

Interview with Meg Conkey

Douglass W. BAILEY*

Margaret W. ("Meg") Conkey is Professor Emerita of the University of California at Berkeley, which she joined in 1987, after positions at the State University of New York at Binghamton and San Jose State University. Meg took her PhD from the University of Chicago and has made significant contributions to our understanding of the European Palaeolithic, prehistoric art and symbolism, and a feminist and gendered archaeology. Current fieldwork includes the Between the Caves project in the French Midi-Pyrenees. In 1997, she was awarded the 1960 Professor of Anthropology Endowed Chair at Berkeley. In 2009, Berkeley awarded her the Chancellor's Award for Advancing Institutional Excellence for her work to promote diversity and equal opportunity. Meg has also won the Distinguished Teaching Award (1996) and the Award for Educational Initiatives (2001). Meg has served widely and with distinction both locally as the Chair of the Department of Anthropology and as Director of Berkeley's Archaeological Research Facility, as well as being the President of the Society of American Archaeology. Her 1984 article, *Archaeology and the study of gender*, written with Janet Spector, and her 1991 book *Engendering the Past: Women and Prehistory* (co-edited with Joan Gero) are widely regarded as the seminal statements in the history of a gendered and feminist archaeology.

Douglass W. Bailey (DWB): Let's start with your interests in diffusing power and authority structures. Archaeology at Berkeley has had a history of doing things in a different way in terms of the hierarchy of power. What do you mean by diffusing power?

Meg Conkey (MC): It comes from Helen Longino, feminist epistemologist at Stanford. She has laid out what she sees as the virtues of doing epistemology as a feminist, and what it would look like. Out of the feminist movement comes a realization that there are power structures in operation and that if you are going to make it in whatever world you are in, you had better understand those power structures: how power is used, how it is abused, and how it works, so that you can navigate in the worlds that you inhabit. That ranges from everything in your community and your neighborhood to your academic department, and your job. What I like about the idea of a diffusion of power is the fact that traditionally archaeology has been simultaneously collaborative and hierarchically authoritarian, in the sense that there has always been the project or site director and that traditionally, of course, the director has been a male.

There have been many unsung women directors. It would be very interesting to look at these people, and I am thinking in the USA of somebody like Cynthia Irwin-Williams or Patty Jo Watson. Kathleen Kenyon on the other hand is someone who adopted the male authoritarian powers in order to get by; that may have had a lot to do with her personality and the part of the world she worked in as well as the era and time period in which she worked, when it was very different in terms of the number of women in archaeology. So, it would be interesting to go back and look at women as directors, to look at people like Marie Wormington or Cynthia Irwin-Williams or some of the others.

How does archaeology resolve this tension between employing (in one way or the other) paid, unpaid or exploited (or properly taken care of) students or workers, on the one hand and on the other hand having a need for everyone to collaborate and share their data and share their ideas? How does this compare with the present day archaeology which is so inter-disciplinary, so multi-disciplinary,

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where you cannot really do an excavation without a project geologist or palynologist or the many other types of specialists, and where there are issues of who gets access to the data? So here you are depending increasingly on a free exchange of information in a situation where, in order to run it you are relying on (as organizational people will tell you) someone at the top, someone making the decisions.

It is like team-teaching a class; in some ways, team-teaching a class is harder than teaching a class by yourself. Everyone thinks that it is easier, but you can't just make up your mind and just say that you will do "x". You have to consult who you are team-teaching with. In terms of diffusion of power, I think that it gets down to the basics, for example, of being on a project whether it is excavation, survey or lab work, and keeping everyone informed. It is an issue of communication. Rather than coming out and announcing, "this is what we are going to do". It's about inviting input, so that you have a diversity of opinions. Even if they are undergraduates, people on the project have ideas. For those of us who find ourselves getting unwittingly constrained in a box of "this is the way that we have always done it", we need to have an external voice that says, "why don't you try 'x'?", and you might say, "well I hadn't thought of that". So it starts with communication in a much more open situation, rather than this top down, "you do this and you do that". It is a case of your having to explain it, because in the course of having to explain it you learn something.

And then, of course, it all comes down to the whole decision making process, and it comes down to collaborating on papers, and it comes down to not taking other peoples' ideas without citing them. We have a number of figures in our discipline who are well known for taking other peoples' ideas and not citing them; in doing so they affect the power relations. Sometimes it is with people who are students who get abused. I think that there are entire cultures that train archaeologists that this is what you have to do to get ahead, whether it is a word, a concept, a way of doing something. I think that it would be much more interesting if we cited all of our sources and talked about where our ideas came from, because it makes for an intellectual history, rather than an individualist, signaled individual. It would be very interesting to sit down with a lot of people, to have a conference to talk about how do you diffuse power so that things still get done and get done effectively.

DWB: So how do you do this, particularly in a tradition where there are unavoidable traditions and laws about who can do what, and when and where they can do it: for example, whose name is on an excavation permit? How do you break through this? What are the examples of successes at diffusing power?

MC: I think that it plays out in a most challenging way where people are trying to generate truly collaborative projects with indigenous peoples. In the United States, it is working with Native Americans. There is a lot of tension about what they want to know and what an archaeologist wants to know: what they don't want to do, what I want to do, but I want to work with them. And this is the same in Australia and other, what some might call, "settler countries", like South Africa. So those folks are really working it out on the ground in many different instances.

DWB: Are the archaeologists doing this because they are forced to do this, because there is no option but to do so if they want to work in a specific area or with some particular material?

MC: In part, this is the case, and certainly there have been prescriptions that have come out of the Native American Graves Repatriation and Protection Act, though they have something different in Australia and they don't really have this in South Africa. I think that there has been a change in the building of conscience among archaeologists in many of these countries. Of course, not everyone is onboard this ship, setting sail to do a new type of archaeology, but there are enough archaeologists doing it and there are enough indigenous people who say "wow, look what we can get out of it, look what we have learned". There are a number of great examples, and I can think of a number of the Society of American Archaeology meetings where people have heralded some of these examples of collaborative kinds of negotiations. The really interesting examples didn't involve archaeology per se, but they have involved cultural heritage.

There is a great book by Michael Brown called *Who Owns Native Culture* (M. Brown 2003). In it, he has a chapter on a place called Devils Tower in Wyoming that is an incredible, geological, sort of

tectonic thing oozing out of the ground that has formed a huge natural tower. It is a place that has had long standing symbolic, religious, and spiritual significance for native groups, and it still does today. Devils Tower is also a favorite place for rock climbers, and it is also a place where a group of motorcyclists has decided that it is where they want to meet. Michael Brown reports how they took into account the Native American's religious concerns. When they go for their religious activities, they hear the rock climbers hammering pitons into the Tower, and they feel that that is an affront. Brown talks about the process of negotiation and how you come to a settlement where you can satisfy to a greater or lesser extent the different stakeholders. He talks about how they worked it out. There are many other examples where this is happening.

Another example is Kent Lightfoot's work at Fort Ross in California (K.G. Lightfoot et alii 1991, 1998; O. Parrish et alii 2000). They sat down with the native groups, the Kashaya Pomo, and they put out what they called a ritual blueprint for how to work. This had ramifications that one would never have expected. For example, the Pomo did not want the archaeologists to go right in and excavate, because penetrating the soil and going below the surface was considered to be a ritual act of pollution. In addition they did not want the archaeologists to take away the artefacts. They developed several archaeological methods to deal with this. They developed what they called the catch-and-release survey method; you take your survey area and you divide it into 1.0 m squares and you map out the survey area. You pick things up according to that 1.0 m grid, you bag the finds based on that grid, and you take the finds back to the laboratory. You look at the material, you make an inventory of it, and then you take it back out into the land and you put it back in the same general survey unit that you found it in. As everyone recognizes that everything on the surface is already out of place, you don't have to put things back exactly where you found them. In the excavation at Fort Ross, of course, they also took advantage of non-invasive geophysical techniques to see what the structure of the site was before they dug anything at all.

Next, as far as the Pomo Indians were concerned, menstruating women were not allowed on the site. In the US legal context, you cannot deny women differential access to a place or an activity. As Lightfoot's project was funded with federal money, they had to figure out what to do. Kent devised a system whereby everyone on the team, men and women, students and professionals, would rotate among different archaeological sites according to a calendar of menstruation. The students said, "wait a minute, this is a violation of my personal life". It was applied to other areas. Even in camp life, menstruating women could not handle dishes, could not serve food, and could not handle the food. They even had the director of the excavation, Professor Lightfoot, taking a meal for his wife where she was sitting at a different table, and then later collecting her dishes and bringing them back, and washing them.

DWB: I can see this concept of diffusing power, but aren't we just shifting the power structures somewhere else and saying that we are just replacing one system of authority with another, in this case saying that menstruating women cannot participate?

MC: What we are doing is recognizing that there are different parameters for how to run things, and that there are different ways of working. So, they may have been able to do some things that Kent brought to the table, such as doing a particular type of survey here or putting in an excavation unit there. In exchange, the native groups were saying yes, but we will do it this way. If you did not have this opportunity of working with indigenous peoples, but are only working with your own group (which may be the case in Romania as well as in my work in France), then you have to decide who is going to take responsibility for making certain decisions. So, it does create more complex decision-making processes. Everyone comes to the table. For example, one would say, "OK, if you are responsible for organizing the wet-sieving, then what is your plan, and how is that going to interface with my plan for having people numbering artefacts?" Sometimes there are so many things going on that a lot of site directors don't pay any adequate attention to all of them; things slip by the side and they don't get done. Then someone blows up about having 500 bags of wet sieving and they have different systems of numbering on them to the rest of the site's materials.

The call is for us to build-in some sort of sense of accountability, even for people who are farther down the line in the hierarchy, making them accountable and responsible for some part of the project, of spreading the responsibility around.

DWB: I can see how this would work in the field. How would it work in the classroom?

MC: Ruth Tringham and I have written about this (M. Conkey, R. Tringham 1996). There is the idea that when you teach you are standing up in front of the classroom and you adopt a "banking method" of teaching: you deposit knowledge in students' heads and later you withdraw that knowledge in some structured way, like an exam or a test. There are many other, more liberating, kinds of pedagogies and ways of teaching. In the way that we came up with at the undergraduate level, and the way that has been the most fun, we decided that we don't think that these students need to know all the names for all of the different culture periods. They need to know that we give names to culture periods, and that we recognize that there can be different constellations of artefacts that might look different one from another. Whether we call it the Vin a 1 or the DaDah 2 or whatever, and if we then test them on that knowledge, it is totally useless knowledge in terms of their future, unless, of course, they are going to go on and be a specialist.

DWB: I can see that this works very well where students are in a liberal arts education, where some of the students will become archaeologists, but most of them won't. I can understand that what you are teaching them is thinking and critical inquiry skills, but most students in an archaeology class in Bucure ti are either going to become archaeologists or museographers or teachers.

MC: Well, you just have to decide what is the knowledge that they have to have and how best to get at it. So, Ruth and I decided that instead of the two of us standing up at the front of the class and giving a series of lectures during which we deposited all of the knowledge into the students who had to write down and then to give back to us in some sort of way, we designed and developed something that we called Panels. At the beginning of the semester we decided on a list of topics. So, for a class on the prehistory of Europe, one of the topics might be "what do we mean by the diffusion of ceramic technologies?" Students would sign up a particular Panel topic. They might sign up together with a friend. Each Panel group would be responsible for the presentation of a particular topic. They could do it any way that they wanted to. They could decide what were the important features, and they would share them with the class. We have had students do short plays or skits. You will remember Ruth's famous radio show from her article in *Engendering Archaeology* (R. Tringham 1991); so the students could do a radio show for their Panel with students from the class arguing different positions on the topic.

In one of my classes on Palaeolithic Europe, I have had students working on the question of whether or not you could learn how to make a complicated object without language. So, the question becomes what was the role of language in the development of technology? The students divided up the class into several groups and they said, "OK, for this group, someone will use language to show you how to make one of these very fancy origami Japanese folded paper objects. Someone else will show other groups how to do it but they will not use language when they show that other group how to do it". In order to get them to think about it more, the questions became, what kinds of things are produced with a language and what kinds of things are produced without a language. It doesn't have to be about making stone tools, and it can be about making something like the origami objects.

Turning the class over to the students, and asking them to come up with what they want to know actually takes a lot more work for the professor. It is much easier to write your own lecture and stand up and give the lecture to the students. This is especially the case if you have been teaching the same subject for a while. In the alternative that we have used, the students come to our office and we give them some key references, and we may lend them some images or tell them where to go to find the images. You send the students off to work together, which is a part of what they will have to do regardless of what they will end up doing with their lives; they will end of having to work with people who they have never known before. They will have to deal with the dynamics of some people doing their fair share and other people not doing their fair share. It gives them another way of learning and another way of interacting. We assess students individually and as a group. Ruth and I have written about this process (M. Conkey, R. Tringham 1996) and there we have written more about where we found our inspiration.

DWB: Is it possible to inject this concept and practice of a dissolved authority into something that may be more traditional, at least in terms of the knowledge that needs to be acquired? For example, if a student is studying the Neolithic of Romania or southeastern Europe, he or she will need to know a series of patterns and trends in ceramic forms and types of surface decorations.

MC: Well, we didn't turn the entire class over to Panels, so there was time for us to do some presentation of information. However, I could imagine that you could have different groups of students that were responsible for communicating to the rest of the class what one series of formal pot shapes were or about the use of one type of temper. Students internalize information much more by learning this way. As professors, we know this; if I hadn't had to stand up in front of a class and talk about a specific subject, I would not know very much about that subject. Circulating the responsibility and the accountability works.

DWB: What are the obstacles and the challenges at our level (as professors who are people in authority) for practicing and teaching in this way, and how do we get past those obstacles?

MC: One issue is that there is basic fundamental knowledge that students need to have, and if they are in charge of it, we may not get all of that information. Another problem is that in a lot of institutions, team-teaching does not count in terms of a professor's contracted workload; one's colleagues and a department's accounting system may think that you are not carrying your full load when you team-teach. Because of this, you may need to team-teach two classes in order to get the credit that comes with teaching a single class. At Berkeley, we have managed to work on this not only in the Panel teaching but also in regular teaching. In the core courses that students are required to take, such as the history and theory of archaeology class (required of first year graduate students in their first fall term), and also in the archaeological research strategies class in the spring, we have told the Department, "this has to be taught by two people". The Department has accepted that as the way that it has to be.

In addition, team-teaching is great for the faculty. The pairings of faculty members have as much to do with whose particular schedule it is and when they want to teach, as it has to do with two people who want to teach together. I have taught it with people who I have never taught with before and with whom I do not share a lot with. You learn a lot from hearing their different perspectives. For the students, of course, it is fantastic. But, it is true, you do have to stand up to some of these structures if you can.

DWB: At Berkeley it seems that you have a critical mass of people who want to work this way. It might not be so easy if one person was on her or his own trying to do this.

MC: It is hard, that's true. Kent Lightfoot and I joined the Department in the same year. Ruth Tringham had been here before. When we arrived, there were no required courses for the graduate students in archaeology. The admissions' process was that each faculty member selected whoever he wanted; it was not a collaborative sort of thing. We changed all of that. We instituted these collaborative, team-taught, required courses. We instituted a double advisor system so that each student coming in to the program had two advisors. If one or the other of your advisors was not working out for whatever reason, or if you wanted to change your field area or your subject and someone else was suddenly more relevant, you could go with it. People did not get possessive about their graduate students.

DWB: People often become possessive out of fear, especially if they are insecure about their position. They accumulate hoards of graduate students, and they want to be directors of dozens of different field projects or excavations.

MC: What's interesting for me is that if you look at Professor Pat Kirch here at Berkeley, you will see that he has consistently had a core of students who have worked in the Pacific region. This makes sense. He has the Pacific students, and when he has parties at his house, he always invites his students. They have special seminars on Pacific archaeology. I have never had a core of just Palaeolithic Europe students, or even just Palaeolithic students. In the end, I have probably supervised more PhDs in historic archaeology than in the Palaeolithic. It happened because of the topics that people are interested in, especially as gender (for a long time) was something that a lot of

historic archaeology students were interested in. For a while, I was working a lot with Jim Deetz, who is a historical archaeologist, before he retired.

Then Laurie Wilkie, another historical archaeologist, joined the department, and she and I have been on a lot of dissertation committees together. Ruth Tringham has done this in working with many people who are working in the digital world or in household archaeology. Thus, Ruth has worked with a lot of people who are not focused on southeast Europe. Ruth's cadre of students may have had foci on lithics or southeastern European early on in her time here, but for a while now it has been broadening out to household archaeology and digital things. So, if you look at my students, I can only think of four or five who are specifically Palaeolithic archaeologists.

DWB: You have talked before about something called an archaeology of distributions. Will you tell me what that is?

MC: Jim Ebert wrote a book in the late 1980s called *Distributional Archaeology* (J. Ebert 1992). He was a student of Lewis Binford, and the book built on some of the ideas of Al Ammerman who was an early advocate for the centrality of field survey: that it was not just the hand maiden to "real" archaeology, which was excavating a site. There is a parallel to some of the self-critiques that ethnographers have done about their focus on the village and our focusing on the site as some sort of natural, inherently bounded unit: the problem of not thinking beyond the physical borders of the site, and not realizing that people were on the move (and not just hunters and gatherers but also agriculturalists, and not just those people in special roles, such as traders). So, what do we know about how to study distributions? What methodological or field-based practices do we have?

In our work in France (the *Between the Caves* project), we did what had been shunned for a century. We looked at what went on between the cave sites when the latter had always been seen as so attractive and so well preserved. The traditional understanding sounded as if people in the Palaeolithic just landed in a cave site (where they didn't live all year round), and then they suddenly showed up some other site. Of course, archaeologists had said that these people got their food from out in the open but they also seemed to feel that that activity wasn't going to be interesting and it wasn't going to be anything archaeological. In the *Between the Caves* Project, in our field surveys, of course, we have found that in our survey area (an area covering the 10-30 km between two caves with other caves in between), was that those Palaeolithic people were all of over the place. The work was just surface survey, though we eventually got into test trenching, and now are excavating a fairly intact open-air site. Everyone said that you won't find anything in your survey. In fact, they did not want to give me a permit, because they said that I would not find anything. And you have to ask them, "why hasn't anyone found anything, has anyone looked?" And they reply that no one has looked.

Suddenly you get into the issue of evidence. They then asked, what good is it if you have all of these artefacts from across the landscape, which are differentially distributed, sometimes more or less in a relationship with the source of flint or water. The question becomes, how do we deal with these objects and these patterns archaeologically. It is just too messy. It is not contained. It is not bounded. So, you have to start thinking that these are distributions of material culture across a landscape, and the possibility exists for you to make some inferences about where people went, and what they did. When you are out in the open and you find flint that comes from 200 km away, you are faced with the questions of why is it here. We are good at thinking about sites, but we have to stop and think about distributions of artefacts and sites. This is also the case at a higher level. It is not just the distributions of artefacts across a landscape, but it is the distributions of sites and the way that we depict them. Distributions of settlement system sites; I call them maps of caviar, because they look as if people have taken caviar and spilled it across a table. There are all of these little dots. What do people do with them other than the type of studies that were popular in the 1970s with geographic notions of different types of settlement systems, and the hierarchy of settlement systems based on a theoretical notion of the world being an undifferentiated plane. We have not learned how to think about these things; I do not know how to go about these things myself, except that I am confronting it right now.

One of the other things that I am doing is leaving most of the direction of the excavation site to my junior colleagues. This is part of my diffusion of power. So I say, "you are in your early 40s, early in your career, here is a site. Take it." You know that there are a lot of people who won't do that. There are a lot of senior scholars who will hold onto their sites, and then they won't do anything with them, because they have other things to do. Both here in the States and in France with my French colleagues, I have seen people retire and then spend all of their time writing what they probably should have written up earlier in their careers. My plan has been to get out of it sooner. It is not "my" site. It has never been "my" site. Also, I have intentionally not stayed at the excavation site the whole time. I will come either at the beginning or at the end. Usually at the beginning to get things set up, but then I just let them go. They do not need me. They tell me that they need me, and I think that this is very nice of them to say that, and of course, I will do things that they would like someone like me to do for them, but I don't want to be the mother who picks up all of the pieces or gets everyone going. They can handle it on their own. They can figure it out for themselves.

DWB: I can see this working in your project, and I can see this gradually happening over time. I can put this in the context the emergence of a feminist archaeology or of archaeology in the United States. What if we put this into another context, where there have been changes in education and in society (revolutions literally), but there has not been as great a shift in the authority structure of science or archaeology? It seems that change in methods and pedagogy can come from deep processes and patterns percolating through the system over long periods of time, or they can come from radical action and deviant behavior.

MC: I always say that one generation's solution is the next generation's problem. What happened in Spain is interesting between what was the situation under Franco at the end of the 1970s and then during the post-Franco period. Spanish archaeology really changed. It had been very traditional and very culture historical, and then post-Franco it spawned all kinds of people who were interested in social questions, in more symbolic issues. It did not give up on the culture historical questions. None of us can give up on culture history, because we need it as a foundation and a framework within which to work; but the Spanish began pushing culture history in different directions. Someone like Antonio Gilman, a Marxist who works in the Bronze Age, would have a very interesting take on how that could happen in Spain, whether or not it was primarily due to the political shift in the post-Franco era. My impression is that things were bubbling before that happened. The political change probably gave it impetus, but there must have been other things going on. Certainly there must have been entrenched senior archaeologists who would have been resistant. I have always thought that it would be a good project to go back to the pre-1970s articles and publications in the major Spanish journals and see what has happened since. The young Spanish archaeologists are extraordinarily dynamic. It would be interesting to compare, because it came from a more oppressive regime to a less oppressive one, though of course now they are really struggling with an economic crisis that is not good for archaeology. They also have different ethnic identities throughout the country, for example with the Basque.

Manuel González Morales has written a little bit about how some of the groups who were able to claim a certain kind of identity were getting much more government support for their archaeology than others were getting. Many of the decisions were being made about these identity conscious groups that were pushing their own particular agendas. This is unlike French archaeology, which is grindingly making really small, minuscule changes. With two colleagues there, I have been chasing the begrudgingly small recognition that maybe there were women in the past. I have watched the Spanish archaeologists of the 1980s come charging out of the gate in terms of really exciting ideas. By then I had switched to France, having done my dissertation in Spain. I made the switch after having nearly been blown up in one of the Basque separatists bombings. This is also why I am not in Jordan right now at the World Archaeological Congress. I really wanted to go back to Jordan, because that is where I did my first fieldwork. I did it while I was in college and I have not been there since. My family said that who knew what was going to happen, especially with what is going on in Syria. So, I didn't go.

DWB: Does your decision to work in France and not Spain and your choice not to attend the conference in Jordan link to what you were saying earlier about dissolving authority? You have given authority away or shared authority with other people in your life about your own personal and professional decisions.

MC: I think that this is very important. There has not been enough of that. When I read my contribution to a recent collection of interviews of archaeologists (W. Rathje et alli 2012), I said, oh gosh, did I really want to say those things about certain individuals about who had been helpful and then by implication who had not been.

DWB: If you were sitting down with a new cohort of graduate students, what advice would you give them?

MC: The most important thing for any of them is that when they think about a topic for their research, that topic has to be one in which they can really engage. If you cannot get behind a topic that you are supposed to study, then you had better find another topic, or do something else. It is not the end of the world if you decide that graduate school is not for you. Most people feel that they have to pursue it, that they have to stay in there. However, if they are not engaged, then they will not do a good job. It is not a failure if you decide after a year or two that this isn't going to work. You could go back to it later if you wanted to. There are plenty of ways for you to use your abilities and skills. Some people stay in it and shouldn't have, and then they become a burden to their professors, because their professors will have to write letters of recommendation for them and won't always be enthusiastic. That's another thing. When you are asking people for letters of recommendation, you have to say to them (and I know that it is very hard), "can you give me a good letter of recommendation?" Some people will say that they will write a letter of recommendation, but if the letter is not good, then this can do more damage than help.

So, the first thing is to pick a topic that you really want to work on, and then find the people and the support system that will help you do that. At Berkeley, we try to do some of that weeding out before students even come in. We have people who apply who have top grades, come from a great institution, have some fieldwork experience, and they look like they are wonderful students, but they want to work on a topic that none of us can really help them with. You just have to say, we are not the place for you, and you have to give them some suggestions of alternative places and people. In some instances for undergraduates who are not really focused yet, we suggest that they go somewhere and get a master's degree, and then reassess if this is really what they want to do. Have they found what they really want to work on?

DWB: Does this require that the professors let go of their egos, and stop saying that they want 50 students studying the Neolithic with them, and thus not accepting everyone who applies to study with them?

MC: That's right. I would tell new graduate students that they need to find a topic, and that they need to learn to manage their situation. Sometimes it will be a little dicey. Try to develop experiences that will give you the positive feedback that you will need. Get yourself involved in activities, in experiences, and in relationships with faculty or with other professionals that will be positive and that will lead to their positive support. You know that I am still writing letters for my students from the 1980s, and they are now getting prizes and awards, and I am also writing letters for colleagues. I have dozens to do this week. Make sure that you realize that no matter what field you are in, but certainly in archaeology, it is a network and people talk, people communicate, and people have opinions. You are in the field with people, and lots of young students don't realize that when they do silly, stupid things in the middle of the night when you are in the field, people will remember these things. They will sometimes probably never forgive you for some of those sorts of things. It is like having stupid email addresses that do not suggest that you are very professional. So, rather than thinking that a field opportunity is a time to let loose and do whatever you want, you need to realize that you are building impressions with people who you may have to come back to for their permission to analyze some data that they have, and they are not going to be very interested in doing that for you. One has to be en garde all the time, and I think that a lot of our youth do not realize that. Again, I think that one of the main things is to think carefully about the topic that you are going

to do. Becoming someone who is doing the same work as the main professor (or work that is subservient to that work) will make it very difficult for a student to develop his or her own identity.

DWB: How does a student who does not yet have any of this dissolved or shared authority that you are talking about succeed in carving out their niche without ruining their career or ruining their relationship with their supervisor?

MC: It is very difficult, and the solutions will vary with different personalities, not just of the student but also of the senior person as well. I remember the strategy that Clark Howell had. He had an imperial notion about understanding the Palaeolithic. In some ways it played out favorably for his students. He wanted each student to take a part of the world and a time period and then to do a summary of it so that he could have the information. Thus, Sally Binford did the Middle Palaeolithic of Middle East, and Richard Klein did the Middle and Upper Palaeolithic of Russia, and Maxine Kleindienst did the Lower Palaeolithic of Africa. Then each of them was able to have his or her own field, and his or her own success and area of accomplishment, even if it was subjected to being assigned by a professor.

When Clark Howell found out that Richard Klein spoke some Russian, he said, you are going to work on Russian material. At one level this may seem terribly authoritarian, but when one sits down and analyzes it, that may be okay, because the area might become your arena of expertise. What you have to do is think about how you can use your own skills and interests to advance the goals of your supervisor, but also make it enough of your own project so that you can carve out your own identity. It might take a lot of talking and consulting with people. Even making lists. What do I do? What can I do? I have these languages. I can do this kind of work. I can work in lithics. I can learn these new techniques, or I am really good in quantitative or digital skills. Think about what your particular skills are as a student, and try to think about how those skills can be best used even if it is to satisfy the goals of the head-honcho, while at the same time allowing you to develop your own identity. It is always good to say, "I need your help". Go to the superior person and say that you are trying to figure out what to do. It is always good to go and say, "I need your help, here are the kinds of things that I can do. What do you think would be good for the project and what you think should be done in the field". Go to them, but bring them your skills and the kind of things that you want to do.

DWB: Is there a positive role for the hypercritical graduate student, who aims to rip things apart? I can think of some Romanian archaeologists whose work is very critical especially of foreign scholars and teams. What advice would you have for them?

MC: If we take the example of Cambridge in the 1980s, it was a case of what people did and how they did it. But someone has to do something substantive, something more than just a criticism or critique. This is why, I think, someone like Chris Tilley has succeeded. He and Michael Shanks came in and did things that we now think were just completely over the top in the red and the black books (M. Shanks, C. Tilley 1987a, 1987b). People who think of themselves as processual archaeologists still point to things that were in those two books that they see as characteristic of post-processualism without realizing that it is not the be all and end all of post-processualism, and that they themselves are probably already doing things that are that post-processualist without even realizing it.

I think that Tilley went on and did his work with rock art that was really empirically based (C. Tilley 1994; C. Tilley, W. Bennett 2004), as well as the work that he did with Barbara Bender about the landscape (B. Bender et alii 2007). So he made a big critique but at the same time he went on to show how doing things differently can be done working with "real" data. Shanks, I think, has fallen off the deep end by doing his archaeology-as-performance. His work on Greek amphora lasted just a year or two (M. Shanks 2004). So, in thinking about people who shook things up but then went on to continue to be a contributor, I think that Tilley pursued a path that is much more successful. Shanks may be much happier; he loves what he does, he is doing what he is doing, he got himself a much better paying job in the United States. I don't know what constitutes personal and professional rewards. He may be very happy, but in terms of archaeology, if he wanted to stay in an archaeology where you are engaged in and involved with people of that sort, I think that Tilley did the better job.

For better or worse, I think that there still is a preference or a bias for people who work with archaeological materials. I am not sure that that should always be unquestioned. It still is the case. I worry about people like Colleen Morgan who is very, very good with the digital world, but when push comes to shove, I am not sure how it will work out. Here is an example: Marcia-Ann Dobres has a Berkeley PhD. She did work with materials for her PhD dissertation. She worked with engraved bone; she worked with bone and antler artefacts from Magdalenian sites in the Pyrénées. She completed her dissertation, and she had a National Science Foundation grant to do her microscopy work and did all sorts of very interesting things. She got very involved in the social life of technology. It came time for getting a job and she did not have a field project. She said, and this is the case for many people working in the Palaeolithic, that she could place people on excavations. She said that she did not have to have a particular field based project, and that she had a research project that would involve a continuation of the things that she had done in her dissertation in handling real objects and artefacts. But it wasn't a field project where she directed students. She never got a job. She has an adjunct position in Maine and she has been teaching around.

DWB: Tell me about your career trajectory.

MC: I have a trajectory that (on paper at least) some of my senior colleagues at Berkeley might say I never should have gotten anywhere. I went to undergraduate school at Mount Holyoke, a women's college. I was an ancient history and art history double major, and I had an opportunity to go to Jordan and I did archaeology before the 1967 war. I was in what is now the West Bank, and I was doing biblical archaeology. It started as a joke in the dorm room. I decided that I was very interested in archaeology. I like the idea that it blended some of my intellectual interests with being out doors. Of course, being in Jordan in 1964 meant that I wasn't actually doing any digging, because they had hired workmen, and I couldn't even work with the workmen because I was a young female. I was in charge of the pottery and I had an older man who was in his 60s and a young boy who were working with me because they were the only categories of males to whom I could give any directions.

It was a very interesting experience. I traveled a lot; I went to Petra, and we stayed there for a week. Anyway, I liked it and I thought that this is what I wanted to do, and my area of interest was something in the Middle East. I wasn't really interested in biblical archaeology, or early agricultural settlements. I applied to graduate school. I applied to both the University of Chicago and the University of Pennsylvania. Both of them said the same thing. I had applied to the anthropology departments, but I had never had any anthropology. They did not have anthropology at Mount Holyoke at the time. They both said that I needed to take a year of undergraduate anthropology classes before they would give me a final admission. Also, I had managed to apply to the Oriental Institute in Chicago, and they accepted me.

Another crazy opportunistic thing happened. When I graduated in June, one of my friends and I decided that we want to go to New York for the summer and work. "Mother and Dad, you take our stuff how and we are independent. We are going to New York City." So, we went to New York, we found an apartment on the Lower West Side on 14th Street, and we got interviews based on ads in the New York Times. I got a job with an organization that I had never heard of before called the Wenner-Gren Foundation for Anthropological Research. They were looking for a librarian. I was hired by Wenner-Gren and my roommate started off at Time Magazine doing research; she was a history major. I worked at Wenner-Gren, and after a few weeks, they realized that I would probably be better as a help for analyzing grants, and so I became a grant analyst. Then, I found out that I had been accepted by the Oriental Institute, and I thought that I had better go there. The then Director of Wenner-Gren was very generous and she said "Meg, you will probably need a job when you get to Chicago." I said, you are right. I am the eldest of five siblings and my father had said, "you want to go to graduate school? Bye, have a nice life, and come home for Thanksgiving." Even though he had graduated from the University of Chicago. I couldn't even play on his alumni sensibilities. The Director said, "we publish a journal there called Current Anthropology, and I will contact Sol Tax, the then editor, and I am sure that he will hire you."

So, I arrived at Chicago, got some roommates and had this part-time job working as an editorial assistant at Current Anthropology. All of these flukes, these opportunities, you have to take advantage of. Somewhere along the line, in the spring of my first year in graduate school, Sol Tax

pulled me aside and asked how it was going over at the Oriental Institute. I said that I was a little disappointed, but that maybe it was just because it was my first year. He said that he bet that he knew what I was disappointed about, and I said that the work was all about languages and objects, and that there weren't any people. He said that I belonged in anthropology and told me to get my papers together, and he transferred me into the Anthropology Department. So, I started graduate school all over again.

When I took my first class, it was one that was required for all graduate students regardless of their field. The class was taught over three quarters; it was about social systems, cultural systems, and psychological systems. We were in the social systems class, and this was the fall of 1966, and I looked at the reading list and the only book that I could find in the library that was available was by Claude Lévi-Strauss. I read the chapter. It was something about kinship, and I had no background in this whatsoever. I could not figure out, for the life of me, who was "Ego". I thought Ego was a person. I knew that there was Plato, so I thought that maybe Ego was another classical Greek philosopher. It took me a while to get up to speed.

At Chicago I took courses with Bob Adams and Bob Braidwood who were team-teaching the archaeology sequence that was called *The Human Career*, with early humans, agricultural humans, and city humans or urban archaeology. I found this interesting but my interest in early agricultural societies was not sustained. In the next year, I got a job as a research assistant in a little cluster of Palaeolithic archaeologists with people like Les Freeman who was married to Sol Tax's daughter, Susan Tax Freeman who is a very well known socio-cultural anthropologist working in Spain. Also there was Clark Howell and Karl Butzer. It was a very strong group of Palaeolithic people. I started taking some classes, but what was really going on in Chicago at the time was that Fred Plog was there, John Fritz was there, and Chuck Redman came in the year after me. All were graduate students. One year, we found that we had no faculty at all, so we decided that we would teach our own courses. This was where the second generation of New Archaeology came in. Fritz and Plog wrote the famous article about hypothetico-deductive reasoning in archaeology. Chuck Redman started going off with all of his systematic sampling; he was working with Braidwood in the Middle East. The whole furor of the New Archaeology was going on then, and this is where I got the inspiration for doing my dissertation: trying to understand (and this is classic Binfordian archaeology) the nature and significance of variability of material in the archaeological record. Trying to use design and style in engraved bone and antler to see if I could make some inferences about social groups and social relations.

By then, 1967, John Fritz and I had gotten married. I did a Masters thesis under Les Freeman who had suggested that I review how people had interpreted Palaeolithic art. Les was always interested in it, and in his later years he wrote about it, but at this time he was doing all sorts of the new factor analysis and function versus style in stone tools. He was very supportive, and there were not very many Americans involved in Palaeolithic art who might bring a solid anthropological perspective to it. From there I moved into working on the Spanish materials on the Magdalenian for my PhD dissertation.

John Fritz then got hired at the University of California at Santa Cruz, and we moved out to California. Within a year, Alicechandra, our daughter was born, and I had gotten a job at San Jose State University without finishing my dissertation. If there is one thing to slow you from finishing your dissertation, it is starting teaching four courses a semester and having a little baby. So, it is kind of strange that I ever finished, but you have to finish if you wanted to keep going. The first two years I was at San Jose State, I was full time temporary lecturer; this is standard procedure in the California State University system. Then the university developed one the first environmental studies department in the country, and they wanted someone who could teach anthropology, someone to teach the long-term perspective on human relations with their environment and human ecology. This was in 1971 or 1972 and there was a lot of literature available, (for example, Roy Rappaport and Pigs for the Ancestors): all of these ways that humans could regulate their environmental relations. It was easy to move into some of that, and to do the long-term things. That was a tenure-track job in environmental studies, and I was the house anthropologist and I taught the required course in human ecology that all environmental studies majors had to take and a variety of other courses.

A student in one of my classes, Nancy Wilkinson, thought that I should meet another professor named Les Rowntree who taught in the Geography Department. Les and I were married to other people at the time that we met in 1975 and all four of us became friends. But, a job opportunity in anthropology/archaeology came along at the University of Binghamton (part of State University of New York) because Fred Plog was leaving that department. Chuck Redman was there already and Al Ammerman was there. They wanted to replace Fred Plog and they thought that they should get a southwestern US archaeologist. John Fritz and I interviewed for that job together. We worked out a position and a half with Binghamton, and this was pretty original at the time. One year one of us would teach full time, and one of us would be half time. They next year we would switch. However, on the way to Binghamton, John and I decided we would set up separate households. Although John soon left the teaching at Binghamton and set up a project in India. For a time, for one or two years, he happened to own a house two doors away from my house in Binghamton, which was just fine, especially for our shared daughter. He eventually moved and I stayed connected with Les Rowntree, who had also divorced his wife, but he was teaching still at San Jose State in California.

Les and I commuted for ten years, coast to coast. In the end, we counted those ten years as 20 semesters, and for ten of those 20 semesters we managed to be together, but we never really knew when or who was moving or what we were doing. Les' daughter Erica was living primarily with her mother Heidi in Los Gatos in California. My daughter Alicechandra was primarily with me, though spending some time with her father who was in New York and doing a lot of traveling; this was convenient for him and it was fine for me to have her with me. There wasn't much of the pull apart tension that sometimes happens in these situations. It was in this context that I developed the idea of doing the survey project in France.

So I developed the project in France with Les because we decided that since we were commuting during the year and if I was going to go off for fieldwork in the summer, we would be separated again. So, we decided to pick something that we could do together in an environment that had good stuff for him and for me. As a historical geographer, Les had done his dissertation in the Alps in Austria. So, I suggested a mountainous region and we decided on a survey project where someone who had a sense of historical landscape use in geography would fit in. So we decided to go and do this together. Our first season was in 1993. We had an exploratory grant from the National Science Foundation; they give small amounts of money to people for projects that they have no idea how they are going to turn out. No one had done any survey, no one had reported anything, and we decided to see if we could find anything. If we succeeded, then great; if not, then that is fine as well. NSF still has this program; they are called high-risk projects, and they allow people to do new and different things and to have a funding source. It was a small grant: \$25,000-\$30,000, enough to get a number of people going. I had to develop relationships with French colleagues, and get permission from the regional archaeological service and get a permit.

By then, by 1993, I had come to Berkeley. In 1986, they were looking for someone to come and teach for a semester. So I came out for the spring semester. Alicechandra was 15 at the time. She was so mad, she was furious: "Mom, you are taking me out of school for a semester. What am I going to do?" New York State has a very rigid set of Regent's exams that you have to take to qualify for college. "My life is going to be ruined." This wonderful teenage sturm und drang. So we came out to California, and we found a small private school here called the College Preparatory School in Oakland. If looks could kill, the day I dropped her off at that school I would have been dead on the spot. So we go through the spring semester and it was quite lively. Desmond Clarke was about to retire. There was a big festival and celebration in his honor. I taught a couple of courses including an undergraduate course on the history and theory of archaeology. It comes to be June and I say, "Ok, we had better sit down and plan the move back to Binghamton" and Alicechandra coolly told me that she was not going back to Binghamton, and I said, "what do you think that you are going to do, young lady?" She said, "I am staying here and Gina's mother said that I can stay with them". So, I moved back to Binghamton all by myself.

It was during that next year that Berkeley advertised the job that I eventually took. It was horribly controversial. Some of the Berkeley faculty wanted an Africanist. Desmond had retired, so he had no more say in it, though he understandably expressed a major concern that the Africa program was not continuing, even though there were some offers to researchers in Africa and elsewhere in the

Old World. Of course Glyn Isaac had left and unfortunately, tragically, died very prematurely. Clark Howell was still here. Tim White was still here. They wanted an Old World archaeologist who worked in the Palaeolithic. That was me (sort of) but it wasn't their idea.

DWB: After the hire of Ruth Tringham, was your hire the next building block in making the department what it was?

MC: Kent Lightfoot and I came in the same year. We came in the fall of 1987 and then in January of the next year we had another position in Old World, and the hire there was Pat Kirch. In the period of a year and a half we had Kent, me, and Pat. John Rowe had not yet retired, and Jim Deetz was still with us. John Graham, a Mesoamericanist was still there. Then Deetz and Graham retired and John Rowe had retired before them. In 1994, the entire University of California system had a big early retirement program: what was called a very early retirement program. So Howell retired, Vince Sarich retired. Deetz retired and then went to the University of Virginia, and next John Graham retired. That led the way for us to hire Laurie Wilkie to replace Jim Deetz. Deetz was a pioneer in historical archaeology, and by the mid-1990s it was becoming increasingly popular and there were many more people doing it. So Laurie Wilkie was hired and then Christine Hastorf.

DWB: It is interesting that the hires were all females.

MC: Yes, Kent Lightfoot once said, and I think it was after Laurie's job talk, "will you girls invite us out for a beer?" The other person we hired around the mid-1990s was Junko Habu, who does historical ecology, and she has done some very interesting work on identity and archaeology. Now she is working on a big multi-million dollar project funded in-part by the Japanese government about sustainability and how can the study of small-scale societies contribute to understanding small-scale societies today. We also hired Rosemary Joyce as the Director of the Hearst Museum of Anthropology.

DWB: This may sound like a sexist thing to say, but was there something about this influx of female academics who came out of a specific set of academic contexts to make the Department at Berkeley what it was? You all appear to have been producing important work on closely linked themes.

MC: Though she is more recent and has just been awarded tenure Sabrina Agarwal is a great example of how one's intellectual/scholarly culture can influence you in positive ways. She is a bioarchaeologist, a bone biologist. She did a dissertation on osteoporosis among men and women in historic England. She has recently formed a field called social bioarchaeology, and has published a big companion to social bioarchaeology with Wiley-Blackwell. She is someone who came into the program and hadn't thought much about the fact that she was actually doing things related to gender or the more social dimensions of what you could learn about from the embodied social experiences. It was the case that even before she arrived we recognized that when she got here she would change, that she would shift slightly. So yes, there is a dynamic of ideas and researchers and we all have been variably influenced and certainly enriched by each other.

DWB: You have now retired, Ruth Tringham has retired, Rosemary Joyce is working half time in the graduate dean's office as an Associate Dean.

MC: We have hired two new people. Jun Sunseri, who was a student of Diane Gifford-Gonzalez and Judith Haubicht -Mauch. His dissertation work was in the American Southwest, in New Mexico, doing a colonial context project. Jun is also an archaeozoologist, and he does a lot of ceramic analysis. In addition he has an ethnoarchaeology project in South Africa. His wife, Charlotte, is an historic archaeologist and has a tenure track job at San Jose State University. The other hire is a woman, Lisa Maher, and she works in the Middle East, in Jordan, on a huge, fantastic Epi-Palaeolithic site, an aggregation, hunter-gatherer site out in what is the desert today (though it was a fantastic landscape back then). She does landscape archaeology, but also micromorphology and lithics. She and I are giving a paper together at the Society for American Archaeology meetings; at her site she has these hunters' huts, and in France we have a structure that looks like a stone-slab house. We are doing a comparative study about how people treat structures for hunter-gatherers. People call them

huts or cabins, but as soon as you get to the Neolithic, they are called homes. She is a Toronto PhD, and she spent five years in Cambridge on a post-doctoral fellowship; she has an impressive publication record. So, we have some new skills for our graduate students, and they are skills that transcend wherever you work in the world. Ruth and I are very excited to have them "in our places" and we, at the same time, have our own projects, such as the Center for Digital Archaeology (CoDA).

DWB: Let's move from the past to the future. As with other interviews for *Studii de Preistorie* we ask that imagine that you are to be stranded on a desert island. What books would you take with you?

MC: I would take something by Ursula Le Guin, at least one book and maybe two. One that I might take is a book of short speeches and talks that she gave and which cover a whole range of subjects. It is a book that I go through often. I recently reread Le Guin's *Left Hand of Darkness*, which is an amazing book. Also, I would probably take something that I would need a lot of time with; there is so much in them that it might be one of Henrietta Moore's recent books about the nature of anthropology.

DWB: You also may take one luxury item with you on the island. What would that be?

MC: I would probably take family photographs. I suppose there wouldn't be any Internet connection, so I will take the family photographs, though I guess that they could be in electronic formats.

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