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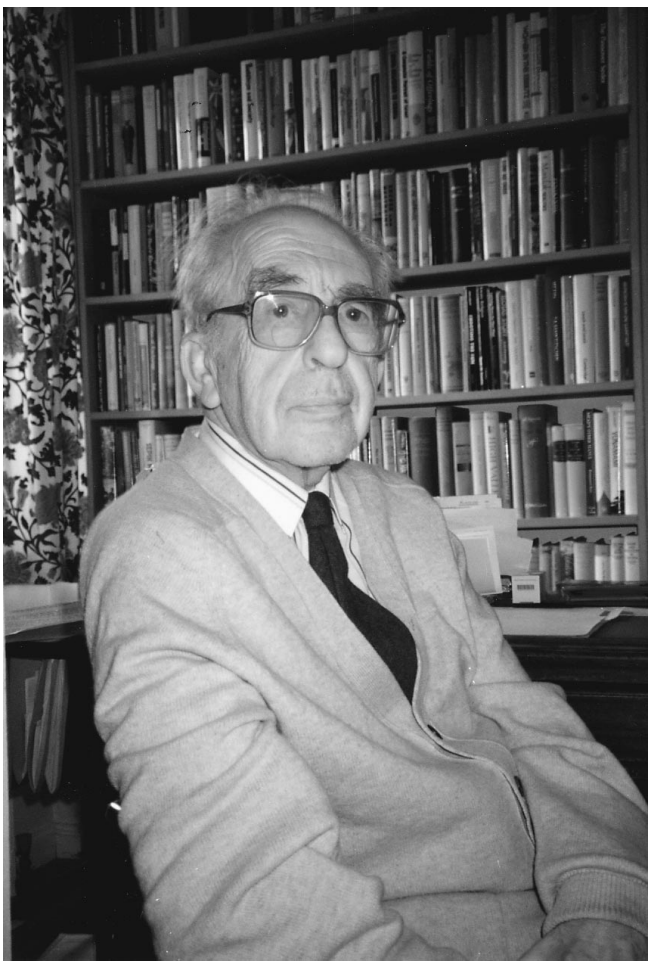
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Reports

An Interview with A. L. Epstein[1]

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A. L. Epstein.

Introduction: A. L. Epstein's scholarship has helped to define key moments in social and cultural anthropology—urban anthropology, qualitative research methods, the study of ethnicity, and the anthropology of emotions. Born in 1924 in Liverpool, he trained in law at Queen's University Belfast before turning to anthropology. His first fieldwork was conducted in 1950 on urban courts on the Copperbelt, in Northern Rhodesia, now Zambia. He returned in 1953 for research on the political organizations of mineworkers for his Ph.D. thesis at the University of Manchester. This thesis became *Politics in an Urban African Community* (Epstein 1958), which deals with the politics of ethnicity and class and dependent development in a colonial territory, anticipating by many years interest in this kind of subject and a perspective that assimilates a complex web of structure and agency. Epstein took an academic appointment at Manchester, becoming one of the pillars of the so-called Manchester School of urban ethnography in South-Central Africa (see Werbner 1984, 1990) and publishing important papers on the application of social network analysis and on the case-study method (e.g., Epstein 1967, 1969). Epstein and his wife, the anthropologist T. Scarlett Epstein, then moved to the Australian National University, beginning fieldwork among the Tolai of Matupit, the Gazelle Peninsula, Papua New Guinea. He conducted fieldwork there in 1959–60, in 1968, in 1986, and in 1994. Epstein's interest in psychology and the anthropology of affect became more pronounced during the 1960s and led to an interest in identity. It is these interests that are evidenced in his influential 1978 book *Ethos and Identity: Three Studies in Ethnicity*. In 1972 he returned to England, accepting an appointment as departmental chair at the University of Sussex. Since his retirement from Sussex he has remained an active scholar, continuing to publish path-breaking work on Tolai emotional life (e.g., Epstein 1992*b*).

KAY: At the beginning of his CURRENT ANTHROPOLOGY interview with Aidan Southall, John Burton says [1992: 67] that the value of these interviews is in educating those who know the subject primarily through his or her work and that these sorts of narratives are intended to seek edification about beginnings. With this in mind, I was hoping that we could construct a life history for this audience—one that would include an intellectual history as well. Might we start with some personal recollections of your youth, your family, your education?

1. © 1997 by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3802-0008$1.00. This interview took place in Hove, England, July 6, 1996. I gratefully acknowledge the receipt of a Presidential Young Faculty Award from the University of South Florida which made the research and travel for this interview possible. I thank Cathy Newman for her work in the preparation of the interview transcript.

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ALE: Yes. I think family is quite central to any understanding of why I became an anthropologist. My grandparents were Jewish immigrants from Austrian Poland, and their children, including my father, were born in Liverpool, where I myself was born. I grew up in a three-generation family, and I spent much of my early childhood in the company of my grandparents. I was, for many, many years, the only child. In particular, I was the first-born son of my grandparents' first-born son. So I was surrounded with all of this, you know; I was the center of their universe. And I suppose I really couldn't stand anybody getting in on what was my territory. And I was really a very aggressive little boy. I recall an occasion when my father's next-younger brother, who had a very important role to play in my life, bought me my first pair of boxing gloves. And one day he came on a visit with some friends of his and another little boy who must have been around about the same age as myself, round about three, four. And without any introductions I immediately proceeded to attack this little fellow. I was cocooned, you see. And the great big world around me was something that I knew nothing about.

This factor, I think, became even more important when we moved to Belfast. The cardinal point about the North of Ireland is that it consists of two basic categories: Catholics and Protestants. And we belonged of course to a small Jewish community, and in that kind of system you couldn't find a place for this third category. There is a joke which is frequently told that the question commonly asked in Belfast is "Are you a Catholic Jew or a Protestant Jew?" I can assure you that in my own case it was not a joke. You'd be minding your business, walking down the road, and somebody would suddenly spring at you and say, "What are you, a Mick or a Prod?" And you would reply, "Well, I'm neither." And then would come the fists. So one was aware, you know, in these different ways, right from the outset, of being an outsider. The real move, as it were, towards anthropology came I suppose in adolescence, when so many youngsters begin to seek, to discover who they are, to search for their identity—in my case, more particularly perhaps, to seek my beginnings. And, curiously, this led almost directly to my interest in anthropology. In fact, I was searching for the subject, in a way, before I even knew that there was such a word. At the age of 16 I began to keep a personal journal, and I still recall the very first entry because it carried a reference to Herodotus. I was reading, at that point, Herodotus. This was a way of looking back to the past—I was looking for beginnings. So I moved from Herodotus to writings on ancient Egypt. And not long after that I went up to Queen's University Belfast to study law. There was no anthropology at Queen's at that time, and there weren't too many books on anthropology. But by chance—this was round about 1940—by chance in the law library I came across a copy of [Bronislaw] Malinowski's *Crime and Custom in Savage Society* [1926], which I read, and I was hooked. So much so, indeed, that I wrote to Malinowski at the London School of Economics to ask about the possibility of doing research. What I didn't know at that time was that he had gone over to the States and had died, I think in '41, at Yale [Malinowski died in New Haven in 1942]. But somebody in fact had opened the letter and replied to it, suggesting I do some more reading. And I began to read, in a way, more anthropology than law. It was a most curious assortment of stuff, because it depended on what was on the shelves in the library; I must be one of the very few who has actually read things like [E. B.] Tylor's *Researches into the Early History of Mankind* [1878]. And then I got on to [W. H. R.] Rivers.

At the end of my degree course I went into the Navy, and at one place—in what is now Sri Lanka—I found myself posted to look after the camp library. So that gave me an option to do a little more reading. By chance I came across a little volume by Morris Ginsberg called *Sociology* [1934], and now I had decided. The early chapters I think dealt with primitive society—as it was in those days—and I thought, "Yeah, this is for me." So when I was demobbed, finally, in '47, I got in touch with Ginsberg and went to visit him at Cambridge. He was an LSE man (during the war the LSE had been evacuated to Cambridge), and he thought that I might come to LSE and do an undergraduate course. That was really quite out of the question, because I couldn't really see myself asking my parents to support me for another three years. My dad persuaded me to go back to Queen's for a year to the law department so that I could be called to the bar as a barrister, and since, like so many others coming out of the forces at that time, I really had no very clear idea what I wanted to do—except for this fantasy that I couldn't see myself ever achieving—I did go back. Then, by one of those bits of sheer chance, I discovered on the notice board one day that the Colonial Office Social Science Research Council was offering fellowships for research in the colonial territories. I quickly saw that this was an opportunity to use my law training—such as it was—as a bridge over to anthropology. So I applied, and I was very fortunate—I was awarded one of these. I went to LSE for a year, and that's how I came to make my first trip to what was then Northern Rhodesia, now Zambia, to do a study of the African urban courts on the Copperbelt.

At that time it was common practice for young people going out to the field on their first trip to have a local supervisor. There had been one or two cases of people who had become ill or for one or another reason really hadn't made it. And in my case it was decided that I would go to Kenya and be supervised by a man called Günter Wagner. Wagner had been quite a well-known German anthropologist who'd worked in East Africa; he did a chapter on the Bantu of Kavirondo in *African Political Systems* [Wagner 1940]. But he had been a senior officer, I think, in the German army during the war, and some sensitive soul at the Colonial Office must have decided that it was hardly appropriate for a young Jew to be supervised by someone who might well have been a former Nazi. I don't know whether he was or not, but anyway . . .

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KAY: Right.

ALE: So, instead of going to Kenya, as had been proposed, I found myself eventually on my way to Northern Rhodesia. I think what had happened was that the governor of Northern Rhodesia was in town for something or other, and I suppose somebody collared him at a cocktail party and said, "Look, we've got this youngster hanging around, and he's a pain in the neck! Surely there is something he can do out there?" And so, I set off for the study of the urban courts.

I left one point hanging in the air when I referred to the importance of my dad and my turning to law: The thing is—I didn't discover this until some years after I was married, and it was [T.] Scarlett [Epstein] who had had a long chat with him and later informed me about it—that it had been my dad's ambition, which he would never have been in a position to fulfill, to be a lawyer. So when he supported me to go and do law it was really an attempt at vicariously satisfying his ambition through his son.

KAY: So, the law takes you to Northern Rhodesia and the study of the urban courts. This was under the auspices of the Rhodes-Livingstone Institute?

ALE: Initially I went out on this scholarship, so I had no affiliation. But towards the end of my year there my funds were running short. Looking back now, it's amazing how one survived. I think I had about £150 for my fieldwork, and in those days there was no question of an anthropologist, even one working in a town, having a car; I knocked about on a bicycle. There were really no guides at that time; remember, this is the beginning of 1950. I had no models. I was thrown in at the deep end, so to speak, to undertake this study of the courts. What form should the study take? How was I to tackle it? One of the things that had guided me there was that during my stay in the law department—this was during the war years, and of course we'd had large numbers of American troops stationed in the North of Ireland—one day our prof had invited two Harvard law professors to come and talk to us. They spoke about the teaching method that they followed at the Harvard Law School, which was what they described as the case method. One of the few books I had read at that time that dealt with the anthropology of law was Llewellyn and Hoebel's *The Cheyenne Way* [1941], but I had come across a short article in the *Yale Law Journal* that stressed "cases, cases, cases," and that stuck in my mind. When I began work, I decided that I really wasn't interested in a sort of code of law—you know, simply setting out the rules. The question that really interested me was how the judges arrived at decisions. And I suspect that that kind of an interest may well have been rooted in my own family background, because, not to put too fine a point on it, my parents didn't have too happy a marriage. On one occasion, when I actually asked my father was he happily married, did he love my mother, he said, "Of course. She bore my son." Which, I think, speaks for itself. And they spent an awful lot of time arguing, squabbling. And I would find myself going to and fro in my mind, Who was right? Who was wrong?—getting involved. And I think this may well have carried through into my own early work.

KAY: You wrote a lot about the case-study method [e.g., Epstein 1967]. Could you give an example?

ALE: I would sit in court regularly, every day, and listen and record as well as I could. And then the following day, before they actually started the session, I would sit with the judge. I had been working through the material, and I had my questions, and we would work through the cases. This went on in the town of Ndola for about six months.

Most of the cases one heard were matrimonial. They dealt with quarrels between husbands and wives, and so on. And at some point it struck me that I was getting perhaps a distorted view—simply from within the court. Really I ought to be going out to see at first hand something of the background of these cases. And so I started going down to the African housing area and doing some very crude interviewing in which I asked various questions about family life, how they got married, things of this sort—themes which I really didn't take up until my final trip to the Copperbelt in 1955 or '56 [see Epstein 1981]. Would you like me to go on about the Copperbelt?

KAY: Exactly. That was my next series of questions.

ALE: Well, although the first study was of, I'd say, a legal kind, in fact it led me directly into the next study, where the focus was politics.

KAY: At this point you have made contact with the Rhodes-Livingstone Institute?

ALE: That's right.

KAY: And this is where you meet Max Gluckman?

ALE: At the RLI, at that time, all the fellows used to gather regularly for a conference when they would talk about their fieldwork, exchange ideas—a chance to get out of the field, be amongst themselves. And it so happened that [J.] Clyde Mitchell was beginning his social survey of the Copperbelt towns, and we ran into one another. He asked me if I would give a paper on my work, and the next get-together they had was actually in Luanshya, where I was based at that time. So I gave a paper which went down very well. All the old hands of the Institute were there then—Clyde himself, Vic Turner, Ian Cunnison, and Bill Watson too, I think.

KAY: John Barnes?

ALE: No, John was of the earlier generation. John and Clyde. John, Clyde, and Elizabeth Colson made a three-

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some. They were the earlier batch. I came in from about '51. My stint with the Institute was '51 to '56, when I made my last visit to the country.

I think I was on the point of explaining the link between the court study and the politics. What I quickly recognized was that there were many issues affecting the courts which were not of a purely legal kind. There were lots of complaints made by the educated Africans of the day about the way in which the courts were being run. The court members were drawn from the rural areas, sent down by prominent chiefs, and the basis for selection was the relative strength in terms of numbers of the different ethnic groups in town. I started what I suppose they would call today focus groups—a group of educated, English-speaking Africans with whom I would have regular discussions—and I pursued a lot of these issues with them. It became clear that many of their complaints were really about leadership. Their view was that leadership should come from within the new urban communities, not hark back to the rural areas. And so I felt the next step should be to look more closely at the political system. In fact, when I met Clyde, Max had by this time arrived in Manchester and was embarking on his study of the judicial process of the Barotse. I think he was keen to have an assistant who could read the stuff and give him what assistance he could, so I was appointed to Manchester, having gone back to the LSE for one term in order to write up my report, which I passed on to Max. That was one of the most embarrassing experiences of my life, because I don't think there was a page that escaped his red pencil and the marginal comment "bad sentence," "bloody awful sentence." You know, I didn't regard myself as completely illiterate, but it was very embarrassing.

KAY: Is this the article that appeared in the *Journal of African Administration* [Epstein 1951]?

ALE: That was the very first piece I did, just a short thing. The whole report was published in the Colonial Office Research Series as *The Administration of Justice and the Urban African* [Epstein 1953].

KAY: So, so you go from LSE to Manchester?

ALE: That's right. Max felt I couldn't proceed to a Ph.D. on the strength of my urban court study. I think in those days departments were much stricter than some are today. You had to be registered, and you had to carry out your research under the supervision of the department. So they wanted me to go back to Northern Rhodesia, and I was actually keen to do a rural study now, because the view was still very strong in the profession that only a rural study really counted as anthropology. Urban really was not anthropology. In fact, I remember a remark of Edmund Leach: A group of us in Raymond Firth's seminar had these Colonial Office fellowships, and Leach was talking to Audrey Richards and made a rather cutting remark in my hearing that "these people are not going to become anthropologists!"

KAY: Because of your urban work?

ALE: Because we were geographers, we were lawyers, you know, we hadn't really "been" anthropology students or the like.

KAY: So your anthropological training at this point is the one year at the LSE?

ALE: That's right.

KAY: What went on during that one year before you went out to do the court work? Maybe we can contrast that to what went on in Manchester?

ALE: The course I went through consisted in what was I think fairly typical of anthropology in those days, particularly postgrads—M.A., Ph.D. We attended lectures which were given by people like Edmund Leach, who was then a reader. Leach's manner was really quite remarkable. There is a famous scene I think from *Henry V* where in the course of his sermon (and I may've got some of the details wrong) the archbishop scatters his papers, and I still remember this scene when Leach was giving a lecture and his papers, notes, scattered all over, and there he was, scrambling around and picking them up. Audrey Richards lectured on East African ethnography. She was actually my anthropology supervisor. There was a man called Arthur Phillips who'd been in the legal department in Kenya and had written a government report on what were called the native tribunals in Kenya, so he supervised me from the law side. Then the other major things were the seminars. Firth ran a seminar. There was also one he ran together with Daryll Forde. And I think I was not the only one to find those experiences quite freezing, because they would pick on each other's students. Even people much senior to myself, like Elizabeth Colson, told me that they too used to find it freezing. But Firth was a magnificent seminar chairman, as I recall. He has this gift of being able to summarize in three or four points and then keep the discussion to those points. He didn't like people to stray too far. And we always had interesting visitors. So one was rather ambivalent about these seminars. I think I learned a great deal.

KAY: At this time now, could we say that the "Manchester School" as it later became known to us had been formed?

ALE: I think it was still at that point very much an idea in Max's mind. We were and became a very close-knit group, but it was only—and this is not unusual—only when we went outside that we became known as a group. When, for example, we all went down to London for an ASA [Association of Social Anthropologists] meeting, I think we were known as the cloth-cap boys—the cloth cap being the mark of the working class. Also, because we were known to go to football matches. We went as a department, and if there was a

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really big match on, then someone was detailed to stand in a queue in order to get tickets, even if it meant that you had to cut your class that day. Not everybody did go, but many of us were genuinely keen. There were others who played golf. Bill Mackenzie, the professor of politics, Max, and Bill Watson were a threesome of golfers. But, curiously, Max even brought into the football group some Americans. And we also went to cricket matches in the summer. There was the occasion when the American sociologist George Homans was spending some time visiting us and Max took him to a cricket match at Old Trafford. And George apparently was getting the games confused—he thought he was watching baseball, kept shouting all sorts of things in his excitement!

We lived very much in one another's pockets—a good deal of intra-visiting. And, of course, basically, we were a Central African group, so we had much to learn from talking to one another about our material. The seminars were very different I think from the LSE seminars. Max's approach was quite different. Firth would pick on people. I mean, to begin with, for example, we were setting a program for the term—how we would proceed. And it was, I think, one of the very first. And he had decided that the family should be the focus of our discussions. And almost immediately he turned to me and asked would I like to present a paper. Well, I suppose I could say I was shaken rigid. But I did produce a paper, and so did the others, and at one point Firth thought that the general quality was so good that they might make a little volume. Happily they didn't! I mean, they were just juvenilia. But he did spring on people. There was the occasion—it was a hot, stuffy room, from the radiators. Of course in winter it was particularly stuffy—very conducive to sleep. And I remember the occasion when Maurice Freedman was sitting with his back to a radiator at the other end of the table and gave at one point a very audible snore. And Firth immediately lit on him: ''Ah, Mr. Freedman, you have something to contribute to our discussion?''

KAY: And so in Manchester it was a different atmosphere?

ALE: Yes. If you didn't feel you had anything to say, you could be quiet throughout the seminar. And it took me a long time to feel completely at home. There were a number of us—Ian Cunnison was also very quiet.

In those days we had money so that we were able to have visiting speakers, and we got people from nearly every department to come round and give papers. So we were not really an introverted group; we were in touch. I'm not quite sure when it was—it was a little later, I think—that Mary Douglas finished up a review article by saying, ''It is evidently time to salute a 'school' of anthropology.'' I think it was a review in *Man.*

KAY: I have it here somewhere [Douglas 1959]. And then it's decided that you need to go back to Northern Rhodesia to do work for your Ph.D.?

ALE: That's right.

KAY: Why don't you tell us about how this came to be and how the Ph.D. thesis became *Politics in an Urban African Community* [Epstein 1958]?

ALE: Well, the usual procedure was that you went to the headquarters of the Institute, which was two miles outside of Lusaka, the capital, and gave a paper on what you proposed to do. And mine was really very thin, because I had very little to go on. You know, usually you build up a research project or an outline on the basis of some material, and there really wasn't a great deal. And I thought what I ought to do is look at the various organizations that had developed and look at their relationships. And since I was clearly dealing not with one of these repetitive situations but with an utterly dynamic one, where new bodies were coming into existence and disappearing, I couldn't take the usual kind of functionalist approach. And then, of course, willy-nilly, I was into the politics of it. And central, here, were the trade unions and, beginning to appear on the scene, the African National Congress—the beginnings of nationalist politics. So I was very much in on the ground floor with a wonderful opportunity. But, again, as I mentioned in the case of the court study, you know, what models? I really didn't know a great deal. In fact I knew damn all about trade unions! I think what I decided was that I had to think of the trade union not as some particular kind of group but as a *group* and approach it in this way. So that meant, first of all, I had to get the support of the leaders of the unions, which wasn't likely to be forthcoming so easily, given all the tensions in the mines.

KAY: Tensions between Europeans and Africans?

ALE: Between Africans and Europeans. Nobody dreamed that within ten years Zambia would have gained its independence. The atmosphere of the time, when I went back in 1953, was quite nightmarish.

KAY: In what way?

ALE: It's a long story. I have discussed it—There's a paper on oral aggression which deals with that situation in some detail [Epstein 1992*a:* chap. 8]. But if ever there was a situation that was made for paranoia, that was it, because everywhere there was talk of spies. There was one little episode which I think sums it up. There was a man called [F. M. N.] Tony Heath who was the district commissioner, the senior government official, and he was very, very supportive—very, very helpful indeed. It was he who took me out to introduce me to the mine management and explain why I was there and what I hoped to do. I had a house which had been occupied by Clyde [Mitchell] and his family, and he would pop up to discuss things with me from time to time. I mean, he was a man of vast experience, whereas I was just this

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kid. I was seeing Heath, socially, quite a bit. And this particular Sunday morning, I had gone down—I'm not quite sure for any particular reason—and there at his house was the very senior government official known in those days as the secretary of native affairs. In all my innocence and naiveté I blurted out a piece of news that I'd picked up: Had they heard about the strike? Apparently, the African Mine Workers Trade Union was about to call a strike. And I assumed that if there was to be a strike, surely the D.C. of all people would know. But nobody knew, and very soon, apparently, the wires were humming between Lusaka and Luanshya. Well, Heath had agreed to take me to the European club [the Mine Club] that evening to meet some of the whites working on the mine.

KAY: Administrators, or administrative-level employees? Not miners?

ALE: Just whites who were employees of the mine, in different capacities. And I welcomed the opportunity. So we went out that evening, and we were sitting I think having a quiet drink when somebody approached the table and tapped me on the shoulder and asked me if I would like to step outside—he would like a word with me. He immediately proposed that I could be of help to them—the assumption being that, you know, I was going to be in touch with African mine leaders and others, and so on, so I would be a very useful source of information. I blurted out that I hadn't come there to act as a spy. It didn't please him very much. Actually, he began by saying, "I think I can tell you that you've been passed fit for human consumption." Subsequently I discovered that as I moved around I was being tailed.

There's an even more remarkable story of this kind, about Hortense Powdermaker, who was there at the same time and who actually shared my house. She had gone off on her own to watch some of the tribal dancing on Sunday. Perfectly innocent activity, you might have thought, just going to see some people dancing in the compound. The next day she was called before the African personnel manager and given a real dressing-down. Didn't she know that going alone she might have been raped?! And thereafter they appointed a young Canadian official to act as her chaperone. Unbelievable.

KAY: After this incident, what happened next? No doubt by this point you realized that you were walking a tightrope, so to speak.

ALE: I did indeed. Because I was now invited to see a man called [Jack] Thompson, who was the manager at Roan Antelope—that was the mine located in Luanshya—and this was to pass on a message from the Chamber of Mines that I was now *persona non grata,* that I would not be allowed to continue my work on the mine township. Well, of course, I at once got in touch with Clyde, who was then director of the Institute, and he was really most supportive. He fought my corner. He tried to persuade them to change their minds. He even got in touch with the governor, Sir Gilbert Rennie. And, of course, the mines in those days were a law unto themselves—in fact, much more powerful than the administration. All company towns tended to be very tight and very edgy about people snooping around. But Clyde was determined that I should stay on. The Chamber of Mines would really have liked to have seen me off the Copperbelt altogether, but I think that that would have been beyond their legal powers, and so I stayed on. A number of Africans in leadership positions now knew what was going on, and some of the more hot-headed were even promising a strike. I said, "For God's sake, that's only going to make matters worse!" But they said they would come and visit me in the house. And then really I had to proceed *sub rosa.*

When I was last in Manchester, Scarlett got a job at what was then known as the CAT [College of Advanced Technology], a sort of a new technological university which later became the University of Salford. (In Manchester we couldn't both have appointments in the same department, and so she joined the CAT.) And there she had a colleague—another economist—whom she brought round for tea or a meal one day, and it turned out that this man had also worked in Zambia. While we were just chatting casually he turned to me and remarked, "I know more about you than you know about yourself." He had been working in the office of the Chamber of Mines, and there he had had access to all the records on me; I was regarded as a subversive. And just one point in support of this: While I was upcountry I had met a very charming young district officer with whom I became quite friendly and who was subsequently posted to headquarters in Lusaka. When I was about to leave the country I thought I would go down and say my farewells to him, and when I left his office we were standing briefly by a door and were spotted by a senior provincial commissioner whose office was a little farther down. Afterwards I was told that the provincial commissioner had come up to speak to my friend and asked him what he was doing—didn't he know that I was a subversive? There had been a series of rolling strikes on the Copperbelt, and there were some people who seemed to believe that I was responsible for them. Life simply became so impossible that I decided I'd better call it a day. I couldn't see myself doing any further useful work. So I set off for Lusaka; I think I stayed around the Institute for another month and then back to England. But as some indication of the way things worked in those days—when we went off to Australia the idea always was that Scarlett and I would go up to New Guinea, but our application for visas was turned down. In Scarlett's case, believe it or not, it was because when she was in Vienna she had lived in a block of apartments known as the Karl Marx Building, and in my case it was because my record had followed me from Africa.

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KAY: An interest continues to follow the Manchester School and its work in Northern Rhodesia,[2] and part of this continued interest has to do with the relationship between anthropology and colonialism. On the one hand, you have people like [Adam] Kuper in his *Anthropology and Anthropologists* writing that "perhaps other colonial powers have been greatly helped by anthropologists, but the reality is that British anthropologists were little used by the colonial authorities, and despite their rhetoric when in pursuit of funds, they were not particularly eager to be used" [1983:116] but also that "it is nevertheless inescapably true that the colonial situation constrained the development of British social anthropology at the theoretical level, while no doubt facilitating access to funds and to field-sites" [1983:119; see Brown 1973:174; 1979:539, 540 on the Manchester School specifically]. But from African scholars, on the other hand, there's the early critique by [Bernard] Magubane in CURRENT ANTHROPOLOGY [Magubane 1971] which you and Mitchell and others responded to very strongly. Magubane at this time (1971:420) was writing things like

> We are presented with a situation in which there was a one-dimensional response—in which colonial values and psychology were accepted without question. The anthropologists implicitly believed not only in the inevitability, but also in the rightness of white conquest of the African. Hence, the colonization of the African personality that accompanied economic and political subjugation was assumed to be natural. The studies that are being critically reviewed here do not even take a city as a unit of analysis. Their unit of analysis is the individuals rather than the social formation of colonialism with its complex social relations. It is quite obvious that these studies tend to obscure the colonial, racial, and class structure of the society.

and saying of informants like William and Chanda that they "emerge as bastardized conformists, as passive accomplices of their own enslavement, for they have been reduced to stereotypes in order to rationalize the alienated condition of the Africans who lived in towns under most cruel conditions" [1971:422]. And he went on to suggest that it would have been more fruitful perhaps to consider the ways in which colonialism and hegemony under colonialism caused the kinds of behaviors that you outlined with respect to the status that accrued to European clothes and forms of consumption.[3] So, there are two sides. There are two explanations, looking at this question. What is your reaction to this?

ALE: The first thing that strikes me is that anthropologists are taught that it's a cardinal principle to look at things in the context in which they occur. And what some of these people are doing, I think, is introjecting notions and concepts that are around in the '90s which were certainly not part of our understanding at the time. But perhaps the other important point is the idea of seeing the situation through the eyes of the people they were working with. And on that point, this business that we were there doing the mental work of the administration is really quite laughable. One could mention all sorts of things. The Institute, since it was funded by government as well as other bodies, felt obliged to try and help in a variety of ways. And one of the things it did was to circulate a note to people in the administration asking if there were issues that they felt should be taken up by way of research. And we just got no response to these sort of things. The only item I can recall from the files that we were ever asked to do—and that in fact nobody ever did—was research on the African system of naming, which was an issue of some concern to the administrators simply because many Africans tended to change their names as they pleased. They had all kinds of names which they adopted themselves. But perhaps even more to the point was a minute that once came round from the governor himself, in which he was protesting about the kinds of reports that we were publishing in the RLI series and asking that all of these should be reduced to a single paragraph. There's no love lost between anthropologists and administrative officials. We were too close to the natives, and I guess this was in itself seen as a threat to the whole system of race relations. You see, to talk to an African was to put him on a footing of equality. For example, you went into the post office, and you joined a queue, and Africans were simply coming up to shake your hand, to greet you. They were testing you to see how you were going to respond in that situation. In a sense, by the fact that one was prepared to live in an African village—one couldn't live in an African township, that was simply out—by the fact that one was an anthropologist, one was actually challenging the system. And I see no indication that, in any of the stuff that has been written about our work, this point has ever been recognized. I think I actually do touch on it somewhere in the introduction to *Scenes* [Epstein 1992*a*].

KAY: Speaking of your relations with Africans on the Copperbelt, I was interested, in a methodological sense,

2. See, for example, Brown (1973, 1979), van Donge (1985), van Teeffelen (1978), and Werbner (1984, 1990); see also the recent debate in the *Journal of Southern African Studies* between James Ferguson and Hugh Macmillan (Ferguson 1990*a, b;* Macmillan 1993; Ferguson 1994 [which I thank Stuart Coupe for pointing out to me].)

3. More recently, Peter Ekeh writes that "obviously, the taint of colonialism on anthropology has not been easy to wipe clean. This is not to say that social anthropology and its works and products are dead in Africa. On the contrary, much of the organized social knowledge produced by Africans tends to be reaction formations against pursuits of social anthropology during the colonization of Africa" (1990:665) and that "the inner character of British social anthropology is best revealed in its mission of assisting colonial administration in understanding colonial peoples. With frequent exceptions, social anthropology was in fact an applied science, with its theories invented to address pressing problems of governance in the alien circumstances of colonial rule" (1990:666).

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in your use of your informant Chanda. My colleague Alvin Wolfe uses your paper [Epstein 1969] as an example for his methods courses, and he calls Chanda a "surrogate researcher." What was your relationship with Chanda like? What kind of a person was he?

ALE: One of the things that must be striking to anybody who reads that paper is how unselfconscious Chanda was. He could not have written much of that stuff if he had been aware of himself. Others among the African assistants were made of quite different metal. One man, Nyirenda, was a natural-born anthropologist. He was tremendous. Chanda saw things in very personal terms and reported them quite unselfconsciously. And he was always in scrapes, he was always looking for money, in all sorts of ways. He was a scamp or a rogue, and I liked him!

Could I just add one point? I think it's brought up by van Donge [1985]. As I recall it what he was trying to say was that although many years had passed since that work had been carried out, much of it still had relevance to the contemporary situation in Zambia, and he was trying to get people to take a look at it again. Now, Magubane says we should have used the concept of social formation. OK, from the time I left until relatively recently, the social scientists at the University [of Zambia] all worked with social formations. The place had become dominated by marxists and neomarxists—to which, as such, I have no objection. But I ask, what did they produce as against the work that was done by myself and my colleagues? And, of that work, I don't feel that there was anything that I or they have to be ashamed of. I suspect that if one were to follow up some of the ideas set out in the politics book they would still have some relevance to the contemporary situation in Zambia. And I find it very disappointing that there was this long hiatus—a period of years—when none of them would get off their behinds, get out of Lusaka and go out to look at a village and see what's *actually* happening, instead of arguing simply in terms of theory. [Marshall] Sahlins, in *Waiting for Foucault* [1966], says something like "Ethnography is anthropology, or it is nothing." What he is stressing is the importance of ethnography. And I sometimes feel myself that perhaps when all the theories are forgotten—and, he says, one of the inevitabilities is that all of these theories will be forgotten—if there is anything that will survive, I think it may be in the ethnography. And by and large I think the work that was done at that time was very, very good ethnography. It's history.

I gave a seminar on the Copperbelt in which one of the members, a man called Eli Devons, a professor of economics and a brilliant man, said at one point, "Your grin belies your objectivity"—in other words, that the paper was objectively presented but my facial expression showed where my real sympathies lay. There was never a question about where my sympathies lay, but in the Copperbelt situation it really wasn't simply a matter of black against white, even though that was in fact a dominant theme. One is almost reminded of Hobbes's situation of every man against the other man. Africans were themselves divided one man against the other.

KAY: So at the end of the Copperbelt work you headed for Australia?

ALE: Yes.

KAY: What took you there?

ALE: John Barnes, who had also worked in Northern Rhodesia, suggested that I might like to go out, and there were personal reasons for finding Canberra attractive at that moment. So I went out on a research assistantship, and Scarlett came out with me. And because the director of the School of Pacific Studies was just establishing its economics department she too was able to get a job.

KAY: So you were married by this time?

ALE: We were married, yes. And so we went off to Tolai country. And I think I've mentioned to you my reception when I arrived at Matupit. That was always a very difficult place in which to work because really it wasn't until the end of our first stint of fieldwork, when we were going back to Australia, that I realized just how much I was really *in.* There were times when we would walk around the village and we'd see a group of people standing around. And as we approached we could see them turning away and almost asking themselves, "What the hell does this guy want with us now?" It wasn't till the very end when the whole village, actually three villages, all got together and put on a tremendous feast for me and Scarlett, and then perhaps most moving of all was, they all lined up, and one by one they all marched past, and presented me with vegetables, shell money, and cash, Australian money. And I felt so small. Something had gelled there.

And then, as I think every anthropologist has found, when you go back the reception is quite different. Nobody ever expected you to come back, and in coming back you show that, in a sense, you're a member of the community. When I went back, when we came back for our second stint, wherever I went around the village women would start wailing, weeping. Naturally, I was puzzled. And the explanation was that my return reminded them of their own close relationships and friendships with men who had been their husbands, and so on, which is a characteristic Tolai response to anything that reminds them of the dead. It always sets off crying, particularly on the part of the women.

KAY: You were teaching at the Australian National University then?

ALE: At the Institute of Advanced Studies. The university then was in two main divisions, the Institute of Ad-

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vanced Studies and what was then called the School of General Studies. General Studies had undergraduates; Advanced Studies had postgraduates only. And our department, I think, had nine, ten faculty. Nine or ten students on their Ph.D.'s. It was a fantastic set-up.

Things happened in Australia two years after anywhere else. They're always bringing up the rear. The students' movement in California—we didn't know it in Canberra till a couple of years later. I was in charge of the department when our students began their protest. But it was utterly derivative; conditions for anthropology students were in many ways quite ideal. I remember one American student who trained at Los Angeles who was going to work in one of the highlands groups. Practice was that you had three or four months, sometimes longer, to prepare yourself for your trip, and before you finally departed you'd be invited to give a paper on your plans, on your project. And this young man started off by explaining that he was going to go into the field without any preconceptions and he was going to learn the language as a native did! And in the department at that time was a man called [W. E. H.] Stanner, one of Australia's most distinguished anthropologists, and he said, ''How long do you propose to spend among these people, 150 years?'' That was a time when people talked in that way—that you can start and learn the customs just as a local child does.

KAY: In *Ethos and Identity* [Epstein 1978] you combined your Copperbelt work with your Tolai work and work on Jews in the United States using secondary sources, and I wanted to ask how you started to relate things and how you got interested in the anthropology of affect [e.g., Epstein 1984, 1992*b*].

ALE: If I can just go back to my adolescence for a moment, when I thought about a possible future it was always a sort of toss-up between anthropology and psychoanalysis, both of which are founded on the notion of being outside. What happened, of course, was that as I moved into anthropology I got caught up in professional concerns—the kinds of problems my colleagues were working on—and so the other interest was sort of left to one side. But I think that in much of my work basically I've been following my nose. That's to say, things would strike me in the field and it would seem to me that they represented a challenge. Here were questions that my own data thrust under my nose, and somehow it seemed I simply couldn't ignore them. And this was how my interest in psychoanalysis was revived. This is the paper that now appears as the final chapter of *Scenes,* the paper on oral aggression [Epstein 1992*a:* chap. 8]. This was in the context of some of the attempts to establish a Central African Federation, which brought Africans and whites into bold confrontation. The Africans saw this as an attempt which, if successful, would entrench European power for all time, leaving them where they always had been—and this at a time when in other parts of Africa countries were beginning to think of independence. These events were followed very closely among Africans in Northern Rhodesia. And this was when I encountered at first hand the ''scare of the poisoned sugar.''

It all started when I got back to the country in 1953 to find that the federation business was really heating up. And I had gone back for a short visit to Bemba country, a part where I was known, and, curiously, had been very well received. Initially I stayed with a chief, Munkonge. Then I went on to another village, where people didn't know me, about 12 miles up the road. It was a time of the year when there wasn't much beer around, and my reception had been distinctly cool. It occurred to me that the best thing to do was go back to the chief's village and collect some calabashes of beer. So the beer was brought. In Central African villages there's a little shelter in the middle called *nsaka,* and that's where I placed the beer. In the ordinary way, you know, when people know beer is flowing, they all flock, but this time there was no flocking—at first nobody came. Gradually, and very sheepishly, a few people appeared, and it was very uncomfortable. I tried to get a conversation going by asking them a few innocuous questions, but I was quite unsuccessful. I thought, ''Well, there's no point in carrying on like this,'' so I went back to my tent. I had with me a young man from another part of Bemba country to carry water and collect firewood and do things like that for me, and he told me that the people feared that I had poisoned the beer. Eventually I went back to the Copperbelt, where my main work lay, and I discovered that this business of poisoned beer was not something that was known only in the rural areas—that rumors had been flying around. In fact, two men were arrested; they were caught printing out a document that purported to come from the House of Lords to the effect that African men who bought sugar and ate it would be rendered impotent—they would be rendered sterile, and their womenfolk would become barren. And this related to a speech made in the Legislative Council by the leader of the Europeans, a man called Sir Roy Welensky, who—I don't quite understand how he could have been so stupid—was saying in effect that if the Africans were not prepared to go along with them over federation, then they would suffer the fate of the American Indians, which the Africans, of course, interpreted as a threat of annihilation. And that was what lay behind these rumors. And for a time—to come back to something I said right at the very beginning about the atmosphere being quite nightmarish—everybody was talking about werewolves, vampires, and saying that all those who had indicated that they were in support of federation would be seized by these vampires and injected so that they lost their will and sometimes carried off and killed. One of the things that interested me was that, although there's been a great deal written about the history of the country around that time, very few seemed to take this seriously. In fact, it was pretty well swept under the carpet, and I felt that intellectually this was quite dishonest. I knew enough about psychoanalysis at the time to feel that if I knew a little more, perhaps I might find some way of getting to grips with the

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problem. And then I went off to Australia. I remember Scarlett and I were driving from Canberra down to Melbourne, and all the time I was thinking, "I've got this sabbatical coming up—what am I going to do?" Most people spent their sabbatical joining another department. And I thought, "What's the point of going to another department to do the same sort of thing I've been doing for years?" And then it suddenly hit me: "Why don't I use this opportunity to try and learn a little more about psychoanalysis and see if it will help me to get to grips with this material?"

In Belfast I had known a friend, a neighbor, who had gone on to become a very successful analyst, so I got in touch and went up to see him. I put my problem to him, and through him I was able to spend the time as a visitor at the clinic of Anna Freud in Hampstead. And that was a very exciting period, because it seemed suddenly as though all kinds of ideas that had been getting buried were beginning to pour out. And suddenly I found I was writing this paper. It was too long. I couldn't find a publisher for it at that time. Eventually I was persuaded to do an abbreviated version which I did publish in an American periodical that dealt with psychoanalysis [Epstein 1979]. But I felt that at some point I would really like to bring out the paper as a whole. When I finally published it, it seemed I think in some ways a bit oversimple, but it was very important for me because increasingly I became interested in affect. Again, this was not so much a matter of thinking it out in the abstract or theoretical terms; it was because I had observed behavior in which emotion was being quite clearly expressed. There was a dispute on the island of Matupit, and I was sitting in the crowd listening to what was going on and seemed to be following it quite well. Then suddenly the whole thing ended and the assembled village was crying. It seemed to me something very important was going on, and, again, I felt this was something that I ought to pursue. I was, in a way, fighting against my anthropological past, because I was brought up in school—not just Manchester but British anthropology as a whole—where emotions, psychology, anything to do with psychology was a dirty word. And, although Max Gluckman had himself undergone analysis, he never allowed us to introduce any of it into our professional work. At the end, however, it caused a great deal of amusement around the place that his book on the Zulu King Shaka finished up with a sort of psychoanalytic explanation that, as I recall it, turned on Shaka's impotence.

KAY: In *Ethos and Identity,* which has actually had a very wide reading outside of anthropology—

ALE: I suspect that that'd be my most successful book in the sense of most widely read.

KAY:—there's the study of the Copperbelt, there's the study of the Tolai, and then what one reviewer—I think unfairly—called a study "more in the nature of an anthropological footnote" [Kuper 1979:243] on the role of grandparents in ethnic identity. You started off the interview by talking about your grandparents.

ALE: Yes, yes. Very important.

KAY: I wonder in what ways they influenced your sense of self in terms of ethnicity?

ALE: Anthropology in some ways is a very personal kind of exercise in which, often, one is saying much more about oneself than most people would readily find acceptable. Particularly in the earlier years, we were all supposed to be objective, and so on, and so to write out of one's own psyche was a cardinal sin. But I can see nothing wrong in that. I think that unless one taps one's own experience one is not going to get too far. The point is to be able to exploit yourself, as it were, to make use of insights which spring not from your fieldwork but from your personal experience. Insofar as you do that it gets to be more than a purely personal experience you're relying on; others will see to what extent it's an idea worth pursuing. Clearly, my chapter on grandparents drew on my experience of my own grandparents, my relations with them. And others have found that quite a stimulating idea, because, by and large, grandparents are ignored. One social psychologist, a colleague at Sussex, after reading it started to discuss the matter with his own students, and he told me that indeed their grandparents had had a very important bearing on their own development. And many of the other things that I referred to in this paper on oral aggression, many of these associations must have sprung from my own unconscious, and again, I see nothing wrong with that, if it's brought out into the open so that other people can test it out themselves and see that it's not just my own personal experience. With the recent stress on reflexivity, one might expect much more of this.

KAY: What is it about grandparents that is the key?

ALE: [A. R.] Radcliffe-Brown, I think, got the crucial idea that it's the alliance of alternate generations cutting across the opposition of adjacent generations. With your parents, things are often very tricky, full of tension, but with grandparents, much of that tension has gone. There is a danger here when one starts generalizing about human relationships, because it doesn't always work out that way. Not all grandparents are good grandparents, and not all children see their grandparents in that way. Very much depends on the age at which the relationship is established. The older you are, the more difficult it is to stand screaming youngsters around the place, so, you know—All one can do is point to certain recurring features.

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With Scarlett Epstein at a dance at Matupit.

KAY: What has it been like being married to another prominent anthropologist? What has been Scarlett's influence on your work?

ALE: On the first question I think the answer is really simple: fine. Which is perhaps implicit in what has been a splendid and enduring partnership. Our interests do not overlap, and to that extent we complement one another. Scarlett really considers herself a development economist. I am more of a straight anthropologist. I think it works well!

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The Archaeology and Anthropology of Mining: Social Approaches to an Industrial Past[1]

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The mining and manufacture of metals have formed a major aspect of production and social reproduction over the past 6,000 years and have employed millions of people throughout the Old and New Worlds. All industrial societies were erected on foundations of metal and still require vast quantities of metals for their existence. Substantial areas of the earth's surface have been transformed by mines themselves or by the pollution that was until recently a common adjunct to mining and metallurgy. Mining and metallurgy in preindustrial settings were often imbued with rich symbolic content the study of which illuminates various aspects of culture that have little to do with metallurgy per se. Only within the past decade, however, have archaeology and cultural anthropology embarked upon some innovative studies of the social, spatial, and ideological dimensions of past and present industrial communities. Whereas many cultural features associated with mining and metallurgy are known from ethnohistoric documents or ethnographic fieldwork, archaeology has had to develop its own devices to assess the material components of an industrial past. The use of ethnohistoric and archaeological evidence as contrasting data sets and the attempt to integrate archaeological, anthropological, and historical approaches fit squarely within current efforts to develop a socially based, human-science approach to the study of the past.

Recognizing this interdisciplinary dynamic, with David Killick (Arizona) we organized an international conference whose express aim was to integrate these diverse approaches and to shift scholarly perspective in a new direction. Having applied to the Rockefeller Foundation, we were ultimately invited to hold our conference at the Foundation's Bellagio Study and Conference Center at Lake Como, Italy, July 22–26, 1996. Twenty-two scholars (9 women, 13 men) from five countries (Australia, Cyprus, New Zealand, the U.K., and the U.S.A.) presented papers spanning the past 4,500 years; the geographical coverage consisted of the Americas (including the Arctic), Australia and New Zealand, Egypt, sub-Saharan Africa, Europe, the Mediterranean, China, and Southeast Asia. The immediate goal of the conference was to acquaint scholars with the current status of research in other fields and to explore how these closely related but still diverse approaches might be better integrated. The intention of the organizers was always to raise new questions, to stimulate new ways of thinking about mining communities, and to provoke participants into reconsidering long-held notions in their respective fields. We believe that the intermingling of divergent scholarly perspectives—the exposure of one discipline to the concepts and methodology of others—will significantly advance the study of an industrial past and lead to a more dynamic, social synthesis. The medium-term goal is to publish substantially revised papers from the conference, which will mark an important advance toward a more socially aware, interpretive, interdisciplinary approach to the archaeology and anthropology of mining.

CONFERENCE THEMES

The history of mining and metallurgical technology is a topic well-represented in both the academic and the popular literature (e.g., Craddock 1995, Tenfelde 1992). Archaeologists have considered the technology of mining and the productive process overall (e.g., Ehrenreich 1991, Hardesty 1988) but have yet to integrate fully into their work the socioeconomic, spatial, and ideological dimensions of past industrial cultures (see Childs and Killick 1993). Anthropologists have considered economic, ideological, and organizational aspects of mining (e.g., Godoy 1985, 1991; Nash 1993) but often from opaque Freudian or structuralist perspectives (e.g., Eliade 1978, Taussig 1980). Social historians of mining and some historical archaeologists, however, have written extensively about gender, class, age, spatial segregation, and other issues directly relevant to the themes of this conference (e.g., Herbert 1993, Hosler 1995, Simmons 1989).

In order to harmonize these approaches and to orient divergent perspectives in an explicitly social direction, our goal in Bellagio was to focus specifically on the concept of the mining or metallurgical community as revealed in the material, ethnographic, technological, and literary or ethnohistoric records of various cultures worldwide, both past and present. Focusing on the social context of the mining or metallurgical community offered a unique way to engage the wide range of interests and expertise represented at the conference. The general, overlapping themes were gender, power strategies, labor exploitation, imperialism and colonialism on the mining frontier, production and social reproduction, and production and technology. Such topics have relevance to any country whose culture and history are bound up with the mining and exploitation of natural resources; they also highlight the significance of archaeological or ethnographic fieldwork to cultural heritages

1. © 1997 by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3802-0006$1.00. We are grateful to David Killick (Department of Anthropology, University of Arizona) for his contribution to the organization of the Bellagio conference and to various ideas mentioned in this report. We also thank everyone at the Rockefeller Foundation's Bellagio Study and Conference Center, who facilitated our stay and made possible a very rewarding conference.

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and ethnographic histories and to understanding and negotiating difference across changing societies.

The literatures on past mining and metallurgy have yet to achieve a synthesis of the technical, economic, and social aspects of these activities. Archaeologists and archaeometallurgists have developed an impressive capacity to reconstruct the *chaînes operatoires* of past miners and metalworkers from the surviving material evidence and through them to document the growth of technical knowledge. As yet, however, they have made few attempts to investigate the social reality of early mining towns or camps. Cultural anthropologists and historians have produced some extremely rich accounts of the social life of miners but seem largely unaware of the wealth of evidence to be found in material culture. The organizers encouraged participants to consider carefully and to emphasize certain more specific themes with the express intention of getting scholars from different disciplines to make use of the full range of evidence at their disposal: (1) examining social structure and social interaction in mining towns and camps, (2) considering mining as an aspect of imperialism, colonialism, and frontiers, (3) understanding earlier technical practices and technological knowledge, and (4) reconstructing the social organization of metal production from material remains.

Archaeologists and archaeometallurgists have made extensive use of material remains, both in the field and in the laboratory, and are often able to reconstruct the nature of work in past mines or metallurgical facilities in extraordinary detail. Evidence of long-distance trade and/or technology transfer has been found in the composition and microstructures of various metal artifacts. At the conference, papers dealing with these topics were presented by Robert Ehrenreich (Canada/Arctic), Vasiliki Kassianidou (Cyprus, Iron Age), James D. Muhly (Cyprus, Bronze Age), Robert Murowchick (China, Bronze Age), and Vincent Pigott (Thailand, Bronze Age). Historians of mining, however, have rarely made appropriate use of the available material evidence to reconstruct either the working or the home environments of past miners and metallurgists, and the historians who took part in the conference were encouraged to incorporate more physical evidence into their presentations. There is growing interest in a number of fields—anthropology, industrial archaeology, art history, folklore, rural history—in the use of material culture as historical documents. Of widespread current interest in the social sciences, this issue was treated in conference papers by Terry Childs (Africa), Susan Lawrence (Australia), Stephanie Moser (mining as metaphor for archaeology), and Alexandra Simmons (western/northwestern U.S.A., New Zealand).

Conversely, issues of power and resistance, poverty, and risk are central to much that has been written about mining by social and labor historians and by cultural anthropologists. Miners in the recent past were prone to sudden death and traumatic injury, were generally poorly paid and housed, and were often ruthlessly exploited by powerful employers. In many precapitalist mining camps they were slaves or convicts. These conditions led to frequent strikes or revolts, which constitute a recurrent theme in the literature on mining. Clearly, the enormous benefits that metals have bestowed upon humankind in historic times must be balanced against the toil and misery by which they were won. These issues were explored at the conference in papers by Robert Ehrenreich (Canada/Arctic), Peter Bell (Australia), Donald Hardesty (western U.S.A.), Justin McCarthy (Tasmania), John Rule (Cornwall, England), and Thomas Sheridan (southwestern U.S.A.).

The social structure of mining towns and camps cannot, however, be reduced to simple class divisions. In the colonial mining settlements of Australia and the Americas, linguistic and ethnic divisions within the working class also structured the communities, and it was not uncommon for miners from a particular ethnic group to constitute a ''labor aristocracy.'' In all but the most rudimentary and most isolated mining camps there were also gender divisions, with a wide variety of domestic arrangements. Papers at the conference that dealt with gender and/or ethnicity were presented by Peter Bell (Australia), Terry Childs (Africa), Eugenia Herbert (precolonial Africa), Susan Lawrence (Australia), Stephen Shennan (Europe, Bronze Age), Thomas Sheridan (southwestern U.S.A.), and Alexandra Simmons (western/northwestern U.S.A., New Zealand)

Whether such specific social and cognitive issues may be investigated without documentary evidence provoked interesting debate at the conference, particularly from prehistorians. To provide a conceptual bridge between the prehistorians, on the one hand, and the social historians and ethnographers, on the other, we had invited several historical archaeologists who have worked in 19th–20th-century mining and metallurgical communities in North America and Australia/Tasmania (Susan Cheney, Robert Ehrenreich, Donald Hardesty, Justin McCarthy, and Patrick Martin). Some of the papers they presented, using a mixture of documentary and archaeological evidence, explicitly tackled questions related to class, power, and ethnicity and whether such factors may be recognized in the spatial patterns and distribution of material culture in excavated mining towns. Papers by Aedeen Cremin (Spain, Roman), Carol Meyer (Egypt, Byzantine), and Ian Shaw (Egypt, Bronze Age) considered these themes in precapitalist contexts, whilst Vasiliki Kassianidou (Cyprus, Iron Age), Robert Murowchick (China, Bronze Age), James D. Muhly (Cyprus, Bronze Age), Vincent Pigott (Thailand, Bronze Age), and Stephen Shennan (Europe, Bronze Age) reported on excavations of mining settlements in strictly prehistoric contexts.

Studies of mining and metallurgy have the capacity to expand and alter our understanding of processes of colonialism and imperialism and the challenge of frontiers. Deposits of metal ores are unevenly distributed over the surface of the earth, and empires often have established mining colonies at a great distance to extract metals for home consumption. Several papers specifically examined issues related to mining colonies or col-

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onies established to trade in metals by precapitalist empires: Peter Bell (Australia), Aedeen Cremin (Spain), Eugenia Herbert (Africa), Patrick Martin (midwestern U.S.A.), Carol Meyer (Egypt), and Ian Shaw (Egypt). These studies should produce an instructive comparison of mining in ancient and modern polities, a comparison that will be of interest to anyone who deals with issues related to colonialism, postcolonialism, and imperialism.

We now consider briefly how these themes are treated within their disciplinary contexts and how the whole may be linked discursively to a more unified, social approach to an industrial past.

ETHNOHISTORY AND ETHNOGRAPHY

In ethnohistorically attested and modern mining or metallurgical communities, certain ecological, demographic, and politico-economic patterns recur. Mining populations, for example, tend to have high fertility, not least because of higher infant and adult mortality or debility but also as a result of early marriage for women, low child-rearing costs, a minimum number of females in the labor force (at least outside the home or brothel), and the existence of menstrual or sexual taboos (Godoy 1985:206; Herbert 1993:72–73). At most stages, mining is a labor-intensive activity, and the successful mining operation necessitates a reliable and inexhaustible pool of labor. Historical archaeologists working in the American Northwest have demonstrated from material and documentary evidence alike that the frontier prostitute was a significant and ever-present component of this labor pool (Simmons 1989).

The politico-economic organization of permanent mining communities, at least, is conditioned in part by locational factors (isolation and inaccessibility), which, at least in historically recent periods, have given rise to self-contained communities as diverse in type as unstructured, open-system camps (Bolivia, Colorado), paternalistic company states (Arizona, Chile, Peru), and the hermetic, totalitarian institutions of South Africa (Godoy 1985:205). In order to obtain not only capital and labor but also food and other supplies for the physical operation of a mining camp, there may be attempts to integrate the surrounding regions into a coherent economic sphere.

The social organization of mining is conditioned partly by the physical and/or sociocultural isolation of mining communities and partly by the harsh working conditions and labor requirements of the extractive and productive phases of mining (Godoy 1985:205). To cope in such situations, miners adopted either a strategy of opportunity—to maximize their resource gains—or a strategy of resiliency—to survive under sudden environmental or politico-economic change. Whereas the maximizing strategy might reduce variability in material culture (e.g., the rapid adoption of the most effective, standardized tools and technology), the resiliency strategy might be expected to increase variability (e.g., experimenting with ways to cope under stress, changing the organization of labor or tools to cut mining costs) (Hardesty 1988:112–14).

Such considerations offer another possible way to bridge the gap between ethnohistoric or ethnographic and archaeological approaches to the study of mining communities and industrial society. How else do the information and evidence established by ethnographers and historians contribute to an understanding of prehistoric mining and material culture? How different are the approaches—often based on production and technology—typically used by archaeologists?

ARCHAEOLOGY

The mining community provides the focal point for information about human activity on a mining frontier. The social interactions that occurred and the social and ethnic stratification that developed in such contexts are evident to varying degrees in artifact morphology and settlement patterns, in the size, age, and sex composition of the human population, in the extent and quality of prestige items and imports, and in other industrial, ceremonial, or habitational features preserved in abandoned or even buried mining "sites." The social and economic organization of mining communities can of course only be inferred from these material remnants, their spatial arrangement, and their contextual coherence within a framework built upon the productive, technological, and ideological dimensions of mining. Where documentary evidence is preserved, another arena of investigation may be opened.

The material record of mining sites, rural/agricultural villages, and production/distribution centers is well suited to a study of archeological variability from a broad comparative and chronological perspective (Knapp 1996*a*). The key factor here is to relate mining "settlements," directly visible archaeologically, to the more abstract concept of mining "communities" (Hardesty 1988:101). Within many mining communities, the variability that results from differences in ethnicity, place of origin, status, gender, and class may be visible in the material record and thus should form one of the primary areas of interest in an archaeology of mining. The individualism typical of miners might also produce high variability in the assemblages of mining households, in contrast to the cooperative behavior—and more limited variability—that might typify the material assemblages of the classic peasant or agricultural community (Hardesty 1988:103–4).

Despite their social and spatial remoteness, mining communities—past or present—are inevitably linked into broader social, communications, transport, and economic networks by virtue of their ability to supply a raw material in demand to a regional (or world) system. Links between the mining frontier and the outside world involve three types of interaction sphere (Hardesty 1988:1–5): the material (transport networks, including food and supplies), the social (labor pool, in-migration, gender ratio of population), and the informational (symbolic, financial, ideological). These

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interactions too may be visible in the archaeological record in the form of settlement layout, trash dumps, luxury goods and "mass-produced" commodities, imports, skeletal remains, cult paraphernalia, etc. Such interaction spheres obviously would have overlapped: the transport of primary smelted copper from mines to production, refinement, or distribution centers, for example, may have necessitated travel through regions controlled by different communities. This would have involved the negotiation of social relations beyond those required for the supply and distribution of a resource in demand (Lechtman 1991:46).

OTHER CHALLENGES

At least four challenges became evident during the presentation and discussion of papers at Bellagio. Here they can be only briefly noted.

Mining and agriculture. Mining interests and activity may complement or compromise agricultural interests and activities. This is a case of "informal" mining (MacMillan 1995), where mining is combined with other production activities. Here we may cite the overlapping modes of production in the African setting, where mining is often a seasonal activity and agriculture the norm. But in 20th-century Cyprus, to take another example, agriculture has often suffered because mining companies discouraged drilling for water or any other activity that might distract potential workers from filling the company's insatiable need for cheap labor. As an economic activity, mining is very sensitive to changing commodity prices on world markets, and mining companies will brook no interference from other modes of production if it is within their power to do so. The traditional agricultural base of Cypriot villages diminished dramatically during the middle decades of the 20th century, when mining activities reached their production peak.

The mining landscape. Within the mining community, various factors dictate the appropriation of the local landscape: natural resources, agriculture, production and dwelling sites, communications, ideology or religion. The relationship between mining and settlement may vary according to the scale and organizational level of production (Raber 1987:301–2). Thus the intermittent small-scale, localized production of the Medieval period on Cyprus contrasts significantly with the larger-scale industries and major labor forces typical of the Imperial Roman period. Whenever the production of copper exceeded the local or regional scale, mining communities were perforce linked into interregional, national, or global networks of communication and exchange; invariably this impacted the mining landscape in the form of imports, migrant labor, possibly even new settlement plans in the face of other socio-structural changes.

Gender and the archaeology of mining. As Susan Lawrence pointed out in her conference abstract, "To make any meaningful and defensible inferences [about gender] involves establishing the social context of the material remains and under what conditions any associations might pertain." The archaeology of mining has for so long presented men as gender-neutral that it is nearly impossible to separate mining terms from their androcentric essence or to break down approaches that privilege techno-environmental phenomena. Publications and fieldwork of an entire generation of social historians, anthropologists, and historical archaeologists demonstrate that women were almost always present in the mining community, working in both industrial and domestic contexts. In the tin mines of Bolivia, for example, miners emphasized how firmly the women were committed to the historic confrontations between the workers and the bosses (Nash 1993). In precolonial African iron production, women provided essential labor for the preparation of ores and fuels, even if they were usually excluded from the overall metallurgical process because they lacked access to supernatural assistance (Herbert 1993). A more focused, less biased examination of written and material resources will help us to challenge the dominant androcentric portrayal of the mining experience and to recognize that both women and men were fully integrated into the sociocultural mainstream of the mining community.

Technology, mining, and social process. By now it is nothing more than a truism to state that innovations in technology had deep-seated and long-lasting social and ecological effects, placing constraints as well as conferring benefits on mining and miners. Indeed, the technology of mining involves much more than extracting minerals or metals from rocks and crushing and smelting ores. Technology as a social process cannot be separated from the people involved, their daily life and human abilities, their ideology and beliefs, and their capacity to negotiate complex social, economic, and political relationships within the mining context (Childs and Killick 1993). A rich body of symbolism, based in part on the conspicuous and prestigious nature of metals such as copper or gold, together with a complex set of rituals, offered multiple, historically situated images to rural cultivators who first became involved with the extraction of native ores, to others who were enslaved by colonial regimes, and to still others who became industrial workers answering the call of supply and demand from a global economy.

CONCLUSION

As archaeologists inclined to look at the active role played by material culture in people's past social practices (variously labeled "postprocessualism" or "interpretive archaeology" but better termed "social archaeology" [Knapp 1996*b*]), we feel that there are other directions that might be taken in examining the nature of a mining community—for example, how social life and settlement were interlocked and how the whole social entourage of the mining community created, engaged, exploited, idealized, modified, and—in most cases—abandoned the mining landscape. Can the accounts presented by social historians and historical archaeologists inform prehistorians about individual hu-

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man agency and historical process, about gender, cognition, ideology, and *mentalité*? Would everyone agree that archaeology is social in nature, that social practices are concerned with meanings, and that social interpretations deal more with understanding than with explanation?

The archaeological record has an "infinite capacity to surprise" (Wylie 1993:15) and to shape or constrain our interpretations of the past. Archaeological interpretations, moreover, will continue to have many voices even as we aspire to find common ground amongst different archaeologies. The "meanings" of material things in the past were never fixed in the objects themselves but instead were "read" into them from other experiences and expectations and then acted upon. Social life, therefore, is enacted within material conditions, and these material conditions are constantly reworked according to different people's interpretations of material reality. The archaeology of mining requires us to examine many different factors, social and cultural as well as physical and technical. By relating information on mining settlements, agricultural resources, communications routes, and gendered activities, we may view technology in its social context and sharpen our vision of the mining landscape.

Mining as a politico-economic activity had the capacity to establish social relations between individuals that transcended community boundaries and transformed regional interactions. Mining villages were often built rapidly and carelessly to satisfy entrepreneurial or state needs; occasionally they evolved from agricultural villages in cycles of boom and bust, when villagers and farmers became urban workers or industrial pawns. The cycles of mining and agriculture in many mining regions suggest that we must concentrate not just on mining as an activity but also—or perhaps instead—on the history of the people who made up the mining community.

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Rethinking the Energetics of Bipedality[1]

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The origin of hominid bipedality has been the focus of considerable research in human paleontology (e.g., Carrier 1984, Hewes 1961, Lovejoy 1981, Wheeler 1991). Increased efficiency of transport has often been suggested as a factor contributing to the evolution of upright movement (McHenry 1991, Pilbeam 1986, Washburn 1963), but remarkably few studies have directly examined the energetic costs of human bipedal movement in a comparative, evolutionary context. Therefore there remains no consensus regarding the relative energetic benefits probably associated with the evolution of hominid bipedality (see Leonard and Robertson 1995, Steudel 1996).

The most extensive data base on the comparative energy costs of locomotion has been compiled by Taylor

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and colleagues (Taylor, Schmidt-Nielsen, and Raab 1970, Taylor, Heglund, and Maloiy 1982). Taylor, Schmidt-Nielsen, and Raab (1970) showed that human *running* was much less energetically efficient than moving quadrupedally at the same speed. Subsequently, Taylor and Rountree (1973) demonstrated that in two primate species (chimpanzees and cebus monkeys) there were no differences in the energy costs of moving quadrupedally or bipedally. These results led Taylor and colleagues to conclude that the evolution of human bipedality could not be attributed to increased locomotor efficiency.

Later work by Rodman and McHenry (1980) questioned this conclusion, noting that human bipedal *walking* is at least as energetically efficient as generalized quadrupedal locomotion and more efficient than chimpanzee quadrupedality. Recent work by Steudel (1994, 1996) supports the arguments of Rodman and McHenry that modern human walking is more efficient than quadrupedal locomotion; however, she has concluded that efficiency is an unlikely explanation for the origin of bipedality, since it appears that the earliest australopithecines did not possess the complete suite of morphological adaptations necessary for efficient bipedal movement.

What is notable about most of the previous studies of the energetics of human bipedality is the similarity in their approaches and in the sources of data brought to bear on this issue. We believe that this relatively narrow focus has limited understanding of the question. Consequently, in this paper we offer a different perspective, drawing on additional data from primate and human ecology.

Most of the previous studies on the comparative energetics of bipedal and quadrupedal locomotion have drawn on relatively little of the available information on the energetics of human movement. Indeed, with the exception of Steudel's (1996) recent work, all previous studies have relied on a single source for information on the energy costs of human movement, Margaria et al. (1963). The problem with this is twofold; the Margaria study examined only two individuals, and it focused on energy costs at running speeds. Hence, much of the work done so far on the evolution of bipedality has not examined what is a fairly large body of literature on human locomotor costs for both walking and running (e.g., Coates and Meade 1960, Cavanagh and Williams 1982, FAO/WHO/UNU 1985, McDonald 1961, Norgan 1996, Zarrugh and Radcliffe 1978). In this study, we use information recently synthesized by the World Health Organization to examine energy costs of bipedal walking over a wide range of speeds and body weights (FAO/WHO/UNU 1985). Additionally, in contrast to most previous research, this study will examine energy costs of locomotion in kilojoules (kJ) and kilocalories (kcal), units more familiar to most readers than liters of O_2.[2]

2. Where necessary energy costs were converted from liters of oxygen to kJ or kcal assuming a respiratory quotient of 0.90 (i.e., 4.92 kcal/liter or 20.6 kJ/liter [McArdle, Katch, and Katch 1986]).

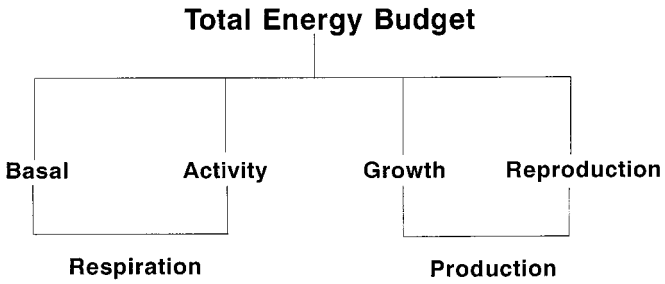


FIG. 1. *Components of an animal's total energy budget. Respiratory (or maintenance) energy costs are those necessary for keeping the animal alive on a daily basis; they include basal metabolism and energy expended for activity and thermoregulation. Productive energy costs are those required for growth and development and reproduction (e.g., pregnancy and lactation).*

A second limitation of earlier studies has been that they have evaluated the potential benefits of bipedality by focusing almost exclusively on physiological efficiency, that is, energy costs per unit weight per distance traveled (e.g., ml of O_2/kg · meter). While this approach is useful for comparing energy costs across animals of different sizes, as in the comparative work of Taylor et al. it does not effectively measure what is "adaptively" important about differences in energy costs. Indeed, what is important from an evolutionary perspective is the total amount of energy that can be allocated to the production of surviving offspring. As is shown in figure 1, this component of an animal's or population's energy budget is denoted productive energy (for growth and reproduction) and is differentiated from maintenance energy expenditure, the costs associated with keeping the individual alive on a day-to-day basis. Adaptations that lead to significant reductions in maintenance costs (i.e., basal and/or activity energy expenditure) should allow for more energy to be allocated to growth and reproduction. Thus, for locomotor costs, the same increase in efficiency is likely to be adaptively more important for the animal with the larger range, since the greater efficiency will produce a much larger reduction in the animal's maintenance energy budget.

This issue becomes important when considering human evolution in that it seems likely that the day ranges of early hominids expanded during the Pliocene as ecosystems became more open and food resources more patchily distributed (Foley 1992, Foley and Lee 1989, Vrba 1988). Such an interpretation is supported by the data on day ranges for extant primates and human hunter-gatherers. As may be seen in figure 2, the day ranges (km/day) of modern hunter-gatherers (male and female data for the !Kung of Botswana and the Ache of Paraguay) fall completely outside the range of modern apes (see Leonard and Robertson 1992, 1997). The average for human foragers is 13.10 km/d, as compared with 1.77 km/d in the apes ($t = 4.44$, $P = 0.01$).

The consequences of these differences are explored in

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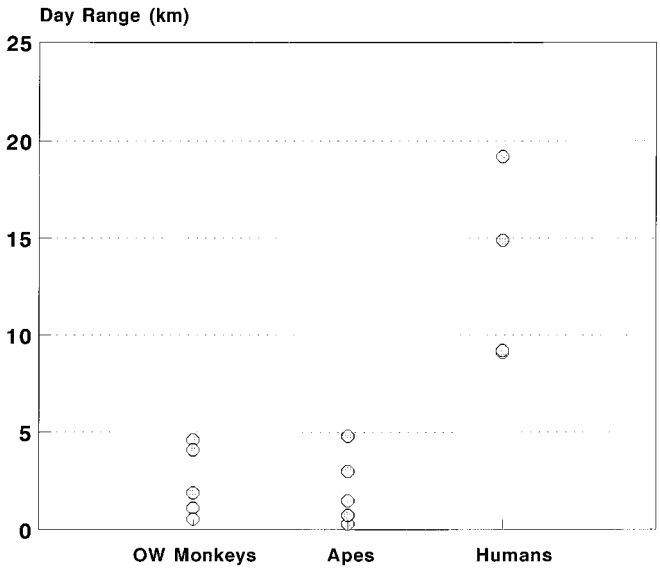


FIG. 2. *Day ranges (km/day) of selected nonhuman primate species and human hunter-gatherers (!Kung and Ache). Data were derived from Leonard and Robertson (1997).*

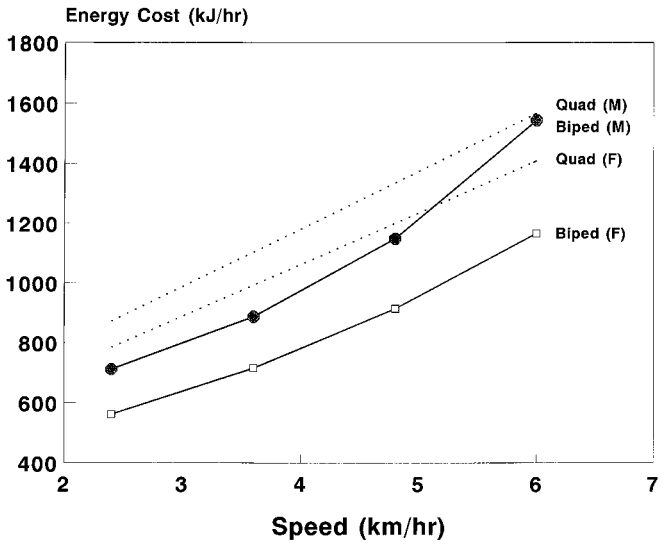


FIG. 3. *Energy costs (kJ/hr) of bipedal vs. generalized mammalian quadrupedal locomotion for males and females at speeds of 2.4 to 6.0 km/hr. Values are average costs for women of 50, 60, and 70 kg and men of 60, 70, and 80 kg. For both males and females, bipedal energy costs are less than quadrupedal costs over the entire range of speeds.*

table 1, which presents a hypothetical example of locomotor energy costs of two 60-kg mammals, one with the day range of a hunter-gatherer and the other with the range of an ape. In each case, the "average" estimate is derived from Taylor, Heglund, and Maloiy's (1982) "general quadrupedal mammalian" locomotion equation, and the "efficient" estimate arbitrarily assumes a 15% greater locomotor efficiency than average. For the larger day range, the increased efficiency produces a 438 kJ (105 kcal) reduction in total energy cost, equal to about 7% of the animal's basal needs. In contrast, the energy savings with the ape-sized day range is 68 kJ (16 kcal), about 1% of basal metabolic requirements.

Thus, it seems clear that the energetic benefits of moving more efficiently will tend to be greater for animals that move longer distances over the course of the day, all other things being equal. The question that we now need to address is *how much* of a savings a hominid was likely to accrue by shifting from quadrupedal to bipedal locomotion. Figure 3 presents the mean energy costs (kJ/hr.) of walking at different rates (from 2.4 to 6 km/hr.) compared with the predicted quadrupedal requirements based on Taylor, Heglund, and Maloiy's (1982) mammalian model. Averages were calculated on the basis of male body weights of 60 to 80 kg and female weights of 50 to 70 kg. For both sexes, bipedal energy

TABLE 1
Energetic Consequences of Differences in Day Range for a 60-kg Mammal with a 15% Difference in Locomotor Efficiency

	Locomotor Energy Costs (kJ)				
Range Size	Average	Efficient	Difference	BMR (kJ)[a]	% of BMR[b]
Human	2,915	2,477	438	6,314	7
Ape	450	382	68	6,314	1

NOTE: "Average" locomotor costs were estimated from Taylor, Heglund, and Maloiy's (1982) "general mammalian" equation. "Efficient" costs were estimated as 15% less than the "average" costs.
[a]Basal metabolic rate (BMR, kJ/day) estimated from Kleiber (1961).
[b]% of BMR = locomotor energy difference as a percentage of BMR.

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TABLE 2
Percentage Difference (by Sex) in Energy Costs of Bipedal versus Generalized Mammalian and Primate Quadrupedal Locomotion at Speeds between 2.4 and 6.0 km/hr.

Comparison	Percentage Difference	
	Males[a]	Females[b]
Bipedal vs. generalized quadrupedal	−13.3	−24.6
Bipedal vs. primate quadrupedal	−40.5	−47.6

NOTE: Estimates of generalized mammalian and primate quadrupedal energy costs derived from equations of Taylor, Heglund, and Maloiy (1982).
[a]Estimates for male body weights of 60, 70, and 80 kg.
[b]Estimates for female body weights of 50, 60, and 70 kg.

costs are lower than the quadrupedal costs over the entire range of speeds. Reductions relative to the mammalian model are greater among the females.

Table 2 summarizes the percentage differences in energy costs over the entire range of speeds of bipedal movement relative to the mammalian quadrupedal model (as presented in figure 3) and the primate-specific quadrupedal model also presented in Taylor, Heglund, and Maloiy (1982). Relative to the mammalian model, human bipedality is 13% less expensive for males and 25% less expensive for females over the range of normal walking speeds. Compared with the primate model, the energy savings is even more striking, with bipedal walking being 40–50% less expensive in both sexes. These results are notable because the differences in energy costs between bipedal and quadrupedal walking are considerably larger than those previously suggested by Rodman and McHenry (1980). The greater energy savings among females likely reflects the fact that their metabolic costs in general are lower than males', in part because of differences in body composition (see Arciero, Goran, and Poehlman 1993, Nelson et al. 1992, Poehlman and Toth 1995). Relative to men, women have a higher proportion of adipose tissue, which has substantially lower metabolic demands than muscle (McArdle, Katch, and Katch 1986).

Extending the analyses a step farther, Figure 4 shows differences in predicted energy costs of bipedal versus mammalian quadrupedal movement for a hominid the size of *Australopithecus afarensis* for several different day ranges (female 29 kg, male 45 kg, after McHenry 1992). The rate of movement is assumed to be intermediate between that of a chimpanzee and that of a modern human, about 3.6 km/hr. (see Leslie, Bindon, and Baker 1984, Leonard and Robertson 1997, Wrangham 1977). The total savings associated with bipedality (fig. 4, *top*) ranges from about 100 kJ/d (25 kcal/d) for the 3-km day range to 450–620 kJ/d (100–150 kcal/d) for a 15-km range. Figure 4, *bottom,* expresses these energy

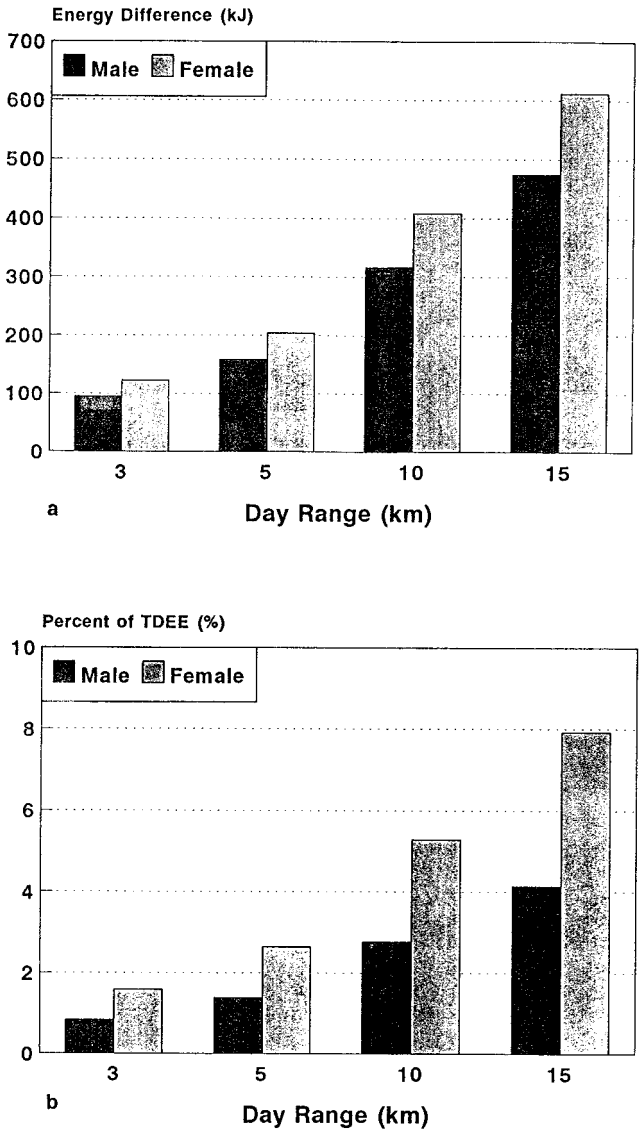


FIG. 4. *Energy savings of bipedal vs. generalized mammalian quadrupedal locomotion for* Australopithecus afarensis *males and females at different-sized day ranges.* Top, *total energy savings (kJ/day);* bottom, *percentage of total daily energy expenditure (TDEE). TDEE was estimated as 2·BMR, and* A. afarensis *body weights were estimated as 45 kg for males and 29 kg for females after McHenry (1992).*

savings as a percentage of estimated total daily energy expenditure. The analyses assume a total energy budget of two times basal metabolic requirements, a reasonable estimate given recent studies of total daily energy expenditure in subsistence-level human populations and nonhuman primates using the doubly-labeled-water technique of indirect calorimetry (e.g., Diaz et al. 1991, Heini et al. 1989, Nagy and Milton 1979). For smaller day ranges (3–5 km) the savings amounts to 1–

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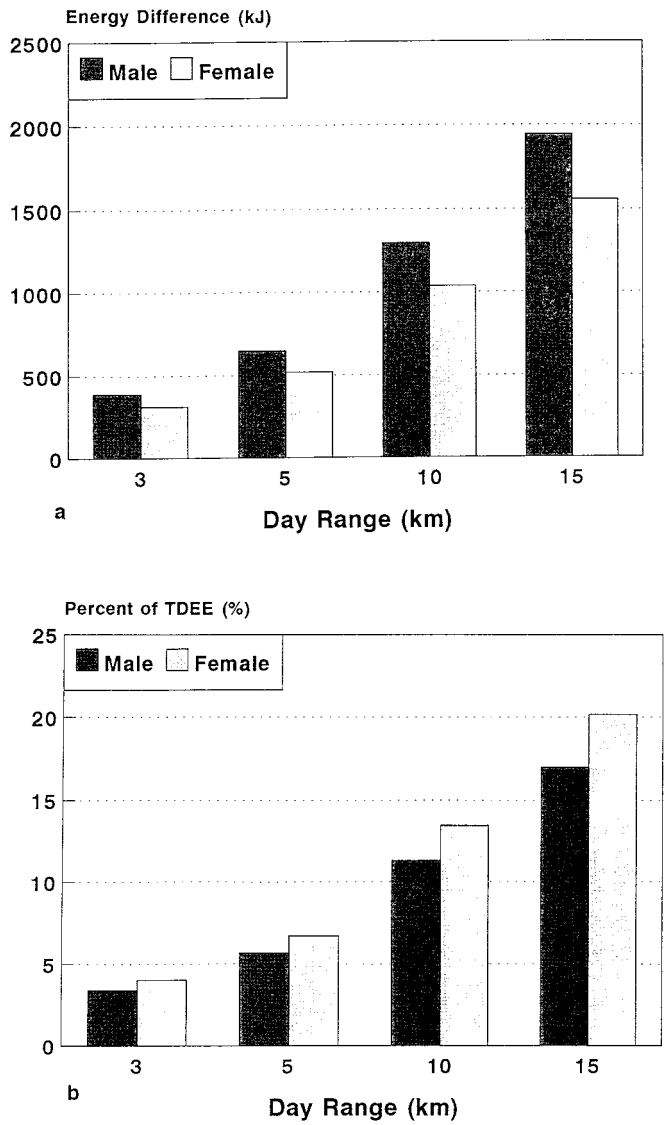


FIG. 5. *Energy savings of bipedal vs. primate quadrupedal locomotion for* A. afarensis *males and females at different-sized day ranges.* Top, *total energy savings (kJ/day);* bottom, *percentage of total daily energy expenditure (TDEE). TDEE was estimated as 2 · BMR, and* A. afarensis *body weights were estimated as 45 kg for males and 29 kg for females after McHenry (1992).*

2% of total daily energy expenditure, whereas for the larger ranges it accounts for 4–8%. Thus, even for small-to-moderate-sized foraging ranges, the energy savings of bipedality is relatively substantial.

Comparing bipedality with primate quadrupedal movement using the same set of assumptions produces larger energy differences. Figure 5, *top,* shows that the benefits of bipedality range from about 400 kJ/d (100 kcal/d) for a 3-km day range to 1,500–2,000 kJ/d (350–500 kcal/d) for a 15-km range. These differences are quite substantial when expressed as a percentage of total daily energy expenditure (fig. 5, *bottom*). For a small day range the estimated savings represents about 4% and for the largest ranges some 15–20% of the daily energy budget.

These results suggest large potential energetic benefits of bipedal movement for early hominids. While it is true that the earliest australopithecines do not display all of the morphological correlates of bipedality seen in modern humans, there is currently debate as to whether or not they were, in fact, less mechanically effective bipeds (see Lovejoy 1988, Stern and Sussman 1983, Susman, Stern, and Jungers 1984, Wolpoff 1983). Moreover, even allowing for the fact that the earliest hominids were less efficient than modern humans, because bipedality is *so much less* costly than either generalized mammalian or primate quadrupedality it is still likely that there were energetic benefits associated with the locomotor shift.

Indeed, what is striking about the locomotor data collected by Taylor and colleagues is how *inefficient* apes (chimpanzees) are with any form of terrestrial movement. Whether moving on two limbs or on four, chimpanzees spend 35–50% more energy than expected for a mammal of their size (Taylor and Rowntree 1973, Taylor, Heglund, and Maloiy 1982). In the context of the information presented here this is not surprising, given that chimpanzees and other great apes continue to live in relatively closed environs with small terrestrial day ranges. Thus, the relative energetic benefits for becoming more efficient have likely been small for the great apes.

Consequently, if our Miocene hominoid ancestors moved on the ground with the same efficiency as any of our three comparative groups (i.e., chimpanzees, primates, or generalized mammalian quadrupeds), the relative energetic benefits of even the early bipedality of the first australopithecines would likely have been great, especially if environmental changes necessitated the expansion of daily foraging ranges. Moreover, the fact that both chimpanzees and cebus monkeys can move bipedally at no greater cost than quadrupedally despite lacking the morphological adaptations for bipedal movement suggests that the morphological changes seen in pelves and lower limbs of the earliest australopithecines would have produced reductions in terrestrial locomotor costs relative to their prehominid ancestors.

Steudel (1994, 1996) has recently voiced concerns about the utility of Taylor and Rowntree's (1973) chimpanzee data, noting that since both animals were juveniles[3] their locomotor efficiency may not have been comparable to that of adults. Nevertheless, even if the

3. This is not stated in the original paper. The chimpanzees were indeed small (average weight 17.5 kg); however, they could have been small adult females, especially given that laboratory primates tend to be considerably smaller than those in the wild (see data in Leonard and Robertson 1992).

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chimpanzee data are disregarded it is still clear that human bipedality is considerably less energetically expensive than either average mammalian or primate quadrupedal movement. Consequently, it appears that the evolution of bipedality during the Mio/Pliocene likely had important energetic consequences for early hominid populations, allowing them to expand their day ranges and more effectively exploit their more open environment.

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Aboriginal North American Population and Rates of Decline, ca. A.D. 1500–1900[1]

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The magnitude of the Native American population decline which began north of present-day Mexico sometime after A.D. 1500 and continued to about 1900 is still a topic of debate. Important issues in this debate have been the timing and magnitude of "Old World" disease epidemics and their effects on aboriginal populations. It is well documented that soon after European arrival epidemic disease devastated native populations in areas of present-day Mexico, the Caribbean, and Central and South America (see, e.g., Cook and Borah 1948; 1971–79; Dobyns 1966; Smith 1970; McNeill 1976:1–4, 176–207; Cook 1981, 1991; Joralemon 1982; Guerra 1988; Denevan 1992 [1976]; Merbs 1992; Dobyns 1993; Larsen 1994). The role of epidemic disease in the population decline of North American Indians has also been extensively discussed and examined (for 20th-century discussions see, e.g., Mooney 1928; Cook 1935, 1945, 1973; Stearn and Stearn 1945; Duffy 1951; Blasingham 1956; Borah 1964, 1992 [1976]; Ewers 1973; Crosby 1976; Milner 1980; Johansson 1982; Joralemon 1982; Zubrow 1990; Decker 1991; Herring 1992; Merbs 1992; Dobyns 1993; Larsen 1994).

Some scholars (e.g., Dobyns 1966; 1983:15–23; Upham 1986; Stannard 1992; see also Dobyns 1993) argue that "Old World" diseases moved northward very early in the 16th century from European settlements in the Caribbean and Mesoamerica. These diseases, according to these scholars, infected native populations in both the Southeast and the Southwest of the present-day United States during the initial decades of the 16th century and frequently culminated in epidemics and pandemics which devastated Native American populations of not only these regions but other regions of North America as well. Consequently, they assert, the aboriginal population of North America, originally large, was greatly reduced by epidemic disease prior to significant historical documentation.

Scholarly research has generally refuted arguments regarding continentwide pandemics of smallpox and other diseases during the 16th century (e.g., Henige 1986, 1989; Ramenofsky 1987; Snow and Lanphear 1988; Snow and Starna 1989; Blakely and Detweiler-Blakely 1989; Thornton, Warren, and Miller 1991; Herring 1992; Larsen 1994). Larsen (1994:109), for example, reports that "archaeological, historical, and bioarchaeological studies provide compelling evidence that the arrival of Europeans did not occasion a sudden pandemic of smallpox in the early sixteenth century." Significant population decline in the Southeast and perhaps in the Southwest did begin *sometime* during the 16th century (see Ramenofsky 1987, Smith 1987, Thornton, Warren, and Miller 1991). Some research (e.g., Ramenofsky 1987; see also Smith 1987) supports the notion that it was caused by epidemic disease in the Southeast and the Mississippi Valley region. Similarly, it is possible that smallpox was present early in the Southwest (Upham 1986; see also Reff 1987, Upham 1987). Still debated is whether 16th-century diseases in the Southeast—and by implication, the Southwest—occurred as regionwide pandemics or as more isolated epidemics or even mere episodes (see, e.g., Milner 1980, Smith 1987, Blakely and Detweiler-Blakely 1989, Zubrow 1990, Thornton, Warren, and Miller 1991).

Most scholars now agree that the baseline for assessing aboriginal population decline—Mooney's (1928; see also 1910) classic population estimate of 1,152,950—significantly underestimates aboriginal population size for the area north of the Rio Grande.[2] No consensus has been achieved, however, on a higher aboriginal population figure. Contemporary estimates vary from around 2 million (Ubelaker 1988, 1992; see also Ubelaker 1976, 1992; Thornton 1980; Thornton and Marsh-Thornton 1981) through almost 4 million (Denevan 1992 [1976] : xviiii–xxi), about 7 million (Thornton 1987:32), and more than 9 million (Dobyns 1966) to as many as 18 million (for north of Mesoamerica) (Dobyns 1983:42).[3] Substantial depopulation eventually did occur following the arrival of Europeans and Africans in North America. The Native American population of the United States, Canada, and Greenland reached a nadir of perhaps only 375,000 around 1900 (Thornton 1987: 42–43; see Ubelaker 1988 for a somewhat higher nadir figure).

CAUSES OF POPULATION DECLINE

Epidemic disease was certainly important in the overall population decline of Native Americans north of Mexico, although the pattern of disease "was a patchwork affair, striking some populations and not others at various times" (Larsen 1994:109; see also Zubrow 1990).

1. © 1997 by The Wenner-Gren Foundation for Anthropological Research. All rights reserved 0011-3204/97/3802-0005 $1.00. This paper was prepared for presentation at a special session honoring Charles B. Nam at the 1996 meetings of the Population Association of America, New Orleans, La. I acknowledge the research assistance of Tim Miller. I thank D. Ann Herring and an anonymous referee for helpful comments on an earlier draft.

2. The dates for the *regional* estimates from which Mooney's overall estimate was derived varied widely, from A.D. 1600 to A.D. 1845, depending on the region in question. A primary reason for his underestimate of aboriginal population size, scholars now realize, was his assumption that little population decline had occurred prior to his dates for the beginning of an extended European presence in a region. On the contrary, substantial prior depopulation had likely occurred in every region (see Ubelaker 1976, 1988).
3. A recent discussion of the wide variety of hemispheric and North American estimates may be found in Daniels (1992).

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Disease affected, at one time or another and to one degree or another, Native American populations in all areas of present-day United States and Canada.

While important diseases such as tuberculosis and treponemal infections were present in the western hemisphere prior to the arrival of Europeans, there was a virtual if not complete absence of infectious diseases producing epidemics such as occurred in the eastern hemisphere (see Thornton 1987:44–47; Merbs 1992). ''It is quite clear,'' Merbs (1992:36) concludes, ''that the two worlds of disease were different enough so that the post-Columbian effect of Old World diseases on the Native Americans was devastating.'' Native American populations had no acquired immunity to European and African diseases; these diseases caused ''virgin-soil epidemics,'' whereby almost all of a population would be infected at once (Crosby 1976). Native American populations apparently were also remarkably homogeneous genetically; thus, viral infections were preadapted to successive hosts rather than encountering a wide variety of new immune responses (see Black 1992). Specifically, Native American major histocompatibility complex (MHC) alleles lacked genetic polymorphism, a characteristic both reflecting a lack of historical contact with many diseases and making Native Americans more susceptible to the diseases from the other hemisphere. However, neither the epidemic disease pattern in North America nor the depopulation of Native America by epidemic disease is fully understood.

Human populations constantly change in composition as members are born, die, or move in or out. The determinants of the way in which fertility, mortality, and migration interact to produce population growth, decline, or stability over time may be termed a ''demographic regime'' (see Thornton, Miller, and Warren 1991). Such a regime is typically relatively stable and influences the population's ability to respond to disturbances such as those caused by disease episodes (see Wrigley 1969 and Bideau 1984 for further discussion). It was probably not the *direct* effects of any single epidemic or even any single disease that produced the long-term reduction of most Native American populations.[4] Disturbances such as epidemics may result in only short-term population decline, with populations returning to predisease levels of population growth, decline, or stability. Thornton, Miller, and Warren (1991) have simulated this for smallpox epidemics, Herring (1994) has pointed to the recovery of a Native American population following the influenza epidemic of 1918–19, and Boyd (1992) has shown the temporary effects of a smallpox epidemic as well as the longer-term effects of a measles epidemic. Similarly, the historian William McNeill (1976:150) concluded that ''the period required for medieval European populations to absorb the shock of renewed exposure to plague seems to have been between 100 and 133 years, that is, about five to six human generations.'' Population recovery may even occur following repeated cycles of different diseases; medieval European populations suffered not only repeated exposures to the plague but also typhus, influenza, and measles epidemics and eventually recovered (Gottfried 1983:xv-xvi, 129–35, 156–59).

The indirect effects of disease episodes appear more important in population decline. These include the social disruption accompanying epidemics, such as that described by Neel (1982; see also Neel et al. 1970, Neel and Weiss 1975, Neel 1978) among Indians of South America and discussed by McGrath (1991), and the reduced fertility accompanying reduced fecundity due to the disease or to the loss of a spouse. The nature of Native American societies, including preexisting patterns of social organization, also influenced population reduction and/or recovery, as has been shown for the Tolowa of northern California (Thornton 1984*b,* 1986; see also Zubrow 1990, Decker 1991, Boyd 1992, Larsen 1994).

Native American population decline resulted not only from European and African diseases but also from the many effects of colonialism, subtle or otherwise (see Thornton 1987, Larsen 1994). Larsen (1994:110) suggests that the emphasis on disease ''has overshadowed a host of other important consequences of contact such as population relocation, forced labor, dietary change, and other areas.'' Colonialism also interacted with disease to produce population decline; Meister (1976:165) notes that ''later population decline resulting from disease was made possible because Indians had been driven from their land and robbed of their other resources.''

Native American societies were removed and relocated, warred upon and massacred, and undermined ecologically and economically. All of these effects of colonialism caused population decline through reduced fertility as well as increased mortality (see Thornton 1988; see also Stannard 1990 on fertility decline among Native Hawaiians). For example, the Cherokee ''Trail of Tears'' from the Southeast to Indian Territory produced substantial population losses, partly from the mortality of diseases such as cholera but also from reduced fertility and increased mortality due to malnutrition and starvation (see Thornton 1984*a*). Southern California Indians were missionized, which confined them in new disease environments that took a demographic toll via both fertility and mortality (Walker and Johnson 1994), and eventually displaced, which resulted in selective out-migration and lower fertility as well as assimilation (Harvey 1967). Northern California Indians were subjected to pseudo-war and outright genocide as well as the destruction of their traditional patterns of subsistence (see Thornton 1984*b,* 1986; Walker and Thornton 1996). Plains Indians lost much of their social and cultural life and most of their economic basis when the great herds of buffalo were destroyed (see Thornton 1987:51–53).

Indeed, in the early 20th century the fecundity and fertility of Native Americans—particularly ''full bloods''—were of considerable concern to government officials (see U.S. Bureau of the Census 1915:157–60).

4. Some Native American groups are far larger today than in 1492, depending on how their populations are defined (see, e.g., Thornton 1990, 1996; Walker and Thornton 1996).

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TABLE 1
North American Aboriginal Population Estimates and Yearly Rates of Decline

Estimate	Yearly Rate of Decline (to 375,000 in 400 years)
1,153,000 (Mooney 1928)	−0.28%
1,894,280 (Ubelaker 1988)	−0.40%
7,000,000 (Thornton 1987)	−0.73%
9,800,000 (Dobyns 1966)	−0.82%
18,000,000 (Dobyns 1983)[a]	−0.97%

[a]North of Mesoamerica.

Moreover, the 20th-century recovery of the Native American population of the United States has been driven by Native American fertility increases and fertility levels higher than those for the U.S. population as a whole (Thornton, Sandefur, and Snipp 1991), along with reduced mortality.

The data presented here show that small disturbances to the demographic regimes of Native American populations are enough to account for even the largest asserted magnitude of native population decline in North America north of Mexico. They also show that reductions in Native American fertility levels may have been extremely important in this long-term population decline. Spectacular long-term mortality from epidemic disease is therefore unnecessary to account for the decimation of large aboriginal Native American populations.

RATES OF POPULATION DECLINE

Table 1 presents five aboriginal population estimates for North America north of Mexico (or, in one instance, north of Mesoamerica) ca. A.D. 1500.[5] They represent the wide range of 20th-century estimates that scholars have offered (see Thornton 1987:26; see also Daniels 1992:300, table 2). Assuming that a nadir population of 375,000 occurred at A.D. 1900 (see Thornton 1987:42–43, but see also Ubelaker 1988),[6] corresponding yearly rates of decline may be ascertained. This is accomplished by assuming that the populations were stationary (i.e., neither growing nor declining) at A.D. 1500 (i.e., prior to the period of decline) and that the rate of decline was exponential and then using the simple formula for rate of growth to ascertain rates of decline. Certainly, neither assumption is totally correct.

The assumption regarding stationarity is relatively unimportant; it is just simpler to model the decline starting with a zero rate of growth. (Starting with a growing population ca. 1500 would increase very slightly the rate of decline, while starting with a decreasing population would reduce it very slightly.) A growth rate around zero does seem realistic, however, for the total Native American population in its "aboriginal state" (see Thornton, Miller, and Warren 1991). Growth rates around zero have been observed for specific Native American populations in the early contact period (see, e.g., Meister 1976, Engelbrecht 1987). Some Native American populations of ca. 1500 were undoubtedly increasing somewhat, while others were undoubtedly declining somewhat (see Thornton 1987:36). In any event, it seems safe to assume that the total Native American population of that time was neither increasing nor decreasing dramatically.

The assumption regarding an exponential rate of decline for the entire period is much more important. Certainly, there were fluctuations in rates over the period for the total Native American population and, obviously, for specific tribal populations or even regional ones. (For example, Ubelaker's [1988] figures show a relatively small rate of decline from 1500 to 1600, a relatively large rate of decline from 1800 to 1900, and a steady rate of decline of −.25 to −.30% from 1600 to 1800, with an overall rate of decline from 1500 to 1900 of −.32%.) Making this assumption simply allows an understanding of how relatively small changes in fertility and mortality *could have* produced large declines over an extended period. Assuming particular patterns of decline has already proven heuristic for estimating aboriginal population sizes (see Thornton and Marsh-Thornton 1981).

The total Native American population discussed here is obviously a mere composite of specific tribal populations, each with its own pattern after ca. 1492 (see, e.g., Meister 1976). There were also regional differences in patterns of decline (see, e.g., Ubelaker 1992) depending at least in part on the time of extensive contact with Europeans and their diseases. Once again, it is heuristic to consider the total population, since detailed population histories are not available for all tribes. The analysis illustrates how small changes in demographic regimes could produce a large population decline and examines the role of fertility in this decline. In this regard, it matters not whether total, regional, or tribal populations are considered.

The rates of population decline required to attain the 375,000 nadir population (table 1) are not particularly high. Even the largest population estimate—Dobyns's 18 million—would require a rate of less than −1.0% per year. More moderate estimates of aboriginal population size require rates of only about −.75%, while the lower population estimates require rates of only about −.25.

CHANGES IN FERTILITY AND MORTALITY

Assuming that the 400-year rates of decline are realistic, what may account for them demographically? They

5. Kroeber's (1939) estimate of 900,000 is not included. It was derived from Mooney's estimate by merely lowering the total for the California area; however, subsequent research indicates that the California area population suggested by Mooney should be increased (see Cook 1978, Thornton 1980).
6. Ubelaker (1988:291) suggests a nadir population of about 530,000; using his higher figure would only lower the magnitude of decline and thus the rates of decline.

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TABLE 2
Contributions of Changes in Birth Rates and Death to Population Declines

	Growth Rate		
	0	−.5%	−1.0%
Changes in birth rates			
Birth rate	40	34.72	29.85
Death rate	40	39.72	39.85
Mortality level	3	3	3
Life expectancy	25.0	25.0	25.0
Changes in death rates			
Birth rate	40	38.76	38.03
Death rate	40	43.76	48.03
Mortality level	3	2	1
Life expectancy	25.0	22.5	20.0
Changes in birth and death rates			
Birth rate	40	–	33.47
Death rate	40	–	43.47
Mortality level	3	–	2
Life expectancy	25.0	–	22.5

SOURCE: Coale and Demeny (1966, 1983).

would not require the spectacular mortality of large-scale epidemic disease (although epidemic disease surely was involved). Rather, they would result from long-term alterations to the demographic regimes of Native American peoples that prevented population recovery from the devastation of epidemic disease, and these alterations could have been relatively modest changes in fertility and mortality. Model life tables may be used to establish what changes in crude birth rates and death rates are implied by the various rates of decline. They will also *suggest* whether changes in birth rates or changes in death rates were more important in population decline (see Coale and Demeny 1983; see also 1966).

Perhaps the most appropriate life tables for aboriginal North American populations are the Model West Female life tables with the lowest rates of life expectancy at birth: mortality levels 1 (20 years), 2 (22.5 years), and 3 (25 years). These life tables are consistent with data on skeletal remains (see, e.g., Goldstein 1953:4; Johansson and Horowitz 1986:245–49; Lallo and Rose 1979: 332; Ruff 1981:150; Storey 1985:530; Ubelaker 1974:64), historical Native American populations (Meister 1976, Engelbrecht 1987), and such contemporary "aboriginal-like" populations as the Yanomama of South America (see, e.g., Howell 1976:33–38; Neel 1978:373–74; Neel and Weiss 1975:36–37). The Model West Female Level 3 life table with a zero rate of growth has birth and death rates of 40 (table 2). Using rates of growth of −.5% and −1.0% to reflect the declines discussed above, the death rate stays virtually the same (39.72 and 39.85, respectively) while the birth rate drops to 34.72 and 29.85. The changes in the overall mortality levels necessary to produce the −.5% and −1.0% growth rates are also shown in table 2. Here, death rates of the level 3 life table at zero growth may be compared with those for level 2 and level 1 to ascertain the required increases in mortality while holding fertility more or less constant. The level 2 life table at a −.5% growth rate has a death rate of 43.76 (and a birth rate of 38.76) while a level 3 life table at a −1.0% growth rate has a death rate of 48.03 (and a birth rate of 38.03).

Lastly, table 2 shows the combination of a decrease in birth rates and an increase in death rates to produce the −1.0% rate of growth. The original birth and death rates of 40 of the level 3 life table change to 33.47 and 43.47, respectively. Neither change is particularly dramatic, although fertility decreases considerably more than mortality increases. These changes are particularly significant because the −1.0% rate of growth accounts for the largest magnitude of population decline depicted in table 1. The more moderate rates of decline shown in table 1 would be produced by considerably smaller changes in birth and death rates.

DISCUSSION

A preoccupation with disease to account for the decline in the Native American population has fostered a simplistic view of Native American population dynamics. A more appropriate focus is on how colonialism, including its diseases, interacted with Native American demographic regimes to produce long-term rates of population decline. Fertility reductions as well as increases in mortality seem to have been important in the decline.

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