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# Reports

## An Interview with Edith Turner[1]

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*Introduction:* The following interview is taken from a much longer life history conducted over the course of several months in 1997 as a project sponsored in part by the Historical Archives Program of the Wenner-Gren Foundation for Anthropological Research. The original motivation for that project was to explore the life and writing of Edith Turner, her marriage to Victor Turner, and how the dynamics of gender and marriage affect the production of anthropological work. This interview has been framed to touch briefly on the issues raised in the longer work. In a few instances it has been necessary to write transitional paragraphs in order to give this interview a more coherent form, but an effort has been made to keep the tone, ideas, and progression of the original conversations intact.

ME: When did you and Victor Turner meet?

ET: In 1942, at Carfax in Oxford, which is the main crossroads, right in the middle of Oxford. That's where Vic and I arranged to meet, an arrangement made by my brother Charlie.

ME: Was it a blind date?

ET: It was through Charlie, but it didn't have the feeling of a blind date. Charlie had been at university in Oxford, and then he was drafted into the army, into the same unit as Vic. In this unit there was lots of lifted literary talk and talk about politics. The unit consisted entirely of men who were conscientious objectors to the war. Nevertheless, they were drafted and doing noncombatant work of various kinds. My brother Charlie said, "You ought to meet Vic," meaning something like "My God, he's interesting. He's the most interesting guy in this group, and you should meet him."

ME: So it was out of an interest in conversation and literature.

ET: Yes, that's right. It was so fascinating. But I don't think we even thought of ourselves as being literary, you know? I was busy doing Land Army work, and Charlie and the other conscientious objectors were all reading as fast as they could. I was doing the same sort of reading at the gardens where I worked. I would read at lunch hour and get in trouble with my workmate for not talking to her. It was just a spontaneous thing. We weren't being literary or anthropological, or trying to find lovers, or anything like that.

FIG. 1. *Edith Turner, 1997.*

ME: What were you reading?

ET: I had been reading Bernard Shaw and Henri Bergson. Vic had been reading Kierkegaard, and he was also reading the symbolist poets of France: Baudelaire, Rimbaud, Verlaine, and Marlarmé. I was reading stuff I used to get

out of the library. There was a whole series of Penguin books out, the "New Writing" group of writers. We were reading those like mad, in addition to the new poets. There was a conscious effort to keep poetry and the arts going in the war. In fact, a little later a group from Vic's army unit was formed to publish our writing. It was called Oasis because we were an "oasis" in the desert. We regarded the war scene as a sort of great patriotic desert.

Rimbaud called what he was doing "the reasoned deregulation of all the senses." We didn't go that far, but what Rimbaud saw was the immense beauty of the world if you weren't hedged in by conventions. This is more or less what it was like for us. My mother-in-law later called me a bohemian.

Vic and I married in 1943, six months after we met. For me, finding him was like the discovery of poetry. So it hadn't been a blind date but I fell in love anyway. And later he said that after two weeks he knew I was the one. After the war Vic went back to University College, London, to resume his studies where he had left off on being called up. But during the war we had discovered anthropology through the books of Margaret Mead and A. R. Radcliffe-Brown, so after the war Vic changed his course from literature to anthropology, which was under Darryl Forde at the time. We moved down to Hastings, south of London, to where Vic's mother, Violet, was living. He used to take the train into London for his seminars.

Vic used to read out all of his assignments to me while I was doing chores around the house, and so I was getting a course under quite an interesting person who was a budding professor himself. Though now I feel I'd have given anything to have gone in and written papers myself, I could not because I had three young kids on my hands. But at home it was one long seminar all the time, day and night. We were thinking of anthropology all the time.

And then, Vic came back from London University one day and said, "I've met Max Gluckman, and he wants me to put in for the Rhodes-Livingstone grant. He wants me to get my Ph.D. at Manchester." Vic was very enthusiastic about this offer. Max was a bit of a Marxist and was interested in the Hegelian dialectic, which was a new thing in anthropology. Any kind of idea which could encompass change was new at the time, because British structuralism was the fashion. Max was an innovator, and Vic could see this. With all the political work we'd been doing, we thought it was a great chance to do research into the very heart of human society in Africa. It looked just right.

A lot of good things did indeed happen in Africa, and a lot of them happened because Vic was the kind of person he was and just ate up hard work. Vic worked for a year as a research assistant in Manchester, attending seminars, and I also audited seminars occasionally. Later on in Manchester, Max would show his delight that he'd got Vic around.

ME: When you were preparing to go to Africa, how were you feeling about your role in the whole trip?

ET: Vic's getting a grant and going to Africa and my getting travel money to go, too, simply confirmed that we would go on doing this collaboration. I knew I could do fieldwork among the women, taking for granted I would do so. I was extremely hopeful. It was a matter of not even wondering if I would fit in. I don't remember there ever being any question or doubts or fears or anything like that. It was a matter of "Now we have a chance to do our proper work." We knew how important fieldwork was in anthropology, and, well, I had this marvelous husband, so I wasn't nervous.

ME: What did you expect as a family and as anthropologists in this first trip to the field?

ET: I think we had the old fieldworker's guide, *Notes and Queries* [Royal Anthropological Institute 1957]. Yeah, we had that. Max [Gluckman] didn't run fieldwork preparation classes. In fact, I still don't think they do enough of that in anthropology.

Vic plunged into his research with vigor, at a tremendous rate, and grew familiar with the little enclave of villages around the rest house where we stayed for the first three months. I made friends with a woman called Fatima, who took me to rituals. I saw the girls' "ripening ceremony," Nkang'a. We saw the girl coming out dancing. I was writing rapidly. I had a clipboard, and Vic had a clipboard. We were at it as hard as we could, with no tape recorders. We just simply wrote down rapidly everything that happened. And I was taking a lot of photographs, being the main photographer. At night we would write up the fieldnotes. Sometimes I typed out Vic's fieldnotes for him. Sometimes we just collated numerical material of various kinds.

ME: In *The Spirit and the Drum* [1987], you write about your research assistant, cook, and friend Musona/Kasonda a lot. He also appears in some of Vic's early work, especially *Schism and Continuity* [1957]. Did he always travel with you?

ET: Yes, he did. He regarded it as his "labor migration." We never really went that far from Mukanza [his home village], and we kept on going back there because it was a center of ritual activity. Musona brought his three wives and children with him wherever we were.

ME: Did you talk to the women more than the men?

ET: Yes, on the whole, although there wasn't a lot of sex segregation. I often used to go to the gardens, and we'd talk there. I used to ask the women what it was like to be in a polygamous marriage. The first wives would say, "It's great, it's a good life." And the second and third wives said, "No, it's not a good life. More or less we don't get much of a look-in." And the young third wives, who were usually *kankang'a* [having just gone through the puberty ritual], would be married off to wealthy older men whether they liked it or not. In one case there was

a man about 50 or 60 years old whose young wife ran away. I thought that she ran away because she found him disgusting, but she ran away, you see, because he couldn't get it up! And I liked to hear that, because these girls were really glowing with sexuality. They were superb young women. That was where I caught on to the marvelous sexuality of African life. The Ndembu loved sex. The most pleasant and cheerful conversations were about sex, and their only fears were of witches who were "hot" in their sex lives—too fast and sudden. The women liked it *chovu*, which means gently and quietly. They liked sex to come up gently. Boy, they loved it.

ME: How did having children in the field influence the dynamics between you and the Ndembu?

ET: What do you think, for heaven's sake?

ME: Well, I would think it would make things a lot easier.

ET: Yes, of course! Obviously! Freddie, Bobbie, and Rene all got along with the Ndembu children. Freddie and Bobbie would run off in the afternoon with their gangs—hunting for little animals or just playing around. We would spend each morning on lessons from a correspondence course that I sent away for in Salisbury, but these lessons always seemed to get sidetracked. For Vic and me, having our children made us more human in the eyes of the adults.

ME: So what was the Rhodes-Livingstone plan of study?

ET: One year in the field and one year back for finding out what you don't know, then one year in the field again to fill in what you don't know, and then one year for write-up.

ME: I'm curious to know what you were reading at the time and what types of issues from the first field trip stuck out for you and Vic as needing more exploration during the second.

ET: Well, we were reading a lot of Meyer Fortes. This was the big thing then. Vic was also reading Marcel Griaule around that time. We were interested in what the French were doing, but Vic was very critical because they didn't have a sense of social interactions and social contexts that the British always had. And he was then, as always, very proud of what the British were doing. He thought French anthropology was superficial because it was not alive in human interaction, and that is what Vic was talking about. His version of political anthropology was local-level politics and the actual political rivalries, like those he was to write up later in *Schism and Continuity*. We were also both reading Henri Junod, and I was very fond of his work.

We were encouraged by conversations with Max in the Manchester interim. There were several rituals going on when we got back [to Mwinilunga], and Vic was then fully able to put them in the proper setting of kinship and political rivalries. As I said, Meyer Fortes's work was very useful to us because we realized that those concerns overlap each other and influence each other, just as Meyer had shown among the Tallensi. But we felt this was even more so among the Ndembu, because the concerns weren't only kinship and clanship; they consisted of local political rivalries and illnesses and curative cults and the new influences from the British government and the march of colonial development—all kinds of forces were playing there, doing their work, creating a present, the now.

So we went into the field again, and this time we arranged to stay in Mukanza [Kajima] village for the whole period because it was at the crossroads of many different influences.

This was when our trips out to the Mukanda ritual, the boys' initiation involving circumcision, started. It became clear that these rituals were performed by people who had complex motivations and rivalries and conflicts [see "Mukanda: Rites of Circumcision" in *The Forest of Symbols* (V. Turner 1967)]. Certain people had more power because they were earning money building a new road, and others were the old-fashioned type. In the end it was one of the old-fashioned type, Nyaluhana, who did the circumcising. He just took it over and pushed by everyone else as they laid the boys out; he was there with his knife and did the cutting. They had to hold down the boys because they were only six or seven years old, some of them, and they just wouldn't stay still. So the men played drums loudly to drown out the crying.

Rituals quickly became the focal point of all that we did. I remember that at the beginning of a twin ceremony once, my friend Nylakusa came out of her hut yelling cheerfully, "Let's go!" I can see her now. Ritual is fun, and her shout captured something. I don't know what's the matter with us anthropologists. For instance, as Vic analyzed the twin ceremony in his writing, it was scholarly and showed the detail of the symbolism. I myself would like to have described the ritual in a different way; to have shown something of the swing of the whole thing as a kind of a great event. I'm interested in capturing what that woman felt when she said, "Let's go!" You know? And that's what I feel is missing in anthropology. Many people have felt it incumbent upon them to write with deadly seriousness. It must be said that Vic was needing to write a foolproof Ph.D., because he had a wife and three children to support. The blame falls squarely on the coldness of academic demand.

Spending a whole year and a quarter in Mukanza village was just the right thing to do. It was tempting to us to go from place to place as we had done in the first tour, but the richness of the material was there in Mukanza—the intimate knowledge of personalities, people, the friendships. These were of the essence in this kind of fieldwork. Other disciplines regard much of anthropology as a string of anecdotes and don't think highly of it, because they value statistics and think such results are the truth, produced according to the real scientific method. But when you're staying in a village like Mu-

kanza for a length of time, getting to know an intimate little place, even though it's only a tiny spot on the map of Africa, somehow or other you get in-depth documentation and understanding. Human sympathy with what's "on the ground" is what the anthropologist is talking about. The stuff of life is difficult to bring into relation with a comprehension of the whole human scene, but I think we have to do it.

When Vic went back to Manchester University with these cases, with the Kamahasanyi Ihamba case, and so on, Max Gluckman said, "*Don't* do the ritual first. Do the social structure and make that your dissertation." Vic was, for better or for worse, linked irrevocably with Manchester for his Ph.D., so he wrote *Schism and Continuity*, which is in great part the statistical picture of a matrilineal people, including plentiful case material and a discussion of the implications of marriage locality. Max had thus set Vic an exercise in describing a social system as a preliminary to his writing on ritual.

So we built up these statistics. But there was an occasion in a pub in North Manchester, which I've discussed in the introduction to *On the Edge of the Bush* [V. Turner 1985], about the social drama. Vic was in the pub with Bill Epstein wondering what it was about anecdotes, episodes, and trouble cases that was so important. There was "process" going on here, and not just "social process" but a special form of "ritual process." We had been thinking about what happened in the second field trip, when Sandombu/Samutamba had terrible rows in the village when he was drunk, blaming his wife for not having any children and his mother-in-law for being a witch. Sandombu would roar out the frightful words "*Wanza weyi!*"(Dirt under your foreskin!). He was furious, and this was a real curse. That scene and the quarrels that followed and the trouble that came up in episode after episode, as documented in *Schism and Continuity*, meant that the very roots, the vital existence of the village was trembling and tottering all the time. This was in front of our eyes during the second session in the field. Vic couldn't look at these events as just anecdotes or mere trouble cases. He strung them together later in *Schism and Continuity*, but while still in the field he was taking notes, massive notes, paying attention because of this hunch which he hadn't yet articulated—not until the pub in Manchester with Bill Epstein. The hunch in Manchester was the concept of the social drama and its definable form: breach, crisis, redress, and reconciliation. After the pub conversation, Vic wrote it all down and turned it in to Max as the major chapter in his dissertation. And Max liked it.

There are other stories important to that early work. Once during the second field period we were walking over an old village site, a bit north of Mukanza village, just taking a walk. The old village site had ghosts because people had died in those huts. You would hear voices, and they were talking about you, and they would tell you not to eat the bananas. As Vic and I were walking over this place we were talking, and we came on to the subject of Sigmund Freud, whose work had become very important to us in the field. Vic had got hold of *The Interpretation of Dreams* [Freud 1955] in the field, and it all came out in 3-D for him. During that walk I, too, saw the curious imagery in dreams as echoing everywhere in Ndembu consciousness. We were both excited. Decades later I experienced dreams, as Native Americans do, as truly prophetic. This time, however, the ghosts decided Freud should rule! *The Interpretation of Dreams* powered Vic's work on symbolic analysis and was a real breakthrough in the field itself. So this all came out at that time and we discussed it night and day, and it was marvelous for us. I loved it.

When we returned from the field, we had to get to business and write, get this dissertation through. We reckoned it was from the September term to the spring that Vic would have to do the work, while the grant lasted. We had all these figures to deal with and recheck and we also had to consider which tables would be useful, perhaps introduce some other ones, too. Vic was dealing with the field notes and the main series of cases, the social dramas. We were busy in our rented house in North Manchester, going in to the department very often, and because the children were in school it was possible for me to take part.

Wonderful seminars were being held, and we had library research to do. But we did a great deal of work at home. Vic kept all his main materials and his typewriter and books at home; he didn't have an office at the university. And we began to build up the dissertation, chapter by chapter, very carefully, starting with the geography, means of subsistence, political systems and history, etc. Nowadays, I discourage students who want to do their write-up that way. I tell them to start with something which is at the heart of the topic, which begins to breathe real life into the piece. But those were the days when the old conventions still reigned, and you simply did it this way. Vic handled the main writing. There was only one typewriter. I did the editing throughout, and the tables and the photographs, working all the time. Max Gluckman, when he finally got a complete draft in his hand, with great painstaking care went through every word of it, copyediting in detail.

I liked this work a lot because we used to talk about the subject matter all the time. I did the maps for Vic. These details are important to know, but they don't get very much regarded. Vic's mother came up when Vic got his dissertation, and it was quite an occasion. I was immensely proud, and I bought myself a new hat.

ME: Is there any part of *Schism and Continuity* that you and Vic kept coming back to as something to argue over, agree on, or revel in?

ET: Yeah, the social dramas were all of those. They were the great events in the villages that affected us all. And we wrote it all down and took great pains to record them all in photographs in the dissertation. And yet I felt there was no sense that there was a spirit being passed down in the book, but Vic said there was, because of the Chihamba ritual given at the end. I regarded that analysis of Chihamba as being tailored to fit the theme of the

book as a whole—emphasizing the unifying effect of the cult of Chihamba throughout that vicinage. It's too sociological, although there are hints in the analysis that there's more to Chihamba than what's written and it did lead on to *Chihamba, the White Spirit* [V. Turner 1962]. So OK, there were these differences of opinion. What I was pleased about was the fact that in *Schism* you could see the Ndembu ritual system in action. It was set "in time." And then of course there was my manuscript "Kajima." I was writing myself while doing all this dissertation work with Vic, because I couldn't not do it. Africa had such an effect on me, and I missed the people so much. I had a vivid dream about them, and I just simply had to get the events down the way I personally saw them and experienced them.

ME: The manuscript "Kajima" has always fascinated me. It was eventually to become *The Spirit and the Drum*, some 30 years after it was originally written. But I've always wondered about how you thought of it when you wrote it. Did you think of it as anthropology? How did you and Vic talk about it?

ET: He was supportive about my doing this, but it wasn't a part of the department's research. I never read any of it in the department; it was private writing. I didn't expect that anything I wrote would be given in the seminars.

ME: Why?

ET: Because no wives ever did this, unless they were university-trained. And that was that. Otherwise you were just going to be a bother. As one professor at [the University of] Chicago later said, "We don't want all these Hyde Park housewives around here." Thanks, you know? I'm very angry still. Such a dictum was taken for granted in England at that time, and probably universally. I did go and sit in on the seminars, but the possibility of my contributing simply didn't come up. But Manchester was a comfortable atmosphere, and so much went on outside of the seminar setting. Elizabeth Colson used to do her knitting at the seminars. It was very human, and I was extremely glad to be there at all.

The manuscript stayed in a drawer, and we were busy thinking out what was going to come next. In the late 1950s, a very important thing happened in our lives: we joined the Catholic Church. It was at St. Joseph's, in Manchester. We had been knocking around in Manchester for a few years after the field, a little depressed for a number of reasons. The Communist party, which we had joined after the war in Hastings and which informed a good deal of our first fieldwork, had lost all appeal. African ritual had taken its place, and I suppose that for us there was something of this ritual fever in the Catholic Church. It would be hard to fully explain—or understand—the reaction we got in the Manchester department. A lot of our friends were card-carrying members of the CP, and almost everyone in anthropology was a left-leaning atheist. Joining the Catholic Church was probably the worst thing we could have done. It didn't end friendships, but it did cause tensions with some people. In any case, we wanted to get out. Vic was very devoted to Max but also wanted to get out from under his thumb, so in 1960 he accepted an offer from the Center for Advanced Study in the Behavioral Sciences at Stanford to spend a year there. Soon after that, he got an offer from Cornell University to come as a full professor, which was very rare—especially given his age. So in 1964 we moved to Ithaca, and Vic started teaching at Cornell.

ME: It seems that when Vic was at Cornell your research interests began to expand. I'm thinking here of the Morgan Lectures Vic gave at Rochester during that time.

ET: *The Ritual Process* [V. Turner 1969], which was based on those lectures, was the key to the Cornell period. It was partly a recognition of the developing hippie era and of the demonstrations and love-ins at Cornell. We had transferred the pub discussions of Manchester with the likes of Bill Epstein to Cornell, although it was in our house rather than a pub. We developed a liminal system in our seminars. Somebody gave a presentation—this was the structured part. Then the interval, the liminal time, when we all got cans of beer and had a break. Then we came together afterwards for the "reaggregation," to say more on what we'd been talking about in the beer interval. We would have a discussion and people would be able to hear each other. That system worked like a treat. At least 12 heads of departments have resulted from those seminars, and students with many, many publications—I can't count the number of books that have been written by students who have been to those seminars.

Within a short time the Chicago offer turned up. That appointment was for the Committee on Social Thought, with a joint appointment in anthropology, but Vic was to be paid by the Committee. Without much hesitation he said yes, because he liked some of the faculty a lot. He was also interested in the liberty that the Committee might offer, because he could teach outside of anthropology—courses on Dante and Blake or whatever caught his fancy. We had been at Cornell four years and felt that we could move on. We didn't necessarily want the beauty of nature and the quiet life that Ithaca offered. We wanted to be where the action was.

At the time, Vic's reputation was rising. He and I were producing a lot of work. These books were popping out like mad, and people were reading his papers in different collections. Maybe if Chicago had been like the Virginia department—if it had had the same ethos—it would have been a permanent affair for us. But there was something about the University of Chicago—a kind of tough, bitter steel from the city itself that had gotten into the fabric of the school.

The first impressions in Chicago were of Hyde Park itself. When we first arrived the place was in an absolute uproar because of the 1968 elections and what was happening at the Democratic Convention. The police were

so jittery, I thought, my Lord, this place is very upset. Very, very upset. Vic and I felt for the students a lot. We wanted to be identified with them. Most of the faculty at Chicago didn't feel this way, and there were some ugly rows. But I always thought that the students were like your children, and how could you betray your children? Anyway, this is part of what we plunged into when we first reached Chicago. I know these events played a big role in how we thought about our work in anthropology from then on.

ME: Did you two carry the seminar format developed at Cornell on to Chicago?

ET: Yeah. That seminar went on. It was known as "Victor Turner's midnight seminar." It would start at eight o'clock on Thursdays and just go on and on. The gathering just couldn't stop. Vic and I would wake up late on Friday, about nine or ten, and go along to Walgreen's and have some coffee and sweet rolls. And Vera, the waitress, would always come to our table, and we'd feel totally at peace. We would walk around the Point, out near the Lake Michigan, and then come and have our coffee, talking about what had been going on in the seminar. Those were great days. It was the students who helped us think through everything.

The seminars were the heart of that Chicago period, but Vic also gave courses to students who were interested in Durkheim. He gave lectures on Kierkegaard, William Blake, Dante, and other figures. He was able to do this in the Committee on Social Thought, you see, and he relished it because at University College, London, he had gone deeply into literature. Besides, he and I were continually exploring literature, the various Greek poets, the French symbolists, the American visionary romantics, and so on. It was a very busy time in Chicago, nine years in all.

ME: Did faculty come to the seminars at your house as well?

ET: Some faculty, yes. I wouldn't say a lot. Maybe they were there more than I realized. Fred Eggan was often there. Not many anthropology faculty. Jamie Redfield was there sometimes. Who else? If there were South Asian themes, it would be A. K. Ramanujan and Ralph Nicholas turning up, depending on the topic on which a student was presenting. The only regular was Fred Eggan.

ME: At this point I wonder if we can talk about when you started to work on the journal *Primavera*. I'm also interested in hearing about your experience of the women's movement and how feminist sentiments were coming into the university.

ET: At the time I was more caught up in it than in anything else. I hadn't been totally aware of feminist issues. When I was a little kid I was kind of a feminist, but I didn't attach myself to it because I was so busy with Vic's work and his thinking. *Primavera* was actually pointed out to me by Vic in *The Maroon*, the university's student newspaper. He said, "There's this ad calling for people to work on a literary journal, why don't you do it?" It was his suggestion that legitimized it for me. I would have loved to do it anyway, but since it was coming from him that meant, "OK, that's a go-ahead."

There were more than a dozen of us working on this journal. Some of us were associated with the university and some weren't. We published poetry and articles. I wrote "Girl into Woman" for one issue, which was the first thing I published on the N'kanga ritual. I liked working in a literary style very much. I worked hard on *Primavera*, to such an extent that Vic started to feel he wasn't seeing much of me. But the journal work continued right until we left Chicago. We had a break for one year in Princeton when Vic was at the Institute for Advanced Study. He was there during the 1975–76 school year and then back in Chicago until 1977.

ME: I wonder about the progression of all this. You were writing a lot in the 1940s, and we've talked about the work in Oasis. And then in the 1950s, after the field, you were writing both with Vic and on your own. In the 1960s, when you got to America, you mention editing more than writing, and then, in the 1970s, with this literary journal, writing comes up again. How do you see this history? Were you always thinking as a writer?

ET: On and off, yes. I can see the uneven development in any writer's life. And incidentally, about the Oasis poetry: I much admired Vic, who was writing very precise poetry. It was almost as if every line came out in balance, and I liked that. Of course that was one of the reasons I fell for him. But I tended to regard my own writing as this kind of flyaway stuff. I admire Walt Whitman because he has a free style. And I felt that probably mine was not gifted poetry because others had said it was sort of wild. But the works of the hippie poets actually encouraged me a lot; Alan Ginsberg, Gary Snyder, and the others. Ginsberg just let it all hang out, and that was encouraging for me.

ME: This was also the time that you and Vic started getting into the pilgrimage work, which resulted in your first officially coauthored book, *Image and Pilgrimage in Christian Culture* [1978]. That work strikes me as being of a very different nature from the Ndembu work and more a continuation of the later chapters in *The Ritual Process*. How did you do the work for that project?

ET: It was different from standing with a clipboard in the middle of the bush writing about medicines and ritual. For the pilgrimage work we did do "traditional fieldwork" in the sense that we went on pilgrimages ourselves, but a lot of it was textual analysis. We looked a lot at writing by devotees, which was an important part of the project. And then of course the experience of going on the pilgrimages as Catholics was personal as well as observational. We were worshipping at the shrines; this

did not get into the writing very much, but it did creep in a bit here and there. So, we used more historical material and less straight ethnographic material about what the pilgrims said to us. In hindsight, I feel we didn't have enough time to do any consistent following-up of specific groups of pilgrims. That's why I went back to the field in Zambia after Vic died, because I'd been missing that sort of thing—integration with what the people were actually doing.

ME: Looking back on the course of your work together and trying to situate the different projects, how do you think about the question of authorship?

ET: There's not much difference—is there?—in what we did together. It was just a matter of the political climate of the age. In the 1970s you could begin to say, "OK, this was a collaborative effort," but it was about the same before. For example in *The Drums of Affliction* [1968], Vic had a big chapter on the girls' initiation, Nkang'a. A large amount of that was my material, and the ideas were from our collaboration, but it was a book by Victor Turner, published by Clarendon Press. So it's a matter of the politics of the time. The fact that feminists in the 1970s could raise these issues is very important, and I do thank them. To a certain extent I wish there were more of my stuff in the work, but then, in a way, there is, indirectly. But, you know, it's actually hard to collaborate in writing. It requires you to slow, and we were both in full-tilt with what we were doing, you know? We liked it that way, and we liked the fast pace.

ME: You've mentioned to me before that when you two went on your pilgrimage research trips, you started to write fieldnotes in your own style, which was different from how you approached fieldnotes with the Ndembu. I wonder if you can talk a little more about the process of writing fieldnotes—from the early days with the Ndembu to your latest work in Alaska [1996] and in Ireland.

ET: In Mwinilunga, my fieldnotes were not as academic as Vic's, but they were running accounts of what people did as I watched rituals. I did not give any emotional reactions of my own at all. I tried to get the account objectively, but my emotional reactions were running through my head, and that's why I wrote "Kajima" [*The Spirit and the Drum*]. I wanted to get it before the feelings disappeared. I didn't regard that as academic; I regarded it as a narrative account that I wanted to do because I liked doing it. So, OK, I was writing what happened as a report of various rituals and so on, and I always reckoned that they probably weren't as complete as Vic's and that he would be aware of social processes going on that I wouldn't because of his training. He had a good eye for antagonisms in the village and the curious tangle of personalities that he wrote about in *Schism and Continuity*. I wasn't up to writing on the intricacies, or so I thought at the time. And so, OK, I wrote these reports.

ME: Did you think of yourself as an anthropologist in Africa?

ET: I felt that I was a junior anthropologist, yes, but a strange one because I hadn't been shaped in the mill at University College. I was freewheeling a bit. My thinking wasn't shaped in the professional way, although I had been fairly close to it. And believe me, I had a deep respect for it, or else I wouldn't have helped Vic with all those books. I was a sort of an anthropologist, and an anthropologist's assistant 100 percent.

ME: When you moved to Virginia in 1977 for Vic to take up the Kenan Professorship in anthropology and religious studies, you delved even further into the realm of literature by enrolling in English and creative writing courses. I imagine this all fits into the trajectory you're describing.

ET: I found myself in this new place, and I was getting more and more interested in writing. I felt I could do a degree, and since there are extraordinary writers in Charlottesville, people like John Casey and Greg Orr, and others visiting, I felt I could take some courses in English and the symbolist literature. I was hoping and thinking that maybe one day I would learn how to write what was going to be *The Spirit and the Drum*, you see. I wanted to do it right. Eventually, when I did take courses, John Casey helped me a lot. He looked at that manuscript, once I had done quite a bit more to it, and he gave me a great deal of help.

I eventually decided to enroll full-time at the University of Virginia, in English. I was accepted to the M.A. program, despite the fact that I had never earned a Bachelor's degree. There was a question of whether I should do it in anthropology or in English. Of course, Vic and I had been writing poetry and reading great literature, which is a kind of passion in our family. I felt that I did not want to learn what Vic had been teaching me all the time and take classes on what was prevalent then, which was solid structuralism. I felt that if I did, there would be something slightly invidious about it, like nepotism or something. My hesitation was mainly because I had been living in the element of anthropology all the time, and I had my own rather strong ideas about it—about liminality not being subsumed under structure.

ME: How did it feel being in school at 60?

ET: Oh, fine. I could cope much better than most of those graduate kids because I had been with Vic doing a lot of writing. I was more mature. And they were trying, I felt, to get the grades with more or less the least trouble they could. I guess it was a little strange to be in school because I was older than the others, but as you get older you find things less strange anyway, you know. And I felt capable and experienced. One of the professors gave us Henry Miller to study—you know, *Tropic of Capricorn* [1965], real way-out stuff—and I did the best of all on that.

ME: And the administration didn't mind your not having been to college?

ET: Well, they counted my being a coeditor of *Primavera* as pretty important. They also saw other pieces I had written, and there was a very liberal attitude toward women at that time and women that came into studies later in life. And they knew I had done the editing of Vic's books and that I had been collaborating with Vic all this time.

ME: Did you ever think about a Ph.D.?

ET: Yes, obviously, but I was very involved in Vic's work so it didn't seem possible. Of course, I remember the occasion when the possibility came up, and in a way I regret it, and yet I knew that I couldn't.

After Vic died in 1983 I was appointed a lecturer in the anthropology department at UVA, which was appropriate, I think. It was only part-time, and I immediately had a lot more on my plate—it was as if a great deal of what Vic was doing was suddenly put on my plate—so the part-time lectureship gave me time to deal with all this.

ME: Many of your former students have told me about the "performance" seminars at Virginia that you and Vic led, which is also something I want to touch on because it seems as if the move to performativity was carried out most thoroughly at Virginia.

ET: We were trying to convey an understanding of ritual in a way that reading and writing can't capture. This was something that Vic always talked about in his work—that a lot of ritual couldn't be put into words. We'd try to get the students in the spirit of it, and they very quickly couldn't resist. There were strategies to facilitate this. In these settings, you're not getting the structural relationships—"hot and cold," or whatever it is. You have those as well, but you get a sense of the progression, the process, the body. And one has to "suspend disbelief," as old Wordsworth said, and flow with it. Flow is so important—the actual pacing and sense of being right in the thick of things. You can understand through the nonverbal. And when people go to the field, having done anthropology without having tried any performance, they see people fooling around and they don't know what the hell they're at. They write it down and get the structural relationships, but they remain on the outside, because they would have to drop that criticality to understand. If you go in the field and you haven't performed ritual before, and you see "the natives" acting like that, you look for signs of the social construction of reality and you find them, because you find what you're looking for. There are always people running the temple and doing accounts, so to speak. There are always people at pilgrimage centers selling zillions of blue plastic virgins, plastic bottles with "Knock Shrine" printed on them, and so on. So then you have your social construction of reality and its workings and all, but that isn't much, and it isn't always interesting.

ME: This makes a lot of anthropologists uncomfortable.

ET: Oh, yeah. It used to make me uncomfortable.

ME: When was the switch for you?

ET: Well, after we joined the Catholic Church. You see, there are various ways in which Vic wrote. Very hardheaded, but then sometimes—what's the word?—experiential, and with an infinite respect for what was going on. Such is the way he wrote in *Chihamba, the White Spirit*. And in not a very different era he wrote *The Drums of Affliction*, in which he practically analyzed away the true meaning of the Ihamba ritual. These two things were going on side by side. I was usually in the same mode as he. I often saw him responding in this double way to the anthropological material. To forestall the critiques of this, he took a great deal of trouble with scholarship. This is what has kept the discipline in deep respect of Vic's work. I know I'm not the scholar that Vic was, but still, I'm perhaps even more of a maverick than he. I don't give much of a damn, perhaps acting like the naughty one of the family.

ME: That's your favorite role, I think.

ET: Oh, yes, you've got it.

ME: All of this brings us to some key themes in anthropology. Do you go native?

ET: As much as I bloody well can! To me that's the point. There is a slight limitation, but human beings are extraordinarily pervious to each other. As Vic said, there are these prepositional plugs in everybody—to, for, from, against, by, with, of, within, out. Everyone has these plugs, and they plug into other people.

I'm a woman. In a bygone era, the man took the initiative, and the woman would be trying to work along with the man. I knew this from the environment I was in, and I had quite a lot of practice in it. A part of me would say, "Well, if I'm going to be flexible and take on other people's views—a husband's or whatever—I'll take on a lot of other people's views. What the hell is the difference?" This is something of the way my mind argued. I went the way of Vic being a Catholic. He got the sense of it first and I did afterwards, although when I was given the original sense it was very strong to me. So in whatever we did, I delighted in getting alongside others with their agenda. And "getting" it, if I could.

ME: That's an interesting connection, I think, and an interesting crossover between the personal and the professional. Can you say more about how you approached the idea of a relationship with Vic in that sense? My sense is that it was a very complicated mix. You and I

have talked a lot about how you contributed to Vic's work and how it was very much a collaborative effort. And everyone I've talked to who knew you both has said the same thing, without fail. And certainly the two of you had profound impacts upon one another about how you wrote things up. It's this very complicated mix of give-and-take. There are moments when I think you assert your position within this all and make a point of claiming that partnership and a very active role. I think this is important and an accurate sense of the give-and-take. But the way in which you just described it now—and this ties into other ways you have described it—was taking on his mission, adapting yourself to his culture. So it's curious to me in what ways you see yourself adapting to Vic and in what ways you see yourself as a point of reference.

ET: I think I know what you're getting at. I think it's to do with the fact that in my consciousness I understood what Vic's agenda was, and as it developed I understood it. I don't think there was any point at which I didn't understand it. He's not here to ask, but I think he'd say he was doing a lot of things in reference to me. He was testing things off of me to a certain extent. He would frame things in a way that I could absorb, and this is rather sexual, actually. Because I was there, he would do things in a certain way, or frame things in a certain way. I didn't have to tell him what to say, I didn't have to direct. But he knew I was receptive to certain things, and he knew how my mind was moving and was perhaps telepathic. So he would do this, and I would suggest to him what he was thinking, too. You see? And then he would develop it, and vice versa. This was the collaboration.

But then, when you look at the collaboration, as you've insisted upon doing, you see this thing from his point of view. And if he were here (which he isn't), he would be showing this himself, you know? But I was very conscious in this social world that I was not trained at University College, London, and all the rest. Therefore, I valued very much this part of me that was interacting with Vic, and looking at myself as the adaptable person. I was, in a way, determined to develop this like an art form. I would think about this. And therefore, well, I think he translated this into communitas. It was there. It was conscious, but he didn't look at it as a woman would. It wasn't so personal to him, as it sometimes isn't with men. But he did know what communitas was, and he loved it.

So, I think this is how it was. And there is such a thing as being a woman and being a man. It's absolute rubbish to say there are just human beings, because one is very much sexualized. And true, this is structured in our society. Conscious persons know they have to live in this world and will adapt as they can. That's what was going on. Does that answer your question? Or is there something more?

ME: I think that gives me a sense of the connections you see between the two of you and even "going native." Let's talk more about these ideas of "the man" and "the woman" and the different roles, perceptions, and attitudes. I think talking about these as concrete, essential realities is another strong characteristic of your work, something that you don't shy away from. It's also something that a lot of anthropologists would be critical of—not seeing these as categories that can be broken down. You talk about religion in these terms, too. It's something that's not a social construction—which is a very nonanthropological viewpoint.

ET: Absolutely. I'm highly conscious of this. I've been working away at trying to shift this from all kinds of angles. Yes, I'm quite aware of what I'm feebly trying to do.

ME: So tell me something about your latest work, from *The Spirit and the Drum* to what you've been doing in Ireland over the past five years.

ET: I got the manuscript for *The Spirit* under control in the summer of 1985, when I was on my own and there wasn't anybody in the house at all. There were some places in it that I was bothered about, and I had a chance from that May onwards to have a look at it. And I saw that what I'd got was centered on four rituals; the boys' and girls' initiations, the Tukuka healing ritual, and the Chihamba. In the 1980s, the material was more vivid to me. I was more convinced, for instance, that Manyosa had gone into trance. I was more sure of the symbols, more sure that these were a force in themselves in this situation—symbols that were playing their own symphony, as it were. I didn't have any qualms about the way it was written. I cared if people read it, but I didn't want to put it into an academic frame, really. So I thought about it in much the same terms as I had originally written it in the 1950s, but by the 1980s the material was much richer to me. All the work that Vic and I had done over the years confirmed what I wanted to do, confirmed my own sense of the human story, and it is portrayed in *The Spirit and the Drum*.

ME: The reviews of that book are interesting because most of the reviewers obviously didn't know about the history of the book—that it was first conceived and executed in the 1950s. I'm thinking particularly of George Marcus's [1987] review in *Parabola*, where he said that it was a first-rate account of the postmodern approach to writing narrative anthropology. I thought this was a wonderful instance of how the categories we use are tricks to define ourselves. Do you think of that book as a postmodern text?

ET: I think of it, as well as *Experiencing Ritual* [E. Turner 1992], as evidence that this is where anthropology might be going—the richer the better. Human material is almost impossibly rich, and so we have a mandate now to go ahead and unfold the full richness of humankind to the best of our ability. It's there, and we should all try to show it. It's a marvelous field, anthropology, and I see

*Experiencing Ritual* fitting somewhere into all of this. Whether it's postmodern or not, I want to recount the relevant details to anyone who will listen. There should be an accumulation of these pieces to engage the academic stages of theory making.

ME: When I read *Experiencing Ritual* I was struck by the different ways in which you referred to Vic throughout the text. There are passages that create a sense of intimacy, and there are passages that create a sense of scholarly distance. In some passages it's "Vic" or "my husband," and in others it's "as Turner argues," and so on.

ET: The fat and the thin Vic, really. It's like Philip Kabwita, who had a fat and a thin side. Vic had a lean and muscular mind and rather a fat body! You had to respect that lean and muscular mind in the writing and also the other side.

ME: I assume that the specific ways you referred to him were strategically placed.

ET: Of course. To engage with the academic side of anthropology has meant engaging with the canon, and so I've had to think of him in that way. But he was also a very full human being. These were dialogues of a sort.

ME: I have another question to do with the Ihamba ritual you describe and the tooth you saw. I think there would be a lot of anthropologists who would say that it's all a bit crazy, your seeing a spirit form.

ET: Yes, yes. Some people, including some anthropologists, think this is crazy. I've been helped by Roy Wagner in this. The tooth is a peculiarly strong thing, and so, was this going through the veins? And the concept of a spirit tooth is also somewhat strange. Jesus said, "Put your fingers in the holes in my hands and you will believe." This is a spirit figure, coming after the crucifixion, and yet this poor guy Thomas was able to feel it. And people say this is a myth. How could it be?

I was certain it happened to me. I didn't actually see a tiny little tooth coming out of the skin. I saw the spirit object, a gray blob, come out. I don't know whether a concrete tooth came out of the vein, or a spirit tooth as a gray blob came out. But I saw it, whatever it was. And one does not retract things like that, you know? I know it's hard for people, but if they begin to take in a little of the reports they hear (like Evans-Pritchard walking in the Azande village and seeing a spirit light) then we can get somewhere. We haven't sufficiently grappled with these issues, and yet they don't go away. There are always more coming up. It stays like a tooth in our veins, if I can put it that way. We don't know what to do. I just like to go on with this study on the quiet. It's the same with my work in Point Hope, Alaska, and in Ireland. I will always try to get into the thick of things in this way, whether it's the whale spirit in Alaska or visions of Mary at Knock Shrine in Ireland.

Sometimes I wonder what Vic would think of me now. What would he think of me running shamanistic sessions? How would he think of my Catholicism, in which I say, "God the Mother Almighty"? There's a certain feminism in this. What I'm doing now is an extension of the *Chihamba, the White Spirit* side of Vic, not the *Drums of Affliction* side. I obviously take off from the spiritual side of thinking. I don't get any visions or flashes about what Vic would think, but I'm grateful to that guy, and, God, the communitas. The conversations with Vic were marvelous. We would get breakthroughs right and left. Those were great times.

I think we can go on with Victor Turner's work. My work, of course, is relatively obscure, but it does affect a small range of people, and I think there's a certain communitas in it. I always hope we might get some breakthroughs.

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# New Evidence for Paleolithic Rock Painting in Central Europe[1]

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Despite Paleolithic research dating back to the 1860s, little evidence for parietal art has been documented in the caves of Central Europe. However, on August 3, 1998, Patrick Russell, a member of the excavation team at Hohle Fels Cave, located near Schelklingen, Germany (fig. 1), recovered a painted rock fragment from an archaeological horizon containing abundant Magdalenian artifacts. The find was photographed in situ and belongs to geological stratum 1k. This fragment of limestone preserves a double row of seven and a truncated double row of four dark-red, subcircular dots and provides new evidence for rock painting in Central Europe. While an earlier age cannot be ruled out, stylistic and contextual arguments suggest that the depiction dates to the Magdalenian (Conard and Floss 1999). The rich Magdalenian layers of Hohle Fels are well documented and date to ca. 13,000 b.p. (Blumentritt and Hahn 1991, Housley et al. 1997, Conard and Uerpmann 1999).

Hohle Fels Cave is located at an elevation of 543 m above sea level in the Ach Valley near Schelklingen, ca. 20 km west of Ulm. Along with the nearby Blau and Lone Valleys, the Ach Valley, with its many caves, forms the heartland for Paleolithic research in southwestern Germany (Müller-Beck 1983). The cave is one of the largest of the Swabian Jura, with a 30-m-long entrance passage leading to a main hall with an area of 500 $m^2$ and a ceiling as high as 12 m (Blumentritt and Hahn 1991). The entrance passage to the cave, 6 m wide and 3 m high, opens toward the north-northwest and is situated roughly 100 m southeast of and 7 m above the Ach River.

Hohle Fels has been studied by several generations of scholars beginning with the work of Oskar Fraas (1872) and continuing into the 20th century with the work of Robert R. Schmidt (1912). More recently Gustav Riek conducted excavations in the entrance to the cave from 1958 to 1960 (Saier 1994). Further excavations were conducted under the direction of Joachim Hahn of the University of Tübingen in 1977–79 and 1987–96. These excavations again focused on the entrance rather than the presumably more disturbed deposits in the interior of the cave (Blumentritt and Hahn 1991, Hahn 1997*a*). Hahn reopened the excavation at Hohle Fels with the goal of recovering archaeological material from a chronostratigraphic setting similar to that from his nearby excavation at Geissenklösterle (Hahn 1977*a*). Following Hahn's death, excavation at the site has continued under our direction from 1997 to 1999 and has focused on ecological and economic questions related to the Upper Paleolithic of the region. Beyond the important Magdalenian and Gravettian deposits at Hohle Fels (fig. 2), the site shows potential for yielding both Aurignacian and Middle Paleolithic materials, as is the case at sites including Geissenklösterle in the Ach Valley and Hohlenstein-Stadel and Vogelherd in the Lone Valley (Hahn 1977*b*, Müller-Beck 1983).

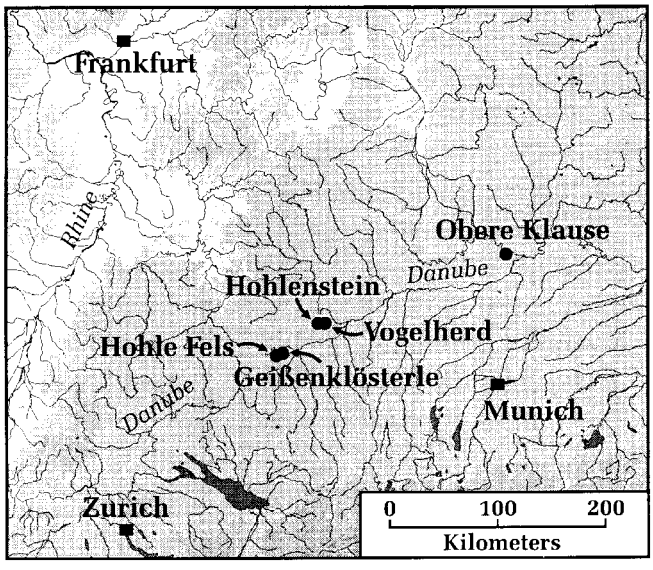


FIG. 1. *Map of Southern Germany showing the position of Hohle Fels and other sites mentioned in the text.*

The debate over the presence of parietal art in Central Europe has lasted for decades and has been characterized by a series of claims and subsequent refutations for the existence of cave painting since the recognition of Paleolithic cave paintings in France and Cantabrian Spain at the turn of the 20th century (Conard and Floss 1999). Over the past three decades, the work of Joachim Hahn has played a central role in regional research on Paleolithic art. Hahn's excavations at Geissenklösterle provided new evidence for Aurignacian figurines (Hahn 1986) that complemented the previously excavated mammoth ivory statues recovered from Vogelherd (Riek 1934) and Hohlenstein-Stadel (Schmid 1989, Hahn 1986). Susanne Münzel's archaeozoological work also led to the

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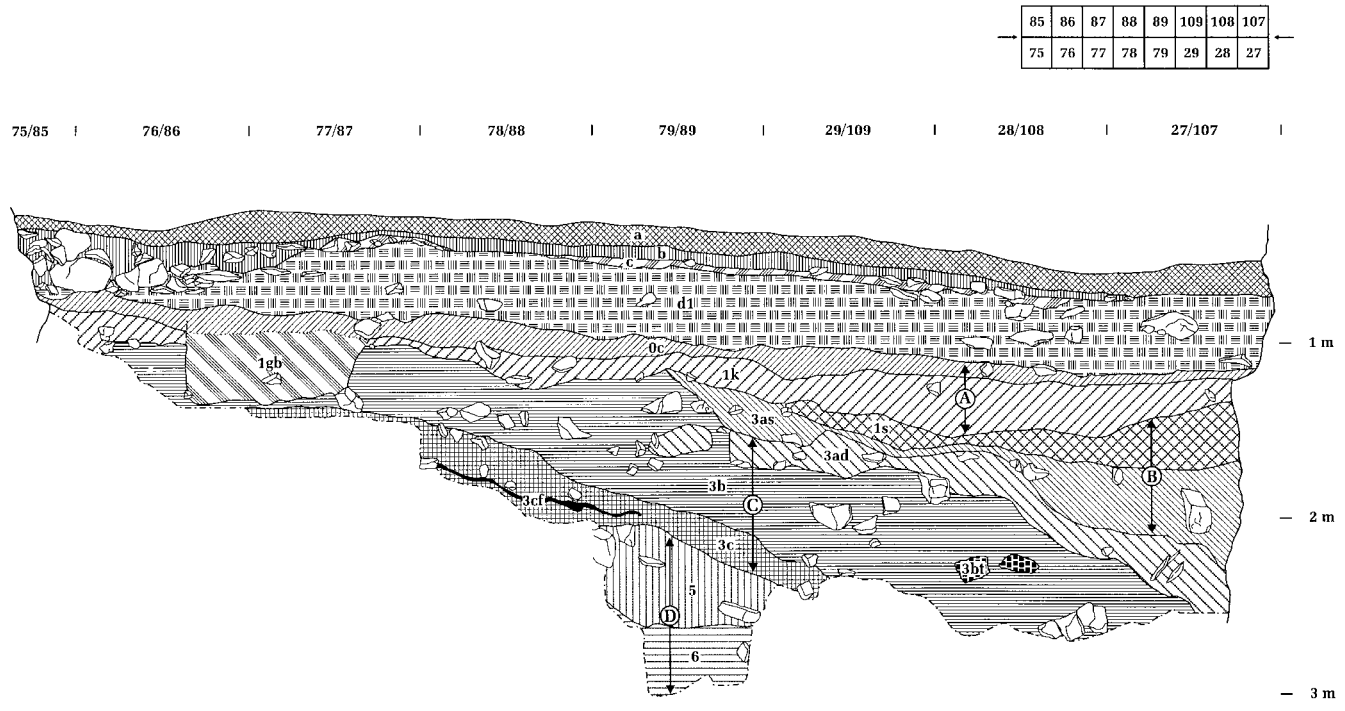


FIG. 2. *Schematic stratigraphic profile of Hohle Fels including four major stratigraphic units (after Conard and Uerpmann 1999):* A, *with finds from the Magdalenian and radiocarbon ages of ca. 13,000 b.p.;* B, *corresponding to the last glacial maximum, ca. 20,000 b.p.;* C, *containing Gravettian finds and dating to ca. 29,000 b.p.; and* D, *containing early Upper Paleolithic finds from a test excavation.*

recovery of the remains of two bone flutes from Geissenklösterle including one made from the radius of a swan (Hahn and Münzel 1995). These musical instruments and the ivory figurines from the site stem from the Aurignacian find horizon II and date to ca 33,500 b.p. with radiocarbon and 37,000 b.p. with thermoluminescence (Richter et al. 2000).

Prior to the 1970s, painted stones had been recovered from several sites in southern Germany. Best-known among these are several painted stones from the Magdalenian which were excavated in 1912 by J. Fraunholz in collaboration with H. Obermaier at Obere Klause (Obermaier 1914, Freund 1963, Bosinski 1982). Additionally, E. Soergel and W. Soergel recovered a 9.4 × 5.5 × 2.2-cm painted cobble of either Magdalenian or Late Paleolithic age from Hohlenstein–Kleine Scheuer in 1923 (Wetzel 1961, Hahn and von Koenigswald 1977), while G. Riek's excavations at Vogelherd (Riek 1934) and Hohle Fels (Saier 1994) provided further examples of stones and rock fragments with traces of pigment. Saier (1994) describes five painted rock fragments from the Magdalenian layers of Riek's excavation at Hohle Fels and has been able to refit one specimen to a broken cobble from Hahn's more recent excavation there. Among the early finds from Obere Klause, Kleine Scheuer, and Hohle Fels, rows of small red dots and faint lines constitute the most common motifs. Despite the existence of earlier ivory figurines from the Aurignacian, female figurines and occasional figurative engravings from Gravettian contexts, and hundreds of figurative engravings from the Magdalenian (Bosinski 1982, Hahn 1986, Scheer 1994), figurative painting is entirely unknown in Germany. Moreover, with the possible exception of a purported painted animal from Býci Skála Cave (Oliva 1996), figurative painting has remained absent in the Paleolithic of Central Europe.

Hahn's careful excavations at Geissenklösterle and Hohle Fels provided more recent evidence for Paleolithic painting in addition to the stones mentioned above. Noteworthy are two finds of Aurignacian age from Geissenklösterle. One is an 8.5 × 6 × 4.5-cm piece of limestone with red, yellow, and black pigment from layer IIb. The other is a limestone fragment from the lower Aurignacian layer IIIa, which Hahn (1988) describes as preserving a black V-shape. Hahn argues that this 10 × 10 × 3-cm piece, which appears to stem from the wall of the cave, was intentionally painted. While this interpretation is plausible, the irregular nature of the black and brown color could be the result of natural processes or incidental human agency. Hahn's excavations at Hohle Fels have also yielded at least three examples of stones preserving traces of red pigment (Blumentritt and Hahn 1991, Scheer 1994). These often poorly preserved finds, as well as the material from earlier excavations, provide

convincing evidence for nonfigurative painting on mobile objects, small stones in particular. Before the discovery of the new find from Hohle Fels, however, no convincing evidence for cave painting had been recovered within Germany. While the new depiction stops short of furnishing definitive proof of the existence of cave painting in the region, it does provide the best evidence thus far for parietal art in Germany.

The new find from Hohle Fels measures 7.6 × 5.9 × 1.7 cm and was recovered from geological horizon 1k. The painted surface is smooth, whereas the reverse side preserves unweathered, angular surfaces. The find preserves two double rows of 4–7-mm oval red dots (fig. 3). One double row is complete and depicts seven subparallel pairs of dots. The other double row includes four pairs of dots and is clearly truncated, indicating that the depiction originally continued beyond the limits of the current piece.

What distinguishes this find from the objects mentioned above is the excellent preservation of the pigment and particularly the recognition that the reverse side preserves unweathered, angular surfaces. This observation indicates that the limestone fragment in all likelihood stems from the wall of the cave. This stone fragment is composed of the same granular Upper Jurassic limestone that forms the walls of the cave. While fragments of the cave wall are extremely common within the Paleolithic find horizons of the cave, this is the first such fragment to preserve unambiguous evidence for painting. The collapse and fragmentation of the cave walls have been closely documented by Hahn (1991) in connection with his study of scratches on polished surfaces of the former walls of Hohle Fels. The apparently ubiquitous fragmentation and collapse of the cave walls of the region may help to explain the scarcity of parietal art in the region. While the theoretical possibility exists that the limestone fragment was painted after it fell from the wall, the truncation of the depiction and the fresh, angular nature of the broken surfaces of the specimen indicate that it was originally part of a larger representation on the wall of Hohle Fels rather than a piece of mobile art.

In the context of southern Germany, the motif painted on the new find from Hohle Fels shows particularly strong similarities with the best-known painted stone from Obere Klause, where three double rows of seven small red dots are depicted on a rounded, 16-cm-long, elongated piece of limestone (Obermaier 1914, Müller-Beck and Albrecht 1987). Although the general absence of parietal art makes comparisons within Central Europe impossible, diverse depictions of red dots and rows of red dots are well known in the Paleolithic art of Western Europe, for example, at Niaux (Clottes 1995), Grotte Carriot (Lorblanchet 1984), and Grotte Le Travers de Janoye (Clottes and Lautier 1984; Bosinski, personal communication, 1999).

This painted wall fragment from a Magdalenian layer at Hohle Fels provides the best evidence to date for parietal art in Germany and helps to fill a gap in our knowledge of Paleolithic art that appears to be in part dictated by the poor preservation of cave walls in the karst region of Central Europe.

FIG. 3. *The painted fragment of limestone from Hohle Fels, scale in cm.* (*Photo H. Jensen*)

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# Cooperative Reproduction in Ituri Forest Hunter-Gatherers: Who Cares for Efe Infants?[1]

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Efe foragers of the Ituri Forest, Democratic Republic of the Congo, share in a unique child-rearing system in which infants receive care from many individuals other than their mothers from birth into early childhood (Tronick, Morelli, and Winn 1987, Tronick, Morelli, and Ivey 1992). Cooperative reproduction is highly unusual from an interspecific perspective and is especially challenging to evolutionary theory, compelling primatologists and biologists to devote considerable attention to parenting behaviors exhibited by alloparents (individuals other than the parent) toward conspecific young (e.g., Reidman 1982, Emlen 1984, McKenna 1987, Small 1990, Clutton-Brock 1991). From an explosion of research on animal behavior since the 1960s, three general hypotheses have come to dominate ecological perspectives on apparently altruistic parenting behaviors: nepotism, reciprocity, and learning-to-mother.

1. *Nepotism* predicts a substantial amount of variation in alloparenting both within and between species (McKenna 1987). Investing in kin is considered an extension of investing in one's own genetic reproduction, as the degree to which genes are shared is expected to predict shared fitness interests. This equation, however, like all evolutionary predictions, is economic in nature and weighted by the relative costs and benefits to individuals of alternative behaviors within a specific environmental context (Williams 1966, Altmann 1979, Emlen 1995). The costs and benefits of particular behavioral strategies are determined by ecological interactions of the social and physical environment and individual life-history parameters affecting survival, growth, development, and reproduction. Human life history sets the stage for at least two important opportunities for kin, as well as others, to care, with important coevolutionary consequences. Parents nurture multiple weaned dependents,

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TABLE 1
*Caregivers Predicted by Alloparenting Hypotheses*

| Allocaregiver | Nepotism | Reciprocity | Learning-to-Mother |
|---|---|---|---|
| Adult male | ✓ | – | – |
| Adult female | | | |
| Reproductive | ✓ | ✓ | – |
| Nonreproductive | ✓ | – | – |
| Postreproductive | ✓ | – | – |
| Child male | ✓ | ✓ | – |
| Child female | ✓ | ✓ | ✓ |

increasing the demand for and availability of care (Lancaster 1997). Consistent with theoretical indications that a lengthy developmental period favors allocare by retained related "helpers-at-the-nest" (Lack 1966, Brown 1987, Koenig et al. 1992), sibling care is the most common form of alloparenting in traditional societies (Weisner and Gallimore 1977). In addition to the prereproductive period, early female reproductive senescence may increase the probability of completing investment in later-born young (Lancaster and King 1985, Hill and Hurtado 1991, Hawkes et al. 1998) and enhance the reproductive efforts of adult offspring through assistance in food getting (Hawkes 1997) or child care (Hill and Hurtado 1996).

2. *Reciprocity*. Other interspecific research suggests that where the costs of aiding unrelated young are less than the costs of leaving the group (e.g., because of predation pressure, low food availability, or social competition), unrelated helpers may assist parents in return for enhanced access to physical (e.g., food, territory) and social (e.g., alliance, mating) resources necessary for reproduction (Ligon 1983, Reyer 1984, Davies 1990). Emlen (1982*a*, *b*) modeled the fitness payoffs of helping between unrelated individuals, concluding that contributing resources to the reproductive success of an unrelated individual would be a successful strategy for both helper and beneficiary if they resided in a marginal environment with highly unpredictable access to the resources necessary for reproduction. Comparative analyses suggest that cooperative breeding is found in environments that are extremely limited in resources or saturated with conspecifics, limiting opportunities for juvenile or subordinate individuals to secure food, mates, space, or other resources necessary for independent reproduction (Clutton-Brock 1991). By definition, small traditional societies lack the intensive stratification that results in large-scale subversion of the reproductive interests of some members of the group to the advantage of others. However, cross-cultural research confirms that demographic, economic, and social limitations commonly impinge on an individual's ability to mate and parent young in these populations (Irons 1983; Hill and Kaplan 1988*a*, *b*; Bailey 1991*a*; Hill and Hurtado 1996). The extent of cooperative behavior exhibited within human groups, on a scale unparalleled in other species, suggests that in some ecological contexts allocare by unrelated but frequently interacting individuals may be included in the suite of shared and reciprocal social behaviors, such as cooperative resource acquisition and food sharing, that characterize traditional behavioral patterns.

3. *Learning-to-mother*. Like the reciprocity hypothesis, the learning-to-mother hypothesis predicts that personal but delayed fitness benefits are associated with alloparenting. Through skills gained from caring for the young of others, prereproductive individuals may increase the chances of survival for their own future offspring without incurring the risks to their young of inexperienced care (Spencer-Booth 1970, Lancaster 1971, McKenna 1987). This hypothesis is based on several observations: (*a*) parenting skills do not appear to be innate among primates; (*b*) survivorship among primate offspring is highly dependent on the quality of care they receive; and (*c*) the infant mortality rate for primiparous female primates is higher than that for multiparous ones. Field studies report increasing reproductive success with age due to increasing reproductive skills in a number of nonprimate species as well (Lack 1966, Charlesworth 1980, Clutton-Brock 1988). Perhaps in no species is the quality of care more critical to developmental outcome and future reproductive success of young than in humans, with important life-history consequences for parents, caregivers, and their wards (Bogin 1998, Charlesworth 1988, Hrdy 1992, Chisholm 1999). As predicted by their future role as mothers, cross-culturally, young girls most frequently perform allocare (Barry, Bacon, and Child 1957, Weisner and Gallimore 1977).

The hypotheses of nepotism, reciprocity, and learning-to-mother suggest specific life-history strategies for alloparenting the young of others (table 1). Evolutionary ecological theory predicts that caregivers will allocate investment on the basis of the inclusive-fitness costs and benefits of providing care, with individuals giving care to the closest dependent who is likely to benefit from child care efforts. The probability and amount of investment should be determined by (1) the degree of relatedness between the potential caregiver and the child and (2) the ratio of the cost of care to the caregiver's fitness to the fitness benefit received by the child. While ecological theory implies an economic impact (i.e., cost or benefit) of behavior on individual reproduction, measuring the survival or reproductive consequences associated with specific behaviors remains challenging (Rogers 1990, Clutton-Brock 1991, Lessells 1991, Kaplan 1997). The potential overlap of individual investment interests (e.g., kinship and reciprocal interests) and the competing tradeoffs of alternative behaviors (i.e., opportunity costs) complicates the task of assigning costs and benefits of behaviors to individual fitness. With regard to these constraints, this investigation assumes a temporally proximate focus: to examine the distribution of Efe infant allocare across caregivers to assess the explanatory strength of alternative hypotheses.

## ALLOPARENTING AMONG THE EFE

While some form of allocare has been described, at least qualitatively, in most hunter-gatherer societies, the most

extreme example of alloparenting in a foraging population has been reported for the Efe of the Ituri Forest. Data collected in 1982–83 showed that the percentage of time young infants spent in physical contact with individuals other than their mothers increased from 39% at 3 weeks to 60% at 18 weeks. During observations infants were cared for by an average of 14.2 different persons, with a range of 5 to 24 (Tronick, Morelli, and Winn 1987). Tronick et al. hypothesized that this communal pattern of care was a cultural adaptation to the thermoregulatory challenges faced by Efe infants. Infants on average weigh 2.4 kg at birth, a weight considered "at risk" in the Western medical context (Tronick and Winn 1992). Peacock (1985) and Hewlett (1991) have suggested that the unique pattern of Efe infant care may be explained by high rates of infertility among reproductive-age Efe women. Tronick, Morelli, and Winn (1989), however, found that the frequency of care by nulliparous adult females did not account for the extent of allocare. A number of studies describe Efe child care from a developmental point of view (Tronick, Winn, and Morelli 1987; Morelli and Tronick 1991; Morelli 1987, 1997; Tronick, Morelli, and Ivey 1992); however, it remains unclear why so many individuals among the Efe forfeit their time and energy to provide care to the young of others. This investigation was prompted by the challenge of alloparenting behavior to ecological precepts: Who cares for Efe infants, and what are the costs and benefits to alloparents of child care services rendered?

## THE STUDY POPULATION

A corpus of research provides details of the environment and lives of the Efe (cf. Morelli 1987, Peacock 1985, Ellison, Peacock, and Lager 1986, Tronick, Morelli, and Winn 1987, Wilkie 1988, Fisher and Strickland 1989, Jenike 1987, Bailey and DeVore 1989, Bailey 1991*a*, Wilkie and Curran 1993). The Efe are widely accepted to be the most traditional population of pygmies in Africa. They associate with horticultural groups in an elaborate exchange system whereby farmers trade cultivated foods and material goods (e.g., cloth and metal) for the valuable forest resources of meat, honey, medicines, and building materials (Wilkie 1989, Bailey 1991*a*). The Efe periodically provide labor in the gardens of the Sudanic-speaking Lese; however, since the end of colonial harvest quotas in the 1960s, the severe deterioration of roads, and the collapse of the cash market since the 1980s, most Lese gardens have contracted to subsistence level. Labor demands are intermittent, and many Efe lack access to opportunities for garden work. While some Efe have established their own gardens, they tend to be small and communal, with low and unpredictable yields (Wilkie and Curran 1993). Only one focal family in this investigation planted a small shared seasonal garden of cassava.

The Efe live in camps ranging from 6 to 45 people, with an average of 21. Typically, they clear a small area of forest (10–15 m diameter) and construct low huts in an open semicircle around a communal space in which most daily camp activities occur. Although descent is patrilineal and residence is virilocal, maternal relatives may also live in the natal camp because of sororal marriage exchange between clans. Nuclear families share a hut, which is primarily used for storage and sleeping, and a cooking hearth, but children, including infants, are by no means restricted from playing, exploring, and even sleeping in other areas of camp. Efe women usually travel together in small groups to gather forest produce, such as fruits, nuts, tubers, and mushrooms, fish in the streams that traverse the forest, or forage for bananas, cassava, and sweet potato in abandoned gardens. Garden labor is highly seasonal; during planting and harvest Efe women may assist Lese women, and Efe men are usually engaged in horticultural work only to fell trees when new gardens are cleared from the forest. Efe males hunt with bow and arrow in groups, using dogs and hunters to flush game to waiting bowmen, or hunt primates solitarily by stealth. Camps move on an average of every six weeks in response to changing access to forest and horticultural resources, from near-village gardens during planting and harvest to deeper in the forest during prime honey, fishing, and hunting seasons (Bailey and Peacock 1988, Wilkie and Curran 1993).

## METHODS

Data were collected between January 1988 and October 1989 in 18 camps within a 36-km radius of the Ituri Project research station in northeastern Democratic Republic of the Congo. The focal subject sampling technique (Altmann 1974, Borgerhoff Mulder and Caro 1985) was adapted to record the behaviors of infant and mother simultaneously across all contexts, with the infant as the priority focal subject. The focal sample consisted of 20 infants (13 females and 7 males) between 12 and 15 months of age. Infants were observed for eight 15-minute sessions sampled across two consecutive and typical days, evenly distributed across daylight hours. Behaviors were continuously recorded as they occurred on a laptop computer that simultaneously tracked real time, facilitating a calculation of the absolute duration of events. Time measures were adjusted to a 12-hour day. Scans recording the identity of all individuals within visual or close hearing range of the infant (i.e., within a reasonable distance to respond to infant distress) were conducted immediately before and after each block of continuous behavioral coding, and departures and approaches of individuals were recorded as they occurred, to calculate the total proportion of time that individuals were in physical proximity to the infant. All exchanges of material goods, including food and other resources, between parents and individuals other than dependent children were recorded, whether they occurred within coding periods or not. Systematic and informal interviews were conducted with mothers and other caregivers in an attempt to elucidate other avenues of reciprocity, including friendships and economic associations. The habituation period included several months of frequent and extended visits to camps. Because data were simulta-

TABLE 2
*Multiple Logistic Regression Model of the Probability of Allocare*

| Variable | Parameter | $p$[c] | Odds Ratio[d] | D.F. | $p$ in Model[e] |
|---|---|---|---|---|---|
| Proportion of time in proximity | *controlled* | | | | |
| Age | *controlled* | | | | |
| Sex | *controlled* | | | | |
| Reproductive status[a] | | | | | |
| Prereproductive | −.204 | .6934 | .815 | 3 | .0152 |
| Nonreproductive | .927 | .0139 | 2.527 | | |
| Postreproductive | 1.155 | .0426 | 3.173 | | |
| Relatedness[b] | | | | | |
| .01 to .125 | .975 | .0401 | 2.652 | 3 | <.0001 |
| .25 | .859 | .0467 | 2.362 | | |
| .50 | 2.873 | <.0001 | 17.684 | | |

Model logLikelihood = 242.958

[a]Compares prereproductive, nonreproductive, and postreproductive with the reference group, reproductively active individuals (i.e., adults with dependent offspring).
[b]Compares three categories of estimated genetic distance of kin (.01 to .125, .25, and .50) with the reference group of nonkin (.00).
[c]Wald statistic based on the chi-square distribution to test the null hypothesis that variable categories are unrelated to allocare.
[d]Approximates how much more likely (or unlikely, if less than 1) allocare is to be performed by the category compared with the reference group.
[e]The −2 likelihood ratio test based on the chi-squared distribution of change in the model with variables added independently with control variables and an increase of 3 degrees of freedom.

neously collected for a longitudinal study of infant socioemotional development (Tronick, Morelli, and Ivey 1992), the observer was often known to the infant from birth or shortly thereafter. Interobserver reliability was established in the field with a colleague and calculated (Cohen's kappa), yielding a mean kappa coefficient of .90 (range .85–.96) across all behaviors.

Demographic records for 8 of the 18 camps have been maintained since 1979 by Ituri Project researchers and local assistants. New records were created to include camps not included in prior censuses and were cross-verified in interviews with group members and members of associated camps and through independent data collected by a local assistant. The ages of many individuals in the study area were known from previous records and local census taking. Unknown ages were interpolated relative to known-age individuals and specific local historical events of known date and cross-checked with dental exams (for children) and interviews. Lineage histories of two or more generations' depth have also been maintained by the Ituri Project researchers. Kinship was independently verified by Efe camp members and local assistants, and new records were created for the remaining camps. The Efe maintain distinctions between consanguineal, affinal, and fictive kinship, facilitating the calculation of genetic distance between infants and others. Individuals identified as consanguineally related to the focal infant are ranked according to estimated genetic distance (e.g., full siblings are related .50 to an infant, aunts .25). Relatives related more distantly than an estimated .125 to the infant are grouped in the analyses. Individuals identified as not consanguineally related to the infant are treated as unrelated. An allocaregiver was defined as anyone other than the mother in physical or social contact with the focal infant.

The question "Who cares for infants?" may be conceptualized analytically in a number of ways. The logistic regression technique (Hosmer and Lemeshow 1989) is intuitively and theoretically appealing, as odds ratios assigned to categorical covariates in a model allow for a biologically meaningful interpretation (see Hill and Hurtado 1996). Because the probabilistic outcome variable is dichotomous (that is, allocare or not), other multivariate techniques are used to take advantage of the sensitivity and precision of the data set, where behaviors are measured continuously. The analyses include (1) general descriptive statistics of care across infants, (2) logistic regression modeling of the probability of allocare among potential caregivers, (3) linear regression modeling of demographic variables on time in allocare, (4) linear regression fit of allocaretaking time to classes of potential caregivers (e.g., boys, girls, adult males and females, postreproductive adults), (5) assessment of the cost of care to actual allocaregivers, and (6) analyses of ecological correlates of allocare. Data were recorded on a program designed by David Wilkie and analyzed in StatView (1998) and SAS statistical programs.

## RESULTS

*General descriptives*. Across the 20 infants in the sample, the total population of *potential* allocaregivers, that is, anyone other than the mother in proximity to a focal infant during observations, is 412. Efe one-year-olds are in close proximity to a caregiver 100% of observed time and spend an average of 85% of observed time in direct care (i.e., physical contact or social interaction with a

TABLE 3
*Multiple Linear Regression Model of Infant Allocaregiving Time*

| | Coefficient | $p$ |
|---|---|---|
| Time in proximity to infant | .011 | .0222 |
| Sex | −.012 | .0151 |
| Age | .016 | .002 |
| Age$^2$ | −.0003 | .007 |
| Relatedness | .124 | .0001 |
| Reproductive status[a] | −.023 | .0019 |

Model $F = 16.307$, $p = .0001$, $r^2 = .20$ [$n = 412$]

[a]Dichotomous, referring to the presence or absence of dependents.

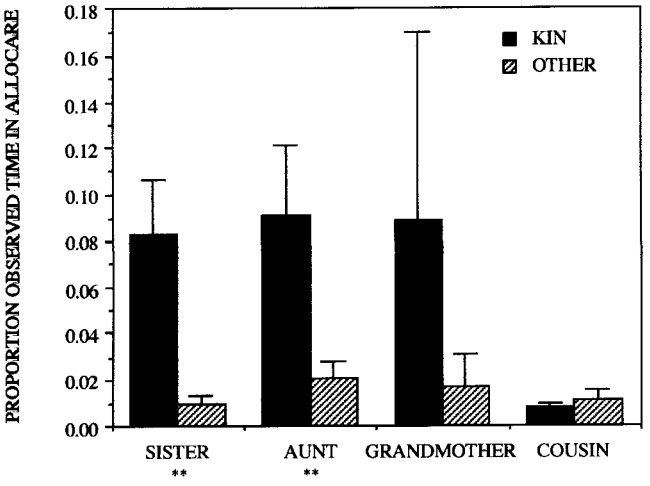


FIG. 1. *Proportion of observed time in allocare by female social kinship category compared with age-matched unrelated females.* **, p = *.0001.*

caregiver, including the mother). Infants receive 41% of care from individuals other than the mother, and fathers present (two died before the infant was one year of age and two were absent from camp on extended hunting trips) averaged 8% of total care and 20% of alloparental care. The mean number of allocaregivers interacting with the focal infant during observations is 11, with a range from 2 to 21. Infant sex is not associated with the number or any demographic characteristic of caregivers or with the amount of care received.

*Logistic regression model of the occurrence of allocare.* Neither the proportion of time in proximity to an infant nor the sex of the potential caregiver is significantly associated with the probability of performing allocare. Age, however, is negatively related to the probability of care (univariate beta coefficient = .023, chi-square = 15.964, $p < .0001$). Table 2 presents the best-fit logistic regression model, controlling for proximity, sex, and age of potential interactant: reproductive status and genetic relatedness are significantly associated with the probability of allocare. There are no significant interaction effects between variables. Odds ratios presented assess how much a particular factor—controlling for others—increases or decreases the likelihood that alloparenting will occur, using the absent category (reproductively active [i.e., with dependent offspring] and unrelated [estimated genetic distance = .00]) as the basis of comparison. Prereproductive individuals (4 to 17 years) are no more likely to perform allocare than reproductively active adults (18 to 49 years), but nonreproductive adults are 2.5 times more likely and postreproductive individuals (50 years and older) 3 times more likely to alloparent than reproductively active adults. The effect of relatedness is substantial: kin are more than twice as likely as nonkin to contribute to allocare, and siblings and fathers (genetic relatedness = .50) are over 17 times more likely to perform infant care.

*Linear regression model of the amount of time spent in allocare.* Variation in the amount of time that individual Efe devote to infant allocare is considerable, ranging from 0 to 5.8 hours per day. In a full model regression of independent variables describing the life-history variation of the 412 potential allocaregivers, time in proximity and the characteristics of sex, age, relatedness, and reproductive status strongly predict the amount of time that individuals spend caring for a focal infant as main effects ($p = .0001$) (table 3), accounting for 20% of the variance in time Efe engage in allocare of one-year-olds. There are no significant interaction effects. Relatedness accounts for only 3% of the variance in time in proximity to an infant and explains 16% of the variance in allocaregiving time. To standardize variance in access to a related infant between individuals within each group, *z*-scores of relatedness were assigned to potential allocaregivers, with similar highly significant results on allocaregiving time ($p = .0001$). The time spent in proximity to an infant is significantly related to the amount of observed time that individuals provide care in univariate analysis ($p = .001$) but explains little of the variance between individuals in allocaregiving time ($r^2 = .03$) and is controlled for in subsequent analyses.

*Comparisons between individuals in allocaregiving time.* The significance of relatedness to participation in alloparental care is apparent when more familiar and socially meaningful categories reflecting sex, age, and kinship are considered. Among females, sisters and aunts spend significantly more time interacting with infants than sex- and age-matched unrelated individuals (fig. 1). While the mean proportion of observed time in allocare is several times higher for grandmothers than for unrelated age-matched females (grandmothers = .09 [$n = 4$], others = .02 [$n = 8$]), the sample sizes are small, limiting significance testing. It is of interest that while grand-

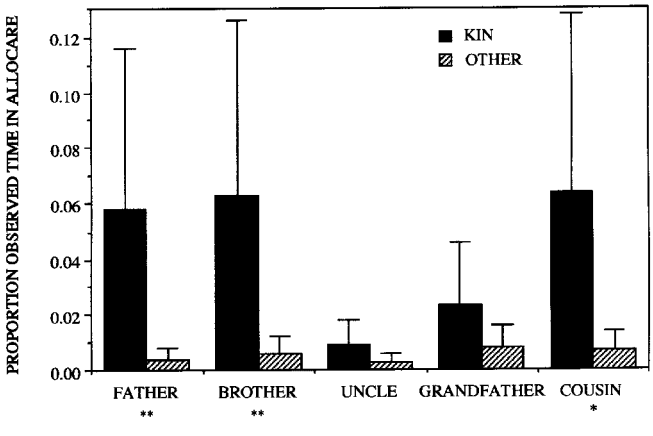


FIG. 2. *Proportion of observed time in allocare by male social kinship category compared with age-matched unrelated males.* **, p = *.0001;* *, p < *.005.*

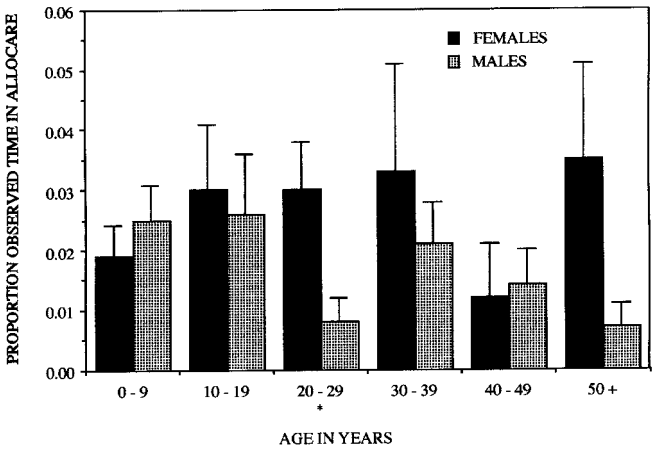


FIG. 3. *Proportion of observed time in allocare by sex and age category.* *, p = .05.

mothers perform more allocare than other older women, because of high mortality very few infants or their mothers have access to grandmaternal care. Among males, only fathers, brothers, and cousins spend more time in allocare than do sex- and age-matched unrelated Efe (fig. 2).

Overall, differences between the sexes in time allocation to alloparenting are significant, but the prediction of increased allocare by females over males does not hold across all ages (fig. 3). When ages are grouped into 10-year intervals, the mean amount of time spent in interaction is actually greater for males than females in the youngest age-range, from 0 to 9 years, although the difference does not reach significance. There is no significant difference between the sexes until reproductive age at 20 to 29 years, when the mean for males declines sharply. When fathers are excluded from the analyses, there is a significant difference in allocaretaking time between males and females throughout the reproductive period (i.e., 20 to 49 years). Sex differences for postreproductive-age adults, 50 years and older, approach but do not reach significance ($p = .08$), with older females contributing more than males to infant care.

Reproductive status has an independent effect on the time that Efe women devote to infant allocare, controlling for the effects of age. Reproduction (i.e., parenting) influences the allocaregiving time of reproductive-age females but not reproductive-age males, whether measured as the presence or number of dependent offspring, the age of the youngest dependent, or the presence of a nursing infant. With the exception of fathers, reproductive-age males perform little allocare. While prereproductive and postreproductive females do not differ significantly from reproductive-age women with or without dependent offspring, nonreproductive women spent significantly more time in infant allocaregiving than reproductively active women (fig. 4). There is a suggestion that nursing mothers are especially constrained (or uninterested) in alloparenting: the mean amount of allocare that nursing mothers perform is half of that of mothers without nursing infants.

*Regression fit to hypothesized allocaregivers.* Separate regressions were fit for categories of allocaregivers predicted by theory (table 4). Only relatedness to the infant weakly predicts alloparenting by adult males (18 years and older) excluding fathers, and no variables predict care by postreproductive (50 years and older) males. The time that prereproductive age males (4 to 17 years) spend interacting with an infant is positively associated with relatedness and negatively associated with the number of dependents (i.e., siblings and foster children) in their own families. If siblings of the focal infant are removed from the analysis, the number of dependents in the family is no longer significant, suggesting that males provide care to infants when there are few other dependents to assist parents. Similarly, allocaregiving by young females (4 to 17 years) is predicted by relatedness and the presence of siblings in their own families. Again, only care by sibling allocaregivers is significantly affected by other dependents in the family. Relatedness alone is predictive of allocaregiving by nonsibling children. Only time in proximity to an infant is related to care by reproductive-age women (18 to 49 years) without dependent young, and only relatedness is predictive of allocaregiving by women with dependent offspring. Time spent alloparenting by postreproductive-age (50 years and older) women is predicted by relatedness alone.

*Reciprocity and resource exchange.* Resource exchanges involving fathers proved impractical to track outside the camp context, and there is no relation between paternal exchanges in the camp setting and allocaregiver participation. Hunted resources are primarily distributed at the kill site, and secondary distributions of meat often occur outside the camp context (e.g., in trade with horticulturalists). Occurrences of resource exchange involving mothers outside of the nuclear

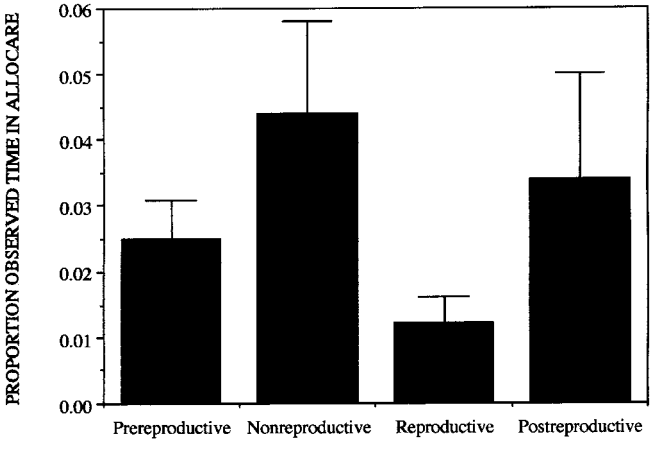


FIG. 4. *Proportion of observed time females spend in allocaregiving by reproductive status.*

TABLE 4
*Regression Models of Observed Time in Allocaregiving Fit for Hypothesized Categories of Allocaregivers*

| Category and Variable | Coefficient | $p$ | $r^2$ |
|---|---|---|---|
| Adult males excluding fathers (86) | | | |
| Relatedness | .005 | .0564 | .20 |
| Prereproductive males (90) | | | |
| Relatedness | .127 | .0002 | .15 |
| Number of dependents in family | −.007 | .0649 | |
| Prereproductive females (70) | | | |
| Relatedness | .533 | .0024 | .28 |
| Presence of siblings | −.19 | .0270 | |
| Nonreproductive females (33) | | | |
| Time in proximity | .060 | .0095 | .20 |
| Reproductively active females (43) | | | |
| Relatedness | .160 | .0584 | .09 |
| Postreproductive females (23) | | | |
| Relatedness | .326 | .0339 | .20 |

household are surprisingly infrequent, and there is no association between individuals who receive food or other resources (including child care) from mothers and the relatedness or allocaregiving contribution of the recipient. Women typically forage together in small groups, but with the exception of fishing and occasional abundant food patches resources are usually collected individually and, in the case of food, distributed after processing to children and other dependents in the household. The Efe possess few material resources, such as clothing or tools, and the transfer of these goods also does not appear to be associated with allocare.

There is also no relation between women identified by mothers as "friends" (described as individuals that mothers tend to associate and share with most frequently or those who might provide care for the mother or her family in case she were ill) and resource exchanges, relatedness, or allocaregiving. It may be of interest that 60% of women identified as "friends" ($n = 25$) have no dependent offspring and that, while nonreproductive women represent 32% of the total population of reproductive-age (18 to 49 years) women, they account for only 12% of alloparental care received by infants. Women with children closely associate with one another but do not habitually share child care responsibilities. Efe alloparenting is not characterized as a creche or nursery system whereby mothers leave their infants in camp with others for extended periods while foraging: 92% of observed time mothers were within visual or close hearing range of their infants. Instead, working mothers are often accompanied by other, less burdened helpers, such as children.

The presence of foster children among the Efe suggests that alloparenting by some juveniles may be reciprocated with provisioning and protection by adults. Indeed, 27% of children have no parents in camp—orphans, children of separated parents, or those living temporarily away from natal camps—and Efe children are commonly transferred from high-dependency-ratio families to low-dependency-ratio families for a period of months or even years. While children 4 to 17 years of age without parents living in the camp do not contribute significantly more time in allocaregiving than children with parents residing in camp (excluding siblings of the infant in the comparison), foster children residing in a subject infant's nuclear family ($n = 11$) spend significantly more time providing allocare than do other nonsibling children in the camp (Mann-Whitney $U$, $p = .0035$). This finding holds when relatedness to the infant is controlled for in the analysis. (Foster children are related on average .10 to focal infants.) In fact, the mean amount of allocare that foster children in the subject family provide exceeds that of siblings, although the difference does not reach significance. The Efe commonly recruit older children, both male and female, who are orphaned or belong to large sibships to assist primiparous mothers or mothers who lack older offspring to assist with infant care. However, not all new mothers have access to the services of

TABLE 5
*Distribution per 12-Hour Day of Mean Time in Care of Focal Infant, Active Care, and Simultaneous Economic Activities by Actual Caregivers* ($n = 222$)

| Caregiver[a] | $n$ | Mean Time (hr:min) | Active Care (hr:min) | Economic (hr:min) |
|---|---|---|---|---|
| Mother | 20 | 6:01 | 1:16 | 1:23 |
| Father[b] | 14 | 0:50 | 0:08 | 0:07 |
| Sister | 15 | 1:08 | 0:11 | 0:05 |
| Brother | 26 | 0:47 | 0:06 | 0:03 |
| Aunt | 6 | 1:16 | 0:12 | 0:07 |
| Uncle | 3 | 0:28 | 0:02 | 0:00 |
| Grandmother | 3 | 1:25 | 0:08 | 0:18 |
| Grandfather | 1 | 0:33 | 0:00 | 0:00 |
| Related female child | 7 | 0:32 | 0:02 | 0:05 |
| Related male child[c] | 10 | 1:02 | 0:14 | 0:05 |
| Related female adult | 1 | 1:42 | 0:00 | 0:00 |
| Related male adult | 0 | 0:00 | 0:00 | 0:00 |
| Unrelated female child | 29 | 0:16 | 0:04 | 0:01 |
| Unrelated male child | 30 | 0:09 | 0:02 | 0:01 |
| Unrelated female adult | 38 | 0:37 | 0:09 | 0:10 |
| Unrelated male adult | 19 | 0:11 | 0:02 | 0:03 |

[a]Categories are mutually exclusive. For example, related female child excludes sisters and related adult female excludes mothers, aunts and grandmothers, etc.
[b]All fathers available ($n = 16$) were observed interacting with infants, but only 14 were engaged in allocare during focal data collection periods.
[c]Of the 10 related male children in the sample who care for focal infants, 7 were foster children. One related female child of 7 was a foster child. Two of the 30 unrelated male child caregivers and none of the 29 unrelated female child caregivers were foster children.

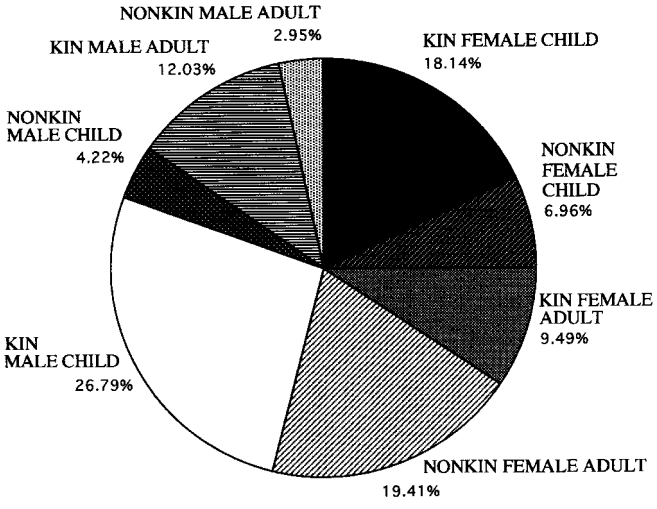


FIG. 5. *Mean proportion of allocare received by infant by caregiver sex and kinship.*

other children. Six of the 20 subject infants do not have older siblings and 9 have no siblings older than five years of age, but only 3 of these have a foster child living in the nuclear family. Foster children tend to live in families with other children and are therefore subject to increased competition over familial resources. Direct assistance in infant care may be one means by which foster children can reduce conflict over scarce food resources within their adopted (i.e., alloparental) family. Efe children, and especially siblings, at times do receive directives from mothers to watch an infant, but the distribution or frequency of maternal directives does not predict children's contributions to allocare.

*The costs of alloparenting*. If alloparenting is a costly activity in terms of time, energy, or lost opportunity, we would expect obvious benefits to be accrued by participants; however, no directly measurable reciprocal payoffs are found between parents and helpers. There are no data on allocaregiver time allocation independent of care to assess the impact of infant interactions on alternative activities; however, the average total time per day spent in allocaregiving and the nature of that care suggest a pattern of potential costs. Table 5 shows the distribution of time spent caring for an infant across categories of *actual* (as opposed to potential) caregivers ($n = 222$). Maternal infant care time well surpasses that of allocaregivers, but the investment of some alternative caregivers is not negligible. Mothers spend an average of six hours a day caring for their infants, but the cumulative addition of a number of related caregivers results in substantial allocare. Of those who provide care, related children (4 to 17 years of age) and adults (18 years and older), including fathers, spend around an hour a day providing infant care. Unrelated caregivers, especially males, spend much less. Unrelated adult female allocaregivers, however, spend on average 37 minutes a day engaged in allocare, and there is no significant difference between the time contributions of unrelated nonreproductive and reproductive caregivers. Figure 5 illustrates the distribution of allocare across actual caregivers from the infant's perspective. Perhaps most notable, males, including fathers (11%), provide an average of 46% of allocare received by infants, and unrelated adult females contribute nearly 20%. Children account for 56% of allocare. While the time contribution per caregiver often may be low, the cumulative time in allocare received by Efe infants is remarkably high.

Time is a global measure of investment; however, the multifaceted demands of infant care present varying costs to the caregiver. A second measure of the cost of care can be inferred from the nature of caregiving. Many forms of child care preclude or circumscribe participation by the caregiver in other activities, such as maintenance and economic tasks, travel, and attention to other children. Active care behaviors, such as feeding, grooming, bathing, carrying, and comforting a fussing baby, enlist a caregiver's attention and participation to a greater degree than do physical contact (i.e., touching or holding) and social interaction (e.g., playing with and talking to an infant) alone. Table 5 shows that while mothers on average spend over an hour per day feeding, grooming, bathing, carrying, and comforting their infants, allocaregivers spend little time engaged in intensive infant care tasks.

A final approach to the assessment of the potential costs of allocare is the proportion of caregiver interactions that are performed simultaneously with economic tasks. While 23% of maternal infant care time is spent in simultaneous economic tasks, including household maintenance activities, manufacturing, water and firewood collection, food acquisition, and food processing (totaling over an hour of maternal time co-occurring with direct infant care), other caregivers spend very little time simultaneously negotiating infant allocare with economic demands (table 5). Mothers are the most pressed for time to complete economic tasks (Peacock 1985), and the time and opportunity costs of allocare for other Efe appear to be low. Efe infant-allocaregiver interactions are characterized primarily by intermittent play and physical contact rather than more demanding forms of care and are relatively free from the burden of simultaneous economic tasks.

*Ecological correlates*. The diverse physical and social contexts of Efe life present opportunities and constraints that may be expected to facilitate or limit alloparenting. There is remarkable consistency, however, in Efe allocare across demographic and physical settings. Group size—the number of camp members present during observation—approaches significance in predicting the number of caregivers that infants experience ($p = .07$) but does not predict the amount of allocare they receive. There is no relation between allocaregiving time and season of observation (wet versus dry or clearing and planting versus peanut harvest, honey, or rainy), camp location (near horticultural villages versus an hour or more's walk into the forest), or the demographic composition of

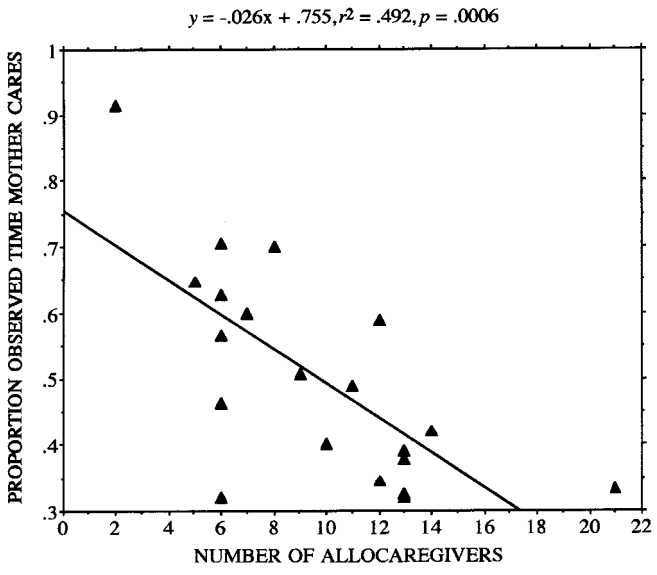


FIG. 6. *Regression plot of the proportion of observed time that mothers spend in infant care by the number of individuals who provide allocare.*

camps (e.g., number of individuals related to the infant, proportion of children to adults, number of nonreproductive adult females, number of foster children).

A clue to the remarkable consistency of Efe allocaregiving across physical settings may lie in Efe social ecology, where an apparent fluidity of social resources exists such that allocaregivers may be recruited or volunteer in the absence of others. As reported above, care by both male and female siblings is negatively associated with the presence of other dependents in the family. Surprisingly, there is no difference in the amount of allocare received by infants with and without siblings (no sibs $n = 6$, mean = 46%; with sibs $n = 14$, mean = 39%). Paternal infant care is predicted only by siblings in the family older than five years of age (Mann-Whitney $U$, $p < .05$), whose presence is negatively related to the amount of time fathers give care, and an absence of siblings in the infant's family is associated with care by nonreproductive women (chi-square 4.615, $p = .0317$). It appears that when parents lack kin to help, unrelated caregivers are recruited. The number of unrelated allocaregivers is negatively associated with the number of relatives available (i.e., in proximity) to an infant ($p < .05$, $r^2 = .30$). Such assistance makes a difference to mothers: Mothers spend more time working when not engaged in infant care ($p < .05$), and nearly half of the variation in maternal care is explained by variation in the number of allocaregivers ($p < .001$, $r^2 = .49$) (fig. 6). There is some indication that alloparenting benefits Efe infants as well: the number of allocaregivers at one year of age is positively associated with survivorship at three years of age (survivors' [$n = 15$] mean = 10.8 allocaregivers; nonsurvivors' [$n = 5$] mean = 6.2 allocaregivers [Mann-Whitney $U$, $p = .05$; logistic regression log likelihood −8.410, chi-square 3.394, $p = .0653$, odds ratio = .664]). No other demographic or behavioral variable measured predicts infant survivorship.

## CONCLUSION

The extensive social distribution of Efe infant care and the lack of impact of physical or demographic setting on the amount of allocare that infants receive suggests a highly flexible child-rearing system that recruits the diverse investment interests of relatives and nonreproductive individuals, especially adult females and children, facultatively in a low-cost, potentially high-benefit endeavor. When the investigator queried a caregiver, "Who cares for Efe infants?" she responded matter-of-factly: "All of us." While this descriptor paints too communal a picture of Efe infant care (on average about one-third of camp members are related to the infant, about half provide infant care, and mothers remain responsible for a slim majority of direct care of one-year-olds), it does reflect a confluence of social interests in the care of young. These data are consistent with previous analyses of Efe care of young infants (e.g., Tronick, Morelli, and Winn 1987, Winn 1991) and older children (e.g., Morelli 1987, Tronick, Morelli, and Ivey 1992).[2] Efe child-rearing demonstrates the central role of human life history and social ecology in providing the opportunity for multiple modes of cooperation in parenting, including nepotism, reciprocity, and learning-to-mother. The suggested lack of significant costs of alloparenting among the Efe reduces the theoretical demand of finding its compensation, and mutualism, whereby individuals gain greater benefit by acting together than alone (see Wrangham 1982), may best characterize Efe allocare. With the exception of a number of reproductive-age females without young, the demographic profile of an Efe allocaregiver is much like what could be found in any small foraging

2. The study sampling methodology was developed to take advantage of technology allowing real-time data calculation, as well as scan-sampling, and was designed to overlap age-ranges of previous research conducted on neonates-to-4-month-olds (Tronick, Morelli, and Winn 1987, Winn 1991) and one-, two-, and three-year-olds (Morelli 1987, 1997; Tronick, Morelli, and Ivey 1992). Therefore, this research replicates previous work employing different data collection techniques. While somewhat different measures are created for theoretical purposes, the results are consistent both within subjects measured across time (only one age period of which is reported in this study) and across subjects used in different data collection periods. (For example, Morelli [1987] and Tronick, Morelli, and Ivey [1992] found that mothers were socially engaged with one-year-olds 49% of observed time [$n = 6$] versus 51% in this later sample [$n = 20$].) Although children's experience of care changes over time from infancy through three years of age (see Tronick, Morelli, and Ivey 1992), internal consistency is demonstrated by measures of the same subjects at different ages. An overlapping sample of subjects in this study was observed at 5 months ($n = 6$) and 8 months ($n = 11$). The percentage of observed time that infants were engaged with adults is 17.6% at 5 months, 15.9% at 8 months, and 16% at 12 months. These infants interacted with children 29.1% of observed time at 5 months, 23.3% at 8 months, and 27% of the time at 12 months. It is also of note that the narrow range of subject age maximizes the sensitivity of the coding system and resultant data to the caregiving demands of specific developmental periods.

population: All human groups are composed of females, relatives, and nonreproductive members, including children and postreproductive adults, begging the question of the uniqueness of Efe allocare. Efe allocare is neither communalistic nor entirely nepotistic, and values concerning infants do little to explain the pattern of caregiving.

The life history and ecology of the Efe hold clues that productive as well as reproductive constraints facilitate allocare by affecting the access that individuals have to food and social resources. Time allocation data (Bailey and Peacock 1988, Peacock 1985, Ivey 1993) and focal interviews (Ivey 1993) suggest that Efe mothers face considerable demands in provisioning their families. Because Efe children are nutritionally stressed (Bailey 1991*b*) but able to gather little in the way of food for themselves (Ivey, Morelli, and Tronick 1994, Morelli 1997), they may defray some of the costs of parental provisioning by performing tasks that increase adult economic efficiency (see Blurton Jones 1993). Mothers are relieved of competing child-care demands to engage in subsistence activities from which children and others benefit. In addition, high rates of mortality and infertility render access to kin low or unpredictable for many Efe, but the reliance on social relationships for success in economic and reproductive activities is high (Bailey 1991*a*). The development of diverse social ties through allocaregiving may enhance the probability of future cooperation (Morelli and Tronick 1991, Ivey 1993), which has especially important long-term benefits in an environment of unpredictable (i.e., high-variance) access to resources (Low 1988). The role of child-care aid in cooperative reproduction remains underexamined, and these data suggest that among some foragers low-cost assistance may moderate the quality-quantity tradeoff of parental investment. Alloparenting therefore has important developmental and reproductive consequences with implications for the evolution of human life history.

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# Shamanism, Phosphenes, and Early Art: An Alternative Synthesis[1]

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The proposition of Lewis-Williams and Dowson (1988, 1993) that many of the abstract marks found in Palaeolithic and Neolithic art can be put down to neurophysiological processes, as determined by shamanistic prac-

tices, has been cause for considerable debate. On the positive side, it has helped open up a fresh approach to this aspect of art by providing some valuable insights as to its probable derivation. On the negative side, it leaves open certain questions relating to cultures in which shamanism is known to be absent but the same or similar motifs are apparent. How is the persistence of analogous motifs in such cultures to be explained? There is also the problem of the growing accumulation and antiquity of geometric motifs dating to the Lower and Middle Palaeolithic.

Although relying on the neurophysiological model as the underlying mechanism responsible for geometric mark-making, recent commentators, including Lewis-Williams and Dowson, have remained reticent as to the exact nature of the proposed cerebral component. This is surprising given that a detailed understanding of this mechanism could provide important insights as to how early marks might have arisen and that recent research concerning the visual cortex has provided a substantial amount of hard scientific data for analysis in this context. To rely on a neurophysiological model without any attempt at specifying the nature of the primary agent concerned involves a substantial leap of faith. This paper presents important evidence for a more profound, pervasive explanation for early abstract geometric art based upon recent neurophysiological research and a detailed description of the mechanisms involved.

## QUESTIONS RAISED BY THE NEUROPHYSIOLOGICAL APPROACH

We can be sure, as Lewis-Williams (1991:153) points out, that the neurophysiology of the visual cortex of Palaeolithic hominids is the same as that of modern humans and this is more than likely the source of geometric phosphenes (see below for further confirmation), but we cannot be so sure that such hominids engaged in shamanistic activities (Bahn and Vertut 1997:182). Moreover, even if this were the case, as is suggested by examples such as the horses at Pech Merle, France, it would not explain the full spectrum of Palaeolithic art, figurative or abstract. Given the appearance in the Lower and Middle Palaeolithic of a growing corpus of simple lines and geometric phosphene-like motifs (e.g., Bednarik 1995, Marshack 1996) lacking any representational content, the argument put forward by Lewis-Williams and Dowson (that iconic components arise, in stages, out of phosphenes as a function of shamanistic practices) would predict that representational features should also be present in the "art" of this period. Considering the existence of iconic sculptural objects in this earlier phase (Bahn and Vertut 1997:24, 99–100), the question arises why there are no representational features associated with the prevailing geometric forms and why abstract geometric primitives seem to predate the representational art of the Upper Palaeolithic by a considerable period.

## THE SIGNIFICANCE OF PHOSPHENE THEORY

Bednarik (1984, 1986, 1990) has proposed phosphene theory as a sufficient explanation for the existence of simple geometric motifs in Lower Palaeolithic art—through the "externalization" of phosphenes by mark-making. Phosphenes have also been implicated as the underlying factor in geometric "primitive" art by Siegel (1980:131–32) and in the abstract forms of Neolithic art by various commentators (Kellogg, Knoll, and Kugler 1965, Bradley 1989, Lewis-Williams and Dowson 1993, Dronfield 1995).

The appeal to phosphenes by Lewis-Williams and Dowson is based upon the commonality of neurophysiological structures of the visual system across various cultural domains and the fact that such phosphenes can be actuated through induced trance states (Lewis-Williams and Dowson 1988, Dronfield 1996). Iconic features are said to arise out of geometric primitives in three stages (Lewis-Williams and Dowson 1988:203–4; Siegel 1980:125, 127, 130), as if representational form were being constructed out of basic phosphene elements, thus providing a kind of mental template for the realization of depictive form the exact nature of which depends, ultimately, on existing cultural parameters—in this case shamanic norms.

The trance states of shamanism are, however, but one means by which phosphenes can be experienced. The shamanistic theory has, accordingly, been criticized by Bednarik (1990), as cultures which are not shamanistic or do not resort to hallucinatorily derived experiences are also found to have phosphene-like motifs in their art (p. 79). Furthermore, Dronfield (1995:539; 1996), although providing some support for Lewis-Williams and Dowson's main thesis, still questions the explanatory power of the model for reasons similar to Bednarik's. Along with Kellogg (Kellogg, Knoll, and Kugler 1965), Bednarik believes that phosphenes are a fundamental universal of early art (p. 77), that all humans experience them including infants and the blind (p. 78), and that their causes may be multiple. For example, Bahn and Vertut (1997:182) have pointed out that hallucinogen- or trance-induced phosphenes account for only a small proportion of such experiences; isolation, extreme boredom, nocturnal hallucinations, drowsiness, sleep deprivation, etc., can produce similar subjective manifestations. Similarly, Bednarik (1986:165) has suggested that for European cave dwellers prolonged deprivation of light during severe winters or dietary privation could equally be the cause of phosphene experience. This points the way to a possible deeper explanation for the existence of phosphene-like motifs in art and ways of accounting for the experience of phosphenes other than by shamanism alone.

In view of the manifest correspondence between early geometric art and phosphenes, the question arises whether there is any evidence for a direct causal relationship between them.

## THE NEUROPHYSIOLOGICAL SITE OF PHOSPHENES

That the central nervous system or the visual cortex are responsible for the manifestation of phosphenes seems to be accepted (Siegel 1980:132: Kellogg, Knoll, and Kugler 1965:1130; Bednarik 1990:78); Lewis-Williams and Dowson 1988:202). Evidence designating the visual cortex as the locus of phosphene experience comes from migraine sufferers. Siegel, Murray, and Jarvis (1975) suggest that geometric forms derived from visual images of those suffering migraine attacks provide information on the lattice arrangements of detector cells in the visual cortex.

Knoll and Kugler (1959), along with Penfield and Roberts (1959), have found phosphenes arising when either the temporal part of the brain or the visual cortex was directly stimulated by electrical impulses. Moreover, Dobelle and Mladejovski (1974) report that only stimulation of the *primary visual cortex* (pp. 559 and 560) in conscious patients produced phosphenes (either round or short line shapes [p. 567]), and Brindley and Lewin (1968) found that stimulation of the same area by several electrodes simultaneously led to the perception of various predictable patterns. Dobelle et al. (1976) report on a prosthesis consisting of electrodes inserted into the primary visual cortex connected to a television camera which allowed a blind person to detect horizontal and vertical lines so that they were able to recognize simple letters and patterns (p. 111).

Tootell et al. (1998) have, in addition, confirmed, by functional analysis of the primary visual cortex using magnetic resonance imaging, that this area has lower contrast-sensitivity—in other words, it is more sensitive to visual phenomena such as lines—as well as being orientation-selective (pp. 815–16). Kosslyn et al. (1999), using the same technique, in addition to verifying this conclusion have found a direct causal link between the primary visual cortex and the perception of lines by asking subjects to imagine sets of stripes of various orientations rather than merely viewing such stimuli. Furthermore, it has been established that damage to the primary visual cortex leads to an inability to copy the simplest geometric forms and, moreover, that in cases where the primary visual cortex is intact but there has been a lesion to other areas of the visual cortex the subject is able to draw local elements of form such as angles, simple lines, and shapes but is unable to integrate such lines into a complex whole (Zeki 1992:48 and 49).

Such studies, and particularly Zeki's observations, all either implicate or directly confirm the primary visual cortex as the underlying neurophysiological causative factor in the perception and depiction of simple lines and phosphene-like forms as well as phosphenes themselves.

## THE IMPORTANCE OF LINES AND THE PRIMARY VISUAL CORTEX

Further support for the primary visual cortex as the seat of the phosphene/artistic primitive forms comes from the way in which line is processed at this level. Line drawings are universally exploited as a short cut to representation by infants, hunter-gatherers, contemporary artists, and, at preliminary pre-representation stages, even chimpanzees (Kennedy and Silver 1974, Davis 1986, Cavanagh 1995, Latto 1995, Morris 1967). Why should this be the case?

Gombrich (1973:201), Bednarik (1984), Halverson (1992), Latto (1995), and Hudson (1998:98) represent only a few of those who have emphasized that artistic primitive motifs are aesthetically interesting not because they reflect properties of the world but because they simulate properties of the visual system. Latto (pp. 67–68), in particular, has explored this idea in some detail:

> A form is effective because it relates to the properties of the human visual system. To describe this process I have coined the term "aesthetic primitive," which, using "primitive" in the sense of primary or fundamental, is defined as a stimulus or property of a stimulus that is intrinsically interesting, even in the absence of narrative meaning, because it resonates with the mechanisms of the visual system processing it.

Latto has also established that this can be applied to a range of artistic traditions by demonstrating that particular graphic depictive strategies can be understood in terms of precise neurophysiological processes. Davis (1986:196), discussing Palaeolithic art, has, in addition, drawn attention to the fact that representation is a specialized process in that it is a reduction or *decomposition* of human vision.

This approach has received corroboration from Hubel and Wiesel's (1980) discovery that cells in the primary visual cortex are organized to respond to the specific orientation of a line and that perception may be fabricated from the accretion of selected features. This is a concept which is crucially important for an appreciation of how the neurophysiological model proposed by Lewis-Williams and Dowson can be extended to include the many exceptions already referred to as well as to explain how iconic forms arise out of phosphenes.

Hubel and Wiesel go on to describe how the primary visual cortex may function as an early stage in the brain's analysis of line orientation and an important aspect of the processing of visual information through recourse to a hierarchy of simple, complex, and hypercomplex cells by which the nature of information concerned with line may become more abstract. Barlow's (1972) feature-detection theory is an extension of Hubel and Wiesel's analysis which proposes that cortical cells forming the bottom layer of a hierarchy of cells respond progressively to more and more abstract geometric features. Hence, cells in lower levels might respond to primitive line components while higher layers respond to simple geometrical patterns such as angles, defined by the activities of particular combinations of complex and hypercomplex cells, leading to the perception of yet more elaborate features, such as rectangles and circles, and so on up to

representational figures—the latter concerning higher-order centres of the visual cortex and the brain. Although this is a simplification of a very involved process which is still not fully understood, recent neurophysiological evidence has tended to confirm this picture by demonstrating how the primary visual cortex processes line orientation, angles, and "T" junctions (Gilbert 1998, Das and Gilbert 1999; see Eysel 1999 for a useful summary).

Marr (1976), in accord with this analysis, has demonstrated that a visual processing model (leading to what he calls the "raw primal sketch") beginning with the extraction of essential edge features as necessary components of line drawing can account for the efficiency of such drawings in depicting representation and carrying a great deal of information. This may explain why early humans began preferentially with an investigation of the possibilities inherent in the use of line, eventually leading to the artistic equivalent of Marr's "raw primal sketch," akin to the outline figures of animals in the Upper Palaeolithic (Halverson 1992:389 and 402). It appears that early humans, in making the first crude marks during the Lower to Middle Palaeolithic (if Bednarik's analysis [1995] is to be accepted), may have been extracting one particularly significant aspect of the optical array for possible exploitation, beginning with the analysis of parts of form in terms of line and line orientation (Hubel and Weisel 1980:36–37) the wiring of which in the primary visual cortex is genetically determined (p. 38). The vertical, horizontal, tilted, circular, zigzag, and lattice lines typical of the Lower-Middle Palaeolithic (Bednarik 1995) would indicate this early processing, the "mandala" motif (from Tata, Hungary [p. 610]) being especially definitive in embodying such diverse features and constituting the probable limit of visual analysis in terms of Gestalt principles at the level of the primary visual cortex (see below). Given enough time, a sufficient "vocabulary" of marks would eventually have accumulated to produce the first realistic drawings found in the Upper Palaeolithic, congruent with the way in which the visual cortex is thought to produce a "representational image" (much as Barlow's feature-detection theory and Marr's analysis suggest). Abstract geometric components deriving from this dynamic continued to be common in the Upper Palaeolithic and would also have appealed to Neolithic artists, as the same mechanism indigenous to the primary visual cortex would have been involved—which, incidentally, explains the evident but perplexing universality of such forms, as specified by Forbes and Crowder (1979).

Another "deconstructive" aspect of visual processing relevant to the origins of art concerning the primary visual cortex is the separate but parallel processing of different kinds of visual information (Livingstone and Hubel 1987). There are, generally speaking, at lower levels, two broad divisions known as the magno system, which carries information about luminance contrast, and the parvo system, which relays information concerning colour—a division which becomes more complex and elaborate in the primary visual cortex and higher-order centres of the visual cortex. At the level of the primary visual cortex the magno system analyses signals for depth, motion, and global form perception, while the parvo system splits into the "blob" pathway, for the analysis of colour, and the "interblob" pathway, for the high resolution of static form perception. The magno system as a visual channel includes decisions about which elements, such as edges, figure and ground, and discontinuities, belong to individual objects in the scene as specified by Gestalt theory (Livingstone and Hubel 1995:61–64) and, consequently, is concerned with the overall organization of the visual world. The parvo-interblob system seems to be more important for analysing the scene in much greater detail. It would not be surprising, therefore, to find that the more primitive magno system is engaged in the essential functions which enable an animal to navigate in its environment, catch prey, and avoid predators while the parvo-interblob system, which is well developed in primates, adds the ability to scrutinize more closely the shape and surface properties of an object—and, accordingly, this system seems well suited to visual identification and association (p. 64). The perception of form would have been critical for survival and, over time, specifically selected for, which could explain why line components figured so prominently in early mark-making.

Thus, different aspects of a painting are processed by different regions of the visual cortex (Livingstone 1988; Zeki 1993:355). The "artists" of the Lower and Middle Palaeolithic, in producing simple repetitive lines and geometric primitives, were therefore, at the level of the primary visual cortex, probably appealing to the parvo-interblob pathway. The evolutionarily earlier magno system would, however, have been significant at levels of the visual cortex beyond the primary centre (involving separate functional "units," e.g., V2, V3, V4, which have larger receptive fields and are more tolerant to line orientation, and reentrant processing) for combining and integrating detail into a structured whole. In view of this analysis it is hardly surprising that animal outlines occur so often in Upper Palaeolithic art, as perceptual analysis of such forms through the magno and parvo pathways would have had important consequences for the identification of a potential threat or possible food source. Colour was a subsidiary element added to Upper Palaeolithic art (Halverson 1992:390), probably as a consequence of the later-evolved parvo-blob/V4 axis.

The obvious similarity between phosphenes and abstract motifs in Lower, Middle, and Upper Palaeolithic art and Neolithic geometric decorative forms, the probability that the primary visual cortex is the locus for phosphenes, and the importance of the processing of line by the primary visual cortex support the idea that this part of the brain is a common predisposing neurophysiological factor. It is therefore the structure and morphology of the primary visual cortex which can account for the particular form of phosphenes, whether the mechanism producing them is the trance states of shamanism, drug-induced hallucinations, migraine attacks, or electrophysical intervention.

## AN ALTERNATIVE EXPLANATION FOR EARLY MARK-MAKING

The question then arises how pre–Upper Palaeolithic and Palaeolithic geometric marks are to be explained if shamanism is unable to account for their preponderance but for exceptional instances in the later period. Given that the universality of the visual neurophysiology is widely accepted, we can agree that the Lower and Middle Palaeolithic marks may resemble phosphenes but their principal source was not the attempted rendition of phosphenes themselves or the altered states of shamanism but the practice of or preoccupation with mark-making itself. Engagement in a visually creative act is said to produce a hyperreality experience akin to that produced by trance or drug inducement in that the artist is in a state of focused awareness detached from the outside world (Bahn and Vertut 1997:182). Furthermore, a hypnotic trance can be induced by having the subject focus intently on entoptic phenomena (Hunchak 1980). As Dissanayake (1992:84) suggests, "the production and repetition of geometric shapes . . . seems to be a fundamental psychobiological propensity in humans that provides pleasurable feelings of mastery, security, and relief from anxiety." Berlyne (1960) has proposed that engagement in such activity produces pleasure, leading to an optimal level of arousal through a feedback loop, and Arnheim (1971:251) has suggested that the economical use of shape and the discovery of similarity are part of a search for order and structure in a complex world—a process in which, I submit, the primary visual cortex has played a preeminent role.

Infant drawing begins, *spontaneously*, around two years of age, with simple repetitive lines that evolve into more complex geometric motifs (Kellogg 1970) similar to phosphenes (Kellogg, Knoll, and Kugler 1965) from which the first representational forms arise by about three years of age. Lewis-Williams (1991:158) makes the mistake of discounting child art with reference to any aspect of Palaeolithic art (he completely misunderstands Bednarik [1990:77] on this issue [Lewis-Williams 1990: 81]). In terms of the particular iconic elements realized this may be admissible, but it is not with regard to the pre-representational geometric forms or the stages whereby representation emerges from such forms. The sequence in infant mark-making parallels the way in which iconic features arise out of phosphenes in drug-induced states. Both sequences, I argue, are contingent on the same neurophysiological mechanism—the primary visual cortex (for phosphenes and phosphene-like geometric marks) and, subsequently, other areas of the visual cortex for outline contours of representational figures. The first primitive marks of the Lower and Middle Palaeolithic can therefore be regarded as a prerequisite for the realization of representational form, a process also seen in the similar universal, sequential development among infants and the emerging iconic features of induced shamanistic states. The difference lies primarily in the rapid access to the underlying neurophysiology in the case of shamanism as compared with the gradual realization through "learning" (i.e., the approximation of self-sufficient marks) afforded to pre–Upper Palaeolithic hominids. Justification for this comes not only from the way in which the visual cortex functions but also from the fact that ontogeny tends to follow phylogeny and the evolutionarily mediated incremental movement from the simple to the complex (as in infant drawing [Arnheim 1974, Slater 1996]) in the same way as speech is thought to have evolved gradually toward greater complexity over a considerable period (Deacon 1989, Pinker 1994). This thesis is additionally supported by the fact that chimpanzees spontaneously (i.e., without any reward) produce simple repetitive lines leading, eventually, to more complex geometric designs up to the "diagram" stage (Morris 1967:138–39), equivalent to those of a two-and-a-half-year-old child.

This analysis explains the exclusive existence of geometric marks in the Lower and Middle Palaeolithic and, moreover, the persistence of such marks alongside of representation in the Upper Palaeolithic, as children also tend to refer back to earlier geometric forms even after the representational stage has been attained (Golomb 1981).

For both infants and early humans the preoccupation with mark-making is an important stimulus for the eye and the brain, helping to promote vital survival skills such as visual memory and perceptual awareness and to improve hand-eye coordination, particularly fine motor skills mediated through the parvo-interblob pathway. Gregory (1970:42) has remarked that vision develops in children through the touching of objects. Similarly, Alland (1983) has argued that "art" results from the interaction of behavioural systems such as exploration and play, which foster understanding of the environment, response to form, which increases awareness of the environment, and fine-grained perception and perceptual memory, which increase the capacity to deal with environmental variation and to exploit resources. Of course, as already intimated, such skills would involve other areas of the brain, such as the arousal system, including higher-order associative functions as part of an active-"passive" feedback loop, the primary visual cortex constituting the crucial nexus for geometric forms.

This model has been vindicated by a recent study using functional magnetic resonance imaging (the Wellcome Trust Sci-Art Project; see *New Scientist*, December 5, 1988), which establishes that artistically naive subjects copying geometric figures experience increased activity in the visual cortex while trained artists show more activity in the right frontal regions of the brain, indicating that the former are slavishly copying the presenting stimulus while the latter are using higher-order functions because of previous training. Hominids involved in mark-making during the Lower and Middle Palaeolithic would, hence, be akin to untrained subjects "learning" how to make simple lines by recourse to positive feedback involving activation of the visual cortex, and in view of the neurophysiological studies already cited this is more than likely to have been the primary visual cortex.

## THE IMPORTANCE OF GESTALT THEORY

It is significant that Marr's ideas made extensive use of Gestalt principles of perceptual organization whereby discrete elements are deemed more likely to "belong together" by having a similar orientation or lying next to one another than those oriented dissimilarly and spaced far apart (Bruce and Green 1990). Siegel (1980:131), similarly, argues that not only straight-line figures but curved ones can be subdivided into straight lines, since the direction of the curve can be defined by the orientation of its tangent—circular as well as straight lines are therefore possible. The Quneitra artifact (ca. 54,000 B.P. [Marshack 1996]) seems to illustrate the importance of this in early mark-making, as do other similar geometric motifs before and during this period. Earlier Lower Palaeolithic marks such as cupules, spots, dashes, and various arrangements of peck marks, when repeated and strung together, can, in accordance with Gestalt principles, also form the basis of concomitant straight or curved lines as a preliminary to the depiction of lines, angles, and squares or circles (i.e., through the laws of continuity, closure, and symmetry, respectively) and eventually to a combination of such forms (e.g., a rectangular design from Bilzingsleben, Germany, ca. 100,000 B.P. and other similar geometric motifs [Bednarik 1995]). Many of the simple geometric shapes common to the proto-art of early hominids and Neolithic art can be similarly explained in terms of the way in which perceptual information concerned with line is processed and assimilated graphically in terms of Gestalt theory at the level of the primary visual cortex. It should be noted, however, that it is the structure and topography of the neurophysiology relating to the primary visual cortex which determine the Gestalt principles, probably by recourse to inhibitory and excitatory axons involving short-range and long-range connections within the fabric of the primary visual cortex (Das and Gilbert 1999) and reentrant processing (Zeki 1992:50).

## EVOLUTION AND THE PRIMARY VISUAL CORTEX

The primary visual cortex in primates plays a critical role in visual information processing, as most visual information reaching the rest of the visual cortex is funnelled through this area (Felleman and Van Essen 1991). This "gatekeeper" role may account for this area's being the largest known visual cortical if not the largest cortical area (Tootell et al. 1998). The primary visual cortex is regarded as pivotal in this context because it is the one area of the visual cortex which seems to be mature at birth (Zeki 1992:44) and, together with V2, a feedforward centre, is common to all mammals (Allman 1987:637), which underlines its evolutionary significance. Furthermore, the magno system, as well as being more primitive than the parvo, may be regarded as homologous to the entire visual system of non-primate mammals (Livingstone and Hubel 1995:64) and a precursor to the parvo-interblob pathway (as indicated by the aforementioned redundancy). Consequently, in view of research with contemporaneous subjects which has established the primary visual cortex's preeminence in the perception of line and form and given the evolutionary imperatives, this part of the brain, through commonality, would have been equally if not more important for hominids engaged in early mark-making.

There is also evidence that Gestalt principles may be innately mediated at the level of the primary visual cortex, additionally linking Gestalt theory with functional correlates at this level and reinforcing its importance in terms of evolutionary factors. Bower (1977:43) has demonstrated that two-month-old infants can respond to and therefore perceive closure. More recently Slater (1996), in an overview of similar studies, confirms this finding and cites research which has discovered other corresponding elements of perception to be innately defined. This is borne out by the work of Knoll and Kellogg (1965), who, in attempting to explain their finding of a strong similarity between adult phosphenes and preschool children's scribblings, concluded that "we are dealing here with the activation of pre-formed neurone networks in the visual system" (p. 1130).

From an evolutionary perspective, then, it makes sense that abstract marks should appear before representational forms. Mark-making may be derived from the perceptual processes pertaining to the primary visual cortex, the latter having evolved, in conjunction with the eye, as a means of organizing and systematizing the incoming perceptual array for the purposes of survival. Simple geometric scratches on rock surfaces would therefore constitute a recapitulation of the process by which the primary visual cortex begins to construct a "picture" of the world from simple lines as a preliminary to more elaborate representational features, thus providing a more pervasive explanation than Lewis-Williams and Dowson's for the apparent emergence of iconic elements out of geometric primitives. Accordingly, shamanism is a necessary but not a sufficient cause for such depictive manifestations. This is, therefore, a model which both includes Lewis-Williams and Dowson's thesis and embraces Bednarik's position concerning phosphene theory and neuropsychology.

## CONCLUSION

The fact that the primary visual cortex plays a central role in the initial processing of visual information concerned with line, its obvious connection with the process of making and viewing simple geometric marks, its evolutionary significance, its maturity at birth, and the functional characteristics which can explain the experience of phosphenes and phosphene-like marks confirm that it is fundamental for determining both the proto-art of early humans and subsequent geometric motifs. Primitive marks then become the starting point for the assignment of form according to Gestalt principles (as determined by inherent neurophysiological structures) in the same way that the combining of such primitives into basic configurations may well be determined at the level of the primary visual cortex. Shamanism in this

context therefore becomes simply one means whereby abstract motifs can be generated by recourse to this mechanism—and it is the structural and operational nature of this mechanism which ultimately accounts for the graphic features and instances which could not be accommodated in Lewis-Williams and Dowson's model.

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# A Reconsideration of Population Pressure and Warfare: A Protohistoric Korean Case[1]

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The role of warfare in the emergence of state-level societies has been a concern of scholars since before the Christian era (cf. Haas 1982), but Carneiro's (1970) "A Theory of the Origin of the State" produced a remarkable increase in scholarly attention to the subject and stimulated a great amount of research from a modern anthropological perspective. Carneiro argues that state formation is a result of the interplay of three crucial elements: environmental circumscription, population pressure, and warfare (1970:737; 1972:65; 1988:499). He goes on to suggest that warfare oriented toward the conquest of arable land, stimulated by population pressure in direct association with geographic circumscription, is the only mechanism in the rise of state societies. He does not rule out the possibility of voluntaristic cooperation under particular conditions, but he considers it temporary (1970:733; 1972:65). Although not all scholars agree with Carneiro's proposition, which has not yet been tested in a sophisticated way with empirical data, many have contended that warfare in conjunction with population growth was a significant evolutionary mechanism, in particular, for state formation (Cohen 1984; Haas 1982, 1984, 1990; Jochim 1979, 1981; Kirch 1988; Sanders and Price 1968; Schacht 1988; Spencer 1982; Webb 1975, 1988; Webster 1975, 1976, 1977; Wright 1978). Despite this, the theory continues to be challenged.

Some scholars reject warfare as a primary cause of state formation, arguing that large military conflicts have been a result of the development of complex societies rather than a cause. Other criticisms are that warfare is considerably older and more widespread than the state, that the presence of warfare does not automatically bring about state formation, and that Carneiro's (1970) definition of circumscription is equivocal and relative (Schacht 1988). Furthermore, even if there were many environmentally circumscribed environments with large populations and evidence of conflict, there is still the case of the Polynesian islands (Kirch 1984:207–16; 1988), where state-level societies never emerged. Why did prehistoric people(s) who had controlled their population below the carrying capacity of the limited land supply let it grow so large as to cause conflicts over arable land (Redman 1978:225)? If warfare arose because of the lack of arable land in direct association with overpopulation, unless the winners killed all the losers the fundamental problem—the shortage of arable land (i.e., lack of food)—would have remained unsolved. This scenario of population growth in a circumscribed territory may work for the emergence of social stratification in a given region, but it does not necessarily work everywhere.

Thus, although warfare theory has played a significant role in the explanation of the emergence of state-level societies, there are still many questions open to debate. In this paper I will reexamine the correlation between population pressure and warfare in Chinese and Korean chronicles and examine alternative causative factors. In particular, I will argue that in the Korean case warfare took place not because of population pressure in relation to arable land and food shortage but in order to obtain labor to produce more foods, goods, and services in response to environmental stress.

## DEMOGRAPHIC DYNAMICS AND WARFARE

Evolutionary theorists have emphasized the correlation between population pressure in circumscribed environments and the emergence of the state. In particular, ecological anthropologists have emphasized that population growth in conjunction with a shortage of arable land (i.e., insufficient subsistence resources), followed by intercommunity competition, was the principal mechanism of the sociocultural change leading to the emergence of the state (Carneiro 1970, 1988; Ferguson 1984; 1990:31–33; Harner 1970:68; Harris 1971:227–28; 1972; 1979:102–103; Johnson and Earle 1987:16–18; Larson 1972; Sanders and Price 1968:230–32; Webster 1975). According to Vayda (1974:183), however, "war occurs even when appreciable population pressure is absent and when none of the belligerents either needs or seeks more

land or other resources." Other researchers have also proposed that population growth in conjunction with limited subsistent resources may be the principal cause of warfare, but war is not necessarily always caused solely or mainly by such pressure (Brumfiel 1983:270; Cowgill 1975:517; 1979:58–60; Dumond 1972:310; Gibson 1974; Vayda 1974:184). It also has been pointed out that population growth cannot account for the entire course of history, since it must be assumed that other processes are also at work in any given case (Dumond 1972:309; Harner 1970:68). Also, war may occur "as a corrective response to problems not of overpopulation but of underpopulation" (Vayda 1974:184). That is,

> Autonomous local groups are small enough in much of the primitive world to be subject to considerable fluctuations in size, sex ratio, and age distribution as a result of chance variations in natality and mortality; it has been noted that some such groups, as, for example, some Indian groups in central Brazil, compensate for the effect of these variations by resorting to warfare that involves taking captives belonging to appropriate age and sex categories.

Cowgill (1979:59–60) has argued almost the same view:

> It seems quite clear that conflict *did not* escalate because of growing population pressure or subsistence problems. On the contrary, rulers of states often felt that their states were *under*populated. They needed people as conscripts for the huge armies, and they also needed people to intensify agricultural production in order to feed the huge armies during their long campaigns. Some rulers adopted policies explicitly intended to encourage population growth, and rulers also attempted to induce peasants to flee from other states. . .

Redmond (1994) also finds that, among the Yanomamö and Jívaro in northern South America, tribal warfare is motivated by the desire for looting valuables, abducting women, and taking revenge rather than the desire for subsistence resources. Similar cases in South America have been observed by many others (Chagnon 1983: 170–89; Helms 1994; Johnson and Earle 1987:124, 134). These examples strongly suggest that the relationship between population pressure and competition for arable land is not as strong as many have thought.

The Chinese chronicle *San quo chi*, compiled by Chen Shou (1987) in the second half of the 3d century A.D., is a good source of information on the political configurations, social structures, and economic patterns of the independent aboriginal polities in southern Korea for a time span of approximately 500 years from the 2d century B.C. to the end of the 3d century A.D. These autonomous polities are collectively called the Sam (Three) Han: Ma Han, Jin Han, and Byun Han (fig. 1). The Sam Han consisted of 78 *guks*, the Chinese character for which literally means "states" but which cannot be considered state-level societies as Service defines them. All but a few of them have been regarded as chiefdoms at most. According to the *San quo chi*. Ma Han, the largest, consisted of 54 *guks* located in the western and southwestern portion of the Korean peninsula. Baekje, a polity located around the present-day city of Seoul, evolved into the Baekje Kingdom. Jin Han consisted of 12 *guks*, located in the southeastern portion of the peninsula, largely on the east side of the Nakdong River. Saro, a polity located in the present-day city of Gyungju, evolved into the Silla Kingdom. Byun Han also consisted of 12 *guks*, predominantly located on the west side of the Nakdong River between Ma Han and Jin Han. In the Byun Han area, six of the polities evolved into at least maximal chiefdoms that were collectively called Gaya (fig. 2).

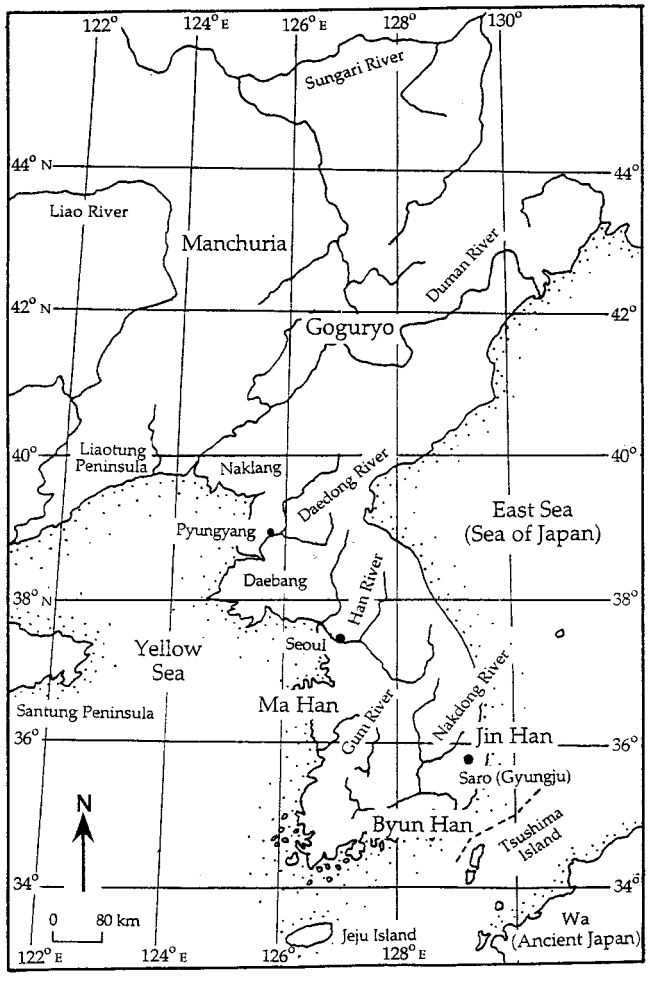


FIG. 1. *The location of the Sam Han in southern Korea.*

The *San quo chi* provides gross estimates of the demographics of a few polities of the Sam Han that allow us to assess the overall population size, distribution, and growth of the Silla Kingdom (i.e., Jin Han), from the 1st

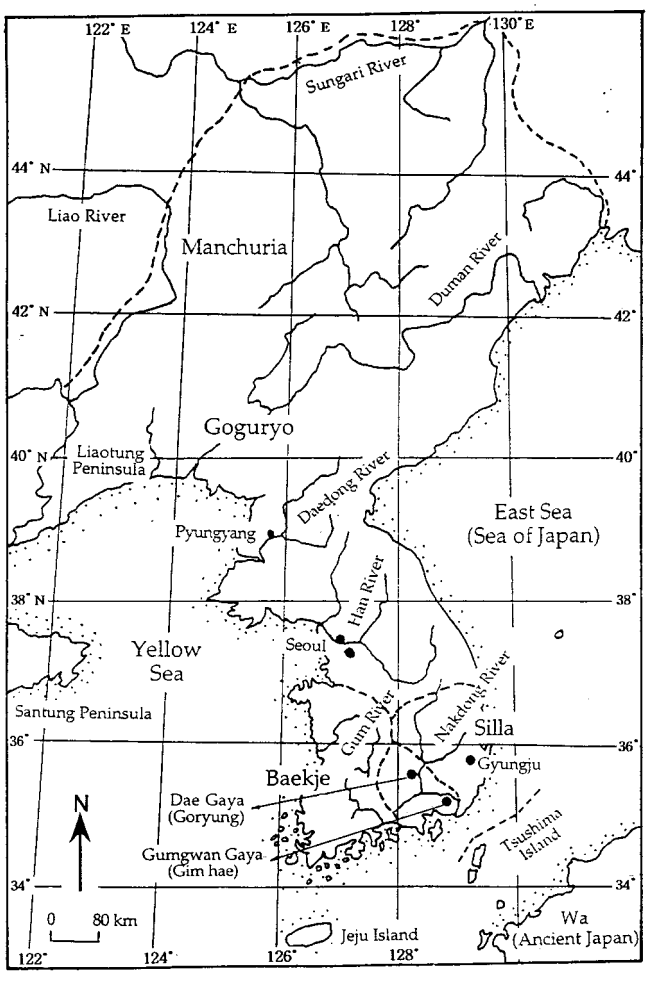


FIG. 2. *The location of the Baekje, Silla, and Gaya kingdoms in southern Korea.*

century B.C. to the 5th century A.D. According to the *San quo chi* (Chen Shou 1987:192–96, my translation),

> There are slightly more than 50 countries [in Ma Han]. Larger *guks* [polities] consist of approximately 10,000 households, and smaller *guks* consist of several thousands of households, making up a total of approximately 100,000 households.. . . There are 24 *guks* in Byun Han and Jin Han [12 each]. Larger *guks* consist of between 4,000 and 5,000 households and smaller *guks* consist of between 600 and 700 households, making up a total of between 40,000 and 50,000 households.

These population figures are summarized in table 1. The population density in the Ma Han region (the Baekje Kingdom) is much higher than that of both the Byun Han (Gaya) and Jin Han (the Silla Kingdom), mainly because arable land is much more abundant in the Ma Han than in the other two regions. Considering all the historical and archaeological data available, Saro seems to have been one of the largest polities in the Jin Han region, and if this is correct, it is not unreasonable that it may have had at least 5,000 households.

J. K. Kim (1974), on the basis of the size of the semisubterranean pit houses in the Bronze Age of Korea, estimated that there were four to five persons per nuclear family household. His estimate, slightly modified (five or six per pit house), has been widely used by Korean scholars as an average family size for ancient Korea (Lee 1982; J. B. Kim 1986:228–29). Although both Bronze and Iron Age people(s) practiced wet-rice agriculture, Iron Age people worked more land with more effective agricultural tools, irrigation, drainage, fertilizer, and flood controls. Thus it is possible that households averaged more than five persons during the Iron Age Sam Han period. It appears, however, that the average number of persons per household during this period was no more than five. According to the Korean chronicle *Samguk sagi*, compiled by Bu-Sik Kim (1977) in A.D. 1145, Jungto Yeon, a son of Goguryo Kingdom's Gen. Gaesomoon Yeon, surrendered to Silla Kingdom in A.D. 666 with 12

TABLE 1
*Population in the Sam Han Region Between the 1st and the 3d Century A.D.*

| | No. of Polities | Household | | Total Households (Midpoint) | Total Estimated Population[a] |
|---|---|---|---|---|---|
| | | Large Polity (Midpoint) | Small Polity (Midpoint) | | |
| Ma Han | 54 | 10,000 | 5,000 | 100,000 | 500,000 |
| Byun Han | 12 | 4,500 | 650 | 45,000 | 225,000 |
| Jin Han | 12 | 4,500 | 650 | 45,000 | 225,000 |
| Total | 78 | 19,000 | 6,300 | 190,000 | 950,000 |

[a]Estimated number of persons per household is 5.

fortifications, 763 households, and 3,543 people. Thus the average household size may be calculated as 4.64 persons in the middle of the 7th century A.D. in the Goguryo Kingdom, which covered an area from southern Manchuria to northern Korea (figs. 1 and 2). Average household size in the Silla Kingdom may have been slightly higher because arable land was more abundant there, but, as will be discussed below, it is doubtful that there was a significant difference between the two kingdoms in this regard.

Further evidence for household composition is found in the Korean chronicle *Samguk yusa*, compiled by Yeon Il in the 13th century A.D. According to this source, there were 100 households with 75,000 people in Gaya (Gumgwan Gaya, one of the largest polities in the Byun Han area, location of the present-day city of Gimhae). This population size is unacceptable because of the implausible number of persons per household that it implies. Some Korean historians interpret the "100" as "10,000," assuming that Yeon Il made a mistake in one Chinese character (Lee 1982:18). If we accept Lee's interpretation, an average of 7.5 persons made up a household during the first century A.D. in the Byun Han area. The Byun Han area is very well known for its fertile land, and consequently it can be assumed that the population density in conjunction with carrying capacity was relatively high.

Again according to the *Samguk yusa*, there were 178,936 households in Gumsung, the capital of the Silla Kingdom during its heyday (ca. A.D. 750). If we accept this number and multiply by 5, the total population becomes 894,680 people in the capital alone. If we multiply by 7.5, the total population becomes more than 1.3 million, which is even more unlikely. Thus this also seems to be an error made by the compiler of the document. The area of the capital (ca. 187 $km^2$) is not large enough to accommodate that many people (which would yield a population density of a little more than 4,784 persons/$km^2$), and therefore it is reasonable to interpret the number (178,936) as the total population size (Lee 1982:30). If we divide 178,936 by 7.5, the average number of persons per household, the number of households in the city becomes 23,858. This number does not correspond with the historical record of the *San quo chi*. If, instead, we divide 178,936 by 5, the total number of households is 35,787, which corresponds with the historical document and is more reasonable and convincing.

All these things considered, the figure of five persons per house is adopted as the average in this paper, and, from the demographic information recorded in the *San quo chi*, the population size for Saro during the 2d century A.D. (midpoint between the 1st and the 3rd century A.D.) can be estimated as follows: 5,000 (households) × 5 (persons) = 25,000 people. Next, combining the demographic figures appearing in the sources it is possible to estimate the rate of population growth for the Saro polity.

To determine the annual rate of population growth ($r$), the following Malthusian model is commonly used:

$$N = N_0 e^{kt},$$

where $N$ is the final population size reached from the initial population $N_0$ after time elapsed ($t$) (Hassan 1978: 69; 1981:139). Thus, the annual rate of population growth ($k$) is determined by the following equation:

$$N = N_0 e^{kt},$$
$$N/N_0 = e^{kt},$$
$$\ln(N/N_0) = \ln(e^{kt}) = kt,$$
$$k = \ln(N/N_0)/t.$$

The initial population size of the Saro polity is assumed to have been 25,000 at around A.D. 250 (on the basis of the *San quo qui*) and to have reached 178,936 in the heyday of the Silla Kingdom around A.D. 750 (on the basis of the *Samguk yusa*). Thus, the annual rate of population growth for the Saro polity for 500 years can be determined as follows: $k = \ln(178{,}936/25{,}000)/500 = 0.003936 = 0.394\%$. This means that the population increased by 3.94 persons per 1,000 per year, which seems relatively high considering that the 17th century was less than 0.6% and that of the European population as a whole about 0.4% (Hassan 1981:234). According to Cowgill (1975:511),

> Surges implying rates of natural increase of from 3 to about 7 per 1,000 per year over regions up to some tens of thousands of square kilometers, sustained over two or three centuries (a doubling of population in about 240 to 100 years), have not been uncommon during the past few thousand years, but they are interspersed with periods of very slow growth or decline. Overall regional trends spanning a millennium or more show net population gains that are rarely more than what would have resulted from a steady rate of increase of 1 or 2 per 1,000 per year (population doubling in 350 to 700 years), and are perhaps never over 3 per 1,000 per year (doubling in about 240 years).

It is questionable whether this annual population growth rate (0.394%) is enough to cause overpopulation in terms of a critical scarcity of productive agricultural land, leading to conflict over arable land.

The following passage from the *San quo chi* (Chen Shou 1987:194, my translation, emphasis added) suggests that underpopulation or depopulation, rather than overpopulation, was a key factor in the occurrence of warfare in southern Korea between the 1st and the 3d century A.D.:

> At the end of Hwan Emperor [A.D. 155–57] and Young Emperor [A.D. 168–70] [of Later Han China], Han [of the Sam Han, either Jin Han or Ma Han, source of the confusion] and Ye [Dongye] became strong and prosperous. Gun [Naklang]·Hyun could

> not control them so that many people [of the Gun·Hyun] escaped [migrated] into the Han *guk*. During the reign of the Gun An [of Later Han China, A.D. 196–220], Son-gang Gong divided the sterile land located south of Doonyu Hyun and made it the territory of Daebang Gun. [He then] Sent Son-mo Gong and Chang Jang to gather the refugees [of the Gun·Hyun]. [So they] Conscripted warriors and *attacked* Han and Ye. People who [escaped and] resided in [Jin Han and Dongye] gradually came out of [Jin Han and Dongye] and came back to Daebang Gun. Hereafter, Wa [ancient Japan] and Han [Jin Han] at last became subordinated to Daebang.

Judging from the context, the Naklang and Daebang Han Chinese commanderies of northwestern Korea may have lost a great number of people who had been engaged in various kinds of productive activities. The reason, though not clearly stated, was probably flight to avoid heavy taxation and frequent levies. If the annual population growth rate was in fact higher than the rate computed above (0.394%), this is evidence that population movement (migration) also played a key role in rapid population increase.

The two Chinese commanderies organized military forces and conducted a military campaign to invade Jin Han and Dongye. Eventually they defeated them in battle and made them their subordinates. More important, they forced the refugees to return to their original territories. Here warfare appears to have been motivated by a desire to obtain captives for labor forces or at least to get their people back. In other words, a sizable population was regarded not as a resource stress but as a source of wealth, since people must have paid taxes and tribute and provided corvée labor. Also, it is apparent that both the Naklang and the Daebang commandery were also interested in the acquisition of territory. Population pressure does not seem to have been a critical factor in this conflict.

In many other cases during the Three Kingdoms period in Korea (1st–5th century A.D.) when relatively large polities like the Baekje Kingdom, Gaya, Wa (ancient Japan), and the Silla Kingdom were involved in either offensive or defensive warfare, they were interested in capturing people and taking them to their countries. (The historical account "Silla recaptured the Wasan fortification and killed 200 Baekje citizens," which appears in the *Samguk sagi* [King Suktalhae 20, A.D. 76], is an exception.) The following historical account (Yeom Sa Chi) from the *San quo chi* (Chen Shou 1987:193–94, my translation) concerns securing captives for labor forces:

> During Wangmang's [reign, during Sin China, intermediate dynasty between the Former and Later Han] Gi Hwang [A.D. 20–22], Yeom Sa Chi, who was Wooguhsa [local official] in Jin Han, heard that Naklang had very fertile land and its people were rich. So, [he] desired to escape [from Jin Han] to come to give himself up to [the Naklang]. He departed from his village, and along the way he saw a man scaring sparrows away in the field. [Recognizing that] his language was different from that of [Jin] Han, [Yeom Sa] Chi asked [what happened]. He replied, "We are Chinese, my name is Horae. While fifteen hundred of us were logging, we were attacked by [Jin] Han. We had our hair cut and became slaves, and three years have passed since then." [Yeom Sa] Chi said, "I am just about to give up myself to Chinese Naklang. Are you willing to come?" Horae said, "Yes." [Yeom Sa] Chi of Jin [Han] took Horae along and arrived at Hamja Hyun [an unknown place in Naklang]. [Hamja] Hyun reported this to [Naklang] Gun. The administrator of the Gun [Hamja Hyun] let Chi be a translator and had him board a big ship at Geum Jung [port] on the way to Jin Han to take back those of Horae's peers who had surrendered [to Jin Han]. Although they took 1,000 people back, the remaining 500 people [out of 1,500] had already died. At this time, [Yeom Sa] Chi made himself clear to Jin Han: "You give us the 500 people back, or the Naklang will send several thousands of warriors on big ships and attack you." [The administrator of] Jin Han said, "Five hundred people have already died; we are immediately responsible for the compensation." Thus Jin Han contributed 15,000 people and Byun Han contributed 15,000 linen rolls. [Yeom Sa] Chi took them and came back to [Naklang] Gun right away. [The Naklang Commandery officials] made a public recognition of [Yeom Sa] Chi and bestowed on him an official cap, fields, and a house which his descendants handed down for many generations. When it reached the fourth year of the Yeon Gwang reign of Emperor An [A.D. 125], his descendants were exempted from the labor draft because of that [their ancestor's contribution to Naklang].

This chronicle is considered exaggerated and distorted in favor of the Naklang Commandery (Im 1959:22–23), but it provides a clear picture of relations between Naklang and indigenous people at the beginning of the 1st century A.D. (Gardiner 1969:22). It carries a great deal of description of the sociocultural, political, and economic aspects of the Jin Han in particular and the Sam Han in general. First of all, this incident took place between the Naklang Commandery and Jin Han sometime between A.D. 20 and 22. Although we cannot determine which of the 12 Jin Han polities was involved in the dispute or pin down its geographic location, we do know that Jin Han existed as a collective entity during that time period. More specifically, Saro, as a member of Jin Han, probably existed in this time period as well.

According to this account, Jin Han captured 1,500 Naklang people and exploited them as labor for three years. If Jin Han had a problem with population pressure (i.e., lack of subsistence resources to support the extra population), it would not have kept the Naklang people for that long unless it expected to obtain something from

TABLE 2
*Environmental Stresses and Warfare Recorded in the Annals of the Silla Kingdom in the* Samguk sagi

| Time | Drought | Flood/ Rain | Typhoon | Snow | Locusts | High Temperature | Hail | Frost | Epidemic Disease | Crop Failure | Total Stresses | Warfare |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| 1st century B.C. | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 0 | 2 |
| 1st century A.D. | 4 | 2 | 3 | 0 | 2 | 2 | 0 | 0 | 1 | 0 | 14 | 17 |
| 2d century A.D. | 6 | 6 | 2 | 3 | 3 | 2 | 4 | 4 | 3 | 2 | 35 | 24 |
| 3d century A.D. | 9 | 3 | 2 | 1 | 3 | 1 | 3 | 2 | 1 | 2 | 27 | 24 |
| 4th century A.D. | 6 | 2 | 0 | 0 | 4 | 1 | 1 | 1 | 1 | 3 | 19 | 4 |
| 5th century A.D. | 6 | 8 | 1 | 0 | 3 | 1 | 4 | 4 | 2 | 3 | 32 | 30 |
| 6th century A.D. | 2 | 1 | 0 | 1 | 0 | 0 | 0 | 1 | 0 | 0 | 5 | 10 |
| 7th century A.D.[a] | 3 | 3 | 1 | 2 | 0 | 1 | 1 | 2 | 0 | 1 | 14 | 85 |
| 8th century A.D.[b] | 10 | 3 | 3 | 4 | 5 | 4 | 6 | 2 | 3 | 7 | 47 | 2 |
| Total | 46 | 28 | 12 | 11 | 20 | 12 | 19 | 16 | 11 | 18 | 193 | 198 |

[a]Outlier, eliminated from analysis.
[b]After unification of Three Kingdoms, eliminated from analysis.

them. Eventually the Naklang Commandery not only took 1,000 people back (after 500 people had already died) but also took as many as 15,000 people from Jin Han as compensation for its 500 dead. This was the outcome of negotiation between the two polities to prevent war. Thus, if warfare had occurred between the two polities, the key factor was not population growth and resource stress but instead the desire to secure labor and/or wealth (contra Carneiro 1988; Cowgill 1975:517; 1979:59–60; see Dumond 1972:309).

We can assume that the captives were forced to become slaves or at least subordinates who engaged in basic material productive activities (depending on specific individual skills) in a hostile country. Thus, as is argued by many scholars, population pressure within circumscribed environments is not necessarily correlated with warfare. For example, Dumond (1965) suggests that population growth may have played a positive ecological role. In other words, population growth can serve as a spur to improvement of subsistence and commerce (Cowgill 1975:517), and these, by raising total income, encourage the growth of further population. Whereas cultural materialists have attempted to relate population growth to the occurrence of warfare from the perspective of an adaptive strategy (e.g., Harris 1971; Larson 1972; Vayda 1971, 1974), this turns out not to have been the case in ancient Korea. The oversimplified viewpoint needs to be reconsidered at least in this case (see Hallpike 1973 for a detailed argument).

In summary, population during the Korean protohistoric period was regarded as a source of labor rather than the burden which many anthropologists and archaeologists have proposed. The correlation between population pressure and occurrence of warfare is weak. On some occasions the reverse is true, that is, warfare may have occurred because of the lack of appropriate population (e.g., unbalanced sex and age ratios) (Cowgill 1979:59–60; Oberg 1955:473–74; Vayda 1974:184).

## ENVIRONMENTAL STRESS AND WARFARE

An alternative explanation for warfare may be environmental stress: sudden and unpredicted climatic change over a short period, including drought, heavy rain, typhoon, frost, unusual temperature fluctuation, volcanic eruption, earthquake, and snow. Along with these variables, geomorphological transformations such as soil exhaustion or salinization and overpopulation beyond the carrying capacity of the area have also been considered as contributing to environmental stress (Haas 1990:177). Environmental stress has been considered important for the explanation of sociocultural change by many archaeologists (e.g., Braun and Plog 1982, Haas 1990, Jones et al. 1999). It has not been documented archaeologically for southern Korea in the time period in question, but the *Samguk sagi* mentions various environmental phenomena that would have affected crop success or failure (leading to abundance or scarcity of food resources) in a given year or contributed to chronic environmental deterioration that might have caused conflict over insufficient subsistence resources. By analyzing some of these references it is possible to examine whether they are correlated with the occurrence of warfare.

The frequencies of various environmental stresses and of warfare recorded in the *Samguk sagi* are represented in table 2. (Comets, novas, lightning and thunder, rainbows, and earthquakes are not included.) Crop failure and epidemic disease are not environmental stresses in themselves, but their close association with environmental deterioration (e.g., Jones et al. 1999:138) makes it not unreasonable to include them in the table. These environmental calamities would not have been recorded in the historical accounts unless they had had some significant effect (i.e., food stress). Therefore the summation of these various calamities is a valid indicator of environmental stress on the human population in the study area.

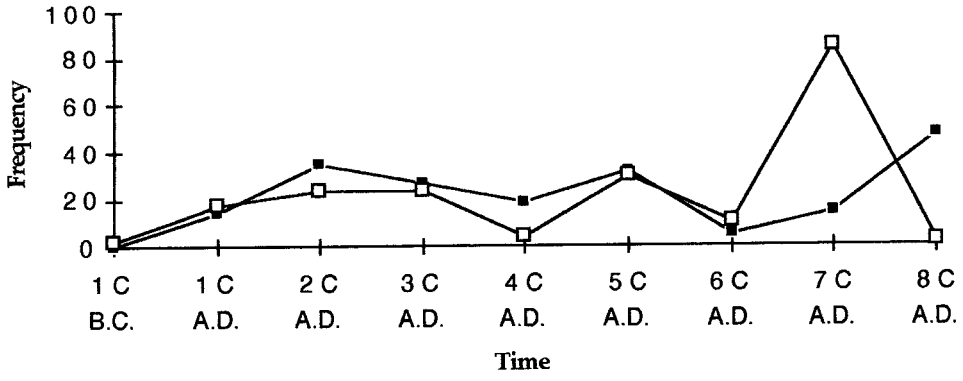


FIG. 3. *Temporal variations of frequencies of environmental stresses* (filled squares) *and warfare* (open squares) *as recorded in the* Samguk sagi.

As can be seen in figure 3, the frequencies of environmental stress and warfare do not directly correspond with each other, but overall the trends are similar: as environmental stress increases, the frequency of warfare increases, and vice versa. A significant anomaly occurred during the 7th century, when there were far more instances of warfare than of environmental stress because of the war for the unification of the Jin Han instigated by the Silla Kingdom. During this time period, sociopolitical phenomena must have played a more important role than environmental conditions, but it is notable that there was a slight increase in environmental stress as well.

Further statistical analysis provides a more detailed understanding of the relationship between environmental stress and the occurrence of warfare. The scattergram of figure 4 shows a positive linear relationship between the two variables. This implies that as the frequency of environmental stress increases, the frequency of warfare may also be expected to increase. Therefore, a correlation analysis employing Pearson's correlation coefficient is appropriately applied to these data.

Appropriate use of correlation analysis requires that the data used meet certain criteria. First, variables must be measured on an interval or ratio scale (Kachigan 1986: 206), a requirement which is met with these raw count

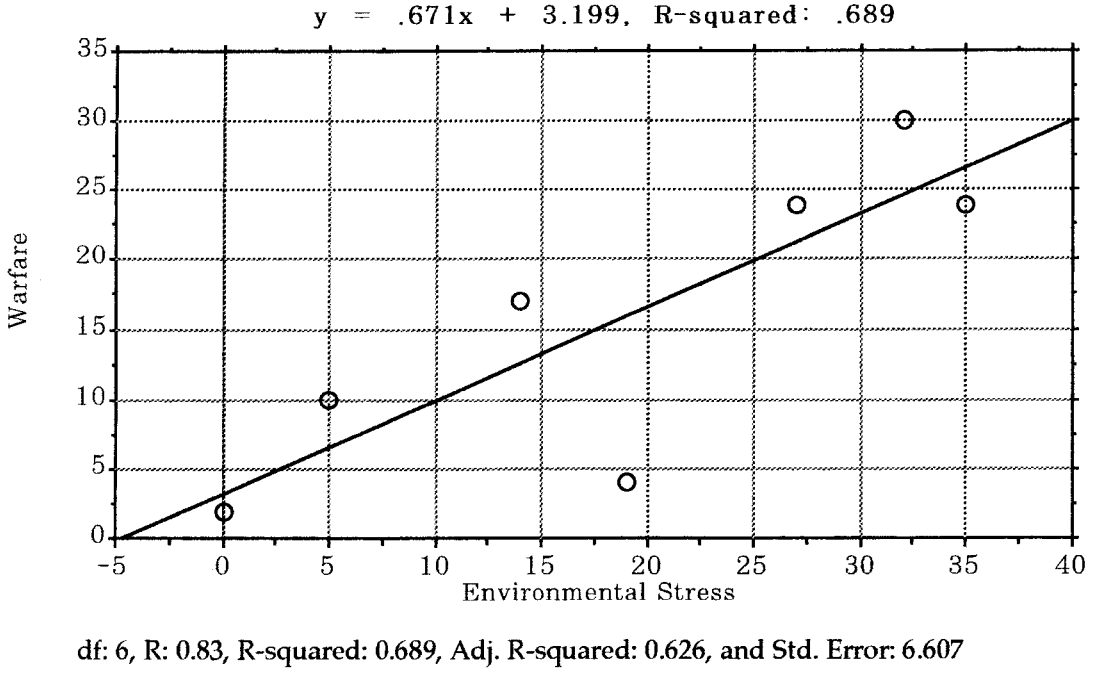


FIG. 4. *Scattergram of the frequency of environmental stress on occurrences of warfare.*

TABLE 3
*Analysis of Variance for Significance of Regression of the Frequency of Warfare on Time*

| Source of Variation | Degrees of Freedom | Sum of Squares | Mean Squares | *F*-Test | Significance |
|---|---|---|---|---|---|
| Regression | 1 | 482.563 | 482.563 | 11.053 | 0.05 |
| Residual | 5 | 218.295 | 43.659 | 0.0209 | |
| Total | 6 | 700.857 | | | |

data. Secondly, the use of Pearson's *r* assumes that relationships between variables are best described by a linear model (Davis 1986:40; Kachigan 1986:205–6; Speth and Johnson 1976:36). All variable pairs except those for the 4th century show a linear trend and thus satisfy the criterion. Visual inspection of crossplots is the best way to detect linear relationships between variables (Carroll 1961:360), and it can be seen from the plot that a straight line would adequately describe the trend in the data.

To examine the nature of the relationship between the two variables, a linear regression analysis was performed. First of all, the outliers observed in the 7th and 8th centuries were eliminated. Since references to warfare recorded during the time period were related not to state formation but to the unification of the Three Kingdoms, they should be excluded from consideration. In contrast, whereas there is no environmental stress in the 1st century B.C. this does not significantly affect the linear relationship, and therefore it is unnecessary to eliminate the zero from the data set.

The least-squares equation for these data is $y = 3.24 + 0.647x$, which is plotted in figure 4. The predicted value of $y$ (the frequency of warfare occurrence) for any value of $x$ (environmental stress) is given by a point on the line and can be calculated according to the equation. The significance of the slope $b$ was also tested using the sample regression equation. The basic assumption is that if there is no relationship between the two variables of environmental stress and warfare, then the slope of the regression equation will be expected to be close to zero. A statistical test for slope $a$, a $t$ statistic ($t$ = estimate/standard error), was used. The null hypothesis should be rejected if $t$ is greater than a given value of $\alpha = 0.05$ significance level using a one-tailed test with degrees of freedom $= n-2$. Since $t = 3.31$ is greater than 2.015, we can reject the null hypothesis ($0.01 < p\text{-value} < 0.025$) (see table 3).

According to this statistical analysis, the data from the *Samguk sagi* suggest that the occurrence of warfare is positively correlated with environmental stress. To measure the strength of the positive linear relationship, the correlation coefficient was computed. Given seven pairs of observations, the computed correlation coefficient $r$ was 0.8297.

## SUMMARY

This analysis of historical documents has led to the conclusion that while warfare played a significant role in sociopolitical development in ancient Korea, the causes of that warfare differ from those proposed by Carneiro (1970, 1988). According to the conventional model, population pressure within a circumscribed environment has been considered a source of all kinds of conflict in human social evolution. However, the case of ancient Korea does not support this model. Instead, warfare apparently occurred as a result of underpopulation; populations were considered not just food/goods consumers but food producers and sources of wealth for elites (e.g., the objects of taxation and corvées). Furthermore, statistical analysis confirms a strong positive correlation between the shortage of subsistence resources caused by environmental stress and the occurrence of violent conflict. Thus, rather than assuming that Carneiro's warfare theory applies everywhere, it is important to consider the unique cultural, historical, and environmental context of the specific study area under examination. Warfare theory may provide a plausible explanation for the emergence of the state-level societies in the prehistory of Korea, but the causes of the warfare may not be the same as Carneiro proposed.

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