

# Reports

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## An Interview with Claude Lévi-Strauss<sup>1</sup>

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MM: Let's start with *The Elementary Structures of Kinship* [1969(1949)], if you don't mind. I've been struck by its completely new way of conceiving the nature-culture opposition. The prohibition of incest, you say, is the process by which nature transcends itself. Would you mind explaining your point of view on this?

CLS: Listen, you're tackling a very hot issue here. The nature-culture opposition is the subject of very passionate debate among anthropologists today, and most of the reproaches I'm subject to are precisely for my use of this opposition, which the postmodernists consider a creation of our own civilization and completely foreign to the character of the peoples that ethnologists study.

MM: But the idea of nature's transcending itself is, in my opinion, an extraordinary conception.

CLS: I don't see anything extraordinary about it at all. Basically, the paradox of the human sciences is that our object of study is humankind, but we mustn't forget that this term is inseparable first from life and then from the whole range of phenomena that make up the world and the universe. Humankind, then, is at once nature and something more—nature, in a sense, and more than nature, in another sense.

MM: Yes, yes, and this is what I find striking. Now I would like to touch on *Tristes tropiques* [1967(1955)] for just a moment, particularly on your remark about the ethnographer as a symbol of expiation. It seems to me that this represents the establishment of ethnology as a fully mature discipline. Would you mind saying a word about expiation, culpability, attempts at redemption?

CLS: You know, this is a perspective that probably would



*Claude Lévi-Strauss with Marcello Massenzio.*  
(Photo Mario Nutile)

not be adopted by every anthropologist and on my part results from the fact that I am an Americanist and Americanists are constantly confronted with the observation that we have destroyed our own object of study or, in any event, reduced its proportions. Thus we constantly approach the South American Indian with both the attitude of the scientific researcher, trying to be objective, and the consciousness of being part of a civilization that has committed a kind of unpardonable sin—in my opinion the greatest sin ever committed in the history of humanity, which is to have destroyed or attempted to destroy half of the richness of humankind.

MM: I entirely agree. Let's move on to *Structural Anthropology* [1963(1958)]. It is hard to imagine another text that has evoked such deep reverberations in the human sciences. I would like to invite you to reconsider some of its main issues, beginning with the method and especially with the analysis of the conditions that you see as underlying the convergence of linguistics and anthropology.

CLS: I believe that what you are calling the reverberations of *Structural Anthropology* were essentially the product of historical circumstances. Basically, structural thought was very little known in France, probably even among philosophers themselves, and it was simply chance that made me the vehicle for bringing the structuralism of its masters Trubetskoi and Jakobson home

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to France. For my colleagues, my contemporaries, it was obviously something new, but my role in that was almost a passive one.

MM: No, no, I disagree! The chapters devoted to magic and religion seem to me to have opened new horizons, and the one on symbolic efficacy raised, among others, a fundamental issue. The world of symbolism, you said, is infinitely various in its content but always limited by its laws. Could you clarify your speculative itinerary in this regard?

CLS: I have been gifted with a very poor memory, and old age is not going to improve it, so reconstructing an itinerary is extremely difficult for me. I would say that it arose from contacts that I had when I was very young—still in high school—with psychoanalysis. It so happened that the father of one of my high-school friends was one of the first psychoanalysts, and he encouraged me to read psychoanalysis even though I was only in my first year of high school. I was fascinated by what to me is Freud's fundamental contribution—the possibility of rationally understanding things that are entirely irrational—but at the same time uncomfortable with the content of psychoanalysis, since I have come to conceive of the unconscious activity of the mind less as an activity based on memories from before or after birth than as essential rules of functioning—which was what linguistics has taught me. Thus what you are talking about, I think, comes from an intersection between what psychoanalysis—or, more exactly, the reading of Freud—brought me and what I learned from structural linguistics.

MM: *Structural Anthropology* gave rise to a great debate about the line of demarcation between ethnology and history. People objected that this delimitation of roles seemed to preclude any integration of ethnology and history. How did you respond to this criticism?

CLS: I answered that there was a misunderstanding based on dates. The article in question was first published in 1949 and dealt with the state of traditional history before the advent of the *Annales* school, and so it tried to establish a kind of contrast but at the same time a complementarity. Even in that article I said, if I remember correctly, that neither history nor ethnology can do anything without the other, but in any event I showed that traditional history and ethnology adopted different and complementary points of view. But, of course, since that time a lot of things have changed: historians have become interested in ethnological work that they used to despise. We mustn't forget that even Durkheim, in his early years, mistrusted ethnologists (and they made him their target). Now I would say that history and ethnology are the same thing, with the slight difference that we study societies spread out in space whereas history studies societies spread out in time. Our work is entirely comparable and mutually fertilizing.

MM: Yes, yes. Now let's talk about *The Savage Mind* [1966(1962)], which is in my opinion the richest in ideas of all your works. In contrast with evolutionism, you maintain that so-called primitive thought is thought in the full sense of the word because it is based on the same requirement of order that is at the root of all thought and it is in terms of that requirement that one evaluates the function of the sacred and of ritual. Would you mind illustrating your position in this regard?

CLS: *The Savage Mind* is actually inseparable from another, smaller book entitled *Totemism* [1963(1962)]; for practical reasons we had to separate them. Those two works represent a turning point in my thought. Up till then I had been almost exclusively concerned with problems of marriage and kinship, and when I became part of the Religious Sciences Department of the Ecole des Hautes Etudes I began to be interested in mythology. Thus I had undergone a kind of conversion, and my objective in *The Savage Mind* was, on the one hand, as you just said, to place the thought of people without writing and that of so-called civilized people in some sort of equality, on the same plane, but at the same time to resolve a contradiction that had obsessed me from childhood—the opposition traced by European philosophical thought between the perceptible and the intelligible. For me, with my extremely changeable tastes, passionately fond of art, objects, plants, and animals, and receiving a Cartesian education in philosophy class, this was an opposition that was hard to resolve. I tried to resolve it by turning to a kind of thought that seemed to me both of the same quality and on the same level as ours but for which this opposition did not exist, having been resolved without any difficulty, because it built intelligible systems on its sense perceptions. This was in a sense a preface to my work on myths.

MM: Yes, yes. In *The Savage Mind* you present magical thought not as a beginning but as a well-articulated system, and thus the opposition set up by evolutionism between magic and science loses its importance.

CLS: No, I don't want to exaggerate, I don't want to distort my thought: I have, after all, great respect for and a deep belief in scientific thought.

MM: But at the same time you have great respect for magical thought . . .

CLS: Yes, yes, but I have to admit that scientific thought works and magical thought doesn't, that it was an attempt—actually, I am wrong in placing it in the past, because magic still exists and all of us are magical in one way or another. But the idea that humankind, which is part of nature (we were talking about that few minutes ago), could at the same time, by its actions and its words, behave like nature was not an absurd idea. It was an idea that made sense to me.

MM: Yes, and a very innovative one, it has to be added.

In the chapter devoted to the logic of totemic classification, you say that theoretical knowledge is not incompatible with feeling, and you add that taxonomy and friendship tend to meet in primitive thought. This is a fascinating subject. Would you mind talking about it a little more?

CLS: You know, for a long time we thought that primitive languages were inferior in their structure and mode of expression to ours, but in this we were ignoring or disregarding the attention they paid to concrete realities. Did you know that Inuktitut, the language of the people once called Eskimos and now called Inuit, contains 60 different words for different states of snow? Well, this is something that is also found in all our technical and professional languages. And at the same time we were overlooking the fact that these languages are capable of abstraction just as ours are. For example, certain American languages from the Northwest Coast use abstract terms where we would use concrete ones. For example, instead of saying, "The woman puts a huge quantity of leaves into a small basket," they say, "The woman puts a huge quantity of leaves into *the smallness of a basket*," and so on. These are languages, then, that can distinguish and classify, on the one hand, and on the other hand do not, for all that, make an abstraction of the link between humankind and natural realities. And if one gathers a food plant, one has a word to refer to it, but this does not prevent one from making an offering to its spirit to be forgiven for having taken it.

MM: Let me say a word about the famous quarrel you had with Sartre: it seems to me that over the years this quarrel has assumed paradigmatic significance. It seems to me to embody the contrast between a system of thought—yours—that is capable of understanding and appraising diversity and another system, ultimately ethnocentric, that is not. Do you agree?

CLS: I do not want to appear to be among those who consider Sartre passé. I respect and admire him; I consider him a genius for his ability—which unfortunately I lack completely—to express himself in various ways, through philosophy, fiction, theater, and so on. That seems to me admirable. However, you speak of a quarrel: there was no real quarrel. Sartre was not much interested in what I said in *The Savage Mind*, and he thought—he said so two or three times—that I did not understand his *Critique de la raison dialectique* [Sartre 1960]. It was completely coincidental that Sartre's book, in which he attacked anthropology and was contemptuous of exotic peoples, appeared while I was writing *The Savage Mind*, and as an anthropologist I had a duty to respond to it. I was giving a seminar on the book at the Ecole des Hautes Etudes, and the two things collided.

MM: *The Savage Mind* is a kind of prelude to the series entitled *Mythologiques*. I am thinking, for example, about your definition of mythical thought as intellectual

bricolage. Do you mind if we talk about *The Raw and the Cooked* [1969(1964)]?

CLS: It's my pleasure.

MM: I'll begin with a personal reflection about the whole tetralogy [*Mythologiques*] and also *The Jealous Potter* [1988(1985)], *The Story of Lynx* [1995(1991)], and others. Thanks to the depth of your analysis, we have replaced the very vague idea of myth with the notion of mythical thought, the properties of which are well defined. This first essential and lasting result is one that transcends all the criticism and controversy aroused by the novelty of your analysis. Do you agree?

CLS: I'll answer that indirectly. When you came to see me for the first time the day before yesterday, you brought me a book that you had just written, and I immersed myself in it. Your own analysis in that book seems fascinating—I leave that aside—and in addition you present a synthesis of the Italian masters' thought on the history of religion. I knew them a little, and from reading you I can see better why they were hard for me to understand: it is because they talk about myths and the sacred but do not push their reflection far enough about what these things are. They take them for granted. And what I tried to do is to begin, before speculating about mythology or religion or about a particular mythology in one part of the world, by asking what a myth really is, what it is made of. In other words, before asking what role my watch might play in my emotional life—making me very impatient when an appointment is late, eager when the woman I love is about to arrive—I would open the watch, take it apart, and see how its insides work. Here I tried to take myths seriously and tell myself that they were objects and objects that required long and very patient analysis.

MM: Yes, I entirely agree! Then, mythology and music: we can start with your very beautiful dedication to music—you remember, music the "mother of memories," the "nourisher of dreams"—music, you say, and mythology are both time-canceling machines. This idea is very exciting, and I ask you to develop it.

CLS: I would say that it is almost a consequence of the links I was trying to create between them. But in speaking of a time-canceling machine I wasn't trying to say anything profound or important. I only wanted to say that what is important for understanding a myth is not following the progress of the story but recognizing that it is made up of superposed slices like the parts of a score and therefore has to be grasped outside the linear time that we are accustomed to, just as when we have really listened to a piece of music our memory reassembles the phrases we have heard into a whole. I don't think there is any philosophical message here!

MM: But I think it's a very beautiful formulation!

CLS: Yes, but it's a purely technical remark.

MM: Technical but nonetheless fascinating! The structural analysis of myths offers a possibility that is of considerable speculative interest, that of gaining access to a dimension in which the mind is, in a way, reduced to mimicking itself as an object—an object among objects. Do you want to talk about this important subject?

CLS: Well, I would say that it is part of the program I seem to have followed—not something I planned but something that just happened. In fact I began to studying the rules of marriage and of kinship systems, that is, creations, of course, of the mind but subject to practical and experiential constraints—since there are men and women in different proportions and of different ages and all this produces constraints. And when I turned to the study of myths it was, in a way, to test the theoretical ideas I had developed earlier by saying that if we are in a domain where empirical constraints are relatively unimportant—where the mind is apparently completely free, so free that we usually consider the plots of myths absurd, unbelievable, and impossible and it is as if the mind were given free rein and no longer under any kind of control—then if I find constraints in that domain, where they are not supposed to be, it means that they really exist. It was Tylor, one of our great predecessors or even founding fathers, who said that if there are laws anywhere they must be everywhere.

MM: The "Finale" of *The Naked Man* [1981(1971)] fascinated me very much. This book concludes your tetralogy, and looking back at the huge work you have produced you write a very articulate assessment of it. This final reflection has to do with the primordial opposition between being and nothingness that is the source of all the other oppositions contained in myths. It is a famous opposition—the "to be or not to be" pronounced by Hamlet—and you add, "Between being and nothingness it is not humankind's place to choose." Would you mind explicating your point of view?

CLS: Here I'm going to respond directly: no! Because at the end of that work I allowed myself a kind of daydream, and I would be at a loss to comment on it. Like the mind in myths I gave myself free rein, and I wrote some sentences that probably aren't worth very much and don't mean much either, except that at the end of this enterprise I had a sense of its vacuity—a sense that reducing thousands of myths to a sort of common form was a little bit, on an infinitesimal scale, reducing the story of humankind itself, with its many different cultures and civilizations, which, in apparently irreducible forms all meant the same thing and which, like the myths, will one day disappear without a trace—a kind of pessimistic reaction at the end of a very long effort that I realized probably had no real justification except the one I had always given myself, of never being bored!

MM: This time I disagree! I think that the end of *The*

*Naked Man* is the most beautiful part of your tetralogy. This time the author is not the best judge of his work.

CLS: The author is *never* the best judge, in my opinion.

MM: Well, now we can move on to *Anthropologie structurale deux* [1973], which, once again, gives a general picture of your system of thought and your method. The essay devoted to Rousseau, "Jean-Jacques Rousseau fondateur des sciences de l'homme," is in my opinion—and I am not alone in this—one of your most important texts. Saussure and Rousseau are your privileged references. Would you mind talking about your discovery of the philosophy of Rousseau, "the most ethnographer of philosophers," as you call him?

CLS: You know, the text you are talking about is a speech I gave in Geneva on the occasion of Rousseau's 250th anniversary. I feel a little uncomfortable about it, however, because my position on Rousseau has evolved over the years. As an adolescent I was essentially interested in Rousseau's writing style—the way he expressed himself was always extremely attractive to me because of his ability to say very concisely what today we can express only with very complicated circumlocutions. Besides, his ethnographic work and field experience among the people and in the countryside were very important. We mustn't forget that Rousseau was a field man, and this seems decisive for the birth of the human sciences. We are always being told that for Rousseau humankind was naturally good, that humankind in the state of nature had been perverted by civilization, and so on, but this is completely wrong. When one reads Rousseau carefully it is clear that this is not what he wanted to say at all. He distinguishes first a state of nature in which humankind was neither good nor bad, judgments and criteria of morality not having yet arisen. This was humanity in a zero state. Then came the first state of society, the one that today we identify with the Neolithic (although these categories did not exist in Rousseau's time), which, for him, because it was reduced to essentials, was really the best. Then, because of demographic expansion and the invention of the mechanical arts and civilization, there was a second state of society, and this one corrupted humankind compared with the previous one. And all of this was expressed in Rousseau, naturally in a language that is no longer ours and with some conjectures that we could criticize today, but this view seems to me, for his time, the most profound that had yet been suggested and veritably establishes Rousseau as the founder of the human sciences.

Now, let me explain what made me move away from Rousseau later. It was his political thought, which gave rise to the French Revolution and, in a way, atomized the individual in relation to the sovereign principle, that is, the community. And I became increasingly aware of the importance for a well-balanced society—and this brings me closer to the tradition of Montesquieu rather than Rousseau—of the existence of intermediary bodies that create a succession of screens, of moderating buffers,

between public power and individuals. Thus my admiration for Rousseau is in many respects intact, but his political thought proper does not seem to me as . . .

Of course, I could say that what appeals to me about *Le contrat social* is the difficulty of the text, which constantly forces one to think about what exactly Rousseau had in mind and wanted to say. And actually, it is only a fragment of a book he never wrote, isn't it?

MM: Beyond the reference to Rousseau, it seems to me that your work is deeply rooted in French culture but transcends it. In the field of anthropology Marcel Mauss represents for you a very significant reference point. Would you mind talking about your relationship with Mauss and especially about the link between the *Essai sur le don* [1954] and *The Elementary Structures of Kinship*?

CLS: Marcel Mauss is someone I didn't know very well. I never took a course from him, because my program was entirely philosophical and it was only after entering into professional life that I turned to ethnology. And when I had the opportunity to go to Brazil, Dr. [Paul] Rivet, who was the founder of the Museum of Man and for whom I was to gather specimens for its collection, sent me to meet Mauss, who welcomed me kindly, but I only saw him a few times. It was less his person than his work that was decisive for me. My reading of the *Essai sur le don* was a kind of revelation. I wrote that sometimes in reading it I had the same feeling that Malebranche must have had in reading Descartes for the first time, and there were two main reasons for this. The first was his ability to assemble facts from entirely different times and places and compare them not with the old comparative method, which is content with only superficial resemblances, but by bringing them together for analysis in depth. And that was a methodological idea, and therefore, I suppose—I didn't know this at the time—one of the essential bases of structuralist thought, although Mauss was certainly not consciously a structuralist. And then this foundation that Mauss established about social relations and exchange and reciprocity was illuminating for me because he was doing scientific research aimed primarily at social relations. And this study of invariants, which I probably owe in large part to Mauss, as well as to Saussure and Jakobson, is the basis of my thought.

MM: You dedicated *Anthropologie structurale deux* to Emile Durkheim on the centenary of his birth. In your very beautiful dedication, you declared yourself an inconstant disciple of the founder of *l'Année sociologique*. What did you mean by that?

CLS: You have to understand that when I was studying at the Sorbonne, where naturally I heard a lot about Durkheim, my bachelor's degree was in ethics and sociology. At the time, I have to admit, I was not at all interested in ethnology. My interest in it developed when I started reading English and American authors and people who had direct experience of fieldwork, and it is

thanks to them that I really came to understand what ethnology was. And so at the time I felt hostile to Durkheim, and then, subsequently, precisely by reading with great pleasure the works based on fieldwork, I realized that Durkheim had understood everything, or nearly everything, about Australian societies even before fieldwork had confirmed what, in a way, he had intuitively guessed from the first field studies. But my Australian colleagues are the first to pay homage to Durkheim today by saying, "He understood long before us many things that we have since rediscovered." And so I wanted to make up for my youthful infidelity to Durkheim's thought by joining his ranks again.

MM: In the essay entitled "Ce que l'ethnologie doit à Durkheim" you underline the fundamental importance of *Les formes élémentaires de la vie religieuse* [Durkheim 1912], the work that was in a way the origin of ethnology as a discipline. If I am not mistaken, this work does not seem to be considered a classic. What do you think about that?

CLS: The *Formes élémentaires* has two aspects: a reflection, as I have just said, on Australian societies and their beliefs—and here, as I've told you, the most modern Australianists pay homage to Durkheim—and a much larger project of reconstructing how religious belief may have arisen. Now, obviously, one can no longer agree!

MM: Let's move on to *The View from Afar* [1985(1983)]. I will limit myself for the moment to the last part. The unifying thread in the various essays is the relation between constraint and freedom. You say that there is no opposition between the two, that instead they support each other. Can you explain?

CLS: I have the feeling that to have sense and content freedom cannot exist in a vacuum—that it is always a matter of having experienced an obstacle of some kind and managed to overcome it. And nothing could be more apt than the words of my colleague and friend Roland Barthes in his inaugural lecture at the Collège de France: "Language is fascist." Why? Because language has laws and constraints. And it is because language has laws and constraints that we can have such beautiful poetry, which consists precisely in using those constraints and knowing how to overcome them in every domain. *The View from Afar* has a little more polemic, perhaps because it is more directed at the contemporary world. Some of my contemporaries have a tendency to say that we need freedom everywhere, that a child needs to express himself exactly as he wants and all constraints, all obstacles are harmful. I wanted to react against all of that and say that painting is fighting against brushes, tubes of paint, and canvas and intellectual or philosophical work is a kind of fight against wrong ideas or bias in the minds of one's contemporaries.

MM: I want to ask you just one last question, because I don't want to impose on your kindness! I read "Race et

culture" [1983(1971)] with interest. Would you mind comparing it with your famous essay *Race and History* [1952]?

CLS: Well, those are two separate texts, I believe, with a distance of 20 years between them, but they have something in common because they were both commissioned by UNESCO. *Race and History* was for a collection of little books about the race question, and in it I tried to explain why the notion of race was unacceptable. It became almost a little catechism against racism. It is taught in schools, and every year high-school students call me and say, "We have to write an essay about *Race and History* and we don't understand it. Can you explain it?" Reading it like that, however, involves only half of it, because while I was trying to explain why the notion of race was unacceptable I was also trying to show the dangers that are associated with a standardization that could lead to antiracist bias, and that hasn't been seen at all! Then, 20 years later, UNESCO asked me to do a conference to inaugurate an International Year Against Racism, probably thinking that I would repeat the same catechism that they remembered from the first book. I don't like to repeat myself, and in addition a lot had happened in the preceding 20 years—notably the emergence and development of population genetics, which made collaboration between anthropologists and biologists possible once again. It was impossible in the time of the old physical anthropology, which involved measuring skulls and imagining that civilization was based on differences between lengths and widths of skulls. And so I tried, on the one hand, to accentuate what had not been seen in *Race and History* and, on the other hand, to take into account the progress that had been made with regard to this possible collaboration. This was extraordinarily poorly received by UNESCO, which had the feeling that I was completely betraying myself compared with what I had said before. *L'Humanité*, the newspaper of the Communist party, quoted a passage to show "how Lévi-Strauss has changed," but this passage was word for word in *Race and History*!

MM: So, the people at UNESCO tried to prevent you from speaking?

CLS: Not exactly, but they managed to preface my intervention with another speech that was so long that I had to shorten mine considerably!

MM: That's a very terrible kind of censorship! But you did manage to speak?

CLS: I managed to speak, and UNESCO finally published the speech.

MM: Really! So it was a real success after all! Now to conclude, *Paroles données* [1984], a text I really like, a text that documents your teaching activities: I would like to conclude this interview by thinking about the final part of your last course at the Collège de France.

You take your leave by underlining the efficacy of a principle that is the leitmotiv of your work: in your opinion the principal mission of ethnology is to see to it that we have irreplaceable testimonies to the richness and diversity of humankind, and so I ask you to devote your last response to the fundamental value of this richness and diversity.

CLS: It is always perilous for someone very old to express himself pessimistically about the present. This vice was denounced a long time ago by Horace and continues to be so today, but a very long life has made me nearly coincide with my century, and when because of that people ask me, "What is for you the event of the century?" I can only give one answer: at the beginning of the century humanity was a billion and a half people, when I started my active life nearby 1930 it was two billion, today it is six billion, and even if demographic expansion slows down, as we are promised it will, or perhaps completely stops, it will be even more in 20, 30, or 40 years. And this is, for me, the major event, the absolute catastrophe that has befallen humanity in an epoch I have had, from this point of view, the misfortune to experience. And what attracts me to ethnology, what has made it my life's work, is precisely that it confronts the richness and diversity of which the weak effective force of humankind was perhaps a sort of prerequisite. A past extending back centuries and millennia and hundreds of millennia to the time when humankind began has presented us with small societies that were to become very numerous, each of which gave rise to original ways of life, irreplaceable beliefs, forms of oral or pictorial or sculptural expression constituting something unique in human experience, and my role as an ethnographer was, to the extent that these have survived until the past one or two centuries and we can approach them through texts and objects and direct experience in fieldwork, to preserve something of this richness and diversity so that humankind in other forms, forms absolutely different from those that existed in the past, will be acquainted with them.

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## The Epidemiology of Infectious Diseases among South American Indians: A Call for Guidelines for Ethical Research

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With alarming frequency, the native peoples of South America continue to become victims of neglect and abuse. Such incidents rarely come to public attention, and in the few instances in which we learn about them remedial action is rarely taken. Who is to blame for this and what can be done to prevent the extinction of indigenous groups during the 21st century are simple questions for some social critics—notably Patrick Tierney, author of the recent and much publicized book *Darkness in El Dorado* (2000). Unfortunately, Tierney's journalistic enthusiasm for sensational allegations directed at a few individuals trivializes the complex causes of the plight of the region's native peoples (Hurtado 1990) and draws attention away from the kind of analysis that can produce lasting solutions.

One of Tierney's most serious charges is that medical scientists and anthropologists caused epidemics among the Yanomamo of Venezuela over 30 years ago. Journalists are not trained to decide such things; epidemiologists are, and they are unlikely to claim to know what caused an epidemic many years after it took place. They find it difficult enough to do so in the midst of an epidemic. The work is very time-consuming and costly and re-

quires experts from multiple public health disciplines with a great deal of experience in collecting and analyzing valid and accurate quantitative data. We may never know the who, when, and how of the origins of the measles and malaria epidemics of the mid-1960s in Yanomamo communities, just as we may never know why a measles epidemic broke out among Angaité communities in the Paraguayan Chaco in January of this year (*Ultima Hora*, January 15, 2001). What we do know is that the vast majority of epidemics occur when medical scientists and anthropologists are absent from these communities.

Tierney's charges could have disastrous consequences. They may give policy makers the false impression that the causes of the poor health of the Yanomamo and other indigenous people are easy for journalists and others to identify. Policy makers so deceived are likely to propose and implement policies that deny South American Indians the right to epidemiological and medical research and intervention. Without adequate knowledge native peoples will not receive the medical care they so desperately need, and without medical care their health will continue to deteriorate, their economic productivity will continue to decline, and their health will deteriorate further (Psacharopoulos and Patrinos 1994).

What policy makers need to be made aware of is the many reasons for the poor health of indigenous people throughout South America. Among the most important insults to indigenous well-being are the complacent and racist attitudes of government officials who approve meager budget allocations for indigenous public health programs, fail to punish rampant embezzlement of scanty funds, and promote proposals that violate native land rights (Centeno 1997). Such complacency intensifies the negative effects on native health of biological host factors such as low genetic diversity and poor immune-responsiveness to infectious agents. Even though a great deal has been published about these and other biological insights since the 1970s (CIBA Foundation 1979), they are generally overlooked by those who plan public health programs or draft guidelines for field research. Consequently, most South American native communities today have not benefited from the scientific research that has been done among them. If indigenous groups have become increasingly hostile to scientists, it is not because medical scientists and anthropologists cause health problems but because these professionals are not helping enough to prevent new problems or to remedy existing ones.

Anthropologists and medical scientists need to be aware of the complex causes of poor health among South American natives in order to make a difference over the long term. Similar biological and social factors influence indigenous health regardless of the number of years that native peoples have lived in close interaction with outsiders, but they do so in different ways depending on the level of acculturation. We will discuss these factors and provide examples of their effects on indigenous health during contact and thereafter. We will go on to argue that this knowledge needs to become part of the process

of developing guidelines for field research. Field scientists who provide little if any medical help are now at risk of being accused of exploiting South American Indians as research subjects—advancing their careers without any concern for their subjects' well-being. This interpretation may be correct in some circumstances but not in others, and at present we lack ways to make this important distinction. This is partly because there are no internationally sanctioned guidelines that clearly specify what constitutes an ethical response to local health problems. Without such standards, anyone who wishes to can easily damage the work of others by accusing them of failing to abide by rules of behavior that they unilaterally decide to be ethical. At the same time, anyone who chooses to be apathetically negligent can do so at will. We will suggest six specific areas of field research that require immediate attention by the American Anthropological Association (AAA). The current guidelines for fieldwork developed by the AAA Committee on Ethics require only doing no harm to the study population, but this is no longer sufficient with native populations. It is time to be specific about what constitutes sufficient concern and action.

Because we are citizens of the United States and teach at a public university in the United States, we address our remarks to our national anthropological organization, the American Anthropological Association. We recognize that similar debates are taking place in other national anthropological and academic organizations in the United States, government agencies in Latin American countries (*El Nacional*, November 24, 2000), and various organizations in other continents.

#### EPIDEMIOLOGY OF INFECTIOUS DISEASE

The epidemiological profiles of South American indigenous groups vary in complex ways across time and space. This diversity has to be viewed against the background of the host factors that their members have in common. Ironically, James Neel, a scientist accused by Tierney of genocide among the Yanomamo, is one of the main contributors to our current understanding of disease susceptibility in these populations (Neel 1971, 1974, 1977). Among other factors, susceptibility appears to be influenced by low genetic diversity (Black 1990, 1994) and macroparasite-induced immune defense (Sousa et al. 1997).

Lack of genetic diversity is sometimes associated with higher rates of susceptibility to all sorts of illnesses (Carrington et al. 1999, McNicholl et al. 2000, Turner et al. 2000, Zlotogora 1997). On average, indigenous peoples have much less heterogeneity in the highly polymorphic loci that control the immune system, the Class I and II histocompatibility antigens (MHC) and the immunoglobulin allotype genes (Black 1994).

The negative effects of homozygosity on native health may be intensified by parasite loads on immune defense against bacteria, mycobacteria, and viruses. Most indigenous groups of South America tend to be chronically infested with macroparasites such as *Necator ameri-*

*canus* and *Ascaris lumbricoides* (Salzano 1988, Hurtado et al. 1997), and they also tend to produce immunoglobulin IgE at some of the highest levels ever reported for individuals who do not suffer from extremely serious and sometimes lethal anaphylaxis (Hurtado et al. 1999). Indigenous persons with abnormally high levels of IgE are healthy and active members of their groups. IgE-driven defense against parasites competes with defense against infectious diseases such as malaria and tuberculosis because some pathways of immune defense against parasites and against bacteria and viruses tend to be mutually exclusive (Beyers et al. 1998, Hurtado et al. 2001). These high levels of IgE production are only in part related to parasitic infestation, since nonindigenous populations that are equally parasitized show much lower IgE levels.

For the most part, enormous deficiencies in the public health systems of South American countries create conditions that further exacerbate the effects of susceptibility to infectious diseases. Most of these countries invest much smaller percentages of the gross national product in health than developed countries (Pan American Health Organization 1994). Very small fractions of this investment are devoted to the health of indigenous people, and even these limited funds are often embezzled by government officials (*ABC Color*, July 15, 2000).

At the same time, international efforts to control disease are hampered by local problems of distribution and surveillance. For example, Paraguay is notorious for the very high rates of tuberculosis its indigenous communities suffer. Unfortunately, the sources of this information are rural and missionary nurses as opposed to the government-mandated national surveillance officials who are responsible for tracking these epidemics (Hurtado et al. 2001). Cases are rarely treated or are treated in ways that promote drug-resistant tuberculosis (Frieden et al. 1993, Farmer et al. 2000). When Paraguay's National Commission of Tuberculosis Control receives a shipment of tuberculosis medications, it generally keeps them in the capital city or, when it makes them available to rural and indigenous communities, does so without the kind of direct observed treatment program that ensures completion of drug regimens (American Thoracic Society et al. 1992). For these and other reasons prophylactic and prompt curative treatment of tuberculosis is not an option for most Indians in Paraguay. To be treated, they have to wait until they develop active tuberculosis—that is, until they have contaminated family and friends with the airborne bacilli produced by chronic cough.

When scientists bring such disheartening observations to the attention of local officials, they often respond that their (inadequate) surveillance systems show no such pattern and claim that in fact infectious disease rates are low among indigenous communities.<sup>1</sup> Most officials either fail to realize or are unwilling to admit that, while many sectors of society benefit from the epidemiological

1. We base these conclusions on the fieldwork experiences of three of us (Hurtado, Hill, Kaplan) in Paraguay, Peru, and Venezuela over the past 20 years.

surveillance systems mandated to generate information that is used to justify public health expenditures, indigenous communities are almost entirely excluded from these systems because of their remote location and cultural barriers. As a consequence, the knowledge that scientists wish to share with officials about disease rates and disease prevention is infrequently if ever used to inform policy.

Thus, it appears that host factors such as homozygosity and high IgE production, among others, in combination with social factors such as lack of epidemiological surveillance and limited if any access to well-timed vaccinations, sanitation, and medical treatment, cause high rates of infectious diseases among South American indigenous groups. These factors influence the epidemiological profiles of indigenous groups with little if any admixture *regardless of the number of years that they have lived in close proximity to non-Indians.*

#### THE DEVASTATING EPIDEMIOLOGICAL EFFECTS OF CONTACT

Tierney is correct in stating that the past 500 years of contact between Native Americans and people of European descent have had disastrous consequences for the former. Epidemics have killed millions. Typically, first face-to-face contacts result in the death of between one-third and half of the native population within the first five years of contact (Hill and Hurtado 1996, CIBA Foundation 1979). The majority of South American Indian groups were exterminated in this way during the first two centuries after European arrival in the Americas (Hemming 1978). Unfortunately, the lessons of half a millennium have not resulted in any significant improvement. If a group of native South Americans that had been living in isolation for some time were to make contact today, the result would probably be equally catastrophic.

In Brazil alone, there are still some 30–50 groups living isolated from face-to-face contact with people of European or African descent (the National Indian Foundation [FUNAI] estimated 55 uncontacted tribes two years ago [Veja, June 10, 1998]). Others exist in Peru and Bolivia and perhaps in Venezuela, Ecuador, and Colombia. As global economic, social, and population forces drive people into remote areas for the purposes of colonization and resource exploitation, contact is inevitable, but there is no contingency planning for managing its medical, economic, and social consequences.

Remote peoples in South America face three major threats. First, woodcutters, miners, colonists, missionaries, and even representatives of government-sponsored organizations are entering their traditional ranges in increasing numbers. Face-to-face contacts with some members of remote groups will occur as a result of these incursions. Eventually one or more of those contacts will result in the transfer of disease organisms to individuals with little resistance to them. Epidemics will ensue, even among people who never actually come into contact with people of European descent, since the natural

response to disease among mobile peoples is to flee to neighboring villages or camps. Second, the habitats of remote peoples are shrinking rapidly. As more areas are colonized, the available habitat to support subsistence is increasingly circumscribed and may eventually be insufficient to support the nutritional needs of the group. In the short run, resource pressure can lead to nutritional stress, disease, and both inter- and intragroup conflict. In the long run, it may result in loss of access to traditional territories as colonists and others establish de facto or legal ownership of them. Third, many local groups have become isolated from other members of their larger ethnic groups because of incursions into their territory. These isolated groups may be so small that they do not constitute viable mating populations (Baruzzi et al. 1979). As a result, small demographic shocks can lead to their physical extinction even without epidemics.

Many anthropologists and indigenous-rights activists believe that uncontacted Indians should be left alone. These people are well-meaning, but they are wrong because they base their position on three incorrect assumptions. First, they assume that the Indians have chosen to remain isolated. They have not. What they have chosen is to avoid those they believe would kill, enslave, and abuse them. There is little doubt that most would immediately opt for contact if they expected trade, affection, help, and support. Humans are a social species and enjoy productive interactions with neighboring groups. Most isolated tribes have difficult lives (Hill and Hurtado 1996). All the Indians that we have ever spoken to gladly accept improvements in their physical conditions and health situation if offered by true friends. None are content with the typical 30–50% child mortality rates that they experience without Western medicine. As soon as it becomes clear to isolated natives that those attempting to contact them are peaceful and friendly and can provide them with technology to ease some of the burdens of their lives, they virtually always initiate a contact.

Those who oppose contact also assume that the Indians will inevitably be decimated by virgin-soil epidemics. This is not true. Two of us, Hill and Kaplan, have been present at contact sites within days of first contact. If competent medical care is available and consistently present during the first five years following contact, few contact-related deaths need occur. The last band of Northern Aché foragers in Paraguay was contacted in April 1978, and Hill began medical care of that group in collaboration with two missionary organizations within days of contact. Only two small children out of a group of 22 died within the first five years, and both had been in poor health at contact. In 1979 another band of Aché contacted a missionary family that subsequently lived with them in their traditional home range down to the present. Only one child from a group of 37 died within the first five years after contact. The key to survival after contact for these two groups was competent medical care 24 hours a day, 365 days a year for several years.

Finally, opponents of contact assume that isolated native groups will survive if not contacted. Population ge-

netics and demographic models clearly show that this is not true. Most isolated populations of less than several hundred are destined to become extinct through accidental population fluctuations. This process is much more rapid in small groups and in situations where traditional territories have recently decreased in size. Almost certainly many isolated groups became extinct in the 20th century without ever making contact. In some cases one or two final survivors may be rescued at the end of such a decline.

Although we now have a body of scientific literature on virgin-soil epidemics among native peoples, the record of years of missionary and governmental experience, and articles providing specific advice on contact situations (e.g., Hill and Kaplan 1989), individuals and organizations making contact with isolated native groups seem as uninformed as those 100 years earlier. Even well-meaning groups and individuals continue to ignore this accumulated knowledge. For example, the planned FUNAI contact with the Korubo of Brazil in October 1996 included 26 individuals who were not screened or quarantined, including 8 journalists and their assistants, but no physicians. When outsiders suggested that the original team might have infected the Korubo, government officials returned to the contact site for a few hours but without qualified medical personnel (see <http://www.nationalgeographic.com/features/96/contact/index.html>).

There is now sufficient documentation of the consequences of first contacts that we can no longer plead ignorance. The remaining isolated peoples in South America should not suffer the same fate as those of the previous 500 years. The American Anthropological Association and other anthropological organizations should form a panel of experts now to develop policy on this issue and advise pertinent government and missionary organizations. The enormous number of deaths that will result from failure to act are *preventable*.

#### DISEASE AFTER CONTACT

Contact is only the beginning of the problems that South American Indians face. Sedentism, poverty, and poor access to health care, in addition to biological influences on disease susceptibility, lead in many cases to deterioration of health status (Psacharopoulos and Patrinos 1994). Conditions do not always improve immediately after the initial period of contact. Among the Aché of eastern Paraguay, in the first ten years after contact the infant mortality rate was higher than prior to contact, and it has returned to precontact levels only recently (Hurtado and Hill 1996). In addition, the effects of novel infectious pathogens such as *Mycobacterium* spp., *Plasmodium* spp., and numerous intestinal parasites have interacted in ways that have undermined the Aché's precontact robustness. Since contact, the Aché have experienced two malaria epidemics during which many adults were unable to provide for their dependents. Their diet has changed dramatically as well, from one based on plentiful animal protein to a nutrient-poor manioc-based diet. In fewer than five years tuberculosis became

a major source of health problems, and by 1994, only a decade after contact, the lifetime prevalence of active cases of tuberculosis among Northern Aché had increased from less than 1% to 18%. In addition, 50% of individuals over 15 years of age now test positive for tuberculosis infection (Hurtado 2000). Other hunter-gatherers have fared equally poorly. Among the Hiwi of Venezuela, who made contact in 1957 and continue to depend on hunting and gathering for their subsistence, leprosy and violence (massacres and murders) have been some of the main causes of morbidity and mortality since the 1960s.

Tuberculosis outcomes are an excellent example of the importance of susceptibility to infectious diseases in combination with social factors many years after initial contact. The Native American case is well-documented (Rieder 1989). By 1900, tuberculosis was the most serious health problem among North American Indians. Even though mortality, morbidity, and risk of infection have sharply decreased, the incidence rate among natives is still 4.4 times higher than the rate of Americans of European descent (Centers for Disease Control 1987). In the South American countries with the highest prevalence of tuberculosis, such as Bolivia and Peru, indigenous populations have been the hardest hit by this disease. Data from other countries suggest that many communities will be devastated by tuberculosis over the next decade. By 1992, the prevalence rates of tuberculosis exceeded 1% among the Cuna of Panama, and most of those infected were not receiving any treatment (Caminero Luna 1995). Even higher prevalence rates have been reported for indigenous groups of the Paraguayan Chaco over the past decade (Galeano Jiménez 1995), and the rates of compliance with treatment are dismal, particularly among hunter-gatherers (Meinke-Giesbrecht, Floto, and Hettwer 1993). Lastly, prevalence rates of tuberculosis infection among the Shuar of Ecuador are comparable with those of the Aché and are lowest in the communities farthest from nonindigenous villages (Kroeger and Barbira-Freedman 1982).

The way in which indigenous populations mount immune defenses against bacterial pathogens is one of many host factors that may help explain high rates of infectious disease mortality not only at contact but many years afterward. Sousa et al. (1997) studied Yanomamo communities of Brazil that made contact in the 1960s and found that the Yanomamo develop immune defenses to tuberculosis infection different from those of other populations. First, the prevalence rate here of 6.4% active cases of tuberculosis is considerably higher than one would expect for a population exposed to the disease for fewer than 15 years, although it is still considerably lower than that observed among the Aché (Hurtado et al. 2001). Second, compared with their Brazilian neighbors the Yanomamo had higher titers of antibodies against *M. tuberculosis* glycolipid antigens (14% versus > 70%). Thus, relative to other populations with exposure to tuberculosis, the Yanomamo mount unusually high antibody responses at the expense of the more effective cell-mediated immune responses that are typi-

cally observed in nonindigenous people. Third, the rates of tuberculosis infection as measured by tuberculin tests show positive responses at much lower rates than one would expect in a group with as high a prevalence of active cases—27%, in contrast to the 90% reported for Eskimos in the 1950s and only slightly lower than the rate for the Aché (32%, all ages). In nonindigenous populations, lower than expected rates of positive tuberculin tests tend to occur only in immunosuppressed individuals such as HIV patients. Thus, if left unchecked, tuberculosis epidemics among the Yanomamo could have long-term consequences as devastating as those of the measles epidemics in the 1960s, particularly *if active cases are left untreated or drug-resistant tuberculosis is allowed to emerge* as a consequence of intermittent treatment. In at least one case in South America, tuberculosis has already exacted a huge toll. Six hundred of the 800 Surui who were alive at contact in 1980 had died by 1986 from concurrent tuberculosis and other epidemics (Fleming-Moran, Santos, and Coimbra 1991).

Data on indigenous groups that are considerably more acculturated than some of those just mentioned show that the future of South American Indian populations many years after contact is bleak indeed. Recent studies using large samples indicate that indigenous people are often less healthy than their rural peasant neighbors (Psacharopoulos and Patrinos 1994). In South America, indigenous people make up about 27% of the rural, and poorest, population (Jazairy, Alamgir, and Panuccio 1992). Moreover, indigenous infants and children have much higher mortality rates than their nonindigenous counterparts in every country where these rates have been measured (Psacharopoulos and Patrinos 1994). In Bolivia, monolingual urban indigenous peoples are two to four times as likely to have been sick or injured in the past 30 days, to have been kept from work for more than a week, to have received no medical help if sick, and to have missed yellow-fever vaccination campaigns (p. 68). Child mortality rates here are three times as high among monolingual indigenous mothers living in poverty as among nonindigenous mothers (p. 89). In Peru, indigenous people are less likely to have been vaccinated against BCG, polio, and measles and more likely to have had diarrhea in the past 15 days than the nonindigenous poor (p. 166).

These are only a few examples of the devastating consequences of pathogens many years after contact. Thus, guidelines that specify the ways in which indigenous people should be protected from the ravages of disease need to address both the contact and the postcontact period.

#### A CALL FOR ACTION

The intent of international and national guidelines for research among native peoples should be to use fundamental scientific understanding of infectious disease epidemiology to serve humanitarian ends (Eades and Read 1999). Science can and should help indigenous people just as it has helped the rest of the world. Recognizing

that it took 100 years for antituberculosis campaigns to reduce death rates from 2 to 0.001 per thousand persons in the United States (Hopewell 1999) and that this is only one of innumerable examples of how science has managed to reverse ills for all humanity, it is difficult to fathom why mortality rates among some South American Indians can be as high today as they were 100 years ago.

In spite of the enormous potential benefit to indigenous communities, many have become increasingly unwilling to participate in scientific projects. In fact, many Indians are suspicious that scientists intend not to help but to exploit them. One of the most important reasons for this rejection and suspicion is that for decades scientists have not adhered to fieldwork ethics that natives can clearly identify as positive for their communities. This means that we need to reexamine fieldwork ethics and develop stronger guidelines in several key areas.

Current guidelines for fieldwork developed by the AAA Committee on Ethics require only that the anthropologist do no harm to the study population, leaving considerable room for interpretation as to what sorts of conduct are indeed harmless. We suggest that ethical behavior goes beyond simply not harming to more proactive behavior. Six areas of field research that require careful consideration in expanding on current guidelines are as follows:<sup>2</sup>

1. *Conduct and disease prevention during contact situations.* Tierney suggests that anthropologists and medical scientists should have taken preventive health measures to avoid excess contact-related mortality in Yanomamo communities. He does not specify what measures, nor is he able to cite internationally sanctioned guidelines that do so. Something needs to be done soon by the AAA and other national and international anthropological organizations to correct this problem. First, a commission must be established to evaluate the status of existing isolated peoples. There is a great deal of information already available through reports from private individuals and government officials who have visited or live in remote areas. This information needs to be analyzed and the status of each group evaluated. Which groups are under immediate threat of contact? What are the likely sources of contact (miners, woodcutters, colonists?) Which groups face probable extinction because of small population size in isolation? Such a commission should include experts in anthropology, physicians and public health administrators, and government officials who are responsible for policy development with regard

2. Several of these recommendations, reviewed in an early draft of this manuscript, were incorporated into a motion passed by the Executive Board at its meeting on November 16, 2000, during the AAA Annual Meeting. The motion charged the AAA Committee on Ethics to consider developing draft guidelines to be included in the Code of Ethics and/or to develop other materials regarding anthropologists' responsibility to provide assistance during health emergencies, the fairness of remuneration and the impact of material assistance, the potential negative impact of factual data, and the issue of informed consent. The motion was disseminated to the AAA membership through electronic mail and published in the *Anthropology Newsletter*.

to native peoples. Second, policies on contact need to be developed. Which groups can be protected from contact? Which groups should be approached and offered contact because they are likely to become extinct in the immediate future? How should contacts be managed in both the long and the short term? Third, medical protocols must be developed and financed. Rapid vaccination and outreach medicine could bring contact-related mortality to less than 2–3% in the first year (Hill and Hurtado 1996, Baruzzi et al. 1979). Since newly contacted peoples have no experience with health posts and Western medicines, a nontraditional medical program must be developed. Medical treatment must be delivered whenever and wherever it is required without relying on the people themselves to seek it. Since isolated peoples remain highly susceptible to foreign antigens for decades, such programs need to be permanently sustained.

2. *Medical relief during health emergencies.* Tierney criticizes scientists and journalists for not spending more time and resources fighting the Yanomamo measles epidemic and not getting more involved in alleviating observed health problems while doing fieldwork. The same charge could be leveled at him and all other observers (e.g., journalists, tourists, missionaries, those who become informed through secondary accounts) who encounter suffering but do not take “sufficient” action to alleviate it. Many anthropologists do provide health care for study populations and other forms of economic assistance, sometimes at great cost to themselves. Other anthropologists are uncomfortable with the notion that they are responsible for providing services that should be the job of other organizations supposedly dedicated to such things (e.g., governments, missionaries, and health and human rights organizations). Some sincerely feel that it is their job to observe and not to interfere in events, and some simply lack the training to provide health assistance to study populations. If anthropologists are expected to provide health assistance, who will provide the funds for medicines, medical training, and transportation and pay for lost work time? Should anthropologists lobby major funding agencies such as the National Science Foundation and the National Institutes of Health to provide each research project with funds that can be diverted into such assistance? The AAA and other anthropological organizations need to consider this issue. There are no clear answers, and every anthropologist who has ever done fieldwork has been forced to make difficult choices in this area.

3. *Fairness of remuneration for research cooperation.* Tierney suggests that Chagnon did little to help the Yanomamo during his 25 years of research with them and that the organization he created, the Yanomamo Survival Fund, was intended to help only by providing data that might be useful to other groups assisting the Yanomamo. The lack of direct assistance to the Yanomamo community appears to be a complaint voiced by many Yanomamo critics of Chagnon. Again, the same criticism could be aimed at nonanthropologists who make incomes from native peoples (e.g., journalists, missionaries, staff members of indigenous rights organizations).

Many anthropologists base their entire careers on work done on populations that are paid only direct informant fees during a fieldwork period. Because the lifetime earnings of a professional anthropologist (including salary, book and film royalties, etc.) are more substantial than informant fees, this opens anthropologists up to a charge of exploitation of native peoples. This charge is reinforced by the commonly held anthropological belief that the value of specific research results to the target population is payment enough for the access and cooperation it provides. Most traditional populations, in contrast, see little practical value to the research results produced by anthropologists and medical scientists. The AAA and other anthropological organizations have never specifically provided guidelines on the level of remuneration (beyond informant fees) that is appropriate and how that remuneration should be linked to the earnings of researchers that result from their focus on a particular population.

4. *The impact of material assistance.* Tierney accuses Chagnon of fomenting violence by giving gifts to some Yanomamo communities. Given that it is impossible to give equal material support to all communities or all individuals at any field site, should anthropologists be held responsible for jealousies and competition over resources that they distribute or the acts committed by individuals who gain advantage through their material rewards? This is a general dilemma faced by all organizations and agencies who provide material assistance to groups, including national governments. The implication of Tierney’s charge is that anthropologists should avoid conflict by giving no gifts at all. Most study populations would find that suggestion cruel and self-serving, and the refusal to pay for services rendered would be considered unethical behavior by many. But all researchers who do fieldwork need to consider the potential impacts of their gifts on study populations, and the AAA and other anthropological organizations have no clear guidelines here.

5. *The impact of factual data about a study population.* Tierney criticizes Chagnon for publishing information and viewpoints about Yanomamo warfare that damage the reputation of the Yanomamo and can be used by their enemies to justify denying them certain rights and privileges. Chagnon counters that he is simply publishing factual data and it would be deceitful and scientifically unethical to conceal or change them. The AAA and other anthropological organizations have not adequately addressed this dilemma, and as a result there are several other similar controversies in anthropology (e.g., reports of prehistoric cannibalism in the U.S. Southwest).

6. *Informed consent.* Tierney accuses a variety of researchers of collecting data without adequate informed consent. A similar charge has recently been leveled at other anthropological researchers using native DNA (*New Scientist* 2000). Some scientists believe that informed consent consists only of an explanation of the methodology, the procedures used on study subjects, and the potential dangers of those procedures. As long as this

information is provided and study subjects are not harmed or disenfranchised in any way, the consent is legitimate. Cultural anthropologists often obtain no informed consent for their studies but assume that if the community tolerates their presence informed consent is implied. At the other end of the spectrum, some people believe that informed consent includes not only an explanation of data collection methods but a detailed explanation of the research topics that will be examined with the data collected. If this view were taken literally, many studies could never be carried out (because informing subjects would change the character of the data gathered), and no post facto analysis of any field data could ever be done (since subjects would not have been informed at the time that a particular use of the data was contemplated). This version of informed consent would probably eliminate behavioral research entirely (since the behavior of study subjects would always be affected by knowing why they were being observed) and preclude a huge number of useful scientific studies done using data initially collected for a different purpose. Finally, no scientifically sophisticated study could ever be done on a relatively uneducated population because its members would not be able to give true informed consent (implying that they understood the purpose of the research). For example, no DNA studies could be done on any population that did not know what DNA was or understand the basics of modern genetics. Both extremes of the informed-consent debate seem unfair or unrealistic, but the AAA and other anthropological organizations should provide some guidelines on what exactly constitutes valid informed consent in anthropological studies. The same standards that apply to material data collection (blood, DNA, etc.) should apply to the collection of any information about a study group.

The success of the development of policies and guidelines will depend in large part on the composition of the teams that are brought together to implement them. Members of indigenous communities, governmental and nongovernmental Indian-affairs organizations from various continents, the World Health Organization, and other major health organizations should be active participants in this process along with large funding agencies, ethics committees, and anthropological associations. These efforts should be motivated by the realization that it is no longer tolerable for researchers who rely on First World know-how, technology, and resources to offer little if any help in implementing solutions to indigenous health problems (Farmer 2000).

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## Risk Sensitivity and Value among Andean Pastoralists: Measures, Models, and Empirical Tests<sup>1</sup>

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Risk sensitivity has intrigued anthropologists because of the role it can reasonably be expected to play in decision making given the uncertainties of food supply and weather patterns and the hazards that surround us (Winterhalder, Lu, and Tucker 1999, Douglas and Wildavsky 1982, Vayda and McKay 1975). Using data from Andean herders, I will operationalize a definition of risk sensitivity and demonstrate how risk sensitivity varies with environmental and social variables. The potential benefits of incorporating risk into models of economic behavior are obvious in the Andes. Andean mountain environments are cold, unpredictable, and limiting (Molina and Little 1981:115–16; Orlove and Guillet 1985:5; Browman 1984:314; 1987; Goland 1993:318). Frequent droughts, snows, and generally dry environments constrain the subsistence choices of Andean peoples, leading ethnographers and archaeologists to assert that Andean people will be risk-averse (Custred 1977; Gade 1975:94; Browman 1984, 1987; Brush 1982; McCorkle 1987; 1990:10; Guillet 1986:210; Goland 1993; Winterhalder 1994; Isbell 1978; Hesse 1982; Aldenderfer 1998). The patterns of risk sensitivity I present are consistent with research in economics (Friedman and Savage 1948, Kahneman and Tversky 1979, Bosch-Domènech and Silvestre 1999, Butler 2000, Morrison 2000), agricultural economics (Dillon and Scandizzo 1978, Elamin and Rogers 1992, Zuhair, Taylor, and Kramer 1992), biology (Real 1991, Stephens 1990), and human behavioral ecology (Winterhalder, Lu, and Tucker 1999).

### RISK SENSITIVITY IN ANTHROPOLOGY

Anthropologists have not overlooked the importance of modeling decision making under uncertainty and risk (Cancian 1972, 1980, 1989; Quinn 1978; Ortiz 1980,

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1983; Gladwin 1975, 1989; Orlove 1986; de Garine and Harrison 1988; Halstead and O'Shea 1989; Cashdan 1990; Winterhalder 1986a, b, 1990; Goland 1993; Fratkin 1991; Smith 1991; Kuznar 1991a, b, 2000). One of the most detailed studies is Frank Cancian's (1972:144–56; 1980: 171; 1989) pioneering work on the influence of status on risk taking among Mexican peasants. He points out that very wealthy peasants are more likely to take chances because their level of wealth is well above crucial thresholds. Lower-middle-class peasants will also engage in risky behavior because the prospect of entering a higher wealth status is so near. Very poor and moderately wealthy peasants will be averse to risk—the moderately wealthy because they have too much to lose and the very poor because they cannot afford to lose. Cancian (1989: 151) finds risk aversion to be widespread among moderately wealthy peasants and rural farmers in India, Pakistan, Kenya, the Philippines, and the United States.

Winterhalder, Lu, and Tucker (1999) review recent optimal-foraging applications that incorporate risk and propose that much risk-sensitive behavior can be understood by employing a sigmoid utility curve to model people's preferences. Utility is a measure of a person's satisfaction with a good or some decision. As far back as 1738, the mathematician Daniel Bernoulli (1954 [1738]) noted that utility does not necessarily vary 1:1 with quantities of actual goods. Milton Friedman and Leonard Savage (1948) recognized that people's utility functions for some good (for example, wealth) tend to be sigmoid, or S-shaped.

In the sigmoid curve (fig. 1), the first part of the curve is convex and the last part concave. Convex utility curves correspond to a preference for risky prospects. For instance, individuals of wealth status  $w$ , offered an even chance of either increasing or decreasing their wealth by  $p$ , will take the gamble because if they win their wealth status will increase by  $a$ , a greater gain than the loss  $b$ . In contrast, risk-averse individuals at wealth status  $x$ , offered the same gamble, will reject it because the most they can gain is  $d$ , which is less than the potential loss  $c$ . Friedman and Savage saw the sigmoid utility curve as a reasonable description of how people's risk sensitivity changes with wealth. People with convex utility functions aspire to the next-highest class and therefore are willing to take a chance at a perceived higher increase in utility from a gamble. In contrast, people in a comfortable wealth class are reluctant to risk what they have for a comparatively small increase in utility.

Winterhalder (1986a, b, 1990) and Goland (1993) employ an approximation to a sigmoid utility curve in their Z-score model of risk-sensitive decision making. Winterhalder (1986a:374) begins by defining risk as "the probability of falling below a fixed minimum requirement ( $m$ ). This might be starvation or some less catastrophic but significant cost to fitness or adaptation." The model is so named because minimum requirements are standardized with the common Z-score. In this model people will make decisions to exploit a set of resources so that they minimize the probability of falling below the minimum requirement (measured as some standard

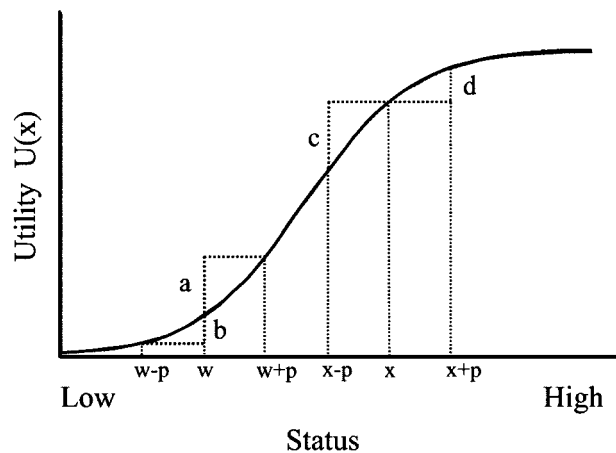


FIG. 1. *The sigmoid utility curve. Risk-preferring decision makers are represented by the convex portion of the curve (origin to inflection point), where a gamble on winning or losing  $p$  is accepted because the potential gain  $a$  is greater than the potential loss  $b$ . Risk-averse decision makers are represented by the concave portion of the curve (inflection point to end), where a gamble on winning or losing  $p$  is rejected because the potential gain  $d$  is less than the potential loss  $c$ .*

deviation below the mean). Goland (1993) conducted an empirical test of the Z-score model in two Peruvian agropastoral communities and found that peasants dispersed their fields to reduce variance in yield because stochastic shocks that reduce yield (frosts, hail, droughts, theft) are unevenly distributed across the landscape.

While yielding important insights, anthropological explorations of risk sensitivity do not all agree, and they generally fail to consider the subjective component of decision making. Optimal-foraging theory applications are consistent with research in biology: some species of animals, including people, close to a starvation income tend to take chances in order to obtain enough food (Winterhalder, Lu, and Tucker 1999:317, 332, 334). In contrast, Cancian's statements on class and risk sensitivity can be translated into the sigmoid utility curve in figure 2, where poor peasants are risk-averse (have a concave utility function) while upper-class peasants prefer risk (have a convex utility function). Furthermore, most applications are tied to etic measures of value (e.g., starvation thresholds, actual variances in income), which people do not necessarily perceive accurately. People's perceptions of the actual variances of events can be very subjective, and most people in most economies are not so concerned with starving as with maintaining or gaining in social status (Douglas and Wildavsky 1982:71;

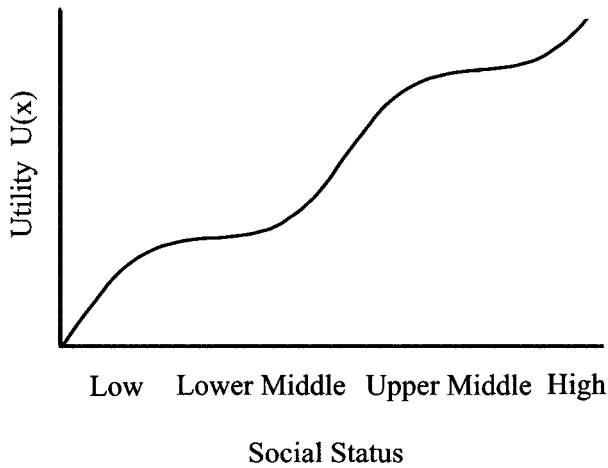


FIG. 2. Translation of Cancian's (1972, 1989) theory of peasant risk sensitivity.

Smith and Mandac 1995; Bar-Shira 1992). A more thorough consideration of economic utility theory will resolve these problems and increase the generality of both optimal-foraging applications and Cancian's results.

#### UTILITY THEORY

Utility theory provides a means of monitoring how people perceive risk and of measuring subjective values by taking advantage of an individual's perception of risk (von Neuman and Morgenstern 1944, Luce and Raiffa 1957, Myerson 1979). The application of utility-theory methods does not require that decision makers have any explicit idea of probability or make explicit mathematical calculations (Rapoport 1966:30). They need only make decisions based on their subjective perception of probabilities. It is assumed by this method that a decision maker's preferences are complete, transitive, and continuous (von Neuman and Morgenstern 1944, Luce and Raiffa 1957, Myerson 1979). Completeness means that a decision maker can compare any alternatives under consideration. Transitivity means that a decision maker who prefers A to B and B to C will also prefer A to C. Continuity means that a decision maker's utility increases continuously such that if A is preferred to C, any option B that is ranked between A and C can be represented by a randomized combination of A and C. Provided that a decision maker's preferences meet these requirements, researchers can use utility-theory methods to monitor preferences and to model decision making. In the application of utility theory I present below, I address the validity of each of its axioms for my particular case study.

Economists, taking an explicitly deductive approach, tend to rely for its validity more on the theory's axiomatic foundations than on empirical demonstrations (Perry 1998, Paris and Caputo 1993). When economists do test utility theory, it is often in experiments con-

ducted in industrialized Western societies (Kahneman, Knetsch, and Thaler 1990, Cubitt and Starmer 1998, Bosch-Domenèch and Silvestre 1999, Butler 2000). Some experimental economists have focused on violations of utility-theory assumptions. Many of these limitations were detailed in a seminal article by Daniel Kahneman and Amos Tversky (1979) in which they noted common violations of utility theory such as unequal weighting of losses versus gains, overweighting of certain outcomes over probabilistic ones, and failure to consider common features of prospects relevant to the calculation of their value. Other researchers have built upon this foundation (Karmarkar 1979, Tversky and Kahneman 1992, Cubitt and Starmer 1998, Butler 2000, Morrison 2000). Despite various limitations, utility theory appears valid when its assumptions can be met, and violations of assumptions can often be overcome with modifications to utility functions (Kahneman and Tversky 1979, Tversky and Kahneman 1992, Butler 2000). As Morrison (2000:194) notes, despite the limitations of utility theory, "a clearly superior model has not yet been identified."

In contrast to critical experimental studies, actualistic studies by agricultural economists (Bar-Shira 1992, Smith and Mandac 1993, Dillon and Scandizzo 1978, Elamin and Rogers 1992, Zuhair, Taylor, and Kramer 1992) tend to support the fit between utility theory and people's actual behavior. For instance, Bar-Shira (1992) found that, when a feasible solution to a land allocation problem for farmers exists, risk aversion coefficients can be assessed and people behave in accordance with utility-theory predictions. Zuhair, Taylor, and Kramer (1992) found that utility functions provided accurate predictions of harvesting strategies among Sri Lankan peasants. Other researchers have applied these methods successfully among slash-and-burn horticulturalists and agrarian peasants in northeastern Brazil (Dillon and Scandizzo 1978) and the Philippines (Smith and Mandac 1992).

Some anthropologists have considered utility theory. Harold Schneider (1974) explicitly used utility theory as the basis for his formalist approach in economic anthropology. Sutti Ortiz (1980, 1983) took into account subjectivity in people's values for goods and in their evaluations of probabilities to discuss the potentials and limitations of economic methods in anthropology. Because of the empirical and cross-cultural nature of their fieldwork, anthropologists can make potentially important contributions to utility theory by testing its foundations empirically in contexts sensitive to cultural and social variables.

#### UTILITY THEORY IN PRACTICE

Utility-theory methodology involves presenting decision makers with options, or lotteries, that have different long-run expected utilities. Expected utility,  $E[u]$ , is important because a probabilistic return (e.g., a 50% chance of winning \$100) either rewards an individual or not, depending on whether the event (winning) occurs. However, if the trial is repeated many times, the expected return will equal the prize multiplied by the probability

TABLE 1  
High-Sierra Utility Interviews

Herder/Sex	Preference Ranking			Probability Premium <sup>b</sup>
	Goats	Sheep	Cows	
1/m	<i>b</i>	<i>c</i>	1	0.45
2/m	1	<i>c</i>	<i>c</i>	0.49
3/f	<i>b</i>	1	<i>c</i>	0.45
4/m	<i>b</i>	1	<i>c</i>	0.45
5/f	<i>c</i>	1	<i>b</i>	0.0
6/m	<i>c</i>	<i>b</i>	1	0.45
7/m	1	<i>c</i>	<i>c</i>	0.4
8/m	1	<i>c</i>	<i>c</i>	0.3
9/f	<i>b</i>	<i>c</i>	1	0.35
10/f	<i>b</i>	<i>c</i>	1	0.4
11/m	1	<i>c</i>	<i>c</i>	0.4
12/m	<i>b</i>	<i>b</i>	1	0.45
Aggregated animal values <sup>a</sup>	$4 + 6b + 2c$	$3 + 2b + 7c$	$5 + b + 6c$	

<sup>a</sup>The sum of the values assigned to each rank for each animal over the set of herders interviewed.

<sup>b</sup>A measure of risk sensitivity in which the value represents the additional probability, *X*, that a decision maker required to be indifferent between a certain prize of 50 animals and a lottery with probability 0.5 + *X* of winning 100 animals. Positive values indicate risk aversion, zero values risk neutrality, and negative values risk-preferring attitudes.

of winning ( $0.5 \times \$100 = \$50$ ). The techniques of utility theory are easily adapted so that they can be applied among Andean pastoralists for monitoring risk sensitivity.

The first step in applying utility-theory methodology was to establish whether it was warranted. The Aymara herders I interviewed in southern Peru had no trouble rank-ordering their preferences among different animals, satisfying the completeness axiom. The herders also demonstrated the transitivity axiom by consistently ordering their preferences among the different herd animals they had available to them. Finally, herders comprehended the choice between a certain prize of animals and a lottery that offered the chance of winning animals (some herders had actually played lotteries), and so the continuity axiom also seemed reasonable.

In order to measure a herder's risk sensitivity, I presented a herder with a choice of prizes, either a particular number of animals of that herder's preference or a lottery ticket that offered the possibility of winning a larger number of animals of that kind. I varied the probabilities assigned to the lottery ticket until the herder had difficulty choosing between the prizes (see Dillon and Scandizzo 1978 and Zuhair, Taylor, and Kramer 1992 for similar applications). If, for instance, a herder was indifferent between a certain prize of 50 animals and a ticket that offered a 50% chance of winning 100 animals, then that herder's value for animals was risk-neutral because the expected utility of a 50% chance of 100 animals was 50, the same as the certain prize. However, if a herder required a higher probability of winning 100 animals, say, 75%, then this indicated risk aversion; the herder required a higher chance of winning the uncertain yet larger prize than what simple expected utility predicts.

The probability above or below a risk-neutral expectation required for the decision maker to take a gamble is called the *probability premium* (see n. 1); in this case it was 25%. If a herder preferred to take, say, a 30% chance of winning 100 animals rather than take 50 animals for certain, this would indicate risk-preferring behavior, and the probability premium would be negative, -20%. The probability premium is a measure of a decision maker's sensitivity to risk; negative values indicate a willingness to bet on long shots, a zero value indicates risk neutrality, and a positive value indicates risk aversion.

Comparing the relative values of different animal species is methodologically more complicated. Theoretically, animal values can be estimated in a similar application of utility-theory methodology. An accessible description of this method is found in Rapoport (1966: 30), and I have used this method to assess the relative values of herd animals among Aymara pastoralists (Kuznar 2000). However, the method can be limited if the arbitrary value of zero is assigned to the least preferred animal as I have done, for this implies, probably contrary to fact, that the animal has no value to a herder. An alternative is to aggregate animal values over all herders' preferences. For each herder, one assigns a value of 1 to the highest-ranked animal and arbitrary values *b* and *c* to the second-highest-ranking and the least preferred animal respectively. Then one can sum the rankings for each of the animals over the set of herders (see tables 1 and 2). Quantitative estimations of animal value are derived by inserting values for *b* and *c*. The limitation of this method is that the values for *b* and *c* are arbitrary. The advantage is that one avoids undervaluing low-ranking animals. I will use this aggregation method and then perform a sensitivity analysis in which values of *b* and

TABLE 2  
Puna Utility Interviews

Herder/Sex	Preference Ranking			Probability Premium <sup>b</sup>
	Llamas	Alpacas	Sheep	
1/m	b	I	c	0.20
2/f	b	I	c	0.15
3/m	b	I	c	0.45
4/m	b	I	c	0.40
5/m	b	I	c	-0.2
6/f	b	I	c	0.00
7/m	b	I	c	0.30
8/m	b	I	c	0.35
9/m	I	b	c	0.25
10/m	I	b	c	0.25
11/m	I	b	c	0.45
Aggregated animal values <sup>a</sup>	3 + 8b	8 + 3b	IIC	

<sup>a</sup>The sum of the values assigned to each rank for each animal over the set of herders interviewed.

<sup>b</sup>A measure of risk sensitivity in which the value represents the additional probability,  $X$ , that a decision maker required to be indifferent between a certain prize of 50 animals and a lottery with probability  $0.5 + X$  of winning 100 animals. Positive values indicate risk aversion, zero values risk neutrality, and negative values risk-preferring attitudes.

$c$  are varied and values in Kuznar (2000) are used to check the robusticity of my findings.

In generating my final data, I had to take care that respondents were giving me their actual preferences instead of the result of their own optimizing calculations. This was accomplished by identifying the forage needs of different species of animals and what forage resources herders possessed. These discussions, which generated self-reports of aspirations and assets, were supplemented by my own detailed mapping and analysis of the vegetation and forage potential of the communities in which I worked (see Kuznar 1991c, d, 1994, and, for maps and forage data, 1995).

I examined risk sensitivity in two Andean herding communities located in contrasting environmental settings, one in the high sierra (2,500 m–3,800 m above sea level) and one in the Andean puna (3,800 m–4,500 m above sea level). Both communities lie along the Río Asana in the Department of Moquegua, eastern Mariscal Nieto Province, Peru. The environment in this region is arid, and the landscape is dominated by bunch grasses and xerophytic shrubs (Kuznar 1995, 1999, n.d.). The high-sierra community is located between 2,500 m and 3,800 m above mean sea level and is characterized by deep valleys, steep terrain, and seasonal rains. Pastoralists move up and down high-sierra valleys seasonally with their herds of goats, sheep, and cattle. The average family owns 142 goats, 13 sheep, and 4 cows (Kuznar 1993:259). Herders raise these animals primarily for meat, although the milk of females whose offspring die is an important secondary resource.

A number of hazards afflict high-sierra herds, among them theft, drought, and predation. For instance, Win-

terhalder (1994) and Kuznar (1990, n.d.) provide measurements on the predictability of rainfall in the Andes based on information theory (Colwell 1974). In general, the annual predictability of rainfall in highland areas is less than 50%. In the communities I report here, droughts, although infrequent, can claim up to 60% of a herd, and predation is a constant low-level threat, claiming 4% to 8% of a herd annually (Kuznar 1991c: 97; 1994:60; 1995:44, 47, 50). According to herders' statements and some personal observations, theft accounts for 10% to 20% of herd losses in the high sierra (see also Custred 1974:287; Orlove 1980).

The puna community is above the high-sierra community in the Andean high-altitude plain between 3,800 m and 4,400 m. The puna is too high for effective goat and sheep herding, and therefore indigenous herd animals, the llama (*Lama glama*) and the alpaca (*Lama pacos*), are the primary animals herded in this zone. Llamas are large and provide transportation for goods to lowland markets where puna pastoralists can obtain agricultural products in trade. In addition to transportation, llamas provide meat and wool for making bags and ropes. Alpacas produce valuable wool that herders trade and use in the manufacture of blankets and clothing. Family herds in this puna community average 82 alpacas, 22 llamas, and 8 sheep. The primary hazards that afflict herds in the puna include snow, drought, predation, and theft. Snow is the most severe hazard, with families losing up to 50% of their herd in a severe snow year. People mitigate other hazards by effective defense of herds with dogs and family networks that cooperate for defense against rustlers.

The presence of clear, unpredictable hazards in these two communities makes them good candidates for investigating risk sensitivity. Two Aymara assistants and I interviewed 12 herders among the high-sierra families (50% of adults and adolescents) and 11 herders in the puna community (17.5% of adults). These communities were very small, so the sample should capture results generalizable to the communities as a whole. More interviews in larger communities would be ideal. However, the relationships I investigate turn out to be extremely strong and highly statistically significant, despite the small sample size. Assessing the representativeness of my data will require further research.

#### RISK SENSITIVITY IN AYMARA HERDERS

High-sierra pastoralists are extremely risk-averse (table 1). Only one individual in the sample is risk-neutral, and no one prefers risk. The average probability premium is 0.38 (i.e., a person requires near 90% certainty of winning a large prize of 100 animals before abandoning a certain prize of 50 animals), with a coefficient of variation of 34.2%. In general, high-sierra pastoralists prefer cows to goats, and sheep have the lowest preference (table 1). There are interesting trends in animal preference by gender (table 3). According to women's statements, they prefer animals with a direct domestic household use, such as sheep that produce wool and cows that produce milk.

TABLE 3  
*Animal Values<sup>a</sup> by Sex of Herder*

Animal	Sex of Herder		One-tailed T Statistic	d.f.	Probability
	Male	Female			
Alpacas	0.83	1.00	0.905	9	0.190
Llamas	0.67	0.50	-0.904	9	0.190
Sheep	0.18	0.40	1.545	21	0.069*
Goats	0.70	0.40	-1.581	10	0.072*
Cows	0.44	0.65	0.759	10	0.232

\*significant at 0.10 level.

<sup>a</sup>Calculated with the aggregation method using values of 0.5 for *b* and 0.1 for *c*.

In contrast, men prefer animals that produce high market values, such as goats, or high market values and prestige values, such as cows. The statistical associations are weak ( $p < 0.1$ ) for sheep and goats, with women preferring sheep for wool and men preferring goats for their cash value. Cows are preferred by both sexes but for different reasons; women prefer them for milk production while men prefer them for prestige and cash value. While the statistical associations are weak, they are in the direction expected according to other researchers (McCorkle 1990: 16), who report gender to be a crucial variable in determining the goals of Andean agro-pastoralists.

Puna pastoralists are more variable in their degree of risk sensitivity and also tend to be less risk-averse than their high-sierra counterparts. The average probability premium is 0.24 in the puna, with a coefficient of variation of 82.5%. There is one risk-neutral person and even one risk-preferring individual in the sample. Although there is variation, there is still a strong tendency toward risk aversion among puna pastoralists. The majority of puna pastoralists prefer alpacas to llamas, with no obvious gender differences (table 2). Puna pastoralists devalue sheep because of these animals' inability to survive snow and predation hazards and their low reproductive rates at high altitude. The comparatively higher risk aversion of high-sierra pastoralists (one-tailed  $T = -2.107$ , d.f. 21,  $p = 0.023$ ) may be related to overall risk load; as I have said, they are subject to higher uncertainty regarding drought and theft.

#### RISK SENSITIVITY AND WEALTH

On the basis of economic and optimal-foraging research, I propose that there will be a general curvilinear relationship between wealth and risk sensitivity in which the poor and the wealthy will prefer risky prospects whereas those with more moderate wealth will be risk-averse (fig. 3). My argument for moderately wealthy and wealthy individuals (i.e., middle levels of wealth) is similar to Cancian's argument for lower-middle-wealth and high-wealth peasants; peasants in the middle have too much to lose to take risks. My argument for what I define as the poor is that, as one gets closer and closer to the edge, one has less and less to lose; desperate people will

take desperate chances to improve their lot. This statement, while seemingly contra Cancian (1972), is entirely consistent with the work of biologists on nonhuman animals and of human behavioral ecologists on subsistence-level economies (Winterhalder, Lu, and Tucker 1999:317, 334). The model I propose is also more consistent with well-established theoretical formulations (Pratt 1964:123), experimental studies (Bosch-Domènech and Silvestre 1999), and studies by agricultural economists (Dillon and Scandizzo 1978, Elamin and Rogers 1992) that demonstrate that wealthy and poor people prefer risky prospects.

Testing Cancian's theory and my modification of it requires first addressing what constitutes "wealthy" in an Aymara herding community. While many factors would influence a family's income and overall wealth (sibling alliances, kin who have migrated to cities, family size, political position in community), wealth in animals is the primary criterion upon which people judge one another, herd size is a family's primary hedge against hard times, and ascending the local political hierarchy is impossible without possessing a large herd (Kuznar 1991a, c, 1995, 1999). The values I use for animals are the aggregated utility values I estimate (see above) for animals in a particular community. Each family's herd value is calculated by multiplying the number of each type of animal in a herd by its aggregated utility value and summing the results.

To calculate a person's wealth status, the value of his

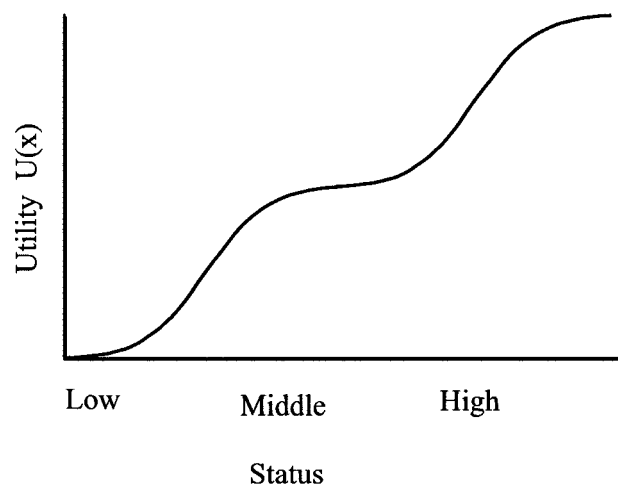


FIG. 3. Proposed relationship between wealth status and risk sensitivity. The very lowest-status decision makers will risk gambles because they have nothing to lose; potential gain in utility is higher than potential loss. Middle-status decision makers are risk-averse because potential loss in utility outweighs potential gain. When the decision maker approaches a higher status, his or her utility curve becomes convex and risk taking is attractive as a means of jumping into the next-highest status.

TABLE 4  
Results of Regression of Risk Aversion against Herd Value as Measured by Z-Scores

Utility Values	Regression Equation	R	R <sup>2</sup>
$b = 0.5, c = 0.1$	Probability premium = $0.443 - 0.273 (\text{Herd Z-score})^2$	0.857	0.734
$b = 0.5, c = 0.25$	Probability premium = $0.446 - 0.276 (\text{Herd Z-score})^2$	0.857	0.735
$b = 0.75, c = 0.5$	Probability premium = $0.454 - 0.305 (\text{Herd Z-score})^2$	0.851	0.724
Values from Kuznar (2000)	Probability premium = $0.441 - 0.265 (\text{Herd Z-score})^2$	0.858	0.736

NOTE: Standard errors for the two components of each equation are (1) 0.039 and 0.046, (2) 0.040 and 0.046, (3) 0.041 and 0.052, and (4) 0.039 and 0.044. For all the coefficients in the equations and for each model,  $p < 0.0001$ .

or her herd is compared with the mean herd value for the community in terms of a standard Z-score. The use of Z-scores allows us to combine data from goat herding and llama herding communities, since herd values refer not to goat or llama herds but to a person's deviation from whatever constitutes the mean value for the community. Using these values, I have been able to compare risk sensitivity coefficients and herd values for 15 individuals.

There should be a curvilinear relationship between wealth and risk sensitivity such that decision makers with levels of wealth near the mean (i.e., within one standard deviation of the mean) should be the most risk-averse, whereas decision makers with high or low levels of wealth (i.e., wealth levels above or below one standard deviation from the mean) should prefer risk. Such a relationship is simply modeled by probability premium =  $c + b (\text{wealth Z-score})^2$ , where  $b$  and  $c$  are constants. Despite the small sample size, there is a strong and highly statistically significant ( $p < 0.0001$ ) curvilinear relationship in which people with either very high or very low levels of wealth (i.e. Z-score absolute values above 1) are associated with low probability premiums and those in the middle have very high probability premiums (table 4, fig. 4). The initial analysis used values of  $b = 0.5$  and  $c = 0.1$  for the aggregated animal values. Sensitivity analyses were conducted with values ranging from  $b = 0.5$  to  $0.75$  and  $c = 0.25$  to  $0.5$  and the alternative values I have presented elsewhere (Kuznar 2000). All models yielded extremely similar coefficient estimates with near-identical measures of fit, indicating a very high degree of robustness in the model despite the varied measurements of value used (table 4).

The risk-preferring behavior of poor herders seemingly contrasts with Cancian's (1972, 1980, 1989) research in which the poorest peasants were risk-averse. Another difference between these results and Cancian's model is that moderately poor herders (people with Z-scores greater than  $-1$  but less than 0) do not prefer risky prospects as Cancian predicts. However, the discrepancy between the models may be due to differing notions of what "poor" means. In the biological examples cited by Winterhalder, Lu, and Tucker (1999) and in my own data, "poor" means near subsistence level. For instance, one high-sierra family had only 16 goats and 19 sheep and one family in the puna only 30 alpacas, dangerously low

levels of wealth in this risky environment. Perhaps Cancian's poor are poor relative to their neighbors but above this desperate level and so are established within a wealth class with which they may be unhappy but that they are unwilling to risk losing. The sigmoid utility curve that fits Cancian's data (fig. 2) is simply a higher portion of the curve I propose for the Andean herders that I have studied (fig. 3).

#### DISCUSSION

Most pastoralists interviewed had high probability premiums, lending empirical support to the notion that Andean herders are risk-averse and generally confirming earlier empirical studies of people in subsistence economies (Dillon and Scandizzo 1978; Cancian 1972, 1980, 1989; Elamin and Rogers 1992; Zuhair, Taylor, and Kramer 1992). However, following research in economics (Friedman and Savage 1948), biology (Real 1991), human behavioral ecology (Winterhalder, Lu, and Tucker 1999), economic anthropology (Cancian 1972, Netting 1993), and cultural anthropology (Douglas and Wildavsky 1982), risk sensitivity and value vary with environmental and social variables (e.g., wealth and social risks); gender has some influence on value. On the basis of the research I have presented here, I propose the following relationships between environmental and social variables and risk sensitivity:

1. As variability of environmental resources increases,

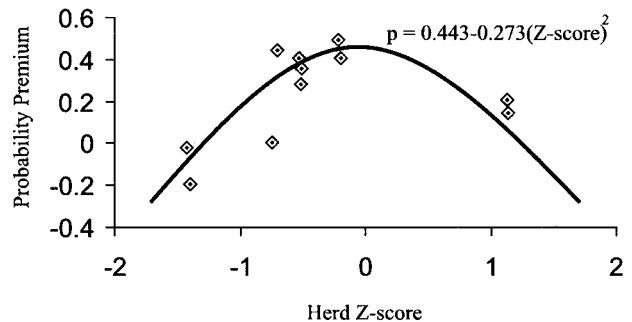


FIG. 4. Probability premium versus herd Z-score for Andean herders.

risk aversion will increase (Winterhalder 1986a, b) unless the level of resources approaches starvation or culturally defined destitution (Winterhalder, Lu, and Tucker 1999).

2. As social risks (theft, lack of trust among community members) increase, risk aversion will increase, following the patterns between environmental variables and risk.

3. People situated within a wealth-status class (as measured in a particular setting and according to their norms) will tend to be risk-averse unless they are close to moving into a higher status (Cancian 1972).

Researchers can use utility-theory methods to generate data on key variables for models of human decision making. Operationalizing such variables contributes to cross-cultural testing of theories of human behavior and evolutionary theories of economic and reproductive strategies that may illuminate how people deal with uncertainties in subsistence, reproduction, and cultural success.

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