Title
An Interview with Sherwood Washburn

Journal
Current Anthropology, 33(4)

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Publication Date
1992

Peer reviewed

Reports

An Interview with Sherwood Washburn[1]

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ID: We thought we might start with your precollege years, and I urge you to go back to as early a period as you want to.

SW: Of course, I was always interested in zoology—mammal skeletons, birds, and behavior. I kept a great horned owl for some years when I was young, as well as crows and hawks.

ID: Were you a Boy Scout? How did you first become involved with bird and mammal skeletons?

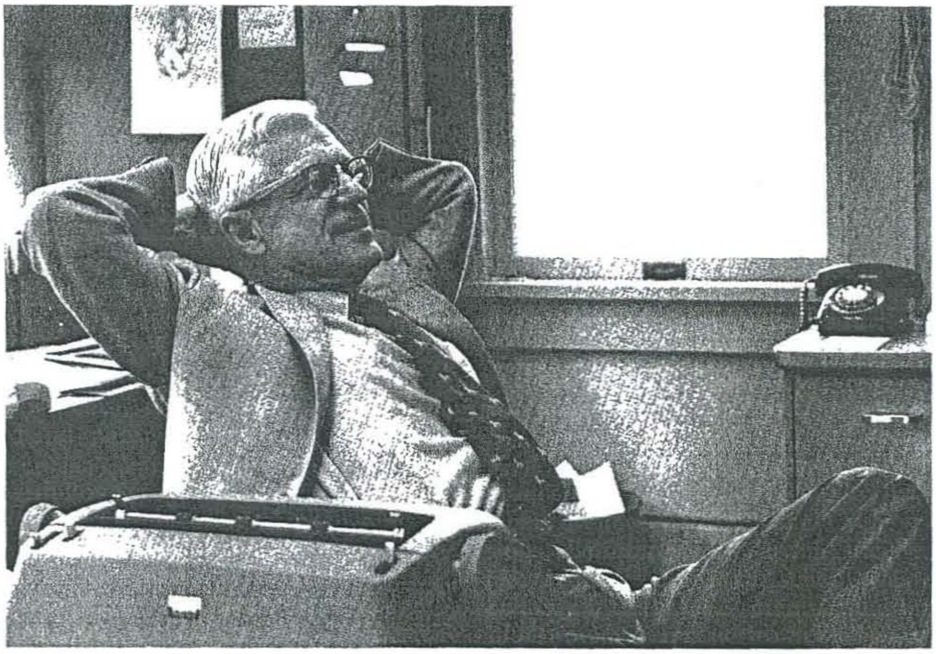
SW: I was bored with Boy Scouts. I think this is because my family had so many interesting things going on which included what the Scouts were doing and were much more fun. In retrospect, I think that my family was amazingly generous in what they allowed me to do.

The first skeleton I had anything to do with was a porcupine skeleton which my brother and I found in the Catskill Mountains. It had dried out and become just bones and a few quills. We gave it to the Harvard Museum of Comparative Zoology. Dr. Henshaw was the director at the time. In retrospect, I believe my father must have called him before we arrived, because when we came in with this absolutely wretched bunch of broken porcupine bones, he welcomed us as if we were giving Harvard a great, valuable specimen. He treated us like adults even though my brother and I were respectively about eight and six at the time. This experience of respect from an admired adult was very important to me.

ID: Where did you go to school at this time?

SW: I went to Buckingham School, which at that time was coeducational, in contrast to Belmont Hill School, where I subsequently spent a year with fewer distractions! Belmont Hill was an excellent school. Heber Howe, the director of the school, was very helpful to my budding interest as a scientist and offered real support like providing me with guinea pigs with which to do elementary genetics experiments when he found out

1. 0011-3204/92/3304-0003$1.00. Alice Davies and Anne Sauter assisted in the preparation of the manuscript.



Sherwood Washburn.

that I was interested in this area of research. In many ways I would have benefited from staying there instead of "advancing" to Groton, which my family thought would serve my future career interests better than Belmont Hill. I could also have continued working for the Harvard Museum of Comparative Zoology, which I couldn't do from Groton.

One blessing was that they allowed me to have climbing irons so that I could climb up into trees and get into the nests of the crows and red-shouldered hawks as well as great horned owls. At different times, I had three great horned owls. One owl in particular had a four-foot wingspread and would come down and land on my wrist very dramatically, and he landed, I've always liked to believe, full of good intent. But in order to prevent his enormous claws from going right through me, I wore a saber glove covered with aluminum and that in turn covered with tape. In spite of all that protection there was a dent in the aluminum where those claws had landed. The crows that we had at that time were ravenous, and they loved soup. We would steal soup from the dining room by using a dropper and put it in a bottle down between our legs, and at the end of the meal we would go out with the bottle full of soup and take it down to the crows.


With great horned owl.

Those boyhood experiments fueled my curiosity and love for the observation of animals.

We built a museum at the school. There was a small collection of birds there, and since I had worked in the Museum of Comparative Zoology during vacations and knew the great curator of birds, Ludlow Griscom, I asked him if we could have some of the stuffed birds that Harvard was discarding for the Groton museum. He said, "Fine. You pick out any ones you like." Being a young upstart who knew something about the intrinsic value of collecting rare birds, I specifically asked for the California condor, but Griscom replied all too quickly, "No. You cannot have that one!" Dad drove us to Groton with the back of the car just filled with stuffed birds, about a hundred of them, and we put them all on exhibition at Groton. My family was always most enthusiastic in supporting the projects I was interested in that developed from my schoolwork.

Groton's cafeteria fed us reasonably well, but the same kind of food year after year is inevitably dull. Dad and Mother used to drive up to school once in a while, and we would go to the Groton Inn, which was noted for fine meals and especially for duck. Usually Brad and I would bring along a classmate, and we would be remembering the last duck we had eaten there and wondering if the current dinner could match the last. What a glorious change from the school food! Brad and I were at Groton for a combined period of eight years, so Mother and Dad came to know a lot of our friends and a great deal about the school.

It was in the third and fourth years at Groton that it became clear that my career there really had ended with the realization that I would never grow big enough to play football or other major sports. In my last year playing football I broke my right wrist twice and my left wrist once. (However, I did quite well in wrestling at Harvard—won six out of seven matches—and only gave up the sport once I recognized that it took too much time away from my studies.) In retrospect, it is obvious that turning to the museum's live crows, hawks, and owls was simply a way to create a life compatible with being the smallest boy in the school for six years! At the time, no one helped me to understand what now seems so clear to me. After the broken limbs, my Mother sent me to Bermuda "to rest." It was wonderful!

Years later when I had my own children, Dad asked me if we had entered our sons on the list for entrance to Groton (which it was customary to do when parents first recognized that they had produced future male heirs to the family name), and I responded that we had not. It was clear to me that what I had learned from Groton was not furthered by the social expectations that go along with an assembly of elect students. To give you an example of how fine an administrator Heber Howe was at Belmont Hill School, when I was on the debate team for Groton, we won a match against Belmont Hill, and Howe came to me and said, "The reason that Groton won was that Belmont Hill had three good team members and Groton had one!" He was a great encouragement to me.

ID: Why don't you tell us more about your father?

SW: Dad was Henry Bradford Washburn, and for many years he was dean of the Episcopal Theological School in Cambridge. Before that, he'd been professor of church history, and before that he was at a church in Worcester, Massachusetts, from where his part of the family had come. He was a very remarkable person in that he was always encouraging his sons to do very different sorts of things keyed to our own individual personalities and interests. Mountain climbing was in a sense a rough business, and he encouraged my brother, Brad, to pursue it. He helped me to find a very good biological supply house in Paris, and I went there each of those summers when we were going through France and got their catalogs and looked at the various things they offered. It was marvelous, like a museum. This was at a time when comparative anatomy was still the most important subject in biology, very different from today. They had beautiful wax casts that were painted and were way ahead of anything that you could buy in the United States. In particular there was a grisly set of casts of cancer of the liver which I've never forgotten!

Dad was a very good Protestant Episcopal-type speaker, and a lot of his sermons were more church history than sermons in the usual sense of that word. And I think that undoubtedly affected my brother and me. We both had successful ways of communicating which I think came directly from the family background. Neither of us ever became interested in religion, and I think this was a great disappointment to Dad and to Mother. My sister has told me that they were frightfully worried when my brother and I went into geography and anthropology on the grounds that this was the Depression and there was no way possible to get a good job or even earn a living in either of these fields.

My brother was "Junior," Henry Bradford Washburn, Jr. He was involved with mountain climbing from the time he was around twelve to fifteen and took a very active part in the Harvard Institute of Geographic Exploration until that program was abandoned. Then Tom Barbour nominated him to be head of the Boston Museum of Science when that was being reorganized, and Brad developed a very effective board and ended up by raising millions of dollars for that museum by the time he retired. He was always a very strong person, and the family went to Chamonix for three summers and spent most of the time climbing when nobody else in the family was interested in it. And I think that experience certainly exerted a powerful influence on me. My mother allowed him to do whatever he wanted to do. He climbed Mont Blanc and the Matterhorn, and Mother would patiently sit and watch him with a telescope through the terrors of avalanches, storms, slides. Brad also produced a film on Grepon, one of the mountains of Chamonix, and it played continuously for about 30 years.

On the other hand, Mother was convinced that I was a sickly child and that I needed to be taken care of, have medical care, take pills, rest, and so on. In retrospect, I think this was unnecessary. However, her influence in steering me away from feats of physical prowess did lead me to more introspective, investigative challenges. I had also experienced three instances in succession where people had come to great harm while climbing, and so I was faced with death quite directly when I was young. I had seen three people who had frozen to death on the slopes and the remains of a climber who had fallen from a great height on Grepon. These two events were followed by being the very last person in a line when an avalanche broke off the part of the mountain on which we had been standing.

ID: Was there any question in anybody's mind that you would go to Harvard?

SW: Absolutely not. I didn't apply anywhere else, and there was no question at all about going to Harvard. My brother had gone there, and so had Dad and his brothers. Mother's family were mostly graduates from Yale. Each year on the day of the Harvard-Yale football game, Mother always had a buffet for everyone, and I saw more family then than at any other time of the year. There were so many of us.

ID: So, how long were you at Harvard, man and boy?

SW: Psychologically, I was at Harvard for many years. I was working at their Museum of Comparative Zoology when I was in high school. At first, I worked during the vacations without pay, and then finally Dr. Barbour had me come into his office and said he was going to pay me 25 cents an hour, and I thought that was great. The result was that I came to know the collections in the MCZ very well before I was even admitted to college, and when E. A. Hooton was lecturing on primates I recognized that I was more familiar with their collections than he was. I can see now, looking back, that working there gave me a great freedom which I would not have had if I'd just been working from the book. If Hooton said something was so-and-so, I would go back to the museum and say, "Is it?"—and a lot of the time I found my argument won out.

At the beginning of college I took Professor Alfred Marston Tozzer's course because he was a great friend of my family. I had never heard of anthropology, and his class was very influential in my training. It had the advantage of incorporating biology and social science, history and functional theory, all in one, and there were issues which were being highly debated in the anthropology department at that time. Tozzer would say, for example, that he wished some of the functionalists would have to repeat their functional studies to find out whether two functional studies done in the same place would produce the same results. This made the class think. It was entirely different from saying, "Malinowski said so-and-so." On the other hand, Tozzer didn't believe in functional studies. He was essentially a Boasian, and so was Roland Dixon.

I found out later on that Dixon and Tozzer had fought and wouldn't even speak to each other. If Tozzer wanted to say what he felt about something that Dixon was doing, he told Hooton, and if Dixon wanted to blow his top about something Tozzer was doing, he told Hooton. Without realizing what they were doing, these key senior people of the department put Hooton in a very, very strong intellectual and executive position in the running of the department. He was the fulcrum around which the department functioned.

Hooton had tea every afternoon. He would come down to the office, give his courses, do the mail, and see whatever students needed him on a formal basis, and then he would go and play golf. And he played golf every decent day. About five, he would be available for tea, and his secretary would tell the graduate students, "Dr. Hooton *expects* you to come to tea." I think a lot of his influence came from those teas.

ID: You were majoring in anthropology from the beginning of your studies?

SW: Yes. Lloyd Warner was at Harvard then, and I was assigned to him as a tutee. He talked the whole time during the meeting with the tutee, walking up and down and talking steadily, and at the end of the fall semester he said, "Washburn, I'm having to reduce the number of my tutees. You're not interested in archaeology, are you?" I said, "Yes, sir, I'm *very* interested in archaeology," although I had never thought of it before. And he said, "Then I'll have to shift you to Walter Cline." Cline was the opposite extreme from Warner. He had no interest in or knowledge of what was going on in social anthropology. He was a good old-fashioned ethnologist and a very nice guy.

The best teacher I had was Lauriston Ward. I learned more from him in terms of what to do as a scholar than from anybody else in the group. He enjoyed archaeology, and he wanted his students to see what they would have to do to get ahead in the field. He gave all of us monographs to analyze, and we had to be able to present a clear picture of why the monograph was useful or not useful and put it into some kind of broader context. And I think that was *excellent* teaching. Yet he was never on the Harvard payroll, and I don't think he ever had a formal position in the department.

ID: Did you take four years to do the degree?

SW: I took four years. Having dropped out for a while, I was short one-half course to get the degree. I took one summer course to make it up, but the administration had not recorded it properly, and so I would probably not have graduated on time. Dad found out about this, and he went around and saw the university secretary and said, "There's been a mistake, and my son in fact has taken all the required courses." So, instead of being delayed for a year, I received a summa cum laude.

As a graduate student at Harvard, I never took an anthropology course. I did audit some anthropology, and I think that's very worthwhile, of course, but it seems to me that after pursuing an undergraduate major you shouldn't have to go on taking more courses. You ought to go on having intellectual experiences of a very different kind than you can have within a structured course. I took comparative anatomy and vertebrate paleontology with A. S. Romer. Those were great courses, and I owe an enormous amount of gratitude to Romer. He was a very interesting man, and by taking comparative anatomy and vertebrate palaeontology with him, I had the advantage of the overlap of the two fields of knowledge.

At the end of the first graduate year, I was invited to go to Thailand with the Coolidge Asiatic Primate Expedition, and because it didn't start until January of the following year, I went to Michigan and did human anatomy with W. T. Dempster and then to Oxford, where W. E. LeGros Clark allowed me to enroll as a special student in his human anatomy class. LeGros loved to go out and walk on weekends, and I took, I suppose, four or five walks with him, and he would chat along about politics or anatomy or fossils or the brain. Then we would go to his house and have tea, and it was very, very pleasant. At that time, I was taking care of some of [Solly] Zuckerman's monkeys. He offered to give me the cadavers to work on when he had finished with various monkey experiments. This would have been a delightful benefit for me, because they would have been fresh monkeys with joints that worked and traditionally studies were done on embalmed specimens that were no longer flexible. Unfortunately, I didn't have the time to pursue those studies then.

ID: What were the course requirements for the Ph.D.? What sort of order did you do things in?

SW: At that time students did a set number of courses and then they were examined by the whole department, and that was a tough exam. Literally the *whole* department sat at one long table with its mentor at the head and took turns asking questions.

ID: And what was your dissertation?

SW: The dissertation was on the skeletal proportions of adult langurs and macaques [1942], and those had been collected on the Asiatic Primate Expedition. A. H. Schultz was very kind and very helpful to me, and he said he would do the gibbons, orangs, and proboscis monkeys and I could have for my dissertation all the lesser monkeys, so to speak, which he obviously was not going to have time to do. He was very nice about it and could easily have taken more of the material if he had wanted to.

ID: Did you first meet Schultz on that expedition?

SW: I must have met him at an anthropology meeting before that, but I certainly had not had any substantial conversation with him. The expedition was Harold Coolidge's idea, and he had invited Schultz, who was a primate morphologist, C. R. Carpenter for primate behavior, Gus Griswold to do general collecting that included birds for the MCZ, an artist, also named Coolidge (Jack), and Harold Coolidge, working with taxonomy. There was someone named Wiley who was going to do big-game shooting. I had a traveling fellowship, and he invited me to go on the trip as the technician for Schultz and Carpenter.

At first, we made the great mistake of doing the collecting and the observations from the same camp. Everyone would recognize that as a disaster now. I had scouted out a location quite a few miles away from the main camp that was a wonderful place for observation, and Carpenter went up there and was very enthusiastic about it. I spent three or four weeks there helping him, but it was just the wrong time to expect me to shift gears from anatomy to behavior, and so after a while I went back to work with Schultz. And the reason Carpenter could do his 1940 monograph in such a very short period of time, less than three months, was that it was the end of the dry season and most of the leaves had fallen off the trees and one could see everything, including a great many gibbons in male and female groups with their young. Any of the common monkeys would have found these circumstances disastrous, but the gibbons were just perfect. Carpenter asked me to shoot some gibbons for him, which just stunned me at first. He had the notion of doing everything in the expedition himself, taxonomy, behavior, and everything else.

ID: Most people doing primate behavior today would be absolutely appalled by some of us going out and shooting primates. What was your attitude toward that behavior then, and what is it now?

SW: When I was working for Carpenter, I could see the animals in the trees doing all the things that involved use of their joints which I'd just manipulated in the laboratory. Taking animals in the field was an excellent way of getting a detailed understanding of what the animals were doing and how they were doing it. I had been quite interested in joints and how they worked, and one of the major problems of primate taxonomy at that time was the limited availability of dissection material. Most of it was juvenile and embalmed. So, when Schultz was doing these gibbons, they would be skinned and he would measure them and then turn them over to me, and I would do the dissections, and what I learned was irreplaceable. Those early studies have now been done and we in the field of primate study should no longer need to take the lives of primates. It was Dempster who first showed me how one relates the skeleton to the live creature, and I think that it takes working with the fresh material to see those relationships at their best. I think you are right that people would be upset about shooting the animals, but it was critical information that we gained that could not have been gained any other way at that time.

If you take the origin of speech, for example, and look at what has recently been written about the relationship of speech to the sphenoid bone at the base of the skull, it's apparent that the people doing the research don't know what they are writing about. Monkey noises and human language are radically different things, and they've got them all confused. They never mention the hyoid bone, which is the skeleton of this critical area. You can't move the larynx down without moving the hyoid down, given that the hyoid and the thyroid cartilages are attached.

I think after a certain point it is useful to be hard-nosed about the essentials and throw out the minor details and then, if you can get a solution that seems to work, go back and fill in the details. For example, you could take a nice atlas of human anatomy and find a little muscle on the side of the cheek, the risorius, and make the assumption, "Aha! This muscle raises the corner of the mouth!" Now, this is a very small muscle, and it's moving thick skin in response to emotional involvement. What are the chances that a muscle of this size could have any importance? If you dissect a couple of faces you will find that the muscle isn't there at all in a certain percentage of the dissections, and these people had just the same facial expressions as people who do have the risorius muscle. In other words, you're simply wasting your time looking at something so small before you study the large face muscles that control facial expression and contrast that information with the musculature of monkeys and apes. Again, for example, we've got three muscles under the eye orbit, and one could argue that they are responsible for explaining why humans have more detailed facial expression than the apes. However, the reason we have these three muscles is that there's not enough muscle to fill the area; in creatures that have a lot more muscle in this area, like the apes and chimps and even some people, there aren't three separate muscles, just one big one.

ID: Tell me more about what came out of the Asiatic Primate Expedition.

SW: One of the most-used mammal collections in the MCZ is the collection of gibbons from that expedition.

ID: How long were you gone in the field?

SW: I was gone for a year. And when I came back, there was a telegram from Tozzer saying that I had a teaching assistantship if I wanted it. Of course, I telegraphed back at once that I was delighted to take it.

ID: What did a teaching assistantship mean in those days?

SW: A lot of work! Seven sections, 140 students, 140 papers a week. I was so glad to have a job, because this was in the middle of the Depression. And all of the material from the expedition had to be macerated and cleaned, which was a massive job. There were at least 240 specimens, not all of them primates. We set up a macerating lab in the basement of the biology building, and at one point we had 90 skeletons being macerated at the same time. Then we would dry them out and fix them up for the museum. Gabriel Lasker aided me through all of the cleaning operations and explained the genetics of the primates to me, as well.

The following year we had this course planned on primates, and Hooton was to give the first part of it as well as the section on dentition, and then I was to come in and talk about the Asiatic Primate Expedition, and Harold Coolidge was coming in to lecture. Well, three or four days before it was to start, I went up to Hooton's for tea, and he turned and said, "Now, I haven't had time to prepare these first lectures. Would you give them?" Even more amazing, when we were through with the first ten days of the course he said, "I'm very busy. Will you just give the rest of the course?" I had to give the whole course on primates! Several things happened that I didn't anticipate at all. In the first place, Hooton really thought that the primates should be described in terms of nonadaptive characters and the families of primates would be done with details of the dentition, and so forth. But the course I presented was organized around studies of adaptation and locomotion, and if teeth were considered at all it was only as parts of functioning complexes. In the final lecture I summarized all of this, and that apparently was the first time that Hooton realized that we fundamentally disagreed about the nature of primate classification, and I've never seen anyone madder than he was. He got up and walked out of the room—which, for me, as a beginner, was a very traumatic experience indeed. And again, I wrote a review of Sheldon's *Varieties of Delinquent Youth* [1949] in which I suggested that this was the new phrenology, with the bumps of the buttocks replacing the bumps of the skull. It made him angry. But I believed then as I believe now that one must take a stand on the basic principles around which research is organized.

ID: Why was Hooton so taken with Sheldon's somatotyping?

SW: Kroeber asked me exactly that question, and I said, "I think he did it because he was bored! Perhaps he got sick and tired of those massive surveys taking the same kinds of measurements." And Kroeber said, "Yes, I got bored twice. Once I took up psychiatry, and once I took up Peruvian archaeology." I think that's just a classic answer! Now, I want to say in defense of Hooton that these were, I think, major confrontations at that time, and we didn't succeed in talking about them in rational ways. However, when Elsie V. Steedman of Hunter College wanted a letter of recommendation about me so that I could teach in the night school there when I was at a very low ebb financially, with two small kids and a low salary at the medical school, Hooton wrote her as strong and positive a letter as anyone could possibly have written.

ID: So you had these rows over intellectual issues with Hooton. Who were the people that you were interacting with or reading who were helping you formulate these views?

SW: A lot of that was done in the summer seminars in physical anthropology at the Wenner-Gren Foundation. We got a grant from the Foundation, and I taught in the medical school for summer salary, and others would teach at Columbia on 116th Street. We did various work so that we supported 10–12 people who were concerned with more or less the same problems. For example, the question of population dynamics and how one deals with it was of concern to everybody. Some of the people at the seminar were still operating under the old typological approach, and others had shifted to the population approach, including Gabe Lasker, who was one of the founders of a more modern physical anthropology. That is where the support was within the group. The purpose was first to get people to consider shifting over to a genetic model—not necessarily that they wouldn't use biometry but that they would use it in a way which is compatible with genetic theory. Secondly, an important aspect was to try to get all students in physical anthropology to take a substantial amount of human anatomy. Most of the people in physical anthropology in fact were M.D.'s and had been trained in the worst kind of 19th-century typological anatomy, which is almost impossible to use in a constructive and detailed way in evolutionary studies. We had human anatomy courses going in the Columbia summer school, and physical anthropologists like Bill Laughlin, Fred Thieme, and others all got their basic human anatomy at those summer sessions.

ID: So, from Harvard you went to Columbia. How long were you there?

SW: Eight years. I became convinced when I was doing graduate work at Harvard that it was impossible to settle the major theoretical problems in physical anthropology and human evolutionary studies by simply describing them and drawing conclusions from these descriptions—and this quite independently of whether or not one used measurements in the descriptions. This was contrary to everything I had been taught at that time. When I had the opportunity at the Columbia Medical School to do experiments, what I set out to do was to try and devise experiments which dealt specifically with problems which were traditional problems in human evolution and anthropology, so that the selection of the experiments was guided by traditional problems even though the methods were not. Now, in order to do this kind of experimental work, one needs marked animals, and the system of marking we used was injecting alizarine dye. The bones that were growing at the time of the injection and immediately afterwards were red, while the bones that grew after that were white. In addition, we did such things as transplanting a muscle or moving a muscle, and it was that combination of techniques which I found very helpful at the time and which are still used relatively infrequently, right down to the present.

This was when Paul Fejos was founding the Wenner-Gren Foundation, and he asked me to come down and describe the work I was doing. I went to his office with considerable trepidation, very hopeful of getting a grant and very uncertain about it, with the skull of a rat that had lost a whole parietal bone on day 1 and had gone on growing for 60 days after that. The skull had grown almost normally in spite of the fact that it had lost this whole bone. It was found in control experiments that if one removed a muscle instead of a bone, then the results were quite significant, and this clearly showed that musculature was the significant form-determining factor in development. Fejos picked up this little bottle with the skull in it and examined it, and he said, "How long will it take to finish the experiment?" I said that it would take a few weeks, and he responded, "How much will it cost?" I told him it would take $1,800 to finish the work. He then exclaimed most enthusiastically, "Ask for it! Ask for it!" He was always very, very positive, and when he liked something, you had no doubt that he liked it. And so that is really when our friendship started. And it was a very important friendship to me, in part because he invited me to come to any of the monthly supper conferences that the Wenner-Gren was giving. Normally, these were organized around a specialty, for example, linguistics or physical anthropology or social anthropology. I went to most of them for a period of about three years, and through them I met the people who were in the Ethnological Society, and in the New York Academy of Sciences, and on the staffs of Columbia and Hunter College. Paul Fejos was a very dramatic, very interesting person, and he really liked to see things change and improve. For example, when Willard Libby discovered C^{14}, Paul immediately arranged to fly out to Chicago to see him, and he asked Libby what he needed to finish up his work. Libby said it would take $25,000, and Paul characteristically responded, "Ask for it!"—and Libby had it almost by return mail. Now, this was at a time when Duncan Strong, for example, was saying, "The last thing in the world we need is some kind of a chemist coming in here and upsetting our techniques." And H. J. Spinden was saying the same thing. He put a date on some lintel, I think, and Libby ran a date on it and it came out to be the same year exactly, and Spinden went around saying, "I told everybody it was unnecessary. You can see I was exactly correct, right to the year." Carbon dating was an irrelevant nuisance to him and to others at the time.

ID: I know in those years you wrote about "the new physical anthropology" [1951]. What response did you get to that?

SW: Most people in physical anthropology at that time, of course, had been trained in biometry, and the notion of an experimental kind of analysis of the things that they were studying came as a major threat. Basically, they didn't like it because it was upsetting. The traditional thing was to describe and then draw conclusions. And the notion of the experimental method was simply to start with a problem, ideally more carefully defined than traditional problems had been, and then do something actively to intervene in what one was looking at. But people didn't like that technique because they thought it was destroying the evidence. I did a lot of experimental work, much more than ever got published, and when I left Columbia I was promised an anatomy lab at Chicago. At the last minute, the anatomy department withdrew its support, so we raised pigs in the basement of the old Walker Museum in Chicago. Students like Neil Tappen, Mel Baer, and others contributed a great amount of time in maintaining the animals under very difficult conditions, and the pig studies were the best studies that we did.

ID: What were you doing with the pigs?

SW: Well, there were two theories about the way the human skull grows, both of which had been well known for a long time. The common theory was that the skull grows at the borders of the sutures, and the other theory was that it grows on the outside and is resorbed on the inside. The material that led to the theory of resorption on the inside arose in a madder factory, where the roots of the madder plant were thrown away with a lot of dye left in them, and the pigs that ate the madder roots had beautifully stained skulls. So, we simply copied this accidental work and stained the pigs' skulls. Pigs are much more satisfactory than rats for this kind of work because you can see what is going on. For example, a single tooth is more than an inch long, and you can see exactly where the thing is growing when it has been stained by the madder dye. What we found was that both processes were going on; part of the skull was growing at the sutures and part of the skull particularly close to the brian was growing on the outside and then being resorbed on the inside. In animals that have thin skulls (rats and humans being in this category) they grow primarily at the sutures, and in animals that have thick skulls (like pigs and elephants) they grow primarily on the outside with resorption on the inside.

Again, how do browridges grow? Well, from this experimental work we found that browridges are the result of two very different processes, one being external apposition and internal resorption and the other sutural growth. These two processes are separable, different mechanically and differentially. For example, the so-called nuchal crest at the back of the skull is a double structure. Part of it grows in response to neck muscles—the traditional expectation—and part of it grows in response to jaw muscles. Now, we can locate the part that is responsible to jaw muscles in the fossils. You can see this very clearly, and it is its greatest size in the larger *Australopithecus*. So, not only can we see why in part the large *Australopithecus* is different from the smaller one, but we have a way of interpreting the difference. And people studying large and small *Australopithecus* are still acting as if the nuchal crest were a simple thing. Again, you can show with the rat work that it isn't, because you can make nuchal crests big, on one side or both sides, or small by experimentally changing the muscles.

I think that a lot more can still be done along these lines. When I went to Chicago I was hoping to shift into analysis of monkeys, and if I'd had the facilities I would have made the shift. Instead, I was stuck with having to use animals which are very easy to raise, rats and pigs, and overgeneralizing about the results, which in general is not a good practice. However, this convinced me that overgeneralizing does have its place, provided you know you are doing it and are willing to change when you see where you have made mistakes. If you don't overgeneralize, particularly in anatomy, then the process of dissection in fact destroys the patterns which are necessary to understand human evolution. Bones, ligaments, muscles have to be discussed together, not as separate entities, and I argued that this was equivalent to the kind of change that Malinowski brought to ethnology. The goal is to look for functional patterns, trying to see how the thing works as a system. It isn't as though one is going to do a distribution of paddles around the Pacific and talk about the reconstruction of history only from paddles. I was simply rediscovering in these experiments precisely the same sort of thing that had already been discovered by Malinowski in ethnology—that you have to see the pattern.

ID: When I was talking with you in Chicago in about '57, '58, getting ready to go to Africa, were you taking the same perspective toward primate field studies—that is, how could you interfere with the system to understand it better? We were talking about moving animals experimentally, and fieldworkers' attempts to do that over the years have been met with tremendous hostility within the academic community for reasons I think are very similar to "Don't upset the applecart. You're destroying data." And it's only after two decades of really long-term and intensive field research that they are beginning to be willing to take an experimental approach. But from my perspective a tremendous amount of the description that went into primate studies could have been bypassed by some elegant work earlier on.

SW: I think that is true. To me this question of whether male langurs do or don't kill infant langurs is a perfect kind of question for experimental work. Having previously studied the langurs and knowing what was their natural behavior and then adding adult males until the system broke down, one would get information very difficult to get any other way. I would be very much opposed to this if the system had not been studied first, but once the system has been studied and it's apparent that there are problems there which will not be settled with discussion, then experimental interference can create a variety of controlled situations you can see. Take Shirley Strum's work with baboons: I urged her to give them meat, and much to my surprise, and to hers as well, when she gave them a hare they wouldn't touch it—and hares were what they were catching and eating themselves in the wild. I don't think anyone would have anticipated that. Jane Goodall used to tell about how one of the chimps that she tested would not take meat at all, but it would reach through the window of the kitchen and take a plucked chicken out and eat it. Now, what appears to have happened here is that food stolen from the kitchen was recognized as food while an animal lying dead on the ground was not. Studying the factors involved can be improved on very much by experimental work, because Jane Goodall's experience remains as just one case and we all know how very misleading one case can be. Let's say that you have conditioned animals so that they will take anything from the kitchen. Now, give them a variety of things and see what the limits are to what they will in fact steal from the kitchen—which is quite a different experiment. I think that a lot of the fieldwork can be made experimental work without in any sense replacing the observation of animals in the natural situation.

The experimental work we were doing in those first days at Chicago was the subject of vigorous, spirited discussion at the departmental meeting in connection with a proposed Ph.D. thesis, with, as I remember it, Fred Eggan and Sol Tax both saying, "This is fine, except it has nothing to do with anthropology itself." [Robert] Redfield sort of leaned back—and he had a way of leaning back that, when you got to know him, you knew was a dangerous sign—and finally he said, "Wasn't there a man named Mendel who made certain contributions which we are using in anthropology all the time, and actually he was studying *plants*! Aren't rats closer to us than plants?" Everything stopped! It was such a succinct statement. I mean, that was that, and the thesis was approved. There is a second issue I am addressing here, and it is what was happening at the department level. Redfield knew the people he was talking to; he knew what the issues were; he knew when to make his pitch. And I think this is the way to run a department. Now, that was a very small department, and you can't do that with 30 people, but that is the way they operated.

Chicago was a super place. The people there were very active, very imaginative, very widely read, completely different and yet they got along marvelously. Compared with most faculty meetings that I've been at, this was just a different world. I think they respected each other, and they knew each other so well that they knew how each one of them thought, basically, and they didn't argue the things that were not going to work.

ID: Now, of course, one of the reasons Redfield, Tax, and Eggan got on so well is that they all shared the structural-functional paradigm, particularly as revealed by [A. R.] Radcliffe-Brown, right?

SW: He made a tremendous impression. Melville Herskovits said, "Chicago brought Radcliffe-Brown to the U.S.A. and destroyed American anthropology."

ID: When I arrived as a student, you had a really magnificent conception of the first year of graduate education. There were three sequences. You, Bob Adams, and Bob Braidwood taught one, which sort of began with the Oligocene, I suppose, and went up through the hilly flanks at Jarmo. The other two were basically an ethnographic sequence and one on social theory and linguistics. And they were required of all graduate students, no ifs, ands, or buts.

SW: The difficulty with it was that graduate students had to make the choices of these sequences literally before they had had any elementary education in anthropology, and they were stuck in those sequences and went through a lot of time without getting any kind of reference outside of anthropology. I still think that a lot of people, for example, would have been better off if they had omitted the archaeology, at least for the first year, and done some work in sociology. That was an eminent sociology department, and it was just a mistake that anthropology existed in the same building with zero communication between them. And you simply couldn't get an adequate education for an archaeologist at Chicago because the students had no time to do geology or basic paleontology. It worked very well if you were in the Eggan-Redfield part of the department, so to speak, but very badly at the periphery.

ID: There were no undergraduates at Chicago, and you went to Berkeley and attracted enormous numbers. Was that a factor in your decision to go to Berkeley?

SW: I had always enjoyed teaching, and I couldn't teach the way I wanted to at Chicago. It was just as simple as that. I think it was the illusion that they were teaching graduates when they were teaching people who had had no experience in the field whatsoever. So I was very happy to go to Berkeley. There I was able to teach my own course for the first time in 19 years!

ID: What was the situation with introductory courses at Berkeley when you arrived?

SW: Well, when I came there Ted McCown and I alternated, and Bob Heizer occasionally taught the introductory course.

ID: And what were the numbers of students?

SW: The low end of the class fluctuated between 600 and 800, and we actually taught as many as 1,284 in one class. Initially, introductory anthropology of human evolution counted as a biological science, and it was very important to a lot of students to be able to count it as a biological science. So the number in the course really bore more of a relationship to the mechanics of bookkeeping than it did to the quality of the course. If the department could show that it was teaching all these people then it could get more faculty positions, so it was complicated academically and politically. And basically it was not high-level teaching. I wanted to teach, had fun teaching. It was very important to me.

Berkeley was a very interesting, very good academic base, very different from anything I had seen before. For example, some years ago, I was going by Sproul Hall, and Ed Feder came along and I invited him for coffee. I asked him, "How's your budget? Can we get another one or two teaching assistants in the department?" And he said, "Why do you need two?" And I explained what the issues were, and he said, "Write me a letter! You'll get them!" So I wrote him a note, and we got two more positions in the department. There were now more than thirty, and this happened very quickly, and largely, I think, without any particular thought on anybody's part about the structure of the department as a whole. So, there was a down side to this more casual growth. We went from two people in physical with Ted and me to six people, and they weren't six people hired for intellectual reasons with substantial discussion of why we were hiring them. And that, I think, is just not a very good way to build a department. The way to build a department is to add people one by one, each of whom brings a different perspective that allows a synergistic atmosphere that makes it easier for them to talk to each other and to develop and exchange ideas. For example, to be able to open for discussion issues like whether or not mitochondrial DNA is going to revolutionize anthropology, or to decide what is the case on South American monkeys, and so on. Otherwise, you could have hired a dozen physical anthropologists and find you'd essentially hired one.

ID: I want to turn a bit to the African fieldwork, some of which I was lucky enough to do with you. To me, part of that work goes back to "The Australopithecines: The Hunters or the Hunted?" [1957]. I gather that you first got interested in going out there as a result of the 1955 Pan-African Congress?

SW: I went out there in October to look at baboons in the Wankie Game Reserve. It was the end of the dry season, and so it was much easier to see the animals than it would have been with a lot of tall grass. It became clear to me on looking at the patterning of bone dispersal that the notion that Raymond Dart had had about atypical distribution of bones didn't prove anything at all. One needed to see which bones in fact were there and then how they got there. What you find is that the most commonly found bone is the lower jaw and the teeth. Why? Because these are the least edible parts of the animals and are therefore the last to go. Basically, what you are looking at are the bones that local carnivores are eating least, which is an entirely different way of judging distribution. And I would argue, again, for an experimental approach, and I would have done this if I had stayed in Africa. I would have put a goat out for a hyena to eat and then followed what became of the bones, thereby getting a lot of information that one would never be able to get from fossils.

I was hoping to get a substantial number of baboons for a series like we had done on the gibbons, and since the baboons are considered vermin there and they want to get rid of them, I thought I would be able to proceed with the work. But when I found that we didn't have enough for our study, I walked down from Victoria Falls Hotel where I was staying to watch the local baboons. There were three troops, and one of them was very, very tame; you could walk right among them. The second troop you could get to the edge of, and the third wouldn't let you get near it. I suddenly realized how much could be learned from this. Here these baboons were precisely adapted to what they could do in this area and where they expected problems and where they didn't expect problems. So I spent the month, I suppose, just looking at baboons instead of dissecting them. And it was very profitable, and I learned a great deal while having fun. For example, in this little park right near the hotel local people went by, and if there were baboons there they would throw rocks at them. Well, the baboons had this figured out within six inches, practically. They knew precisely how far people could throw stones and paid no attention to people who were just a little bit farther away than that. In this context, you could see that they were making all these very sensible and learned adjustments. That was just very, very interesting. There was one particular female in the troop, and one day she was going along at the head of the troop, and I thought, "Well, isn't this interesting? Here the troop is moving away a substantial distance and the female is leading it." I realized a little later on, of course, that she was absolutely at the bottom of the hierarchy and the only way she was going to get any food out of the tree was to get to the front and get her fill in before the others arrived, at which time she was going to get chased out of the tree. All this made me realize that if you're going to get any different observations on animals, you've got to put in some time and see what the animals will tell you, so to speak, about the way they are behaving. It may be very different from what you think.

I hoped to gain some kind of a feeling—rather than scientific facts—about how the baboons were adapting and what were the problems of this kind of adaptation. I have always depended on a deeper level of intuition merged with field experience to suggest the direction of my research. At Amboseli, in particular, there were a lot of lions, and it became clear how simple it was for a relatively small primate that the lions could easily have killed to wander around with these lions and not get into much trouble. I think that *Australopithecus* could have done precisely the same thing, and perhaps even vervets and other small animals, by not getting very far from the trees. To my understanding, this deemphasized the problems of living next to carnivores by suggesting that as long as there were enough ungulates, it might have been very safe for the australopithecines to exist with the large carnivores there.

I have been working on the question of how many carnivores it would take to leave enough food for scavengers, and I think it takes a lot. I think archaeologists at the moment are kidding themselves if they think at some stage of human evolution our ancestors were primarily scavengers. I don't think there is any evidence for this. Hyena females, after all, can eat the meat, get the marrow out of the bones, and digest the collagen. What could a primate female do as a scavenger? Without large canines, she can't break up the bones to get at the marrow. And immediately, I think, in making this comparison, one gets the feeling that it's a very difficult thing to scavenge unless one is adapted as a scavenger the way the hyenas and the jackals are. In the Nairobi Park, there were about 400 baboons and fewer than 40 lions, and it was the lions that would have had the majority of the meat, with little left for the baboons.

ID: It was during that period of the African fieldwork that Henrietta began to show the first symptoms of Parkinson's disease, and I think that changed your life and research strategy in a major way, is that right?

SW: Yes, that's right. If Henrietta had continued to be well—she liked Africa moderately—then I had hoped after two or three additional years at Berkeley to spend roughly half the time in Africa and half the time at Berkeley. That would have been, in a sense, the logical development from the position I was in, partially structured work and partially experimental work with free-ranging animals. But that became impossible. Henrietta and I had different interests, but she was always remarkably supportive and had such a rich sense of humor. While watching the baboons, we were often close to watering holes. I will never forget the time when Henrietta was sitting beside me reading *War and Peace* and I was counting baboons! We were always able to work side by side.

ID: I'd like to move on to the Wenner-Gren conferences you were involved in. The first one that people associate you with is "The Social Life of Early Man" [1961]. Briefly, how did that come about?

SW: Paul Fejos asked me if I would arrange such a conference for the Foundation, and I agreed. What I tried to do was to get some people added to the list who could discuss questions of possible reconstruction of the social life of early man. It's a very difficult topic. It's not a topic that I would have chosen at that time or that I would choose now.

ID: It has become very controversial in recent years, with more and more palaeoanthropologists insisting that the creatures before modern *Homo sapiens*, right up to Neandertal, are really probably very different in their behavior. How do you feel about Neandertal? The pendulum seems to swing back and forth every couple of decades.

SW: The primary issue as I see it is one of language, and this is what I think makes it so difficult, because there are no fossil languages, so to speak. I think that there was a great change within the range of 35,000 to 50,000 years ago, and I think that this is the time when language as we know it today comes into the picture. The issue is that human beings make short meaningless sounds, phonemes, which are then combined into words that are meaningful things, and no other animal does this. This is as unique to humans as is, say, the human foot or the human pelvis. But the difference is that almost everything that we consider in social science is in fact dependent directly or indirectly on language. I think that the origin of language as we know it is the critical event that came about 40,000 years ago—which more or less corresponds with the ability to cross large bodies of water, to get to Australia, to go into the Arctic. Language as sound codes could have led very quickly to very rapid progress, furthering cooperation, kinship systems, and technological systems that would produce better boats, fishing, etc.

ID: So, if I understand you, then even though the well-known cranial capacity of Neandertal is well within the range of *Homo sapiens*, maybe on average larger than in some populations, they were not speaking a language comparable to the language of modern *Homo sapiens*?

SW: That would be my guess.

ID: So it isn't just size, cranial capacity?

SW: On the relationship of the size of the brain to the function of speech, there are two quite different issues. One is that size is not a good indicator of function, and you could have the same size continue for a long period of time without having any particular change in function. And the other is that if, for example, you take a modern *Homo sapiens* who unfortunately has had to have his right cortex removed before the age of approximately six, he will learn to talk just as well as he would with a whole brain. But if you give a neurosurgeon a whole brain and ask him, "Was this a normal person?", he wouldn't be able to determine that. I think capacity has been incredibly exaggerated—not that it isn't important, I am sure it is, but man's capability a million years ago was at what we would consider normal capacity today, with minimum change thereafter. This is one reason I think that identifying skulls like Předmostí, Brünn, and the classic Upper Paleolithic skulls as subspecies is a mistake, because it may simply hide one of the great events in human evolution.

ID: The other Wenner-Gren conference that you were involved in was one that you were allowed to plan from the beginning: "Classification and Human Evolution" [1963]. I would like you to begin with what you saw as the issues, why you chose the people you did, and what you hoped to gain from it.

SW: This was a very important conference from my own point of view, and I wanted to get something out of it which didn't actually happen. Instead, what really happened was that the importance of immunology, which I had not understood before, became clear to me, and it also provided the clearest possible demonstration of the difficulty of instituting a change in what would be considered relevant research material. It was remarkable that the work presented on immunology by Emile Zuckerkandl's classic review of molecular biology and its relation to evolution and taxonomy caused no discussion. G. G. Simpson, who certainly was a highly informed person, refused to pay any attention to these modern techniques, and so did Ernst Mayr. Then Morris Goodman upset them both because he wanted to change the taxonomic terms despite knowing little about taxonomy, and this was a disaster for ensuring any kind of communication. Bill Straus, who was frequently a good person to represent the traditional anatomical kind of view in anthropology, was in his own way as entrenched in his support of typology as Goodman was entrenched in his new taxonomic terminology. The new systematics had not won out by that time. So these were all strong people, and I didn't see at the time any way to achieve unity.

The advantage of that conference was that most of the people there were highly efficient, and most of them made important contributions which have lasted. Both Simpson's statements on taxonomy and Mayr's contributions were very useful. It was one of the last conferences on human evolution at which a large percentage of the important people in the field were present, so that in itself contributed to a dynamic atmosphere, and clearly the beginnings of primate studies were reflected in that conference. Zuckerkandl's paper is, I think, really prophetic. He took the different kinds of evidence and showed how they could be used in evolutionary and taxonomic reconstruction.

ID: Obviously, you have been engaged for more than 20 years in an argument which many of us now feel you've won. But I can remember a time when you were almost alone in holding out for molecular biology with people who were traditional taxonomists, paleontologists. From your point of view, what was going on?

SW: If you look at the primates in terms of the kind of thing that I learned from Dempster and later from LeGros Clark and others, the apes and man form a group. Here I was, practically alone, or at least a downtrodden minority, holding fast to the belief that apes were close to humans in the evolutionary line when most of my colleagues had come to evolve into an anti-Darwinism that suggested they were not so close. I was very conscious of the need for a new kind of evidence to help argue it, and the introduction of immunology was perfect! Why did I jump on the molecular bandwagon? Perhaps it had less to do with good objective work and more to do with its being just what I had believed beforehand!

ID: I was in the audience in 1962 when you gave your presidential address to the American Anthropological Association, with a packed auditorium of at least 1,000 people, and I have just now been looking at how it appeared in print [1962]. Your first couple of lines were, "The Executive Board has asked me to give an address on the subject of race, and reluctantly, I've agreed to do so. Although everyone knows I'm not a specialist on this subject, I have taught it in recent years." Then, without further ado, you said, "In the last year, two books have appeared on the subject of race. One, by Carleton Coon, *The Origin of Races* [1962], is a reversion to 19th-century typological thinking and is of no use to the profession whatsoever. The other, by Theodosius Dobzhansky, *Mankind Evolving* [1962], reflects the best of modern population thinking and is of use to all anthropologists." At the end of it, you got a standing ovation that went on for minutes and minutes and minutes, except, as I looked around, some of the physical anthropologists had not even stood, and many were standing glumly and not even applauding, much less cheering. How do you view that whole situation from a distance of more than 20 years?

SW: The executive board was trying to write a resolution on race, and the more you take a topic like race and struggle for precision and accuracy in every line, the more you don't have a resolution. So as this went around, it became weaker and weaker, and finally Steve Boggs, who was executive secretary at the time, said, "If this is the resolution that the board is going to pass, I resign as of now." This really shocked us, and the question became what to do. Then Joe Casagrande suggested that I give my presidential address on race and the board would endorse that as its position. If this had not been such a volatile and important issue, I would have talked about monkeys or some sensible subject like that.

ID: Why would so many physical anthropologists have taken umbrage at the talk?

SW: It didn't surprise me. First, Carl Coon was a very nice guy and a very good friend, and so I assume some people thought my statements were unnecessary. It wasn't a popular thing to do. And of course, there were a lot of people doing research themselves on precisely the same thing that I was criticizing, and so it came close to home for them. An attack on typological race was in a way an attack on physical anthropology itself, and specifically on the way a lot of people were teaching it then. If I had it to do again, I would handle it the same way, despite the negative impact on some. The amazing thing to me was that here were people living that close to Hitler and that close to the war who really hadn't changed their teaching. That was the shocking thing to me, and I think it has been very bad for anthropology.

There is still so much tacit support for judging some races to be inferior to others. As an anthropologist, I believe you have to fight it or leave it alone, one or the other. The point is, you can do this with a lot of different issues around the race question. The issue of intelligence, for example, has become easier to deal with because Jensen and Shockley have faded into the antiquated background and there has been more research done to draw from than was possible in Coon's day. At that time, you had to go into questions of what various factors affect intelligence, and that's a very difficult thing to do.

I would not have seen it this way a few years ago, but I think when you decide to fight and when you decide to stay on the sidelines is one of the most important decisions we all make. And I should have said in that address, "As far as the study of race is concerned, this is a time to fight. Your executive board is not staying on the sidelines; we have agreed that we want a very strong statement on race."

ID: You were saying yesterday that Sheldonian thinking is clearly not dead. Where do you see this coming back?

SW: The first section of Wilson and Herrnstein's *Crime and Human Nature* [1985] is on biological causes of crime, and in this they go back to the Sheldonian system as a reputable, defensible reference. From what they write, I think they don't understand the system and if they did they would not have included it in the book. This is a very dangerous kind of perspective. If people think that there really is a relationship between crime and a biological cause, then the next step in reducing crime surely suggests eliminating or controlling people who exhibit factors that someone believes are indicators of a potential for criminal behavior. And of course, those factors would be judged from everyone's own individual perspective. For example, there are laws in a number of states allowing castration on the grounds that it will reduce aggressive behavior and the likelihood of rape and violent crime. I doubt that most people are aware of these laws.

ID: My feeling is that some of your objection to sociobiology is what you see as a resurgence of a biological determinism after having spent so many decades of your life trying to purge simplistic biological deterministic arguments from complex human social behavior.

SW: I think that has been a very strong motive for me. But I also strongly resist what I think is its basis in naiveté. For example, the argument that the human species is just a species that is distinct from others the way any other species is distinct seems to me to be nonsense. Humans are distinct in ways which are tremendously important in terms of function and evolution and in terms of interpreting social behavior. And again, I think that language is much more important than most sociobiologists think it is, and I don't see how one can usefully deal with the question of human reciprocity without considering human social behavior. To just drop out all that has been written on reciprocity, particularly by the French sociologists, and act as if this were a brand-new idea is intellectually limited. Basically, sociobiologists act as if there were no such thing as human social science.

ID: You have done some writing on education. What was your basic point?

SW: What human beings value most is being part of some kind of an ongoing project, movement, something that has individual and group meaning. People are very responsive to other people doing things which they regard as important. So, if you are going to teach biology, a substantial part of the task should be to help show kids how this is important, why they should want to know some biology. Now, the biology book that my kids were using in high school was the best excuse for not learning biology that I've ever seen. John Holt said that the dull students forget the facts before the exam and the bright students forget the facts after the exam, and that's an excellent summary of American education. Evolution and anthropology are very important fields of study because they can help people to understand themselves and other people better. And the failure in our schools at this level is, it seems to me, very, very deep. So much of education is about the mechanics, not the fundamental issues. Perhaps this comes out more clearly in anthropology than in anything else. At the last meetings, I went to a session on the teaching of evolution, and it was dull beyond belief! Few people there were up-to-date on what they should be teaching, and their notion of teaching was all technique. I finally got up in irritation and said, "The point is that we should be trying to teach these school kids to understand human beings. It's not a question of what particular technique we use. How are we getting to the most important objective of having them understand people better?" In anthropology textbooks, it used to be very clear, and people like Herskovits had very clear notions of what anthropology should be doing—it should be helping us to understand our own behavior and that of others and the variety of human behavior.

ID: Now that you mention it, I guess that prior to Hoebel's text, each of the previous anthropologists—Linton, Goldenweiser, and even Kroeber—had had a very strong personal point of view. From Hoebel on, we had essentially textbooks written by committee, bland, offending no one, giving a little of this and a little of that kind of approach.

SW: We should have a really first-rate statement that every child should know something about evolution, that we should incorporate anthropology into all high-school programs, and that it should be a fundamental part of all university anthropology studies. In most university departments I know anything about, they are still laboring under the delusion that it is their highest calling to turn out great research people instead of great teachers. I would like to finish our discussion by repeating a passage from "The Study of Race" [1962]:

> Whether we consider intelligence, or length of life, or happiness, the genetic potential of a population is only realized in a social system. It is that system which gives life or death to its members, and in so doing changes the gene frequencies. We know of no society which has begun to realize the genetic potential of its members. We are the primitives living by antiquated customs in the midst of scientific progress. Races are products of the past. They are relics of times and conditions which have long ceased to exist.
>
> Racism is equally a relic supported by no phase of modern science. We may not know how to interpret the form of the Mongoloid face, or why Rh is of high incidence in Africa, but we do know the benefits of education and of economic progress. We know the price of discrimination is death, frustration, and hatred. We know that the roots of happiness lie in the biology of the whole species and that the potential of the species can only be realized in a culture, in a social system. It is knowledge and the social system which give life or take it away, and in so doing change the gene frequencies and continue the million-year-old interaction of culture and biology. Human biology finds its realization in a culturally determined way of life, and the infinite variety of genetic combinations can only express themselves efficiently in a free and open society.

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Guidelines for the Economic Valuation of Nontimber Tropical-Forest Products[1]

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Policy makers often assume that tropical forests have no economic value unless they are logged or farmed (Hecht, Anderson, and May 1988:26; Dove 1983). Besides tim-

 This research was partially funded by grants to Harvard University from the Rockefeller Foundation, the John Merck Fund, and the W. Alton Jones Foundation. We would like to thank the following for providing information on their research methods: Paul Appasamy, Stephen Beckerman, Kenchan Chopra, Wade Davis, Patrice Engle, Daniel Gross, Kristen Hawkes, Kim Hill, Stephen Hubbell, Allen Johnson, and William Vickers. We would also like to thank Peter Ashton, Lisa Curran, Michael Dove, Anil Gupta, Mark Leighton, Donald Messerschmidt, Mark Poffenberger, Kent H. Redford, Ira Rubinof, B. L. Turner, K. F. Wiersum, and David Wilkie for commenting on a draft of this essay. Glenn P. Jenkins, Theodore Panayotou, and Jeffrey Vincent provided technical advice.