An Interview With Irving Rouse
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Reports

An Interview with Irving Rouse[1]

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PS: I would like to ask how you got involved in anthropology and archaeology.

IR: I originally came here to Yale as an undergraduate student, intending to become a forester. Since forestry was a graduate school, I had to do my undergraduate work in plant science. I obtained a B.S. in that subject. The Sheffield Scientific School, in which I was enrolled, had a special program whereby I could take the first three years in plant science and combine the senior year in plant science with the first year of graduate studies in the Forestry School; after the second year in the Forestry School I would have gotten my M.F. degree. When I arrived at Yale in 1930 I put what little money my family was able to give me after the stock market crash the previous year into a bank; the bank went broke, so I had to support myself. I was fortunate enough to get a job cataloging archeological specimens in the Peabody Museum. Cornelius Osgood, under whom I later got my Ph.D., had just arrived at Yale and had discovered that George MacCurdy had catalogued less than half the specimens. There was an item in the budget for this work, so Osgood was able to hire me.

PS: Was there any particular reason you took this job over any others at the university?

IR: No, it was just the first decent job offered to me. Yale had a job-placement bureau. It gave me a couple of jobs raking leaves in the fall, then sent me to be interviewed, and Osgood apparently liked me. He felt strongly that I ought to have some training in anthropology, but he didn't want me to take the undergraduate courses in anthropology, which were then being taught in the sociology department. He didn't think they would help me with the job, so he signed me up for the course he taught in the graduate school. The result was that I took one graduate anthropology course a year throughout the time I was at Yale College. When I came to graduate school, I already had one year of graduate classes. I was required to take a full two years of graduate courses anyway.

Irving Rouse, 1993.

In my junior year, I decided I didn't want to be a forester, and Osgood persuaded me to come into the Yale graduate program. That meant that I had my senior year free, so I took only social science courses in preparation for my graduate studies in anthropology.

PS: Was your background in forestry and botany in any way responsible for your interest in classification in archaeology?

IR: Yes. I was drawn towards taxonomy in botany, but it appeared to me as I went through undergraduate school that it had become a mature subject and that all that was then being done was fine-tuning. I shifted to anthropology because I could see a need there for classification and figured that I could accomplish more than by just fine-tuning the Linnaean classification.

I contrast that with what happened in the 1960s and 1970s. In the early years almost all anthropology departments were associated with museums, often in natural history. In the 1960s and 1970s, with the expansion of

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anthropology after World War II, there was a shift to teaching departments of anthropology within the social science field. There was a shift from people like me, who were trained to study artifacts per se, to social archeologists, who were interested in the role of artifacts in social behavior. In other words, there was a shift of interest from the manufacture to the use of artifacts.

As I look back, I'm impressed by the fact that archeology in the 1960s had reached the same state of maturity in classification that biology had reached when I was an undergraduate. I suspect that if I had come into anthropology during the 1960s I would also have gone into nonclassificatory approaches, but by that time I had become too involved in all the problems of classification. I felt very strongly then, as I do now, that these problems are still with us. We have to use classifications, and there's still room for a person like myself, who's interested in refining the established classifications, so I decided that I might as well continue the rest of my career doing what I had been doing before.

PS: So you had an interest in classification even when you were in plant science?

IR: Yes. I took courses in ecology, but they didn't interest me as much as taxonomy.

PS: Were any particular individuals influential in your development?

IR: Osgood was the primary one, since he was my dissertation adviser. I would say that my main recollection of graduate school was that the professors had diverse and conflicting points of view. That bothered me, because as an undergraduate I had been led to believe that there was a right way of doing things and all I had to do was learn what it was. I'd be told one thing by [Edward] Sapir, for example, and a contradictory thing by [Leslie] Spier, and others by [G.P.] Murdock and Osgood. It bothered me a great deal at the time, but when I look back I think it was perhaps very good for me because it forced me to develop my own viewpoint, and it also forced me to be relatively open to other points of view, which I've tried to do throughout my career.

Another major influence on my thinking was in linguistics. Sapir was the chairman of the anthropology department. He had brought to Yale six linguists who had just obtained their Ph.D.'s. In those days, in the middle of the Depression, there were no jobs. He was able to get them postdoctoral stipends here at Yale. [C. F.] Voegelin, [Mary] Haas, and [Morris] Swadesh were among them. Sapir gave a seminar in linguistics each year. That was the only course in linguistics that he taught. He put me in the seminar, along with Weston La Barre, who was a student of social anthropology, and we sat there listening to all these people talking. I had no training; I didn't even know the phonetic characters. At the end of the semester he finally remembered we were in the course and asked both of us a question, which neither of us could answer. At the time I thought it was a fiasco. Now I realize that the seminar had a great influence on me, because the linguistic methods of analysis and linguistic assumptions sank into me, and the concept of modes I've developed is derived from them.

Leslie Spier was a major influence on my thinking. He was a cultural historian, studying the distribution of traits, a strong Boasian in that respect. Actually that kind of approach didn't interest me so much, but I learned it anyway through him.

Murdock, and his cross-cultural approach, were also very important to me.

PS: That's interesting, because the cross-cultural approach has also been one of the aspects of the New Archeology that has been promulgated by Binford and others [Binford and Binford 1968].

IR: I've been criticized for being hostile to them, but actually I'm not, because I had that background. In fact, Carl Hempel was on the Yale faculty, and I'm sure he had an influence on Murdock.

PS: What do you think is classification's place in archeology now?

IR: You can say that chronology is a form of classification. You can either classify in terms of chronological periods—the dimensions of time and space—or you can classify in terms of attributes, which I like to call the dimensions of form. At the most general level, you do genetic classification. None of those things were being done when I came into anthropology. I participated in the major advances that were made.

PS: What do you mean by "genetic"?

IR: You put the units you are studying into separate classes in terms of their ancestry rather than because they look alike. It's the difference in biology between the Linnaean classification, which is genetic, and numerical taxonomy, which is descriptive. One of the problems in biology is that those two kinds of classification don't always agree. One thing I learned as an undergraduate from my botanical courses is that you can't group the trees together on the basis of all their characteristics, because it would make no sense. You have to select the criteria for classification that mark ancestry. I remember being told that in trees it was reproductive organs that gave you the best way of doing genetic classification.

PS: How would you define "classes" vs. "types"?

IR: The two are often used interchangeably. The thing I came to do, and I think this was also characteristic of the better classifiers among my contemporaries, was to restrict the term "class" to the objects themselves. The artifacts from a collection that look alike are separated out, and each of those groups then becomes a class. It's a mechanical process. You're working with objects, as you are when cataloging specimens. The "type," on the other hand, consists of the attributes which define each of the classes that you've established. This was a major contribution of Jim Ford, among others; you refer to the set of attributes which defines a particular class of pottery as a type [Ford 1954].

PS: So a type is an abstraction.

IR: Yes. There are two ways that one may proceed. You may start by simply grouping artifacts into classes, or you may start more abstractly with the type and set up series of attributes that are definitive of the type and then arrange the artifacts accordingly and see whether it makes a consistent classification. At the time there was an argument as to which of these approaches was better. From the standpoint of the New Archeology, the first approach would have been nonscientific because you're not testing hypotheses.

PS: Do you think one is better than the other?

IR: I think it's a waste of time to say that one is better than the other. In fact, you do both. I agree with what is still being said by many archeologists who do classification—that there is no standard procedure for classifying artifacts. It's an intricate procedure, and you have to classify and reclassify your artifacts, by either of these methods, ten or twenty or thirty times before you finally get a classification that works, as we say. Classification is not really an intellectual operation; it is simply a device for organizing things. When we say that a classification works we really mean that our colleagues find it a useful way of organizing their material to do other, more interesting types of study.

PS: Once you have a classification that everybody agrees works, can the classification ever change?

IR: Yes it can. This I learned in biology. The reason they still have taxonomists in biology is that they're always finding better ways of classification. If you're doing it genetically, rather than descriptively, then you revise a classification because it provides you with a better knowledge of how the development of organisms took place. The same ought to be true in anthropology. We ought to be aiming towards classifications which better show the development of individual types of artifacts and, in the case of assemblages, of whole cultures.

PS: How do you conceive of the term "culture" as an archeologist?

IR: I remember I was impressed at one point by a remark made to me by my Yale colleague Michael Coe, whose father-in-law was the geneticist [Theodosius] Dobzhansky. He said that Dobzhansky once said to him that the central point of study in biology is life, but no biologist knows what life is. They're simply trying to find out what it is. So it's a goal rather than something that you know. And Coe said to me that he thinks the same is true for culture in anthropology. Social anthropologists won't believe this, of course, because society is the central concept for them. But in my opinion, if we're going to take society as a central concept, then there's no reason we're not sociologists.

PS: I think there are some archeologists today who would call themselves paleo-sociologists.

IR: Yes. I know at least one graduate student who's doing that right now. I'm not saying it isn't good, because I think we ought to do everything to learn as much as we can about the past. But I think one of the things that makes anthropology unique is that we have a focus upon culture. This enables us to compare culture with language and physical anthropology, and if you want you can add society to that. But I think, for me, and this is my prejudice, culture would be the central point.

PS: How would you distinguish "culture" from "society"?

IR: It's analogous to the distinction between class and type in the sense that society consists of the individuals who form groups and interact with each other and culture consists of the traits or norms which govern their behavior. This is why Binford [1968] didn't like the normative approach. You can talk about the cultures of different societies, but you can also talk about culture in a geographical sense. When we talk about American culture that's really what we mean.

PS: And American culture can be subdivided.

IR: Yes, into different social or ethnic groups, as the case may be. The thing that I'm trying to get at by quoting Dobzhansky is that culture is a very vague concept and we're trying to find out all that we can about it. I think it's a reflection of the fact that anthropology is so much newer a discipline than biology that we really haven't gotten as far as the biologists have in studying their major focus.

PS: This leads me to a question again on types. Types are constructs of the archeologist, some abstraction. Do you see any value in them, other than for organizational purposes?

IR: Well, yes. If you use "organizational" in the broad sense I was using before, it would include time and space chronology. But the ultimate value for me would be using it to determine origins—in other words, again, the genetic approach.

PS: Do you think that types reflect cultural differences?

IR: Yes. Types are one of the criteria that may be used to distinguish one culture from another.

In January 1944. (Photo courtesy of Irving Rouse.)

PS: It seems that there are people now who have taken modal analysis, for instance, much further in quantification by putting information into the computer and thereby gaining access to many analytical techniques—such as numerical taxonomy. You once told me that you see that as sometimes becoming an end in itself.

IR: Yes, that's true. In this sense Binford and his followers were correct. People were using classification as an end in itself, and they were trying to do things with classification that it can never do. In particular, classification will give you the norms. But I would take the point of view that what we seek to learn in archeology is all possible information about the past. The norms are only a part of the total amount of information available to us, and I think we have to get into the study of variability. It's taken me a long time to learn this, but that of course is what Binford was pointing out in the case of Bordes [Binford 1973, Bordes 1973]. He was using a classification to study variability within cultures, as opposed to classification of the cultures themselves.

PS: Would that be related to the difference between lumpers and splitters?

IR: Yes. One reason that I'm more of a lumper than a splitter is that I don't think we should go any further in classification than we need to for organizational purposes, or for the purposes of working out ancestries of artifacts or human groups.

PS: When you did your classic work in Haiti [Rouse 1939], your inspiration for doing it was to help in clarifying chronology for that region, correct?

IR: Yes, that's right.

PS: It seems that modal analysis is a flexible technique that can include other kinds of interests besides chronology.

IR: It's the same as with types. At this point I need to distinguish between modes and types. I got into this originally because in my doctoral dissertation I tried to apply the concept of type to the pottery of the West Indies, and it wouldn't work. Later, when I worked in Florida with John Goggin, I found that it worked fine there, and I used it in place of the concept of mode because it did work so well [Rouse 1951]. There are two ways in which you can make pottery. One is that you can start with a goal in mind, the kind of pot that you want to make, and just go through the procedure so that you end up with your model. That is a holistic approach, you might say, because you're looking at everything as a pattern; you're putting together a jigsaw puzzle, so to speak.

PS: A mental template?

IR: Yes, that's right. There's another way of doing it, which is in terms of the procedure of manufacture. At each stage in the procedure of manufacture, you stop and think, "What do I want to do next? Do I want to add something here? Do I want to end what I'm doing here? Or do I want to choose between a number of alternatives?" If you follow this approach you end up with an infinite variety of types of pottery. That's the situation in the West Indies. So the concept of type won't work there because there aren't any mental templates.

PS: But then how do we come up with styles, such as Hacienda Grande, for instance?

IR: That has to do with assemblages, not with artifacts. We're talking now about the classification of artifacts. Style is a concept derived from the study of assemblages. You can apply it secondarily to type or mode, as the case may be, but you can't form it that way. The only way you can form it is by classifying assemblages. Take an example from art history. We have what we call the Gothic style of architecture. That was established by studying all the architecture of the Middle Ages and noting that it was similar enough to group together and form a style. It's a classification of assemblages, that is, of all the buildings that you find in different sites in different parts of Europe. Once you've established that style, however, you can apply it secondarily to individual artifacts, and you can say that a building here on the Yale campus was built in the Gothic style. There's no assemblage of buildings in the Gothic style here in New Haven. It's picking something out of the past and applying it to a particular building. People confuse that secondary use of the term "style" with the primary use—the way it was developed. To get back to modes again, the problem you have is that there are three ways you can look at an artifact. You can look at it as an artifact as such, or you can look at it in terms of its manufacture or of its use. Starting off with a natural object, what artificial attributes were produced in that natural object to make an artifact? At the beginning of my time people were interested in the artifact per se, as an object. But when you go beyond that to become interested in behavior associated with the artifact, then there're two ways of looking at it: either in terms of how the artifact was made (behavior of manufacture) or in terms of how the artifact was used (its function).

PS: Today, these are big issues.

IR: I know. In my generation we were primarily interested in manufacture. Today, relatively few people are; they're primarily concerned with use. And that again reflects the fact that they're thinking in terms of societies and how societies use the artifacts. I've noticed that in your work, for example. To me the two approaches are complementary, and we have to do both of them in order to get a complete knowledge of the past. The only thing I object to is doing one over and over again, merely repeating yourself.

PS: When you look at elements of a pot you focus on specific characteristics of it—whether they are technological or stylistic?

IR: Yes, you're really not focusing on the procedure of manufacture itself; rather, you're simply using that as an organizing principle through which to investigate all the artificial qualities that have been produced in the artifact.

To get back to the concept of mode again, what I've been talking about so far is in terms of whole pots. You look at the pot from the standpoint of how it's made or how it's used. But when you get potsherds, that adds a new factor to it, because potsherds have to be reconstructed into whole pots before you can do these things.

PS: Frequently we don't have that luxury.

IR: On the one hand, if the procedure is done in terms of a mental template of the potter, then we can reconstruct that mental template. However, if the potters had the additive approach whereby modes are piled onto one another, so to speak, then there's no way you can get at a mental template to use for classification.

PS: It seems that it's impossible to make a pot without a mental template of some sort, isn't it? There has to be some idea of a goal. The potter knows what he/she wants to make.

IR: The potters know what they want to make, but the point is whether the mental template is small enough to be fully comprehended—so that it actually is a template—whether there are so many options and alternatives as they go through the procedure that they end up with a big confused mass of attributes. At one extreme there is a very simple plain hemispherical bowl, and at the other extreme you'll have a procedure in which they put a pedestal base on that bowl, they make a keel on the side, then decorate the shoulder with different kinds of designs or then put lugs on the rim or flanges on the rim and lugs on the flanges. Well, when you get that type of situation, there's no way that you can produce a coherent mental template at the end. An analogy to this would be in flint working. In the old days they used to classify flint artifacts in terms of mental templates. Now they find that they have to work instead in terms of the procedure of manufacture—the modes that I'm talking about. You will find that certain separate types of artifacts before are simply stages in the manufacture of the completed artifacts. What it amounts to is classifications of different stages of procedure and use. That's what the concept of mode does in ceramic studies.

Anyway, if you have a complex ceramic tradition, such as in the Philippines and parts of Africa, there's no way that you can use the kind of type that you get in the Ford classification. It just won't fit the artifacts.

PS: Why is that?

IR: Well, because his concept of type is a template, a simple template, and you can classify the artifacts into a series of templates. But sometimes you have overlapping templates, and there's no way you can separate them out to get discrete classes of artifacts. So with more complex ceramic traditions you have to use a different approach. That's the situation in the West Indies. As I said earlier, I got the idea out of linguistics. This is what linguists do when they analyze words. Words as such mean very little. You have to break them into their linguistically significant parts—phonemes—for analysis, then use those as your classificatory units.

PS: And those can transfer.

IR: Yes, from one word to another, and be modified as they are transferred. The same is true of pottery. The key there to me is "feature." For me a feature of a pot is any part that would have been recognized as being distinct by the potters. Instead of classifying artifacts as whole objects, in terms of the end product, you classify them in terms of their features. For me, a mode is a type of feature. You group features into a class, and then you establish a type of feature as opposed to a type of artifact. Because that latter dichotomy was confusing when I originally developed it, I coined the term "mode" to refer to the diagnostic attributes of a class of features, as opposed to the term "type," which refers to the diagnostic attributes of a class of whole artifacts.

PS: So mode is the smallest unit of analysis.

IR: Yes.

PS: And perhaps with refinements in a modal analysis you may even be able to break a mode down into finer units.

At workshop on prehistoric cultures and artifacts of the Lesser Antilles held in Rouse's lab at the Peabody Museum of Natural History, Yale University, July 25–28, 1994. Left to right, *Martin T. Fuess, Rouse, Birgit Faber Morse, David R. Watters (back to camera), Aad H. Versteeg, James B. Petersen, and Desmond V. Nicholson. (Photo courtesy of A. Reg. Murphy and Desmond Nicholson.)*

IR: Modes are overlapping just as linguistic characteristics are overlapping. For example, if you look at a particular design, which is a mode, and see how it is applied to different parts of a vessel's surface, you'll find great differences. The best example of that in the West Indies is the curvilinear incised designs on Chican pottery, which were transferred to Ostionan pottery in Puerto Rico. On the Chican pottery, they're on the shoulders on the outside of the vessel, where they're relatively broad. Puerto Rico potters transferred them to their favorite area of decoration, which was ridges or bevels inside the rim. These were very narrow, so they had to simplify the designs. They broadened the ridges more than before and simplified the incised designs in order to fit them into a narrow space.

PS: That reminds me of Jeff Walker's recent dissertation [Walker 1993]. He did some interesting things along those lines but also in seeing designs transferred to stone, such as stone collars.

IR: Yes. When designs get transferred from clay to stone there are differences.

PS: But he could also identify similarities.

IR: Yes that's the study of modes I'm talking about. And that's very different from establishing types of artifacts. Ford was aware of this, but Evans and Meggers [1960] weren't; I don't think they understood the concept of mode.

PS: What happens when people don't understand the concept of mode?

IR: Well, they equate modes with types.

PS: Today there's an interest in such issues as systems of production, exchange, and ideology. Some archeologists are trying to get at these issues through looking at artifacts. Is there a place for modal analysis in light of these interests?

IR: There is if you're looking at ideology, because ideology is structured in terms of concepts. That's what you're really dealing with here—the artisan's concepts of what pottery is like—and you can go on from that to look at what those concepts meant to the artisan. But not with respect to behavior of using the artifacts, which is something else again—and I'd like to get at this from two points of view. There is the difference between the manufacture and use of artifacts. I think that's why, and correctly, your generation of archeologists no longer does classification in the way that we did it. If you're looking at use, you want to look at the attributes which made the artifacts useful, not all the attributes that the artisans built into the artifacts when they made them. Attributes may be made with an eye towards use, but particularly in pottery, where you get so much emphasis on form and decoration, the attributes of use are a relatively small number when compared with the attributes of style, so to speak. Style vs. function is another way of saying it. If you're interested in the manufacture of artifacts, you're interested in the stylistic attributes that were produced in the artifacts. If you're interested in function, you're interested in the functionally significant attributes.

That leads me to a more general subject: How do you reconcile differences between different people's approaches? This became a point of great interest to me in the 1960s, because I was sort of a whipping boy, or one of the whipping boys, of the New Archeologists; I was a normative archeologist. I'm still getting criticized for that by people like Straus [1987] and, more indirectly, by Sued-Badillo [1992]. The implication is that I'm not politically correct, and Straus thinks it's deplorable that I continue to flaunt the term "normative" when it's so discredited at the present time—I'm just being old-fashioned. When all this came up in the 1960s I had a reputation among students around the country for being more permissive than most archeologists in allowing graduate students to do what they wanted. I didn't insist that they do what I wanted them to do. I started with the basic assumption that the aim of archeology should be to learn as much as possible about the past—that any new technique that gave us additional information of a new kind was important and that the worst sin was simply repeating what had been done before to no purpose, thereby wasting time and money.

PS: I think one of the things Binford was saying at the beginning was that it's impossible to know everything about the past because culture has an infinite number of attributes.

IR: That's true. That was the point I was making earlier. Culture is something that we'll never know. All we can do is approximate it. All I'm saying is that the aim is to learn as much as possible about the past. I didn't say "learn everything about the past." That would be impossible.

PS: You need to have a framework to structure your investigation.

IR: Just before the revolution in archeology took place, archeologists had very high prestige in the discipline of anthropology because we knew what we wanted to do. I remember once in the early 1960s I was asked to attend a conference at the University of Minnesota of social anthropologists who felt that they had no real goals and were all going off in different directions and weren't really accomplishing as much as they might. They decided to bring in linguists, physical anthropologists, and archeologists, because they felt that these researchers had a better conception of their goals. We saw what was needed and we were doing it. Then Binford and his generation destroyed all that. To me, archeology has turned into groups of specialists on different subjects who often think that their subject is the only one that's worth pursuing. We've lost the overall sense that we once had.

My feeling at the time was not that the New Archeologists were wrong but that we were both right. The problem was that we didn't understand what each other was doing, and so I set out at that time to try to figure out a strategy of archeology to replace the one we had before that would encompass both sides. That's the basis of the strategy of archeology that I've been talking to you about. The latest version of it is in my book on migrations [Rouse 1986]. What I did was start off with the assumption that you can look at archeology in terms of a series of levels of interpretation. The first level of interpretation, which was the major level of interest when I became an archeologist, was simply the artifacts themselves. I remember, as an example of this, the first review I ever wrote [Rouse 1936], which was of a book by Willoughby [1935]. He wrote a general summary of New England archeology. Well, it was just a description of all the remains that had been found, and then a brief chapter at the end discussing origins where he presented some theories of migrations to explain how these differences arose. I wrote a review in which I pointed out that there was no real evidence in favor of any of these migrations. I received a plaintive letter from Willoughby, in which he complained that I was setting up straw men. In a sense I was, because for him his comments didn't mean anything; they were just opinions that he was putting in at the end of the book. He was primarily interested in describing the artifacts.

Well, that's Level 1, and that's divided into two parts: first the sites from which you obtain the artifacts and then the artifacts themselves. It was followed by the development of chronology. That's what I did in my doctoral disseration [Rouse 1938]. I divided this level, too, into two parts, the setting up of chronological periods [Rouse 1941] and a test of those periods [Rouse 1939]. Each period is defined by types or modes formulated on Level 1; you select out the ones that are diagnostic of each period. Those we call "time markers." Then in Level 2 you look at the distribution of individual types and modes relative to these periods—in other words, trace their distribution, establishing horizons and traditions whose distributions serve as a check on the periods you've set before. You may find there are horizons that don't fit your positioning of the periods. So you have to move the periods up and down to adjust to the horizons. That's Level 2. In effect what you do in Level 1 is look at the data per se, the sites and artifacts. In Level 2 you organize the data in terms of time and space.

Then you go on in Levels 3 and 4 and look at the people who possessed and made the artifacts. In other words, you're turning more towards human behavior, that is, at the manner in which the artifacts were made and used. Level 3, as I see it, is the level of culture, and I would call its study cultural archeology. Level 4 is the level of society, or social archeology. On Level 3, you take the periods that you've established on Level 2 and look at them not simply as time-space divisions but rather as groups of people, each with its own culture. Often you can take a period and convert it into a culture simply by saying that the culture is the culture of a particular period. On Level 4, you can take a given social group and study the culture of that social group rather than the culture of a time-space unit as you do on Level 3.

PS: In terms of this logic, if you come up with a different chronological framework, then that has drastic implications for social concerns.

IR: What actually happened during the period when we were setting up chronologies, during and immediately after World War II, was that two different approaches developed. One was in the Southwest and the Southeast, where they did it the way I've been talking about—starting with chronology and forming cultures. But in the Midwest they couldn't do this. The reason was that in the Southwest and the Southeast they had been digging refuse sites, habitation sites, so they were able to get chronology primarily by stratigraphic excavation of refuse or by seriating house types, or something of that sort. In the Midwest, however, for various reasons ultimately going back to the Field-Columbian Exposition in 1890, a big thrust was made to excavate mound sites to get fancy artifacts to show at this exhibit. They got started doing that and spent the next 50 years just digging mound sites. I remember as a graduate student reading through that literature and feeling very frustrated because they would tell you about the structure of the mounds and so forth but nothing about what the refuse was like and the pottery that was found in association with the mounds. Then when the point came to organize this material they couldn't do it chronologically. They were quite aware that they should, but they just couldn't. So they said, "Since we can't do it chronologically, we'll do it descriptively." That was the origin of the Midwestern Taxonomic System. It was simply a device to use to organize the great mass of archeological material that had accumulated in the absence of chronology. The more sophisticated people among them said that this was only a temporary device—"When we get the chronological knowledge, then we'll convert it into chronology." That, in effect, is what James Griffin did. He started out by being a typical Midwestern taxonomist, but he immediately began doing chronological research. That's why he concentrated on pottery so much. Eventually he came out with a chronology [Griffin 1952]. In all these units I'm talking about, I would make a distinction between strategy and tactics. The strategy is the ideal method of doing it, but there are always cases, as in the Midwestern Taxonomic System, where you can't follow the ideal and so you have to use a tactic which is different.

PS: Something that'll get the job done.

IR: Yes. Actually, in this case they used the taxonomic units, rearranged, as the units of chronology. One of the problems that the taxonomists had, and Binford made a lot of this, is that if you're classifying first in terms of chronology, all the remains of a given period are grouped together, regardless of what the remains were used for. For instance, artifacts from ceremonial and village sites are considered together. Because they're being classified by time-space, artifacts are grouped together as a single culture. In the Midwest they had all of these mound structures, and it turns out that they were used for the same kinds of activities by different peoples. The typical example is what Ritchie [1938] called Hopewell in New York State, which was actually not a Hopewellian culture but simply a Hopewellian mound-building activity that was taken over by a local people in western New York State.

From this point of view, Binford and those who followed him were correct. You've got to take into consideration the activities of the people. But that can still be done on Level 4. There's no reason people can't start with Level 4. In fact, again, if I can quote my colleague Mike Coe, he thought that with their interest the New Archeologists would be most successful in studying historical archeology, because they can obtain so much social information from documents and these can be evaluated along with the material remains. My strategy is inductive. That is, the researcher generally proceeds from lower to higher levels of abstraction, Levels 1–4. That's nonscientific from Binford's point of view, and I'm an empirical positivist. If I were a historical archeologist, however, I would not start with Level 1 and go to 4 but start from 4 and go back to 1. In that case, the primary data are the historical evidence, and archeological remains are used to fill in gaps and to check on the documentary evidence when there are discrepancies, which are bound to occur because people don't always say the correct thing. Archeology is like detective work. The few cues we have determine where we start. If you get clues about Level 4, start with Level 4—don't feel that you have to wait for Levels 1, 2, and 3 to be filled in beforehand.

When I first set up this strategy, before the New Archeologists came along, there were only three levels, 1, 2, and 3. And then along came the New Archeologists, with a different approach. At first, I thought that they were attempting to do the same that I was on Level 3. However, they were saying that these are two alternative procedures; you either do one or the other. One is politically correct and the other is not. Early on, I came to believe that this was not so, but I was so bound up in my own research on Level 3 (the study of cultures) that I just couldn't understand what they were talking about. It took me about 20 years to figure out the difference between these two approaches. You get in a rut of thinking, and it's hard to recognize things that you don't know, especially if they're not within your sphere of experience.

It is important to note that on Level 3 you concentrate on populations or, as I prefer to call them, peoples. The two terms are synonymous so far as I'm concerned; I prefer the simpler form. One archeologist doing postgraduate studies under me was from Japan and another from Germany. That was when I was writing my *Introduction to Prehistory*, in which I stressed the concept of people [Rouse 1972]. They both told me at the time that that concept wouldn't be acceptable in their countries because the term "people" had been so misused during the war by the Nazis in Germany and by the Imperialists in Japan. My answer was that you can't correct something like that just by ignoring it, sweeping it under the rug; it must be used in the right way in order to show that there is a way of applying it that is valid, as opposed to a way that is not. I've been attempting to do that in my emphasis upon people, working on Level 3. A "people" is a geographically and temporally defined unit to which a name is assigned. This is accomplished by classifying assemblages of artifacts rather than individual artifacts. By doing so, we get at the total assemblage of cultural traits that each people had.

PS: Are "peoples" and "cultures" synonyms?

IR: No, a people is to a culture as a class is to an artifact. That is, people are the individuals who were involved, as opposed to the artifacts themselves, which are dealt with on Levels 1 and 2. The culture consists of the habits, customs, beliefs, and artifacts that were possessed by that group of people. When discussing a specific subset of an assemblage, say, the pottery, as we tend to do in the West Indies for practical reasons, the term "style" may be employed. A style consists of all the ceramics. To be more symmetrical about this, one could say you have a school of potters; then the style is the characteristics that were produced by that school of potters. Correlated with the peoples and their cultures, directly or indirectly, are speech communities with their languages and races with their biological characteristics. This is all done on Level 3 of my analytical strategy. Peoples and cultures are defined on Level 3A, and the manner in which they develop is investigated on Level 3B. In other words, peoples are classified in such a way that one is able to work out lines of development or ancestry moving back into the past.

All of this is normative. Norms are the criteria that you use, primarily modes and types. My colleagues and I had a long discussion when we were first working on this about whether types or modes were better for establishing both chronological units and cultures. At the time, I argued that types were more important for chronological purposes and modes for forming cultures. I wouldn't say that now. I now believe that it depends upon the nature of the material. When dealing with pottery, in which features were produced in that complicated way I was talking about earlier, modes are more appropriate to use. In contrast, if you are dealing with artifacts that were produced by a relatively simple mental template, then it's better to use types. Again, the important question is which works better. There shouldn't be any preconceptions about what you use, in my opinion.

PS: It's simply a pragmatic choice?

IR: Yes.

PS: In the West Indies, we focus on pottery for practical reasons.

IR: Yes, if there was good preservation in the West Indies we'd also be including wood carving in this, and we do include stone carving.

PS: What about a ceramic complex?

IR: "Ceramic complex" is the other way of referring to a style—or it's often called a "phase." In the Midwestern Taxonomic System it's a "focus."

PS: Ritchie's projectile points are types [Ritchie 1961].

IR: Yes, that is what he calls them. The term "style" often is used to talk about types of projectile points, but that's poor usage of the term.

PS: A style is more inclusive?

IR: Yes. On Levels 1 and 2, types of sites and types of artifacts are the primary units of study. When moving to Level 3, where a holistic point of view is desired (that is, you look at the culture as a whole), you must work with artifact assemblages of different types and pottery with different modes. In my experience, that's one of the most difficult things for amateurs to understand, because they're interested primarily in types, and they try to read the concept of type into what I call a style. As time goes by, I've become convinced that assemblage is the key to this. Assemblages of types are the units of study on Level 3, as opposed to the individual types that are studied on Levels 1 and 2.

PS: What about variability in all of this?

IR: Variability comes on Level 4, where the units are not peoples but the societies into which the peoples are organized. A people is a group of individuals who have similar customs and feel that they are related to each other because they occupy a given area, an interaction sphere, so to speak, in which they exchange ideas and norms of various kinds. In other words, a people is a stylistic unit. A society, in contrast, is a functional unit in the sense that it consists of individuals who belong to the group not because they have common norms or styles but because they have a common purpose and are interested in achieving certain goals.

In my opinion Binford's greatest achievement is that he stressed the need to study variability. He's saying it with reference to Level 4. At the time, I was trying to apply the norms of Level 3 to achieve the goals of Level 4, which is impossible. The important thing about my strategy is that you have to use different concepts and methods on each level.

PS: It's interesting that in 1935, when Willoughby published his book, your criticism of him was along the lines of what today is called middle-range theory. He presented detailed information on artifacts and speculations on migrations but did not attempt to link the two.

IR: In those early years the orientation was entirely towards the objects. That was Boasian. Boas said that you can't draw conclusions—that it's nonscientific to draw conclusions—unless you have very good evidence about them. As I went through my career, interests in the field shifted to method. I believe that my doctoral dissertation [Rouse 1938, 1939] was a major contribution to this change—at least everybody seems to still like it. I was trying to develop methods for drawing conclusions rather than simply describing artifacts. It seems to me that there has since been an overemphasis on method. I feel partially responsible for that. People became so interested in methods that the results in effect were unimportant. Archeology was held in very high regard by other branches of anthropology when I first came into the field, but its position has deteriorated. We're often regarded now as just playing around with methods.

PS: Part of this problem may be related to the development of computers and the use of them. People became very enamored with the black-box approach to problem solving.

IR: Well, that's true with every new technique that comes along. It started off with radiocarbon dates. Before we had radiocarbon dates, chronology was established through elaborate procedures of stratigraphy and seriation. Many people in your generation now think we don't need those procedures because all time and space issues can be resolved by obtaining radiocarbon dates. That's a great mistake, in my opinion. When radiocarbon dating was first developed, one of the few labs was here at Yale. I was on the board of the lab at the time. People who had charcoal samples immediately brought them into the labs and asked to have them dated. It was impossible to date all of them. We had to think of a way in which we could reasonably select the samples that were worth analyzing. We came to the conclusion that the best way to do this—and I think it was Wendell Bennett who suggested it—was to select samples that had already been dated by some other method, namely, by the kinds of chronological research in my Level 2. For instance, a charcoal sample that could be reliably assigned to such-and-such a ceramically defined period could be accepted for radiocarbon dating. If, however, a sample was found in a site and the researcher simply wanted to know the age of the site, we wouldn't touch it. That's what the New Archeologists were doing. They were saying that it was unnecessary to know the cultural correlations of the radiocarbon dates—that the radiocarbon dates in and of themselves were sufficient.

PS: I think people are less naive now.

IR: You're right, I was talking about the 1960s. The point is that ceramic analysis and radiocarbon dating are two different lines of evidence. You can draw conclusions from each of them, but they both have weaknesses of different kinds. A solid date is obtained by comparing the two lines of evidence and making them consistent. This is what I call "consilience" in my *Tainos* book [Rouse 1992]. That term comes from biology. Conclusions are derived from two or more different lines of evidence which reinforce each other. That's the real chronology; I should amend what I said about Level 2 to include radiocarbon dating.

PS: How has radiocarbon dating affected your initial chronology of Haiti?

IR: When radiocarbon dates were obtained they confirmed the ceramic chronology in some places and in others caused us to revise it. In the West Indies, as in most of the New World, the radiocarbon caused us to push our sequences back, because we had had no way of determining duration from only the artifacts. When I first came into archeology, we believed that the West Indies had only been inhabited a few centuries before the time of Columbus and that we could identify local cultures by using the historical names for the peoples that possessed them. We expected the Tainos, for example, to have extended way back into prehistory. Now we know better.

PS: I have a general question concerning types: Within an assemblage from a given time period, you have several types?

IR: That's the nature of an assemblage. It's a collection of artifacts of different types. Once types and modes have been established they may be used to classify assemblages. This will result in a class of assemblages and will also provide a definition of each class, consisting of a complex of types and modes. I use the terms "complex" and "style" interchangeably.

The last detailed site report I have to write is for one on Trinidad. I haven't done this, partially because I couldn't figure out how to. Arie Boomert had the same problem when he worked in Trinidad. Between us we finally worked it out. It's the problem of plural cultures that I discuss in my book [Rouse 1992]. Trinidad is right off the coast of South America and was subjected to influences in pottery from different directions, coming south from the West Indies, west from western Venezuela, east from the Guianas, and north by way of the Orinoco Valley. Trinidad is situated at the junction of all these routes and was therefore subject to influences from all these places. If the influences had been simply in terms of individual artifact types—as for example in the case of the so-called three-pointer, which our present evidence indicates originated perhaps as far east as Colombia and spread along the north coast of Colombia and Venezuela to Trinidad and then out into the West Indies—then it would be relatively straightforward to determine origins of specific influences. But the problem is that in these sites, entire assemblages came from different directions.

Specifically, in the case of Trinidad the ceramic age begins with the Saladoid series. This starts with the Cedros culture, which is typical early Saladoid. This is followed by Palo Seco, in which vessels are no longer decorated with zoned incision, a typical Cedrosan Saladoid ceramic mode. We also begin to see evidence of modeling coming in from the Barrancoid series in the lower Orinoco Valley. Modeling was already there, so it was only modified; it didn't produce anything new. During the next period, Erin, there is a real jumble of influences, and that's what causes the trouble. Pots that look like Los Barrancos ceramics from the lower Orinoco and others that are obviously derived from the earlier Palo Seco period of Trinidad are found side by side in the same sites. Does this indicate overlapping of occupations, one Palo Seco and the other Los Barrancos? It doesn't look that way, because they're found in precisely the same contexts, just as are vessels decorated with zoned incised cross-hatching and others with white-on-red painting in many Cedrosan Saladoid sites. An analogy may be drawn between this situation and the plural cultures and societies that my former colleague Mike Smith talks about in the West Indies [Smith 1965] and that are of course a big issue in the United States now.

PS: Multiculturalism?

IR: Yes, multiculturalism. That's what I think we have in prehistoric Trinidad. I think I could now go back and really make sense out of that pottery. But I couldn't do it at the time because I didn't have a concept that would account for typically Barrancoid pottery mixed with the local styles.

PS: What about interassemblage variability?

IR: Let me get at this through an analogy to language. My colleague here at Yale, Floyd Lounsbury, once made the comment to me that in traveling from Lisbon in Portugal through Madrid in Spain through France and into Belgium, you'd never visit a village where the inhabitants didn't understand the people in the next village. There's always a language that's understandable to the two neighbors. However, when you make that trip you're going through four different languages, starting with Portuguese and then Spanish, Catalan in Barcelona, and finally French. The point is that there's a difference between speaking of a language as such and of variability within it. When you study both you'll find that boundaries may be drawn around the language territories, but people living along the boundaries are going to be transitional. This is purely utilitarian. They have to be able to converse with each other, so they develop a variant of the language, which is closely related to the next language.

PS: Dialect?

IR: Yes, different dialects. In the case of culture, there is so much contact across the border that they exchange traits, resulting in blurred boundaries and greater variability within and across groups. This is a type of variability that I don't think Binford has shown any interest in, but it exists. The problem is that when you find the boundaries, with transitional forms, the tendency is to assume that they're different cultures, instead of recognizing that they are comparable to different dialects. They are variants on the main culture.

PS: You got involved in archeology fortuitously by needing a job as an undergraduate. How did you get into West Indian archeology? You've also done work in eastern North America. How did that come about?

IR: Eastern North America came first, when I was still an undergraduate at Yale. Professional archeologists decided that they couldn't prohibit the activity of amateurs and that it was better to try to educate them and get them to do work that would be useful for scientific purposes. The National Research Council established a program to set up state archeological societies. In Connecticut the Peabody Museum at Yale was asked to do this. It was Osgood who founded the Archeological Society of Connecticut. I was only an undergraduate student, so I didn't attend the first meeting, but [Froelich] Rainey, who was a graduate student at that time, did. The following year they asked me if I would be secretary of the society. Though still an undergraduate, I agreed. I was the secretary for a number of years, and then they asked me to be editor of their journal, which is called the *Bulletin of the Archeological Society of Connecticut.* I served as editor for about 15 years. Through my association with the Archeological Society of Connecticut I became involved with the Eastern States Archeological Federation. Osgood and I were among the founders of that federation; he was president for a term and I was president for a term. I used to travel regularly to the meetings of the federation.

PS: What about the work you've done in Florida?

IR: Osgood defined the Caribbean Anthropological Program to include Florida. When he came to Yale, he found that MacCurdy had founded an organization called the School of American Research to study European prehistory. MacCurdy eventually turned the School over to Harvard rather than Yale, unfortunately for us. Osgood had to look for a new focus of research. He decided on the Caribbean because when he was a graduate student at the University of Chicago one of his fellow graduate students was Charlotte Gower, who wrote her dissertation on the West Indies [Gower 1927]. It was a typical trait-distribution study. Gower was interested in the fact that the West Indies are a geographical connecting link between North, Middle, and South America. She studied the diffusion of individual traits from the mainland into and through the islands to the neighboring mainland. Osgood continued this research program, and he started in Florida because it was the easiest place to get to. He and Rainey conducted archeological fieldwork for a season in Florida. Then we went on to the West Indies and Venezuela. We never did get as far as Central America.

PS: The Caribbean Anthropological Program was a development out of Gower's dissertation?

IR: Yes. And it included social anthropology as well as archeology. Sidney Mintz was on our faculty at the time. A number of his graduate students, such as Bill Davenport, worked in the Caribbean area. Later there was Michael Smith. So Yale has had a strong emphasis on Caribbean social anthropology as well as archeology.

After World War II it was difficult to get back into the West Indies. Transportation patterns hadn't yet been reestablished. I wanted to go into the field, so Osgood suggested that I work in Florida. At the same time John Goggin came to Yale as a graduate student. He was a Florida archeologist, so we brought him into the program [Goggin 1946, 1947, 1948; Rouse 1951]. Then I went back into the West Indies.

PS: So Osgood had the idea of Caribbean anthropology.

IR: As I said, he envisioned it—and so did I at the beginning—in terms of trait studies [Rouse 1958]. One of the criticisms made of me was that I was just a diffusionist tracing traits. That was what I did in the 1950s [Rouse 1953]. Eventually I grew out of it. After realizing that there was really little relationship between Florida and the West Indies, we dropped Florida from the program. We never went into Yucatán, for the same reason. Thereafter, we limited the program to the West Indies and northeastern South America [Rouse and Cruxent 1963]. We shifted the problems from the study of individual traits, which we had done through classification of artifacts and features, to the study of the origin and development of cultures, which is done in terms of classification of assemblages. My efforts have been largely devoted to trying to counteract the assumption that everything had to come in from the outside. I have become more and more interested in recent years in looking at the developments that took place inside the Caribbean area. If you simply say that migration doesn't exist, you don't get very far, but if you also look at the development that took place there, the whole theory of migration falls away.

PS: You've taken at least two sabbaticals that I know of, one to England and one to South Africa.

IR: The second wasn't a sabbatical. I taught there for a spring quarter. This was at the University of Cape Town in the 1970s. One of the students I had here at Yale was Nick van der Merwe, who's now at Harvard. He had come from South Africa and had gone back to become a professor at Cape Town.

Van der Merwe was a Rhodes Scholar in reverse; Rhodes gave money not only for American students to go to England for graduate work but also for South African students to go to the United States or to England for their graduate work. Van der Merwe had started out at an Afrikaner university in South Africa majoring in chemistry; he came here also intending to study chemistry. He took my course in world archeology, and it interested him in archeology. At the same time he got to know Minze Stuiver, who was then the head of our radiocarbon lab. He started working for Stuiver, extracting carbon from charcoal samples. Eventually he got an NSF [National Science Foundation] fellowship to go on at Yale as a graduate student in anthropology.

He decided that it would be good for his students at Cape Town to hear from outside lecturers. The first one was Binford, who came in the middle '70s. I was the second one.

PS: That was already late in your career. You were well established in your ideas. Given that, was there any further influence on your thinking by going to South Africa?

IR: I was interested in going there because I taught a course on world archeology in which I had come to feel that Africa was not being given the importance it deserved in studies of the origin of man. I was really trying to acquire more knowledge about the prehistory of that area. When I got there I was impressed by two aspects of methodology. One was that the South African archeologists were technically better than we were. They used excellent laboratory techniques. The other was that van der Merwe and his colleagues were using what is sometimes called the direct historical approach. He had started at Yale as a chemist and then worked with the processing of archeological samples for radiocarbon dating. We had expected that he would continue that in graduate school. But midway through his career there he decided to take a year off and go to the University of Wisconsin to study African ethnohistory. He wanted to study modern ironworking practices among the blacks and then, in effect, project them into the past. He has done a very good job of this. One of the problems with much of the technological analysis that's conducted in the United States now is that it's purely technological. Researchers go through the process frequently without relating their results to culture as a whole. Van der Merwe realized that you couldn't really understand what was going on without examining the process as a whole in relation to other aspects of culture, economics, religious influences, and so forth.

PS: When were you in England?

IR: In 1963–64. I went to the Institute of Archeology at the University of London. I was attracted to that institution because it had been founded and headed for many years by Gordon Childe, who had been one of my idols as a student. I was disappointed because Childe had died a few years before. Since he had been a poor administrator, the university had decided to replace him with someone more practical. They brought in a man named [W.F.] Grimes, the English version of a contract archeologist. He had made his reputation recovering remains exposed by bombing during World War II, and then he became head of the Museum of London. He changed the character of the Institute, making it practical instead of theoretical.

The one thing that impressed me intellectually at the University of London was its strong environmental approach. At that time Fredrick Zeuner was there. He was a geologist specializing in the study of the Paleolithic environment. I took his course; unfortunately, he died halfway through it. I also sat in on a number of other courses while I was there.

PS: So you found that a useful experience.

IR: Yes, it was very useful. I was learning new methodological approaches. This is where I acquired the emphasis I've had in recent years on people; indirectly, it's from Childe. Childe looked at cultures in the abstract. I've added to this concept the peoples that possessed the cultures. If you only look at each culture abstractly there's no way of connecting it with what people do and what needs they have. If you focus on a people it provides a connecting point between the culture and the environment that surrounds the culture.

PS: This sounds somewhat similar to what Binford [1977] would call middle-range theory—linking up the abstractions to the data.

You mentioned that Childe was one of your idols. You certainly made a big case for migrations in prehistory. Is that an outgrowth of your interest in Childe's work?

IR: No, that had nothing to do with Childe.

PS: He's often associated with diffusionary arguments.

IR: He was not primarily interested in migrations, although he did assume some of them. Mainly he studied the spread of traits from centers [Childe 1951]. For many years, in teaching my course on world prehistory that was my focus. When moving into a continent, I would first look at the center of cultural development and then trace the spread of traits out from the center.

PS: Is that an age-area approach?

IR: No, because I wasn't interested in it in terms of chronology. It was my way of explaining the origin of peripheral developments and the fact that peripheries often acquire innovations later than those in the center. What I've learned over the years—as a matter of fact, I was stimulated to this to a considerable degree by the year I spent in England—is that peripheries have their own independent developments. That's been a particular trend in England since the time of Childe. He viewed the English people as simply barbarians who were influenced by the innovations from the Mediterranean region. Recent archeologists, such as [Colin] Renfrew, have argued instead that there was very strong local development, which is really a blend of influences going back and forth. So you have to look at cultural contact and interconnections rather than at peoples in the periphery slavishly copying innovations produced in the center.

PS: That's interesting. Today there's an interest in this notion of core-periphery relations [e.g., Chase-Dunn and Hall 1991]. Renfrew is one of the people with this interest.

IR: But they look at it not as the periphery being dependent upon the core but as the periphery having developments of its own which are parallel to the core.

PS: And also in relation to other cores and peripheries.

IR: Yes, that's right. There's one other major trip of mine which you haven't touched on and that is to Japan.

PS: I wasn't aware of that one. What year was this?

IR: This was in the early 1970s. I was about to publish my *Introduction to Prehistory* at the time, and I showed the manuscript to Kimio Suzuki, a young Japanese archeologist. He offered to translate it into Japanese and did so while he was at Yale. His translation was published in Japan soon after the original was published here. That got me an audience in Japan. Subsequent books have also been translated into Japanese. I don't know why, but I may have greater popularity in Japan than here.

PS: Perhaps its being an island?

IR: Well, that's partially it, because I showed them that the way we handle islands in the West Indies could be used in Japan [Rouse 1986]. It's a similar situation. You have a linear string of islands extending from Taiwan and Korea in the south to Siberia in the north. There's a problem of migration and diffusion through these islands. For example, I was able to show that the concept of passage area, which I've developed for the West Indies [Rouse 1982], also applies to Japan. The Japanese believed that they had migrated into Japan by way of Korea. I pointed out that the southern tip of Korea and the adjacent part of Kyushu Island in Japan are actually a passage area, which was a center of development rather than a route of migration. Developments were taking place on both sides, and traits were being exchanged, as in the case of the Mona Passage, between Puerto Rico and the Dominican Republic.

The Japanese have done probably the best chronological research in the world. They had a very good situation because during the protohistoric Kofun period, when there was close contact with Korea and China, many Chinese trade goods were brought in. They are precisely dated in China, and those dates can be applied to the Japanese remains. Japanese archeologists are able to date their protohistoric sites within a range of about 75 years. Of course, they also do excellent radiocarbon dating. They were, however, influenced by their cultural experience to think in terms of cultural purity. They are so homogeneous that they have developed a strong feeling that they came from the mainland as a result of a migration. One of the arguments I made in my book on migrations is that this is not so; their archeological evidence indicates that it's a false problem, as it is in many parts of the West Indies [Rouse 1986]. The Japanese originated in an interaction sphere which is based on the strait between Korea and Japan. You can't say whether it's Japanese or Korean, because it's both.

PS: Is it fair to say that from your West Indian experience you've always had a particular interest in archipelago situations around the world?

IR: Yes, that's true. More important, I've always worked in peripheries. The places you've talked about, where I've been, are all peripheries. Japan is the same way—it has a similar geographical position relative to China in the Far East—and South Africa is peripheral to the rest of the African continent. The fact that I happened to go to those three places reinforced my interest in peripheries. I would say that you have two kinds of archeologists: centrists and peripherists. The centrists are people who study the centers of civilization. One of the reasons that Childe interested me was that he too had an interest in peripheries—that is, he was looking at Europe as being peripheral to the Near East. A problem with studies of evolution is that they tend to concentrate on the centers. One way of teaching world archeology is to teach it in terms of stages, which are based upon the developments that took place in the centers. Peripheries are thought not to be important because they didn't really contribute to the rise of civilization.

After my sabbatical in England, I gradually developed a new approach of my own, based upon the study of peoples and their assemblages. What happened was that I started out to write a book on world prehistory. The first chapter was to be devoted to method, which is the way I taught my course. That expanded into a book in itself, which eventually became *Introduction to Prehistory* [Rouse 1972].

PS: Did you want to say anything about the movements of ideas vs. peoples vs. objects?

IR: I found it essential in my courses on world archeology to define the terms that I was going to use beforehand. When I was writing *Introduction to Prehistory* I found that I was using terms differently in different parts of the book. I finally ended up writing a card for each term, which formed a basis for a glossary at the end of the book. At the time, I thought this was the best part of the book; it would tell you how I was using each of these terms. That's the problem I think you're getting at in this question. Within the dimension of space there are several different kinds of forms that may pass from one place to another. One would be objects that move by trade. A second would be ideas diffusing through the spread of modes from one people to another. The third would be peoples, and their movement can occur on two levels: one is social movement, or immigation, and the other is population movement.

PS: Population movement would be a whole group of people—everybody?

IR: In economics a distinction is made between microeconomics and macroeconomics. Microeconomics is studying the small intimate processes; macroeconomics is concerned with broad-scale changes. That's the difference here. Micromovement (social movement) would be the movement of individuals or families from one population to another, which takes place almost instantaneously compared with the total perspective. Macromovement (population movement) occurs gradually over a long period of time, in the course of which you actually get a replacement of one population by another. I distinguish that from immigration. Included in the category of social movement are the seasonal migrations studied by Binford and others. Precise definitions for different processes are developed in this manner. This has to do with process rather than pattern. Different processes take place on different analytical levels. Separate concepts must be developed for each level of interpretation.

The thing I'm proudest of, from a theoretical standpoint, in what I've done in the West Indies is establishing the idea of passage areas [Rouse 1982]. During the ceramic period—Saladoid and Ostionoid—people on either side of a passage were more closely related to each other culturally than they were with the rest of the islands on which they lived.

PS: One point that we haven't addressed directly is the relationship of migrations to culture change.

IR: I have some opinions on that subject. I've said in several publications that I think archeologists tend to read their own cultural experiences into the archeological remains [Rouse 1986:106]. That's particularly true in the West Indies, because the prehistoric population was completely wiped out in a few years and replaced by people coming in from Europe, Africa, and Asia. That meant a complete change in the human population and also in the culture and the language and a great deal more ethnic diversity than existed previously. New peoples came in from other parts of the world and established separate colonies in various places in the West Indies. Researchers often assume that cultural changes that occurred in prehistory were a result of similar factors. They immediately jump to the conclusion that there's been a repeopling of the West Indies (to use my term) in an attempt to explain culture change. This is a mistake because what we have been dealing with in the past few centuries is something unique in the history of the world, that is, the development of the ability to travel over such long distances, making it possible for people from diverse parts of the world to come together in a place like the West Indies. It's unreasonable to assume that these same conditions would have existed in prehistoric times.

PS: There were also dramatically different stages of technological development between the colonists from Europe coming here and the Native Americans who were here.

IR: Yes, that's another factor. In talking about population movement and differentiating it from social movement, we should note that population movement implies conflict. If one population is replaced by another, there is likely to be conflict over the land. That's really what's going on in Bosnia at the present time, and I'm sure it went on in the West Indies as well. As I read the record in the West Indies, this repeopling really happened only in Saladoid and modern times. At no other times do you find such sharp contrasts between cultures and the opportunity for one population to eradicate another population. It is difficult to identify population movement unless there is evidence of sharp contrasts between cultures, and those come mostly when the new people have come from a long distance away, where the culture is very different.

In this connection, I have cited a concept from Lévi-Strauss that he called "strong" and "weak" interaction [Lévi-Strauss 1971]. One of the characteristics of population movement is strong interaction, that is, conflict and a strong degree of difference between the two cultures, so that one is able to prevail and to eradicate the other. Those are the kinds of culture change that I would see as resulting from population movement, that is, migration of peoples. Changes that occur through immigration, such as what is developing in the United States now, produce cultural plurality—a series of peoples coming in and accommodating to each other but still retaining many of their previous traditions.

PS: I'm wondering if something similar might not have happened in the prehistoric context. Plural society, for instance, could change the complexion of, say, the early vs. late Saladoid cultures.

IR: As I read the record in the Caribbean, some cultures were more or less uniform and others were plural. That's one of the most fascinating aspects of archeology in the area. The case I mentioned earlier of plural Barrancoid/Saladoid culture in Trinidad during the Erin period is a good example of this. In the course of my own career, I began working in the Greater Antilles, which was relatively isolated and where there was no opportunity for the plurality of cultures to develop, and so I acquired a preconception that cultures, and ceramic styles within cultures, would be nonplural. I was greatly surprised when I traveled to Trinidad and discovered that that wasn't the case there. Now we have found plurality again in the case of the Cedrosan Saladoid cultures. There are essentially two ceramic heritages—a white-on-red painted heritage and a zoned incised cross-hatched heritage. I've used the term "ware" to express this plurality, which is a different use of the term from the one you were talking about—ware as a utensil or functional category. There's another way that changes can take place. Rather than getting a penetration, so to speak, of culture brought by an intrusive ethnic group into another culture, cultural traits may spread. The best example I know of that is in Japan, where because of isolation for so many years under the shoguns the Japanese developed a very strong local identity with a concomitant ethnic homogeneity. Then the Meiji restoration led to exposure to Western culture. But people didn't come in bringing their own culture. Instead, the traits themselves came in and were adopted by the Japanese and, most important, integrated into their culture. That's happened many other times. I've always seen a similarity between Great Britain and Japan in this respect, throughout history. They're both island groups on the peripheries of continents. They both have taken traits from the continents and reinterpreted them to conform to their own cultures. That's something very different from the intrusion of a culture, as is happening in the United States, to form a plural culture. One of the things that I have been most often criticized for in the West Indies is my emphasis on cultural contact and my belief that traits spread across cultural boundaries. For example, when I worked in Haiti, my first fieldwork actually, I found that you have first the Archaic cultures, with their flint blades and rather elaborate groundstone tools, followed by ceramic-age people coming in with pottery [Rouse 1947]. I pointed out that the distinctive forms of stone grinding and flint work present in the Archaic cultures persisted into later cultures. Everybody jumped on me for that. They said, "That's impossible." The same thing happened in Cuba. I noted that the shell gouges characteristic of the Archaic people there, the Redondoids, survived right up to historic times [Rouse 1942]. Again, Harrington [1921] said, "That's impossible. They're separate cultures; therefore, they couldn't exchange traits." I don't know why people have that attitude. I think, again, it may be ethnocentrism. They feel so strongly about the identity of their own cultures that they don't see any need to recognize intrusions or influences from other cultures.

PS: So in this case it would be influences from earlier times.

IR: Earlier times, yes, and more primitive peoples. The difference between centrism and peripheralism that I mentioned earlier is also involved. The assumption that everything comes out of the more advanced people in the centers, who bestow their bounty on the poor peoples in the peripheries. But remember, if our archeology is correct, there was an interface, a frontier, between the earlier and later peoples for hundreds of years. Traits could have been exchanged across that frontier.

PS: An interaction between two groups of people—that itself could produce culture change on both sides?

IR: Yes, that's what I meant to say. I have been impressed in this respect with the similarities between the West Indies and Japan. In the original formulation of Japanese archeology there was a Jōmon culture, considered to be very primitive and non-Japanese. I remember attending a meeting of Japan's Society of Archeology at one time. Not knowing Japanese, I couldn't understand what was going on; Suzuki would explain things to me every once in a while. At one point they were having a very strong argument. Somebody had had the temerity to suggest that Jōmon traits might have been adopted by the Japanese as they moved north into Japan—like the Saladoid/Ostionoid movement in the West Indies. That's always been a very sore point with Japanese archeologists. I didn't help it any in my book by pointing out that the direct ancestors of the Japanese were not foreigners from the mainland introducing more advanced cultures but arose in southern Japan from its Jōmon populations, which developed a new culture as a result of influences—rice agriculture in particular—from the mainland. By so doing, that local population became so strong that it was able to advance to the north, just as the Saladoid people did in the Caribbean area. In both cases, the peoples that moved north mixed with the previous populations and adopted many of their traits.

PS: You get blending.

IR: Yes.

PS: It does sound like genetics. People say that in a pejorative way, as Straus (1987) does in his review of your book—like ceramics mating and producing sherds. But to a certain extent it almost *is* that—although obviously ceramics don't reproduce.

IR: As I said earlier, each discipline has a different kind of data and has to use different concepts. But they're often comparable, and that's true in this case. Genes are the basis for studying biological change. Modes are the basis for studying ceramic change. The basis for studying language change is phonemes. They're all analogous, and in all three cases you're looking at similar processes. You exchange genes in a different way from exchanging modes or phonemes, but the effect is the same.

PS: What would you say are or were your principal contributions to archeology?

IR: That's a difficult question. I'm going to rephrase it to say what are the achievements in archeology of which I am most proud, the things that I value the most. I think my major contribution would be to chronology, which I started to study in my doctoral dissertation and have continued with an accumulation of a succession of chronological charts. In terms of the gathering of basic data I never really emphasized the excavation of sites. That didn't interest me all that much.

PS: This was in the 1930s? In the history of archeology that would be when chronological schemes were being developed.

IR: That's right. I think I've also made a major contribution in proposing the concept of mode and in using it to study the kind of ceramics that you get in the West Indies. I wouldn't recommend it everywhere, because it depends on the way pottery was made. It certainly has worked for me and for the interests that I have on higher levels of interpretation. I would say also that I have been more interested in pattern than in process. I think it would generally be recognized that I have contributed to the clarification of what a type and a mode are and how they can be used in archeology.

PS: Well, you are known for chronology building.

IR: I combined two levels of chronology. On the one hand, I set up sequences of periods or styles, which were based upon stratigraphy. On the other hand, I checked these sequences by studying distributions of individual modes. I did this in my doctoral dissertation. The two lines of evidence strengthened the resulting chronology. Then, of course, when radiocarbon dates came along they confirmed our chronology. There are still gaps in it, which are gradually being filled. In this respect, the Caribbean is somewhat comparable to the Southwest. When I first went into archeology the only place you had a really reliable chronology was the Southwest, because of their tree-ring dating, among other procedures. That was the model everybody followed. I think that the West Indies now deserves that same appreciation, within at least lowland South America, because it's the one place in which we've really worked out a detailed and reliable chronology. Which isn't to say that it doesn't require fine-tuning—I think it does. But the main outlines are correct, and most people accept them.

Anyway, I'd say that chronology building and modal analysis are the things that occupied me in the first half of my professional career. There was a break when I took my sabbatical in England. Influences from Childe, Clarke, and others were important in shaping the work that I did during the second half of my career. I'm still criticized for my earlier work—for being nothing but a chronologist or a student of the distribution of traits. But I've gone on to other things since then.

I would say that the greatest contribution in the second half of my career was the development of the concept of "peoples" [Rouse 1965, 1989] and using it as a means of linking individual social groups with the culture that they possessed. The greatest weakness with the New Archeology, as I see it, is that they start with society and never tie it back into culture to any great extent. The missing link is the concept of "people." I try to use it in that way, particularly in my latest books. My other achievement concerns the classification of cultures. In the West Indies we still have not really figured out how to distinguish peoples systematically enough as population units with definite limits in time and space. My contribution has come in how peoples are classified once they have been established. There I was strongly influenced by Gary Vescelius [1980] and have adopted his idea of a two-level classification. The most important thing I've done is to realize that these groups are comparable to language groups—speech communities—and to physical anthropological groups—races—as already discussed. These three different kinds of groups shouldn't be mixed, because they're based upon different criteria.

I'm relatively timid about moving from pattern to process. I'm strong on setting up patterns, such as types and periods. On the level of process, my main contribution is in the question of migrations and population movement. I remember, however, one person who was asked to read and comment on my migrations book pointed out that I could have given a clearer idea of the difference between population movement and what I call social movement. I think that's true, but it's a very hard thing to do.

PS: You said that classification often has become an end in itself—that people frequently lose sight of the larger picture, of why it is they're classifying.

IR: Classification is sometimes applied to situations and problems to which it is not applicable.

PS: Today, in this era of computers, it's very easy to enter numbers into the computer and then to manipulate numbers in all sorts of elaborate ways. You end up often having fancy statistical analyses but losing sight of larger issues. Your modal analysis, I think, is often an inspiring formative study that people frequently refer to as a way to systematize.

IR: I regret the fact that classification in general and modal classification in particular are used for solving problems that they're not designed to solve. Archeologists often fail to recognize what ought to be a guiding principle, that in moving from one analytical level to another new concepts must be developed which are suitable to the new level of inference.

One thing that you haven't asked me is what I thought were my failures. That's also appropriate for someone of my age to mention. The greatest failure is my inability to come to grips with the problem of teaching world archeology. As more and more archeological data accumulated and as approaches to archeological study became more diverse, it became more difficult for me to cover the whole world. I regret now that I didn't write a summary of world archeology when I was younger, when it was still possible to do it with the methods of which I was then aware. I feel very strongly that world archeology is one of the greatest contributions that archeology can make to our intellectual life. It's perhaps the best illustration of diversity, the best means of counteracting the ethnocentrism that we all have. We need to instill in our students an understanding that the United States is not the center of the world and that our culture is not the best in the world, one which all other cultures ought to be emulating. One way of doing that is to teach world archeology, or world prehistory, if you will, in which you follow the development of human culture throughout the world, thereby demonstrating that groups of people in disparate parts of the world have been able to reach similar levels of development. Our route is no better than anybody else's route. I regret very much that I haven't been able to winnow out the data to the extent that I would have been able to write a book doing that.

PS: You have two volumes of unpublished text started there on your bookshelf.

IR: That's the problem. It was too big. My intention was to come back to it in my old age and cut it in half.

PS: To summarize it?

IR: Distill it. Actually, it's a methodological problem. What is needed here is a synthesis. I think the systematic approach in terms of classification is the key. I've succeeded in doing it in the West Indies in my most recent book [Rouse 1992]. I think if I had had time I could probably have done it for the world as a whole. The key to this problem is classification of peoples and cultures. I collaborated with José Arrom in writing an article on Taino art for the Columbus exhibit at the National Gallery of Art [Rouse and Arrom 1992]. We agreed that I should have only a paragraph in it to discuss the prehistory of the West Indies. I could see his reaction; he was thinking, Rouse has been writing long articles on this subject; how is he going to compress it into a single paragraph? Well, I actually compressed it into a footnote; I could do that because of classification. If you approach the problem in terms of cultural series (there are six major series in the West Indies), it's very easy to produce a capsule summary of what is known about West Indian prehistory in a footnote. I could have done that for the rest of the world as well if I had had time.

PS: Well, then, you don't need to write a book. You can write a condensed paper.

IR: Actually, one of the last things Kroeber published was a roster of peoples and cultures throughout the world [Kroeber 1962]. He was working on it at the time of his death. The Wenner-Gren Foundation was intrigued by it, so they published it, even though it was just an outline. In its preface he said that archeologists were focusing on society and were overlooking the concept of culture. Archeologists fail to realize that our audience wants us also to talk about culture. We have a lot to offer to the resolution of problems dealing with cultural contact and cultural conflict, but we don't because relatively few of us are studying it. I would have liked to expand upon Kroeber's outline, and I may end up by doing so. There are two levels on which it can be done. It can be done in outline and then letting a student go on, insofar as he wants to, if he's interested in a particular unit within the outline and learning more about it. This is what I did for the course that I taught in South Africa on world archeology. There I did it a little differently by simply picking a few key sequences in different parts of the world. But I would rather do it, if possible, from a genetic point of view, that is, by tracing the sequence of development or evolution throughout the world. That's where archeology can make its greatest contribution. Our contribution is time perspective and changes that take place through time. My colleagues disparage me because I have this interest in peoples, but I'm still convinced that it's the future of archeology—peoples and cultures. That's the one thing that differentiates archeology in particular and anthropology in general from other social sciences like sociology. It's where we have something to offer, and we're not taking advantage of it. Well, that's my sermon for the day.

One example of processual research that I have been concerned with deals with the difference between what I have called procedural and conceptual modes. A conceptual mode is a pattern that may be picked out of the artifact by analyzing and classifying its features; it's in the appearance of the artifact. A procedural mode is the behavior that may be inferred from that pattern. For example, the formal characteristics of incised designs may be analyzed, or inferences may be derived that deal with the procedures by which the designs were produced. So one has either the incised design itself or the manner in which the procedure of incision took place as different kinds of modes. You raised a third possibility, which I would call cognitive modes—that is, from the pattern one may infer the artisan's thoughts regarding the significance of the pattern.

PS: That may lead into symbolism.

IR: Yes, symbolic meaning. That's something I've always shied away from, but it's becoming more popular, and I think it's important.

PS: Would you say, then, that there is a hierarchy of modes, in a sense?

IR: The problem here is the term "conceptual," because I don't think that's a good one. It certainly doesn't distinguish them from what I am now calling cognitive modes. I don't know what a good term would be.

PS: The point I'm trying to understand is that regardless of the kinds of modes you're dealing with here, you're still looking at the same features of the artifacts.

IR: Yes, that's right. In terms of procedure one starts by analyzing out constituent features, then classifying the features, and finally abstracting the attributes that are definitive of each class. It's your basic unit.

PS: So one could look at, say, design elements on a ceramic pot in terms of manufacture—how the artisan formed the designs—in terms of concepts or thoughts.

IR: What I really mean here is in terms of appearance, rather than the artisan's thoughts about it.

PS: Yet you could also look at those same design elements in terms of symbolic meaning.

IR: Yes, that's right. There are two ways of getting at symbolic meaning. One would be, as it's commonly done in the West Indies, to employ ethnohistorical evidence. The other would be to utilize cross-cultural evidence and to look at designs throughout the world and the major trends—what they mean. That has been referred to as direct vs. indirect or historical vs. general analogy. I think they are going to be emphasized in the future much more than in the past. I'm unhappy about using this approach because you can never be sure, but it's something that interests people, and if our goal is to get at all the information we can about the past we ought to be trying to do this type of research.

References Cited

BINFORD, LEWIS R. 1968. "Archeological perspectives," in *New perspectives in archeology.* Edited by Sally R. Binford and Lewis R. Binford, pp. 5–32. Chicago: Aldine.

———. 1973. "Interassemblage variability: The Mousterian and the 'functional' argument," in *The explanation of culture change: Models in prehistory.* Edited by Colin Renfrew, pp. 227–54. Pittsburgh: University of Pittsburgh Press.

———. 1977. "General introduction," in *For theory building in archeology: Essays on faunal remains, aquatic resources, spatial analysis, and systemic modeling.* Edited by Lewis R. Binford, pp. 1–10. New York: Academic Press.

BINFORD, SALLY R., AND LEWIS R. BINFORD. Editors. 1968. *New perspectives in archeology.* Chicago: Aldine.

BORDES, FRANÇOIS. 1973. "On the chronology and contemporaneity of different Palaeolithic cultures in France," in *The explanation of culture change: Models in prehistory.* Edited by Colin Renfrew, pp. 217–26. Pittsburgh: University of Pittsburgh Press.

CHASE-DUNN, CHRISTOPHER, AND THOMAS D. HALL. Editors. 1991. *Core/periphery relations in precapitalist worlds.* Boulder: Westview Press.

CHILDE, V. GORDON. 1951. *Man makes himself.* New York: New American Library.

EVANS, CLIFFORD, AND BETTY J. MEGGERS. 1960. *Archaeological investigations in British Guiana.* Bureau of American Ethnology Bulletin 177.

FORD, JAMES A. 1954. The type concept revisited. *American Anthropologist* 556:42–54.
GOGGIN, JOHN M. 1946. Ceramic stratigraphy at Upper Matecumbe Key, Florida. Master's thesis, Department of Anthropology, Yale University, New Haven, Conn.
———. 1947. A preliminary definition of archaeological areas and periods in Florida. *American Antiquity* 13:114–27.
———. 1948. *Culture and geography in Florida prehistory.* Ph.D. diss., Yale University, New Haven, Conn.
GOWER, CHARLOTTE. 1927. *The northern and southern affiliations of Antillean culture.* Memoirs of the American Anthropological Association 35.
GRIFFIN, JAMES B. Editor. 1952. *Archaeology of Eastern North America.* Chicago: University of Chicago Press.
HARRINGTON, M. R. 1921. *Cuba before Columbus.* New York: Museum of the American Indian.
KROEBER, ALFRED L. 1962. *A roster of civilizations and culture.* Viking Fund Publications in Anthropology 33.
LÉVI-STRAUSS, CLAUDE. 1971. El tiempo del mito. *Plural: Crítica y Literatura* 1:1–4.
RITCHIE, WILLIAM A. 1938. *Certain recently explored New York mounds and their probable relation to the Hopewell culture.* Rochester Museum of Arts and Sciences Research Records 4.
———. 1961. *A typology and nomenclature for New York projectile points.* New York State Museum and Science Service Bulletin 384.
ROUSE, IRVING. 1936. Review of: *Antiquities of the New England Indians,* by C. C. Willoughby (Cambridge: Peabody Museum of American Archaeology and Ethnology, 1935) *Bulletin of the Archeological Society of Connecticut* 3:91–96.
———. 1938. Contributions to the prehistory of the Ft. Liberté Region, Haiti. Ph.D. diss., Yale University, New Haven, Conn.
———. 1939. *Prehistory in Haiti: A study in method.* Yale University Publications in Anthropology 21.
———. 1941. *Culture of the Ft. Liberté Region, Haiti.* Yale University Publications in Anthropology 24.
———. 1942. *Archeology of the Maniabón Hills, Cuba.* Yale University Publications in Anthropology 26.
———. 1947. Ciboney artifacts from Ile à Vache. *Bulletin du Bureau d'Ethnologie de la République d'Haiti,* 2d series, 2-3:16–21, 61–66.
———. 1951. *A survey of Indian River Archeology, Florida.* Yale University Publications in Anthropology 44.
———. 1953. The circum-Caribbean theory: An archeological test. *American Anthropologist* 55:188–200.
———. 1958. "Similarities between the Southeast and the West Indies," in *Florida anthropology.* Edited by Charles H. Fairbanks, pp. 3–14. Florida Anthropological Society Publication 5.
———. 1965. The place of "peoples" in prehistoric research. *Journal of the Royal Anthropological Institute* 95:1–15.
———. 1972. *Introduction to prehistory: A systematic approach.* New York: McGraw-Hill.
———. 1982. Ceramic and religious development in the Greater Antilles. *Journal of New World Archaeology* 5(2):45–55.
———. 1986. *Migrations in prehistory: Inferring population movement from cultural remains.* New Haven: Yale University Press.
———. 1989. "Peoples and cultures of the Saladoid frontier in the Greater Antilles," in *Early ceramic population lifeways and adaptive strategies in the Caribbean.* Edited by Peter E. Siegel, pp. 383–403. British Archaeological Reports International Series 506.
———. 1992. *The Tainos: Rise and decline of the people who greeted Columbus.* New Haven: Yale University Press.
ROUSE, IRVING, AND JOSÉ J. ARROM. 1992. "The Taínos: Principal inhabitants of Columbus' Indies," in *Circa 1492: Art in the age of exploration.* Edited by J. A. Levinson, pp. 509–13, 575–78. Washington, D.C.: National Gallery of Art.
ROUSE, IRVING, AND JOSÉ M. CRUXENT. 1963. *Venezuelan archaeology.* New Haven: Yale University Press.
SMITH, M. G. 1965. *The plural society in the British West Indies.* Berkeley: University of California Press.
STRAUS, LAWRENCE GUY. 1987. Review of: *Migrations in prehistory: Inferring population movement from cultural remains,* by Irving Rouse (New Haven: Yale University Press, 1986). *Journal of Anthropological Research* 43:381–84.
SUED-BADILLO, JALIL. 1992. Facing up to Caribbean history. *American Antiquity* 57:599–607.
VESCELIUS, GARY S. 1980. A cultural taxonomy for West Indian archaeology. *Journal of the Virgin Islands Archaeological Society* 10:36–39.
WALKER, JEFF. 1993. *Stone collars, elbow stones and three-pointers, and the nature of Taino ritual and myth.* Ann Arbor: University Microfilms.
WILLOUGHBY, CHARLES C. 1935. *Antiquities of the New England Indians, with notes on the ancient cultures of adjacent territory.* Cambridge: Peabody Museum of American Archaeology and Ethnology, Harvard University.

Physical Anthropological Aspects of the Mesolithic-Neolithic Transition in the Iberian Peninsula

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Genetic studies based on synthetic gene maps have recently provided support to the demic-expansion model (Ammerman and Cavalli-Sforza 1973) of the Mesolithic-Neolithic transition in the Iberian Peninsula (Bertranpetit and Cavalli-Sforza 1991, Calafell and Bertranpetit 1994*a*). Population growth associated with the adoption of the Neolithic economy presumably homogenized the genetic scenario of the Mesolithic, confining a small human group, the Basques, on the western edge of the Pyrenees. Accordingly, this population is one of the main sources of genetic variation in western Europe (Calafell and Bertranpetit 1994*b*).

Skeletal materials from these crucial periods are still scarce in the Iberian Peninsula, and some of them have only been partially published in minor journals. Fortunately, Muge, an exceptional sample from the Mesolithic (Cabeço da Arruda and Moita do Sebastiao at the mouth of the Tagus River [Ferreira 1994]) is available and has been exhaustively studied (Ferembach 1974). Recently, another relatively large sample (comprising 15 individuals), dated between 7,570 and 7,640 b.p., has been excavated in Oliva, El Collado, Valencia (Pérez-Pérez et al. 1995). Although analysis of skeletal remains, especially those of pre-Neolithic populations, may furnish valuable data for the debate about the Neolithic expansion, no attempt has yet been made to examine these samples as a whole.

We have compiled the physical anthropological information available on Iberian populations from these pe-