tied in nicely with the Red Oak cemetery that I had been working on at New Brunswick.

MacDougall: So these were archaic period...

Sanger: Yeah. Late-archaic specimens. We now know that some of them went back to the middle-archaic, too. So, we immediately applied for funds from the National Geographic Society and got three years of funding for Hirundo and I wanted to continue my Passamaquoddy Bay research so I applied to National Science Foundation and built in palynology and sea-level rise into that grant. So I had both the coastal and interior thing going right from the get go.

MacDougall: And were already adding the other disciplines to your research. Who did you get involved with?

Sanger: The sea level rise had never been done before around here, and I got Hal involved with that. And he had a grad student, a chap by the name of Thompson, and he went out there and looked at some submerged forests in the Lubec area. And I'd be interested in vegetation change, inside climate change. And Ron Davis had come here the year before I did, and he agreed to participate in that; he took some cores Down East and began working in those three elements: the geology, the palynology, and the archeology.

MacDougall: So, it sounds like one of the reasons you were particularly intrigued by the Quaternary Institute was the opportunity to do interdisciplinary research. Now, did that create any problems for you with anthropology?

Sanger: No. When I came, the anthropology department was already (I believe it's true to say) unique in the old college of arts and science, in that we all had a two plus two teaching load. Dick Emerick apparent went to the dean one day and said "I think we should do a two plus two teaching load" and the dean said ok.

MacDougall: But what does that mean exactly?

Sanger: Two courses each semester.

MacDougall: And is that unusual?

Sanger: Yes. A more normal load at that time was 3 and 3 or 3 and 2. And I'd had enough experience to realize that when you do 3 and 3 there's not too much chance for research. Two and two... ok, especially once you've got the courses started. 3 and 3? Killer. So, I was 100% anthropology, and when the Institute was officially formed, the old personnel action forms had to be redone. And during that summer, Hal initiated the forms. By that time he'd been appointed by President Libby as the director of the Institute. He sent them over here, and Dick Emerick, bless his heart, never looked at anything he signed. He just signed his name, sent the forms back, and in doing so gave away two thirds of my position at anthropology. I was more than a little upset, and it was agreed that if it didn't work out we could pull back the forms and I could go back to 100% anthropology. It didn't make much difference to the department, except that I taught one less course. Even still, I was teaching way more than the other people in the Institute. So, I'd teach two courses one semester and one the next, with the expectation that I'd be getting involved with graduate work somewhere in the future. And in those more naïve days, we were not so concerned with actual numbers of positions in the departments. It wasn't all that difficult to add a new member if you could justify it. Funds weren't as tight in those days. There may well have been colleagues who said, "how come Sanger's not teaching as much as I am?" But it never came to me if that was the situation. So, I would say that, had I been chair I never would have signed that without a lot of questions. But Dick Emerick just signed things. And that's all there was to it.

MacDougall: So he was open to people doing more research... and supported that?

Sanger: Very interesting paradox. If you talk to Jim Acheson you would here that Dick Emerick didn't want anyone to have anything to do with research. He never once made a comment to me about doing research. But I know he and Jim Acheson had some bad times over it. Dick himself didn't do research, as you know. Sandy, of course, was working on things. And Ann Acheson, then Ann Tomerkin, was trying very hard to finish up her Ph.D. so she was tied up. Jim Acheson was getting his research going on the coast. So I think Dick was pleased that I was doing research, taking students out in the summer, getting the anthropology department in the newspaper, you know, covering digs and so on. So I never experienced any reticence to support me as I needed for my research.

MacDougall: Now, I understand, from Hal, that one of the expectations of those who became part of the Institute was that they would bring in significant research funds, and that way support

their own research. Was that your understanding, in the beginning?

Sanger: I knew the University had no money to support research. So, that's why I went to the National Geographic, why I went to NSF.

MacDougall: What do you think has been the Climate Change Institute's greatest contribution, in terms of findings?

Sanger: Findings is difficult. As you know, we started off, it was a very small group. And was very easy to be very much aware of what the other members of the group were doing. We had lots of seminars. When we brought in outside speakers we all went to seminars. It was a very tight-knit group. There were two levels involved. On the campus level, we soon developed a reputation for, "if you want to start an interdisciplinary institute, go and see what Hal Borns and the Quaternary have done." And that's always been kind of a model held up. Except that initially many departments were not willing to have shared salary lines, which always gave the Institute a cohesiveness that many other loose affiliations of people didn't have. Some of us more depended on the contributions of others. As you know, in archeology you can't very well do your own geology; you can't very well do your own vegetational history. You've got to depend on other people. Other folks really didn't need that kind of interdisciplinary effort from folks locally. So, George Denton tended to work with people all around the world, depending on where he happened to be working. So interdisciplinary work, yes, but it wasn't dependent on the people working on campus here to do that. And I found out very early that it's a lot easier to do interdisciplinary work when you can just call up and say, "Let's have a cup of coffee and talk about..." If that person is in California, then we'll see you at the next conference. It doesn't work. You know, maybe you ship a paper over and comment on a paper. But really sitting down and talking about things... And so I think because I was working locally, and Hal was working locally, Ron Davis was working locally, and then George Jacobson came along and was working locally, and they had students who were doing Maine/Northeast work, I really had that advantage. And I really like to think I took advantage of it. And the same with when I got working on the coast and the oceanographers. I remember the first time I went down to NSF and talked to the program manager, who at that time was a social anthropologist. And he said "Institute. Interdisciplinary. Tell me, what is your social organization? Is it just held together with a paper clip or are you people really working together?" And when he heard that salaries were involved, that peaked his interest quite a bit. So, as far as eastern archeology goes, there weren't too many people who were trying to bring together palynology and sea-level rise and things like that. I guess there were none, when it comes right down to it. Especially not building it into the research before you even go into the field. I think it's common for archeologists to do their work and then follow, "Oh, let's see, I've got some soil samples. Who can analyze the soil samples?" We developed a methodology of building all of that in before we started into the field. And I think that was a contribution, a methodological contribution. But, in terms of what the Institute was doing, we had a strong Antarctica program. Hal and George Denton had worked in the Arctic. We had a grad student here, Vebeon Carlen. I don't know if you remembered Vebeon. He married Jesse Kolb. And Jesse and you might have overlapped; I'm not sure. He was a physical geographer from Sweden, and they did some very good things together. So, various people did their own thing, but they were always there to assist and answer questions if you had them. George Denton and I had a program that was about ready to go up in the Canadian Arctic, but it fell down on permanent issues with the Canadian government. So,

people were very willing to be interactive and help out in a lot of ways.

MacDougall: It sounds like one of the areas the Institute has been very strong in is in Maine prehistory. Would you like to comment about that?

Sanger: It's local, and it's so much easier for us to do archeology in Maine than it is for Dan Sandweiss to go down to Peru. We don't have to worry about permits; we don't have to travel far. It's just a whole lot easier. And, as you know, there's a good archeological resource here. So, that's the reason why I deliberately didn't try and start up in the world somewhere. It's just too easy to do it here. And the work needed to be done. Our understanding of Maine pre-European history was pretty weak when I came here in the early 1970s. Some basic outlines, yes, but we didn't even know there were actually Paleoindians in Maine at that time. It took a while to come along, so there was lots of things to develop.

MacDougall: How do you think the geology and paleontology research in Maine has assisted your understanding of archeology?

Sanger: You probably remember that the term we use for the last ten thousand years is the Holocene. And most people think the Holocene means things haven't changed. And the more we began to look at the paleoecology, the geology, the sea-level rise, the more we began to see things have changed a lot. The river systems were not fully integrated at the end of the Paleoindian period. They continued to develop. Major shifts in water coming down the Kennebec and cutting off parts of the Penobscot and things like that. So, a lot of those details we simply didn't know anything about. Water levels in rivers... why was it we were only finding Ceramic Period remains in some of the upstream areas? Well, I think the water levels were so low that access to those areas was really limited until about 3,000 years ago. And, as you know, people have to be able to canoe those rivers, fish had to be able to get up those rivers. And in an earlier time we had the Hemlock decline, around 4700 BP and that really opened up vegetation for the Marst Foods. Bringing in a lot of the animals that people really need to live on. The Hemlock forests are pretty sterile. So, I thought you could make some cases for changes in the environment and human responses. Not one to one, of course, but—when you're talking about being able to get up certain rivers at a certain time—that opens up whole new landscapes to people. And I thought those were important things.

MacDougall: Is there anything that you would like to point to that is maybe one of the most surprising or significant things over the last so many years that you've been doing archeology in Maine?

Sanger: Well, I know you're not asking this the way many people ask it: "what jade idols did I find?" We just don't get that in Maine. I think it's an incremental thing. You build up, little by little, and the picture gets more complete, and that allows you to take a jump into something else. Certain things I keep come back to. I just published a paper, for example, again on swordfish, which I hadn't visited since 1975. That *Arctic Anthropology* paper. Now, to understand the history of the swordfish in the late-Archaic and people on the coast, that really demands that you get into, not only swordfish behavior but also currents, sea level, temperatures. There's a whole series of things that go together. And they all come together and you make a story out of them. There's no eureka moment; it's just a "these all seem to fit together pretty well." And then you throw it out there and you hope that your colleagues will believe you.

MacDougall: Well I understand that there have been some disagreements over the years, among archeologists, about how to understand various findings. One of the things I'm interested in that you have written about is the idea of there being different adaptations to the interior and to the coast. Can you summarize that a little bit?

Sanger: In parts of the central Maine coast, and that would include the Penobscot Valley, instead of a single population moving back and forth, as ??Extromer laid out, and Speck afterwards, that there was an interior population and a coastal population. And trying to show that archeologically was quite difficult, given the lack of preservation in the interior. As you know, you have coastal clans and interior clans. Was this something that went back in time? Were there dialectical differences that went back in time? So I started looking a little more closely into that and trying to see, could I get at that archeologically? Well, one of the things is, if people were on the coast only in the summer there shouldn't be any winter sites on the coast. And I began finding winter sights on the coast based on the former remains. So, in a sense I posed the question back: if we had people living on the coast, sites being occupied the year round, then who's living on the interior? And that's when I started to realize, probably I first published on that in the 1990s, more likely than not there were people who spent most of their year in the interior estuaries and embankments, and then there were folks who lived most of the year further upstream. But I always suspected they came together at places like Eddington Bend or others where, you'd get together for the rendezvous, for exchange of partners, that kind of thing. Which has got to keep a certain amount of homogeneity in the area but still people would know that "home is up here, home is down here." And the more I worked on that the more it seemed to carry through, not only in seasonality, but also perhaps in attitudes towards disposal of animal remains. Coastal people seemed to have... that allowed dogs to chew bones, they were not burning bones. There were some burned bones but it seemed more accidental than deliberate. Whereas in the interior, there's obviously heavy burning of bone. Which makes more sense, in terms of what the Cree were doing and what some of the early ethnographers recorded for New Brunswick and the Nova Scotian area. And it's even there in Speck, if you can see it. And I felt this was more of an interior pattern. Coastal folks didn't seem to have this concern. They let the dogs chew the bones for example. So I thought there was enough there to... Then Jimn Peterson went into cordage twists and at times when cordage was important there seemed to be an interior pattern and a coastal pattern. So, there was an artifactual difference.

MacDougall: To explain to Adam and others who might be listening to this, cords are twisted and pressed into the wet clay to make a design on a piece of pottery. And so if they're twisted in one way the design would go in one direction; if they're twisted in another way the design would go in another.

Sanger: And this is a motor habit that you learned when you were a youngster. So, you rolled it on your thigh this way, then you roll it on your thigh that way. You don't mix it up, cause your mom would very quickly say, "No, no, no! That's not the way you do it. We do it this way." And, it seems pretty picky but it's held up reasonably well since Jim's early work on that. Brian did something on projectile points at one point that seems to follow the same line.

MacDougall: Brian Robinson?

Sanger: Yeah.

MacDougall: Very interesting. I think clearly we've learned a lot over the years from the 1970s till now. Now, how does archeology contribute to climate change information?

Sanger: I don't think it does very well. It was the hope of a number of archeologists that you could go from one to the other. I did a paper with and Heather (sp)Ranquest in a volume that Dan Sandweiss edited in which I pointed out that because of the human nature, hunters and gatherers especially have the ability to roll with the punches. I quoted some of the work of Frank Speck, indicating that when an area got over-hunted, people would move on to another area, usually following some kind of kin lines. Human beings have this ability to reorganize themselves on the landscape on a timescale that we cannot possibly pick up on the archeological record. We're dealing with centuries; they may be dealing in decades at the most. So, in the case of Maine archeology, and I would suspect almost any sort of temperate area like this, trying to go from archeology to climate change is a stretch... I think so. We have been able to make contributions to geology. There's a record of sea level rise in the archeological record which Hal and I agree on, but Joe Kelley and Bill Knap do not. They take all their data points and they smooth them off at the curve. And the question I have for them is, "I've got a site on the coast, guys, and the bottom level is two thousand years old, and then there's a thousand years old, and then the Europeans came. And if your sea-level curve is right, two thousand years ago these guys were sitting back a mile in the woods, waiting for the sea to come to them. For people that are oriented towards the ocean, it makes no sense. And then, up she goes and we begin to see the rapid erosion. That's because I'm taking the individual data points, and they're rounding the curve. So Hal and I continue to talk about this and I just know that our other colleagues in geology, as far as they're concerned, they've published their smooth curves and that's the end of the story. But I do think that when someone comes back again, takes an independent look, especially at sea level rise Down East, and really thinks about the archeological implications of these stratified sites on the coast. They will see things are not a nice linear line, there's a lot of wiggles. As you may remember, in global sea level rise, you've always had people that talked about wiggles and people who wanted to see just sort of a straight line change. So I'm a wiggle person, as opposed to a straight line person. So that was one area I think that... and then, working closely with Alice Kelley in the interior. When I first started doing my work on the interior Hal Borns said, "Well, you know, ever since the ice went out, the Penobscot region and its tributaries have been in an erosional cycle." Well, we found out that's again a matter of scale. As we started working around tributaries, we found out there was very much aggregational systems going on. So we had in some places up to three meters of archeological deposits, interspersed with flood events. Well, you can't date the flood events very well but you can date the archeological strata. So, we were providing the chronology for changes in the rivers and these distinctive areas where there was localized damming going on through bedrock sills and major tributaries and main branch coming in. So, because of the archeology, we went from "The whole thing's an emotional cycle and you won't find any stratified sites here," to "Yes, there are lots of stratified sites, but within a hundred meters of certain geological features." Which we recognized and took from there. So in terms of the history of deposition in the rivers, we have the technique to date that and point out major gaps in the record, big flood events. So I think that was (again, on the local scale) a contribution to the geology. As for the paleoecology, no. You can't do very much there except encourage people to take lots of pollen diagrams. And the whole thing of lake levels, that came about because I got the funding to have Heather Ranquest Jacobson look at the pond up here. And provided the radiocarbon dates and the resources for her to do the analysis and show that there was a significant period of lower water. Well, I'd always

known during the Hipsithermal or Middle Holocene, things were hotter and dryer, but how much hotter and dryer? She had all the techniques and hard work. I just had the cash for it. And we published a number of papers on it. So, you could see, this is why its important to the archeology; this is where the archeology helped paleoecology. But actual climate change, no.

MacDougall: Well one of the things I've heard from other folks is that, maybe one significant change that's taken place recently is that understanding that climate can change very rapidly, more rapidly than we thought before. And that certainly sounds like something that might have an impact on archeological findings. And I was wondering about sea-level changes and whether that is another area where rapid change in climate could have an impact.

Sanger: It could. Climate change on the larger scale, leading to sea level rise, melting of the ice caps. But I think more important for the coast of Maine is localized signal, dealing with the subsidence of the coast. And its specially prominent in Passamaquoddy Bay.... And much less so in Penobscot Bay. So I think the contributions we're making there are more to the geology, and the geology I think is explicable in terms of lineaments.... as opposed to major climatic events. Now, there's a lot of sloppy thinking that goes on in some of these areas. Line up the events here, line up the events here, look across and say, "aha...correlation." Well, it may not be. And I think it's a really hard burden to show... you can show the collations; this is happening here, this is happening here; maybe they're related.... But even so, why is the glacier advancing? You're not going to get at that archeologically. I suppose I was more optimistic 30 years ago than I am now, in terms of what might be done.

MacDougall: Well, a lot of work has been done. Have you found through your career that you've had good support, financially from grants and so on? Has being part of the Quaternary or Climate Change Institute helped with that?

Sanger: I think so, especially in the early days when we were trying to establish the methodology and reputation building. I think it was important. But later on, when I really wanted their help for the kinds of things I was doing, I found myself not going to the founding members of the Institute. They were mostly too busy. My strategy was to pick a young post-doc. For years I worked with Alice Kelley while she was doing geology for me in the field and trying to message that into a PhD dissertation. I think the most significant work we did with looking at the brown stone materials up here at Gilman Falls. Alice found a young post-doc who did his research in the right kinds of rocks in Maine. I was able to hire him for a year or two. He produced a really spectacular report on the rocks that were being used at the site: where they came from, so on and so forth. I worked with Heather. I worked with... And they were not people who were faculty members. They didn't have full time appointments and lots of other commitments. I had the money; they were willing to work for a year. They were professional people, excellent training. I sometimes felt a little bit guilty but not too much. We published together, and that's the important thing, that they got credit. So, you develop a methodology. I had written some of the charter members into a couple of grants and they never really came through for me. Yet I know that having their names on the proposals was probably helpful. Then you get to a level where you can fly on your own, and then you can build in other people, who do have the time and inclination to get involved. So, I would say it sort of shifted through time.

MacDougall: You've been to the arctic, and the arctic is an environment that's not particularly friendly to humans. Did you find it challenging to work there? And if so, what were some of the

challenges?

Sanger: Well, the field work that I did the arctic, I was really moonlighting. I was working as a geologist, and because we had daylight 24 hours a day, and a fair amount of time off, I could go around looking at archeological sites. But I couldn't do any significant work there. I also worked with arctic collections; so I was not involved in the actual excavation of them, but analysis of them, particularly microblade analysis. So, personally, the challenges were not great. Because I was really devoted to full field seasons. But those people that were, and I knew a number of them because many of them came from Canada, back in the 70s and even into the 80s, it was very difficult logistics. Now, there are tourists who drop in on your site and it's changed completely. Back in the early days, it was a lot more challenging to do things. And the archeology being done now is much better because, in those days, decisions had to be made as to what you brought out. There were a lot of bones left behind... they never came back for analysis; just too much weight to get in the airplane. So that was not an issue for the kind of work I was doing.

MacDougall: Have there been any changes in actual techniques that you use in the field?

Sanger: I think the major thing was, back in 1989, I got the... here's where the University did help me out. They invested in a survey station for me. And there was no one else in the northeast using this equipment then. There was a conference in Tucson and I happened to go down to the bookroom. I saw a young man standing there by himself beside a transit. I started chatting with him; it turned out he was developing an archeological program for gathering data on an archeological site. All the data collectors at that time were designed for survey engineers. And he asked me if I'd like to become one of his beta testers, and I said that I would. So, I went and talked with Ray Hence, and got Ray involved. So this changed things completely. It gave a number of readings outside of each feature. And as we went down about 5 centimeter lifts... and then put the whole thing in an AutoCAD program and then print these beautiful diagrams of the features and how they change through depth. And artifact plotting did away with, "so many inches this way, or so many centimeters that way." Just, bang; right into the computer. Downloaded it every night, and ready to go the next day. So, excavation became more accurate and much easier, in the sense of being able to go directly from the transit to the computer, with no translating in between. That, I think, was a major kind of a change. Otherwise, most of the techniques we used were pretty much standard... excavating squares and certain depths. I think I was the first to use column sampling in the northeast here; so little things to try to get more data out of the ground. Tried an early program of soils chemistry, with Victor Conrad down at Hirundo. So, we tried to bring in as much as we could to enhance the data recovery.

MacDougall: Another area I've observed is a greater involvement with the native community. Want to comment on that a little bit?

Sanger: Yeah, I think most archeologists, by 1970, had pretty much taken the attitude that "well, there were some native people around; isn't that interesting. But they don't have much to tell us. After all, Christianity came in here...change everything." In the west coast I had a lot of experience with native people. Because I could talk to people who were born in the semi-subterranean houses. And I had to talk to them through an interpreter. So I was very much aware of what you could do with that kind of a direct historic approach. When I came to the east I wasn't aware of that until I got involved in the... cemetery up in New Brunswick. And the idea

that the local native people might be sensitive to this never occurred to me, it certainly did not occur to my bosses in Ottawa, and it should have. Then, when I came here to Maine, I got to know Ted Mitchell very well. And Ted provided me with a great education. And the more I got involve the more I realized, well, yes, the native people have lost a lot, but down deep they've retained a lot. And then that became far more important, from the point of view of my understanding their sensitivities. And them educating me. So that was important.

MacDougall: Do you ever see yourself as an explorer?

Sanger: Yeah, I sometimes stand at a sight, when I have time to reflect on things, watch everyone working away, and say, "you know, I'm so fortunate; no one else has seen this slice of history" (since 5,000 years ago, or whenever it was), and I'm fortunate enough to be here to do that, have the techniques to recover part of that." So, in a sense, I saw myself as every bit of an explorer as a guy going off into space or going under water. It was all new. And I think for that reason I felt this information has to get out, either through students or in the written word. Because there are other people out there who are interested. So, definitely there were times when I really felt that.

MacDougall: Did you engage much with the history of the region, when you were trying to find out how to put your archeological data into some kind of context.

Sanger: Well, in terms of what we typically call history, in terms of European written records, no, I never did any historical archeology in this part of the world.... I never had any ambitions to tie the archeology of the pre-European period with the written historic period.

MacDougall: I think, at this point, I would just like to ask you if there is anything at this point that you would like to put on record that maybe I didn't think of.

Sanger: I don't think I have anything very pressing. As you know, because the Institute was successful and because archeology had a reasonably high profile within it, we were able to add more archeologists.... So, the administration was always very supportive. And it's one thing for them to say nice things, but it's another for them to say, "Oh, here's a new archeology position." And I do think that did raise a certain antagonism with certain members of the department; many of them come out of the anthropology department. There were ten social anthropologists to one archeologist, and we turned that model on its head. You know, there were more archeologists running around here than there were social anthropologists. That's not the case now. They never replaced Rick Faulkner. I think they are not going to replace Catherine.

MacDougall: But we have two new people coming on board.

Sanger: They're both social anthropologists. So that leaves, in terms of teaching faculty members, Brian and Greg. So, we're down quite a bit, in terms of where we were at one time. And they did replace me.... So the administration, I think, has been very supportive. Do you remember the name Paul Uttormark?

MacDougall: Yes.

Sanger: He was the director of sponsored programs for a number of years. Now, one of the things Paul liked to do, he had all these statistical formulas he liked to use. And he told me once, "I just don't understand the anthropology department. Most departments that come out high on

the money have small student loads. Anthropology, you have high student loads, have lots of students, bring in lots of money, you publish a lot; it doesn't compute." So, he viewed us as oddballs, but good oddballs. And if only more departments were doing that, and I think that's always been something that's... I think the old institute can take a lot of credit for that, because it really helped to build anthropology. Hal was a very persuasive guy. He would go up to the administration and he would talk up, "this is what we need..." and he usually got it. So I think the Institute was instrumental in building certain aspects of anthropology up. So, there was that kind of symbiotic relationship going there. We had most of the students. You know, for a long time we had most of the graduate students in the archeology side of things. All those books down there are theses [points to books at bottom of bookshelf]. And I don't think any other member of the Institute has that many graduate theses. So we got that measure. And if you want to measure by undergraduate students, anthropology looks o.k. Add up all the funding, looks good. So, it balances out nicely.

MacDougall: Well, thank you very much David.

Sanger: My pleasure.

MacDougall: It's been enjoyable and I appreciate you spending a hot afternoon with us.

Sanger: You're very welcome.