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DRUMS AND GUNS: A CROSS-CULTURAL STUDY
OF THE NATURE OF WAR

by

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INTRODUCTION

Throughout the history of the discipline, anthropologists have shown a perennial interest in warfare, generally avoiding the question of universal causes of war and instead describing specific practices in specific societies. These descriptions vary in thoroughness, reflecting both the emphasis upon warfare and war-related activities of individual cultures, and the era and interests of the ethnographers. The quality available to the cross-cultural research worker ranges from the full-blown accounts of North American Indians in the Great Plains during the 19th century to mere passing comments for many societies in Asia and Africa. Between 1950 and 1960, only a few anthropological papers on warfare and one ethnological study, Turney-High's Primitive War (1949), were published. During the decade 1960 to 1970, the literature grew, largely stimulated by concern over the Indochina war. In response to a demand within the anthropological community, a symposium on war was held during the American Anthropological Association's national meetings in the fall of 1966. Subsequently, the papers and some comments from the floor were published (Fried, Harris, and Murphy 1968). It was felt at the time, and by a reviewer later, that the results of the symposium were "not what we have been waiting for" (Fox 1969:315).

Since 1966 the national meetings have each included symposia on warfare and conflict, yet inspection of paper abstracts indicates that the emphasis has continued to be on the specific case and the specific society, with a reluctance to generalize. Those generalizations that have been made are sweeping ones, characterized by the judgment that human actions are explained best by determinist and unitary theories.

After a lengthy discussion of what is right and wrong about these contemporary generalizations, along with the problems inherent in generalizing in social science, I have presented yet another attempt at generalization, using the rationale and methodology of cross-cultural research. The issue of why men kill each other is too complex to be analyzed solely in terms of a few well-chosen cases, either single cases or a small sample subjected to statistical analysis. Large amounts of data can be handled through statistical analysis, the method used in this dissertation. The body of literature is small but growing, and the styles in quantification have undergone changes. Quincy Wright (1942; rev. ed. 1965) provided tabulations early on relationships between "warlikeness" and continental location, temperature, natural habitat, climatic energy, race, subsistence, political organization, social organization, and cultural isolation for 650 societies. He made little attempt to define these variables, and he did not perform any statistical manipulations. Broch

and Galtung (1966) recently used Wright's data to measure associations between cultural complexity and frequency of warfare, using an "index of primitivity." They found that the "level of belligerence varied from 0 per cent at the most primitive level to 95 per cent at the least primitive or traditional level." All other recent research has been carried out using small samples: Naroll (1966) made a cross-cultural survey of 48 primitive societies on the frequency of warfare and military orientation in order to test the deterrence hypothesis: that societies with strong military orientations (armaments, fortification, tactics) will engage less frequently in war. He found the opposite to be the case. A subsequent test (Naroll 1969) of 30 hypotheses related to deterrence using a sample of 20 historic civilizations produced similar results. Otterbein and Otterbein (1965) published a cross-cultural study of 50 societies on the relationship of feuding to the frequency of warfare, fraternal interest groups, and level of political complexity. Their results showed that feuding occurs when fraternal interest groups, indicated by the presence of polygyny and patrilocal-ity, are present but that it is controlled by political authority and declared states of war. Otterbein (1968) also tried to demonstrate, again using 50 societies, that fraternal groups and unauthorized raiding parties influence the frequency of internal war in uncentralized political systems but not in centralized ones, and that the frequency of

external war does not influence the frequency of internal war. Otterbein in a larger work (1970), but still using a sample of 50 societies, tested the survival value of military efficiency in the maintenance and evolution of political communities. Ember and Ember (1971:593), using Otterbein's warfare codings and with sample size varying from 18 to 33 societies, found that "matrilocal societies have purely external warfare [that is, warfare only with other societies] much more often than patrilocal societies," and "if warfare is continual, men will contribute more than women to subsistence unless the warfare prevents them from doing so." I will discuss these cross-cultural studies, particularly Otterbein's work, in greater detail in Chapter II.

My own preliminary work on primitive warfare includes (1) a small study (35 societies) whose central variable is the participation of women in warfare, testing 7 hypotheses based upon variables of military expectations, frequency of war, level of political complexity, dominant subsistence activity, dominant division of labor, residence, and descent. (2) I have carried out indirect statistical tests of my sample and Otterbein's 1965 and 1968 samples following the rationale of Chaney and Revilla (1969), and I have found similarly that statistical generalization based on small samples will differ considerably from those based on large ones (in the test case, 412 societies). The crucial issue of sampling and sample size is also discussed in detail in Chapter II.

In this dissertation I have carried out a large-scale inductive investigation of variables that may be associated with warfare among largely preindustrial peoples. Rather than be concerned with the problem of cause, for reasons presented in Chapter I, my contribution is to make some generalizations about the strengths of association between warfare and other sociocultural phenomena, based on a large standard sample of 186 societies evenly distributed over the 6 geographic regions of the world (Murdock and White 1969). The use of such a large stratified sample allows one to explore geographic variations in the patterning of variables.

Therefore, the problem under investigation is twofold: how does one ask the question "Why war?" anthropologically and what kinds of answers does one get using cross-cultural quantification? In working toward solutions to the problem, my purpose is to make warfare intelligible and to account satisfactorily for human action.

CHAPTER I

AN ESSAY ON WAR

"You should learn not to make personal remarks," Alice said with some severity: "it's very rude."

The Hatter opened his eyes very wide on hearing this; but all he said was, "Why is a raven like a writing-desk?"

"Come, we shall have some fun now!" thought Alice. "I'm glad they've begun asking riddles--I believe I can guess that," she added aloud.

"Do you mean that you think you can find out the answer to it?" said the March Hare.

"Exactly so," said Alice.

--Alice in Wonderland

The riddle is war. What is it? Who does it? When do they do it? How do they do it? But the most important question, Why do they do it?, will have to go wobbling around on weak knees for reasons I hope to make clear in this chapter. It means that I shall offer no macro-generalization, no law-like statements, no cause-and-effect statements, no functional statements, and no predictive models. Well, why not? Aren't I supposed to be scientific? What kind of contribution to knowledge am I making, anyhow? How can my work be useful towards eliminating warfare as the scourge of mankind? Indeed, others have tackled the why question, and their arguments will be presented below and analyzed for what appear to be their strengths and weaknesses. But underlying all attempts to tackle, analyze, and argue is, in my estimation,

the acceptance or rejection of determinism in social science. It is best to begin at the beginning.

Is This Really Science?

To be scientific means trying to figure out the world around us or, more weightily, explaining the empirical rationally. Later on I shall discuss a useful criterion to distinguish between science and pseudo-science, but what I set forth here is the basic philosophical issue of this essay. The empirical refers to experience in the world--few would dispute this--but rationality is obviously less clear. We all claim to use it, yet we come up with competing and conflicting explanations of what appears to be the same experience. Irrationality is ignorance, insanity... or religion and art. We do not expect the artist to be rational, at least not in the same way as the philosopher or scientist, and he is the antithesis of the scientist. In our Western tradition of science and philosophy, we believe that the exercise of reason upon the world will reveal order, necessity, cause-and-effect, and predictability. Anything less, we think, leaves us with randomness, accident, unpredictability, and chaos. As arguments, the former is called determinism and the latter, indeterminism. The force of my criticism of various theories of war is directed against determinism in a variety of forms, and towards a third view that I believe is the reasonable way to make human behavior

intelligible, the primary business of anthropology. Those who deal with the concept culture inevitably must confront the issue of individual free will and responsibility, although many may avoid coming to grips with it. Since indeterminism is a negative theory and is parasitic on determinism (Ayers 1968:6), as relativism is parasitic on absolutism, one cannot understand it without knowing what determinism--the most fundamental and intimate issue in human behavior--is all about.

We ordinarily take it for granted that most adults are normal and that all normal adults are responsible for their actions. Yet there are powerful arguments to show i) that responsibility is incompatible with determinism, since if determinism is true, no one can act differently from the way he does; but ii) that responsibility is also incompatible with indeterminism, since if human actions are not caused at all, not even by the agent, then no one is responsible for them; and iii) that there is no third alternative (Baier 1970:100; italics added).

Or, in greater detail, what Ayers calls the Basic Argument for determinism reasons thus.

The common premiss of both determinist and indeterminist is, of course, the alleged incompatibility of causation and freedom of choice. This supposition is usually supported by the argument that, if an event is in all respects causally explicable, then it could not have been any different, since to explain an event completely just is to shew that nothing else was possible in the circumstances. If one thing is the whole cause of another, then given the occurrence of the first thing, the other must occur. This is the meaning of 'cause'. Likewise, to say that something is a law is to say that anything different is impossible. So if everything that happens is governed by a law, nothing that fails to happen is possible, or ever was possible. On the other hand, everyone agrees, or should agree, that for there to be freedom of choice or, therefore, any real choice at all, the agent must be presented with alternatives that are all genuine possibilities. It follows that a free choice

cannot be causally determined, and it can have no complete explanation (Ayers 1968:1-2).

But indeterminism cannot account for free choice, either, and the determinist seems to hold the high card by claiming that the logical consequences of indeterminism lead to absurdity, refuting his opponent thus:

Let us suppose that some human actions are not caused, that they are indetermined and so really could have been otherwise. This is simply to suppose them accidental, random, unpredictable and unrelated to the agent's personality. How can we even ascribe such an event to an agent as an action of his, unless we can relate it to a specific and intelligible motive, and how can we do this if it is a bolt from the blue? ... Why should a man be held responsible for something supposed to be unrelated to all antecedents and perhaps to all that comes afterwards, something that came into his head from nowhere? (Ayers 1968:3-4).

Most of the basic tenets of the concept culture are antithetical to even the possibility of such bolts from the blue: behavior is shared in common with and learned from other human beings. Yet anthropologists still assume that individuals are responsible and do have freedom of choice within the context of a specific culture.

The only conclusion left to draw, it is argued, is... that responsibility and freedom of choice are self-contradictory notions, requiring that an agent both could and could not have acted otherwise, that his action both was and was not causally determined. Determinism... is no longer presented as the lesser of two evils, the more acceptable horn of a dilemma, but as the only possible conclusion of a rigorous argument (Ayers 1968:4).

Indeterminism is discarded as irrational and determinism retained as rational, albeit nonhuman. This variation of inhumanity seems to be more tolerable to philosophers and

scientists than the imputed chaos of indeterminism.¹

Ayers (1968:4) makes a distinction between metaphysical determinism and scientific determinism: the former is "the doctrine that no person is ever responsible for his actions or ever makes a free choice"; the latter is "the view that every event has a scientific explanation," i.e., it is "explicable by reference to its antecedents and laws of nature...." That is all he has to say on the matter of scientific determinism, since his task is confronting the metaphysical. He does not specify which sorts of scientific subjects he is including--physical, social, or both--but if one makes reference to laws of nature, he must have already decided that such a concept is appropriate to the problem at hand, i.e., he has already accepted metaphysical determinism. Since Ayers' larger task is to defend indeterminism yet make it responsible, he is talking about people. Objects are not responsible. Therefore, while not applicable to the physical sciences, his concern with the nature of human action is obviously relevant to the social sciences. Even if it were limited to physical science, his statement obscures the difficulties of "cause" and "laws of nature" in talking about science younger than Newtonian mechanics.

For some anthropologists concerned with theory, the propriety of either scientific or metaphysical determinism is not at all clear. For others where theory, there determinism. For example:

Every ethnographer--including, of course, the humanist-literary-natural history-"slice-of-life" kind--makes some attempt to tell us how the facts hang together, how they interrelate causally, and what makes the particular society in question what it is.

Yet to assert, for example, that a particular event, x, occurred because of certain other events, y and z, presupposes, more broadly, that events of type x are linked to events of type y and z in certain determinate ways (Manners and Kaplan 1968:5).

Not only do anthropologists not specify the determinate theories implicit in their ethnographies, what little anthropological theory that does exist is flawed, in Manners' and Kaplan's assessment, by subjectivity, ignorance, and ideology. They insist that, in order to be scientific, anthropologists must conform to the hypothetico-deductive reasoning of formal logic. That is, facts and empirical generalizations can only be considered to be explained when they are subsumed under theoretical statements, either deductively or probabilistically, and thus their occurrence made predictable. They dismiss as subjective, and therefore immature, speaking of theories as making something "intelligible" or "understandable," and evaluating theories on the basis of which "satisfies" us the most (Manners and Kaplan 1968:7), enamoured as they are with positivist notions about how we should know the world, rather than how we do know the world.

Nor do they consider the differences between social and physical theory to be inherent, but due instead to the inadequacies of the investigators:

It is not our intention ... to suggest that such differences [between the physical and social sciences] in any way constitute a logical or ontological gap between the two fields of inquiry. Paradoxically enough, greater theoretical demands are made on the social sciences than are made on the physical sciences.... Thus, one looks to the social sciences for answers to the many social problems that afflict us, and one wants detailed answers so that something may be done to correct them (Manners and Kaplan 1968:10).

To which one can only reply, the social need for predictive power does not mean it is possible. The only way I can see that social theory can be predictive is to force people to behave according to the theory.

Marvin Harris, as the clearest proponent in anthropology of the new scientism, cultural materialism, and cultural determinism, is devoted to scientific determinism--which he formulates only as similar variables under similar conditions tending to give rise to similar consequences---and he, like Manners and Kaplan, consequently must view human behavior in terms of metaphysical determinism. Harris acknowledges the classic implications of determinism:

If individual behavior is largely a predictable outcome of technoeconomic, technoenvironmental, and other given conditions, what significance does anthropology attribute to the strivings of individuals to change their personalities or to modify their cultures? Are we all automations fated to act out our particular predestined personal and culture configuration? Is our sense of free will merely an illusion? Can we hold ourselves and each other responsible for the choice of personal and cultural lifestyle that we exhibit? (1971:593)

Yet he dismisses them with the claim that "... the determinism governing sociocultural phenomena is a matter of probability rather than of certainty," and of low probability

levels at that, "although our batting average would be quite respectable among meteorologists and geologists...." By extension then, predicting human behavior is in the same class as predicting the weather and earthquakes. He attributes exceptions:

... errors may be made in data collection and processing; the statement of the initial conditions may be inadequate; the conditions may be undergoing evolutionary change; and finally, the generalization may be poorly constructed. All of these sources of error may be reduced to one: lack of sufficient information, or incomplete knowledge (1971:594).

Harris does not pursue further the problem of incomplete knowledge, and he concludes by reconciling free will and determinism to his satisfaction:

While cultural anthropology is predicated upon the general subordination of the individual to the forces of enculturation, every society nonetheless exhibits a wide spectrum of individual personalities. Enculturation is a form of programming, but our knowledge of the content of the program is always quite incomplete. Thus no individual, even the most heavily psychoanalyzed, possesses anything more than a probabilistic knowledge of how he will act under given contingencies.

But at the same time, it is perniciously false to suppose that all sociocultural events are equally probable and that by mere force of will the inspired individual can alter the trajectory of an entire sociocultural system in a direction convenient to any philosophy. Convergent and parallel trajectories far outnumber divergent trajectories in sociocultural evolution. Most people are conformists. History repeats itself in countless acts of individual obedience to cultural rule and pattern, and individual wills seldom prevail in matters requiring radical alterations of deeply conditioned beliefs and practices (1971:595-96).

Probably most anthropologists would agree with the general drift of Harris' first paragraph, although he assumes the unknowable--that cultural programming is complete, although

we can never know its full content. But in the second paragraph he raises the specter of chaos and god-like omnipotence and then counters it by stating that individual free will cannot override the forces of history, i.e., the wills of other people, something that no indeterminist would claim. Furthermore, he assumes that parallelism and convergence of sociocultural trajectories is due to laws of nature, not the spreading of ideas over time and space.

The gist of all this is that Harris is a determinist, but because he is also a humanist, he is a probabilistic determinist who does not offer a level of acceptance or rejection of a generalization. Do 2 or more events have to appear together in a cluster (independent of other clusters, of course) most of the time, some of the time, 2 times out of 3, 50 per cent of the time, 30 per cent, 10 per cent, or 5 per cent before one can claim the discovery of a causal chain with an arrow pointing in one direction? On the one hand, those whom Harris calls historical particularists--Boas and his students primarily--would never claim that culture manifests itself randomly or deny that things go together in patterns that may crop up in disparate parts of the world. On the other hand, the logical positivists would simply reject Harris' use of predictability as unscientific.

Bidney (1967) offers one explication for our feelings of agreement and disagreement with Harris' conclusions above:

Paradoxical as it may appear, cultural determinism and cultural indeterminism are not incompatible in practice, since the concepts refer to complementary factors in the life of man and society. Cultural indeterminism is postulated because culture is not a closed system, but rather an open one, subject to the directive agency of human intelligence. On the other hand, some degree of cultural determinism characterizes human life, and the cultural anthropologists may study comparatively the significant correlations between culture, personality, and society (1967:17-18).

But Bidney's position is merely an inversion of Harris': Harris allows the individual some freedom within the determined processes of culture; Bidney denies that determinism but also denies individual freedom. Finally, he does not go on to explain what would constitute a "significant correlation." Harris (1968:300) abruptly dismisses Bidney's version of cultural indeterminism and labels Bidney's claim that culture is the product of human freedom in creativity and choice as "the reductio ad absurdum of cultural indeterminism." In Harris' metaphysics culture and freedom are contradictory and yet, as we have seen, Harris does admit that at least some of the time human beings are free.

Berreman, in a brief brilliant article (1972:224) sees the situation as a dilemma in terms of science and humanism: "... how to be scientific and at the same time retain the humanistic insights--the human relevance--without which no account of human beings makes sense," a dilemma that anthropologists solve by avoidance, that is, choosing either scientism or humanism. The former he identifies as

... a retreat to, or preoccupation with, such things as quantification, abstract models, simulation, and highly formal methods of data collection and analysis. It rejects intuitive insights ... [and] results most often in descriptions and interpretations which are reliable but whose validity is questionable....

The latter, humanism, he accuses of rejecting

... any serious attempt at scientific method, relying entirely upon intuitive insights and the qualitative, empathetic ethnographic result thereof ... [and] results in accounts which may be valid, but whose reliability is undemonstrated.

To resolve the dilemma, Berreman proposes that anthropologists develop a method that "... combines rigor and insight, verification and discovery, accuracy and empathy, replicability, and human relevance," and he calls for a "sociology of ethnographic knowledge" or an "ethnography of an ethnography," in which the ethnographer specifies the bases of his intuitive and theoretical inferences, his procedures, and his sources (1972:228). He does not, however, indicate how detailed the ethnography of the ethnography must be in order for verification to set in. He accuses ethnosience of proliferating means of explanation into such torturous channels that explanation is either never achieved or is inhuman, a view with which I concur. He likens the difference in points of view between scientist and humanist to the difference between a European navigator of the open seas, who charts a line to his goal "according to certain universal principles" and subsequently bends his efforts to "remaining on course" or following a plan and secondarily to reaching a distant

landfall, and a Trukese navigator, who knows where he is going and gets there by continually taking into account the context of his actions--the prevailing conditions of wind, waves, tides, and currents. The basic principles of the Europeans are easily taught, but one must be an apprentice to learn Trukese ways (Berreman 1972:225), that is, one must learn the contexts.

Yet European and Trukese eventually arrive at other places and they can tell you how they did it, but which one will tell you more about the world in which he lives? Both are science. This is science, too. Well then, what is this phenomenon--War?

The Problem of Definitions

"Define your terms" is the usual admonition at the outset of a scientific investigation. It is assumed that one cannot or at least should not proceed until this is taken care of. Since argument commonly arises over matters of definition--often to a stalemate--we use operational definitions, specifically designed for the case at hand and perhaps containing "objective" measures of some sort. In submitting to this traditional demand, I advocate a definition of war that has 3 necessary conditions: (1) people organized into a political group, although "political" seems redundant; (2) ^{why} an intention or expectation of the group to do harm to the body or belongings of another group, for which (3) ^{why} armed

conflict is the means. Succinctly, war is collective armed assault. The intention, especially if people go to war only for revenge and defense, may be similar to that of a feud. I have included raiding, which may be considered as violent action midway between feuding and warring, as a form of warfare. My justification for this definition is that violent behavior is a continuum and that it is difficult to specify qualitative differences between one form and another. I think that the fundamental character of warfare is violent action taken by one group of people against another, however they define themselves as members of that group. Furthermore, this intention is to "further the interests of one group at the expense of the other through willful destruction of life and goods. War expresses the apotheosis of self-interest" (Hoebel 1958:508). From society to society, war to war, the size of the groups may vary, as may the principles of organization that unite the members, the weapons and tactics of armed confrontation, and the self-interests at issue, which can range from defense in the face of overt attack to outright conquest of land, expropriation of goods, and enslavement of peoples.

So far, this definition seems common sensical, even obvious. Should it be more specific? Should I stipulate that a fight can only be called a war if there is a minimum of n people involved; or that the result is a minimum of n casualties (lethal or non-lethal); or specify the type,

value, or quantity of what is gained; or require that certain principles of organization be present; or that a certain distance be maintained between combatants using certain weapons; or that hostilities be concluded according to certain principles--do we recognize a war by how it turns out? Several anthropologists have tried to establish criteria to identify "true" war. Turney-High (1949:21-22) lists 4 conditions of war: a group motive rather than merely an individual one; leadership providing command and direction; tactical operations designed to bring warriors to advantageous positions; and the ability to sustain an assault until the goal of the war is attained. Bohannon (1963:306) identifies war by its ends:

"True warfare," if we may call it such, ... has as its ends peaceful settlement with new political conditions, not continuation of fighting. ... when we find it, we are going to find societies in which it is carried out by specialist bodies called "armies" and others in which it is carried out by whole bodies of citizens.

Newcomb (1950), in a typology of warfare, says that true war exists only where people are food-producers and engage in

... a type of armed conflict that takes place between societies, meeting in competition for anything that is valued by the groups involved, usually consisting of territory or certain products of this territory, such as good hunting grounds, oil-producing or agricultural lands (p. 317).

According to his typology based on technology (Newcomb 1960), food-collectors (gatherers, hunters, parttime horticulturalists) do not engage in "true" warfare. The logical implications of this line of reasoning mean that food-collectors,

e.g., most of the Indians of North America, carried out non-war, or "false" war, or that when the Hopi fought the Navaho, the Hopi were at war but the Navaho were not, or that the U. S. Army was carrying out true war against the Dakota, Cheyenne, and Apache, but the Indians were not. This application is not meant to be facetious, but to illustrate the limitations of what appear to be carefully stipulated definitions when they are held up to the real world.

By his definition true war excludes food-collectors and begins with the agricultural revolution. Yet Newcomb also includes hunting grounds as something of value over which people fight. Therefore we should be able to include, for instance, horse nomads of the Great Plains. But Newcomb has previously put such hunters into his Type 2 or "primitive" warfare category, below the category of true warfare carried out by food-producers. The Patwin, Wintun, and Miwok of the Central Valley of California fought and killed groups of intruders in their gathering grounds. They and other California tribes carried out extensive revenge raids and even pitched battles. But people at this level of technology and subsistence would belong in Newcomb's first and lowest category, those peoples identified as not having warfare. What then do we do with such violence?

These examples should show some of the limitations of this particular typology. We know that there exist differences of some sort in the warfare practices of these

societies, and it may be the case that economic criteria are the most fruitful for designing a typology. As McEwen (1963: 160) says, one is forced to make a choice as to what element is to be the most important and which ones must remain secondary. At the very least, a typology would give us linguistic means to talk about variations in warfare in a more abbreviated fashion. Yet I do consider talk about "true" warfare, like "true" religion, to be ludicrous. Furthermore, it is often hard to push cases into the given slots. I do not find that the virtues of Newcomb's typology outweigh its limitations and suggest that we look elsewhere.

Not only must we worry through definitions about whether or not a state of war exists in order to begin counting cases, but also whether or not the study of war in pre-industrial societies is relevant to understanding (1) war carried out by nation-states, regardless of the time period, and (2) war carried out by nation-states in the modern age. These two questions have been asked simultaneously (see Gorer below). The implications of the first question are that wars waged by Egypt of the Old Kingdom, Rome of the Caesars, England of Henry II, Zululand of Chaka, the American Civil War, and the recent engagement of the United States in Indochina all belong in the same category, and that this category is qualitatively different from one in which we might put the Aztecs (but not the Inca), the Comanche, the Rwala Bedouin, and the Dagum Dani. For many critics, the wars of the latter

societies are hard to identify and different from our wars.

For most of humanity, the tribe is the unit within which killing is considered murder, and outside which killing may be proof of manhood and bravery, a pleasure and a duty. Such killing may be done by individuals--head-hunters, scalp-collectors, as part of a vendetta or raid--or by groups; in the latter case the killing is called "warfare." The differences in quality and scope between tribal warfare and modern war between nation-states are so great that it might be useful if different words were used for the two activities (Gorer 1966:31).

Added to the difficulty of identifying primitive peoples in general and in particular is the difficulty of identifying their wars. Primitive peoples only rarely conduct formal hostilities with the object of achieving a tangible economic or political result. Their hostilities are seldom conducted by a highly organized professional military class using distinctive instruments and techniques regulated by an intergroup law applicable only during the period of "war" and designed to render war an efficient instrument of policy. These elements which go to make up the concept of war today are products of civilization, and only their rudiments can be found among primitive peoples (Wright 1965:58, *italics added*).

As for Wright's criterion of formal hostilities, I must ask what would an informal war be like? What kinds of differences are we dealing with if one distinguishes primitive conflict as ritualized and civilized conflict as formalized? Two of the forms that are important to people involved in contemporary wars are declarations of war and treaties of peace. We are all familiar with the semantic howlers that Korea and Viet Nam were not wars because Congress did not declare them to be so. (Cf. Donald Wells, The War Myth.)

I must also ask, when does "modern" begin? Historians identify "modern history" as the most recent phase of a period that began with the Renaissance and Reformation and

ended with the onset of "contemporary history," which "begins when the problems which are actual in the world today first take visible shape" (Barraclough 1967:20). Barraclough himself argues cogently that the years before and after 1890 mark the division between modern and contemporary history, although the latter term may be a contradiction, and we may use "era" instead (1967:20-21).

Furthermore, what do we do about Medieval wars? The Crusades of the 11th century, the Holy Wars of Christendom, are the direct ancestor of total war, a unique practice of the Western world.

After 1053, the idea of Holy War made rapid progress. Precedents there may have been, but in its formulation and execution it was something entirely new. Hitherto, in common with the other great religions, Christianity had condemned war as essentially evil, and the Eastern Church continued to maintain its reservations. War might be unavoidable; it could not be good, still less could it be holy. How, then, are we to explain the ease with which the West cast aside so hallowed a tradition? (Barraclough 1970:13)

Barraclough goes on to suggest that contact between Christian and Norse pagan transformed each: the Norse warriors accepted Christ, but the Christians accepted Thor and Odin. Gentle Jesus became a warrior chief dedicated to conquering, not converting, the rest of the world (1970:13-14).

After 1053 Holy War became the battle cry of the Papacy, an instrument for extending its power and authority ... [and] it had nothing to do, in inception, with the struggle against Islam....

We no longer regard the Crusades ... as a great movement in defense of Western Christendom, but rather as the manifestation of a new, driving, aggressive spirit which now

became the mark of Western civilization. We no longer regard the Latin states of Asia Minor as outposts of civilization in a world of unbelievers, but rather as radically unstable centers of colonial exploitation.... There is perhaps a line running from Bohemund of Antioch to Cecil Rhodes (Barracclough 1970:14,16).

Benedict (1959) uses a simpler taxonomy than that of Newcomb to distinguish primitive from modern war, 20th century war in this instance. She follows the genera-species model of natural history. In her view, the characteristic of the genus War "is homicide that is rewarded with unquestioned acclaim and gratitude by one's fellows" (1959:370-71), as contrasted to the genus Murder, which is homicide with the heaviest penalties. At the species level, primitive warfare is "socially nonlethal," that is, combatants exist in self-sufficient societies and their wars "do not drag to ruin the civilization of both tribes that engage in them" (1959:374). Modern war, however, is socially lethal to all parties, since it shreds the fabric of interdependence that exists among modern nations and thus is suicidal--

You cannot be an international civilization and reap its benefits, and at the same time engage as a national to destroy other nationals root and branch. War in such a society becomes a case of cutting off your nose to spite your face (1959:378-79)

--and drags your civilization down in ruins.

Nonetheless, the views of Gorer and Wright seem representative of anthropological thinking, a view set probably by Malinowski (1941), who defined wars in authentically primitive societies as highly ritualized, self-limiting, and

qualitatively distinct from wars in our time. As other instances, Vayda (1960:1-2) and Chagnon (1968a) identify primitive warfare through the following criteria:

... smallness of scale in military operations, short duration of active hostilities, poor development of command and discipline, great reliance on stealth and surprise, and the great significance of village community or local group in organizing and conducting war parties (Chagnon 1968a:110).

Another often used critical point of difference is distance between combatants, i.e., primitive war is personal because individuals at least see each other and may even have physical contact; modern war is impersonal, because combatants drop bombs on mere topography and shoot artillery from ranges of several miles. Diamond (1968) makes it the difference between personal and impersonal evil. Yet what do we do about labeling the war in Viet Nam, where the acts against the enemy that have raised the greatest outcry at home have been acts of personal evil? It is revealing that we judge such acts as "murder" and therefore outside the category of war, which leaves intact the myth of impersonal combat. We need not go to the more dramatic forms of personal contact. Infantrymen do have at least visual contact with others labeled as "enemy" and they do legitimately kill. Is this not personal?

There are really two different types of human extermination involved in Vietnam, and they perhaps require two different kinds of explanation. First, there is extermination such as the Huey troops engage in--extermination at close range, in which the killer can see (and enjoy, apparently) the blood he sheds. Second, and far more

common, there is extermination at a distance, in which the extent of the killing is so vast that the killer tends to think in terms of areas on a map rather than individuals. In neither case is the victim perceived as a person (such a perception would make modern war impossible), but in the first case the killer at least sees the immediate consequences of his act, whereas in the second case he does not. The "close-range" killers in Vietnam are confronting something.... (Slater 1970:41).

Applying Slater, is Vietnam schizophrenic then--two wars in one? Or should we deal in percentages, i.e., impersonal violence is more common (over 51% of the time), and file this war in the modern category?

I am attempting to show through simple examples that the problems of "when is a war a war and when is it not a war?" and "when is a war primitive and when is it modern?" are not to be solved through setting forth definitions. I contend that Anthony Leeds' protest in the 1967 AAA Symposium--that "... we have had no consideration of what the phenomenon war is and what it is not. What sort of institutional complex is it generically?"--is a question that we really do not have to answer before getting on with the work. Actually we already have an answer. When an ethnographer describes warfare in a non-Western society, he has necessarily recognized and understood an event as being war because it ties into his previous experience (cf. Winch 1958). But a rejoinder might be that understanding is relative and all definitions are equally valid ... and ultimately that cross-cultural comparisons are impossible. What one

ethnographer recognizes as war, another does not. Do we dwindle into cultural solipsism?

But how do we recognize war in the first place? We do it not on the basis of the presence or absence of 1 or 2 variables, but on how those variables look against a background or in a context of human activity and ideas, sometimes difficult to isolate. We know what "war" is--we use the word competently every day and other people listening to us know what we mean; they must, because we are still talking. ^{Huh!} Furthermore, we recognize the metaphorical use, e.g., the war on poverty, the war on crime, the war on inflation, immediately because of the context of a decade of official concern and strategems for collective assault against social enemies. But what about the phrase, the war on health? It does not make sense, at first, until we examine the context--in an article in the New York Review of Books--and find that it refers to and condemns efforts of medical entrepreneurs to defeat measures which would ostensibly give better health care to the population, regardless of financial status.

Competent usage does not require identical understanding of what constitutes war or warlike behavior across cultures. Recall in history the chronic complaint that the enemy does not fight fair, or as gentlemen, or according to the Geneva Convention. Indeed, identical understanding is simply not possible: we cannot take a list of definitional criteria, and hold them up against the world as though they

were color chips looking for a match-up. What would we see if we ran 3 films--Dead Birds, newsreels from Vietnam and a professional football game--first without sound track, and a second time with? What would we see as the same and as different? What would happen if we changed 1 variable, and substituted for our Western observer a Tasaday tribesman from the Philippines? Since the Tasaday are not acquainted with war, is he an "unbiased" observer, objective, purely empirical, unpolluted? Can he identify the facts of the matter and tell us how the people in the 3 films are behaving the same or differently, without actually knowing what they are doing? Such an experiment is within the range of possibility, and I contend that our unbiased Tasaday would not be able to isolate significant differences without absorbing our culture, which of course eliminates unbiased reporting. I discuss further definitional problems of war from the political point of view in Section 4.

Why Do People Do It?

People have been providing causal explanations for war throughout history, with a variety of suggestions. A profile of the causes looks like this: people go to war because of contiguity, habituation, social learning, predation, psychological defenses (rationalization, blaming, denial, counterphobic tendencies), the "host of fears associated with the human condition," territoriality, power, frustration,

biologically rooted aggression, instinct, and sadism (Gilula and Daniels 1969:403). In what follows, I have tried to sort out these "causes" into larger types of explanation, even though in many instances the categories are not mutually exclusive. In the intricate lattice which one inevitably encounters in talking about war, one can get hopelessly bogged down without some calmly applied heuristics, even at the risk of doing some violence to the material. The following discussion of human aggression and war will deal with (1) the biological-instinctual theory; (2) psychological theories--frustration-aggression, social learning, and adaptation-coping; (3) ecological and economic theories; and (4) political theory. The reader will note that the discussion is arranged in a hierarchy from individual human biology to the political behavior of human groups. The discussions are critical, although not exhaustively so. My task is to elucidate the strengths and weaknesses of current theory and to indicate the direction of a possible integrative or alternative point of view.

1. Are wars natural?

"Let the jury consider their verdict," the King said, for about the twentieth time that day.

"No, no!" said the Queen. "Sentence first--verdict afterwards."

"Stuff and nonsense!" said Alice loudly. "The idea of having the sentence first!"

"Hold your tongue!" said the Queen, turning purple.

"I won't!" said Alice.

"Off with her head!" the Queen shouted at the top of her voice.

--Alice in Wonderland

To better understand why things happen the way they do, we are often urged first to become familiar with the nature of the world and the events around us, in the best manner of scientific method using reason and observation. In the interest of changing our world, presumably for the better, it may be argued that if we find that it is not in the nature of someone or something to behave in a desired way, then all our best efforts will be for nothing. We commonly hear as a final reason, justification, and authoritative statement the declaration that something is "contrary to one's nature." "You can't fight nature." "That's human nature for you." When inquiring about the nature of a thing or event, we are asking for its essential characteristics and distinguishing qualities, its essence, its inherent character. As a result of our inquiry, one of the essential characteristics of the subject under investigation could turn out to be that it is, or is not, natural, i.e., inborn, determined, normal to the species. We can ask, what is the nature of love, of steel, of organic gardening, of war. As part of our inquiry here, we are also asking, is war natural? We can look for regularities in the variations of the three necessary conditions posited for war--a group with a common

intention and arms--and say something about the intricate web of relationships among technology and economics, politics, beliefs and values, and war. Or, possibly, we can even say something about the evolution of war in human history. But why the regularities in the first place? Are they due to history and the spread of ideas from society to society? Are they functions of society itself? Or are they due to biological evolution? Sooner or later in our inquiry into the nature of war, we must tackle the question, are wars natural? Is violent group conflict innate, inborn, and normal to the species Homo sapiens?

Basically, there are 2 schools of thought on this matter, with battle lines drawn, cannons at the ready. One school argues for the primacy of biology, particularly of instinct, in explaining human aggression, i.e., man is by instinct an aggressive creature and this accounts for individual and group aggression and violence. Its major speakers are Nicolas Tinbergen (1968), Konrad Lorenz (1966), Robert Ardrey (1961; 1966; 1970), Desmond Morris (1967; 1969); Lionel Tiger (1969), and Derek Freeman (1964). The other school, mobilized in print by Ashley Montagu (1968) with such allies as Geoffrey Gorer, J. P. Scott, E. Leach, Ralph Holloway, and J. H. Crook, argues for the primacy of culture or learning in explaining the rise and expression of destructive aggression. Montagu himself continues to consider it is intellectual and moral responsibility to counter

the influence of the biological determinists, in print and in person.

Let me set the arguments out in some detail. I am devoting special attention to Ardrey and Lorenz because the others have spun off from them, and more importantly, because they are being referred to as expert witnesses by historians and others who understandably cannot themselves go through and analyze ethological data and thus require some summary statements provided by specialists. Konrad Lorenz has been called the founder of the modern science of ethology. Most of his research has been done with tamed ducks and greylag geese. In his book On Aggression, he tries to make a case of instinctual aggression, stating that it is spontaneous, cumulative, and can be directed towards good or bad ends. He continually sees in human behavior analogies to pair-bonding, dominance, and fighting behavior of geese--and a particular similarity between the greylag triumph ceremony and human "militant enthusiasm" or call to battle, identifying both as instinctual. Lorenz does not consider militant enthusiasm an evil, but a response springing from the same source as love and laughter and effort expended to protect the group. But one of the greatest human problems is that we have not developed instinctual ritualizations of intra-specific aggression. His book has been widely read, and his reviewers praise his warmth and love of animals, if not his anthropology. The greatest difficulty with Lorenz' work lies in

anthropomorphism: he interprets goose behavior in human terms and then offers goose behavior as evidence that human beings behave like geese and consequently are subject to genetic programming.

Robert Ardrey is a slightly different case. A playwright who became interested in the evolution of human behavior several years ago, he has produced 3 best-selling "personal investigations": African Genesis (1961), The Territorial Imperative (1966), and The Social Contract (1970). My particular concern here is with The Territorial Imperative, a collection of hypotheses which one finds difficult to tie together that has received an enormous press. Ardrey's position is that we can have a better understanding of modern man and develop more rational solutions to problems if we study man's instinctual endowment (Gorer 1968:76), which we can learn about by studying the fossil remains of pre- and early hominids and the behavior of other vertebrates--from fish to birds to ungulates to other primates. His attitude is that the news may be gloomy, but we must face up to the fact that human beings are subject primarily to biological laws. Ardrey goes beyond Lorenz and attributes human aggression to the "instinct for territoriality," i.e., that each human (male) has an instinctive drive to acquire, maintain, and defend territory; that man builds a fence around his home and feels patriotism for his country because of the same motivations as those of his dog barking behind that fence;

that fighting over the possession of territory is a powerful, inevitable, ineradicable, and therefore uncontrollable instinct. He flatly states, furthermore, that he sees no difference in degree between the psychological attachment of a lungfish to a stretch of muddy water and the psychological attachment of the resident of San Francisco to the city he loves (1966:337). Ardrey cannot find evidence among other primates, especially the great apes, to support his thesis, but he accounts for this gap by declaring man's nearest phylogenetic relatives to be "evolutionary failures."

Ardrey further postulates 3 basic needs in H. sapiens, listed in order of importance. First, the need for identity and assertion of individuality; second, the need for stimulation and relief from boredom; third, the need for security. He states that 2 "institutions," warfare and territoriality, satisfy these needs better than any others. Territoriality provides one with an area of land (or air or water) with which to identify; boundaries for which one competes and which one explores and defends relieve boredom; one's own place gives security. Security is the least important need because in the long run security is anonymous and boring and will be sacrificed to experience identity and stimulation.

War satisfies the need for identity through the possibility of glory and, primarily, position in rank. In contrast, civilian life provides only unbearable anonymity. War is stimulating, the "ultimate release from the boredom of

normal existence" (1966:336), and people like war because pain is more stimulating than pleasure. Finally, war increases security through the acquisition of land and loot, and the possibility of losing a war is more anxiety-provoking than war itself. As modern war has gained in size and sophistication, identification and stimulation have increased concomitantly. The Pax atomica is frustrating because we no longer can have the warfare we want. Therefore, we must have substitute satisfactions for these needs, such as athletic contests.

War has another function in Ardrey's view, which is really a restatement of Thomas Hobbes' social contract and William Graham Sumner's hypothesis of social cohesion (1911). Within a social group, Ardrey sees a condition of mutual hostility (enmity) to be the normal one, a condition reduced only as external hazards and threats to the group rise, forcing individuals to cooperate (amity). Once external hazards are reduced or eliminated, the natural condition of enmity will reassert itself. He even sees hostility between mother and child to be inevitable once dependency is outgrown.

But we still are not clear about human aggression against fellow members of the species. If everyone stayed home, assuming of course that males are in fact territorial, there would be no conflict. But humans do not stay home; they intrude beyond their borders into one another's

territory. Why? Ardrey traces the origins of murder, of man the killer, to man's phylogenetic history as a predator-- hunting other animals for food. Indeed, he makes man the predator and man the killer equivalent terms.

So ... the members of this school see aggression (and war) against other humans as natural--inborn, innate, instinctual.

Montagu and the other members of the opposing school in Man and Aggression (Montagu 1968), mostly professional biological and social scientists, assert the primacy of learned behavior in human beings, not just in recent times, but for the last 2-4 million years. They argue that during human evolution, learning has so suppressed any instinctual endowment that man has lost virtually all instincts. The only remnants are possibly reactions to sudden loud noises and to sudden withdrawal of support (Montagu 1968). Ardrey is accused of reviving several 19th century social doctrines, including Adam Smith's Instinct of Property, Herbert Spencer's Social Darwinism, Freud's Death Instinct, the Victorian thesis of the Innate Depravity of Man and the Myth of the Wild Beast, the American frontier's Rugged Individualism (including Instinct for Status), and the earlier view of Thomas Hobbes that the state of nature among men is a state of war, which people manage to suppress in the individual by forming political groups (Montagu 1965, 1968). Ardrey's opponents state that we have not inherited aggression and

territoriality from our remote biological ancestors. Instead, each generation learns them from the previous generation (Boulding 1968:88).

Among Ardrey's multitude of errors, Crook (1968) points out that Ardrey uses his basic concept, territory, without specifying its multiple meanings used by ethologists. Birds, for instance, have 4 types of territory: the nest or breeding area, which is nearly universal; the area around the nest which may be defended against intrusion from male conspecifics; an exclusive hunting area that predators often have and which social mammals share; a copulating ground or arena where only those males that have acquired it are selected for mates by the females--this is a rare form, e.g., the Uganda Kob (an ungulate) and New Guinea Bowerbirds. Even where these specific territories may be occupied and defended, such action takes place primarily during mating season; for the rest of the year the animals gather in larger groups (Carrighar 1968). An objection to this could be made that since human beings possess year-round sexuality, their "mating season" lasts from puberty to climacteric. While one could use the terms "proprietor" and "property" in such context, as Ardrey continually does, it is confusing because not even in a metaphorical sense are these breeding or hunting grounds like private property among human beings, i.e., transferable, movable, heritable.

As for man the predator creating man the killer, in contemporary hunting societies throughout the world for which we have information, people hunt for food. They are what I like to call "prayerful predators." They attribute thoughts, emotions, and beliefs to other animals; they thank the animals for allowing themselves to be killed; they treat game with respect and ritual; they do not take pleasure in the act of killing, nor do they hunt for fun. The last characteristics seem to be those of amateur hunters, men in food-producing societies who consider hunting not a necessity but a recreation. Furthermore, hunting peoples know the difference between killing other animals and killing other human beings, even though they may not accord the enemy quite the human status they give themselves: "It's wrong to kill a human being but all right to kill an enemy."

How people make this distinction between us and them, human being and enemy, is a tantalizing problem which I shall introduce at this point and return to in social and political explanations discussed below. Psychiatrist Erik Erikson has coined the phrase "pseudo-speciation" to explain this distinction: members of the in-group identify non-members as animals and therefore as game to hunt down and kill (Tiger 1969:213).

But this explanation is not adequate; in fact, I think it in error. When men go to war, they know full well that the creatures they may kill are not bison, deer,

antelope, or bear. If you ask the bigot why he hates and he replies, "because those people live like animals," he knows full well they do not live like the animals in the San Diego Zoo, or in Serengeti National Park, or the hogs in Farmer Jones' sty, or my dog Jason. The Cheyenne called themselves the Human Beings, but surely a Cheyenne recognized a Crow or a U. S. soldier as humans too, although not as human as a Cheyenne. The American soldier may call the Vietnamese a "gook" or "dink" and hate him with fury, even declare that he is not human and therefore deserves to die, yet, upon questioning, admit that in fact he is a human being. While it is not at all clear how people perceive fellow humans, it seems safe to say that they do not confuse them with animals hunted for food.

Predation is a neutral term, describing food-getting activity in terms of ecology and biology. "Killer," however, is a moral term, and Ardrey and others who use the two synonymously create dramatic confusion. We speak of lions and eagles as predators and hunters, describing how they keep themselves alive. But any time we use the word "killer," we are making a moral judgment about the right and wrong of a particular act of killing. It is analogous to the use of the words "murder" and "stealing." For instance, it is incorrect to say, as many cultural relativists do, that in some societies it is all right to commit murder, or that in some societies it is all right to steal. Murder and stealing are

everywhere and always wrong, although the particular context in which the act may occur is variable from one culture to another (Cook n.d.). If we confuse terms and call the Cheyenne hunting bison a "killer," we are implicitly stating that what he is doing is wrong. If one is a Buddhist or Hindu, this is what one means. But if we are not Buddhists or Hindus, and we are concerned with describing the probable evolution of the Australopithecines as savannah-dwelling carnivores, we do not use moral terms. In fact, use of moral terms immediately reveals our moral position on a particular issue. For example, in the quotation cited earlier, Slater uses the word "killer" repeatedly:

In neither case [close-range vs. killing from a distance] is the victim perceived as a person ... but in the first case the killer at least sees the immediate consequences of his act.... The "close-range" killers in Vietnam are confronting something.... (1970:41),

to emphasize the immorality of the entire business.

The issue of the confusion of terms is a crucial one, especially in dealing with such an emotion-charged subject as warfare. It is a confusion of what philosopher Harry Nielsen calls "Socratic truths" and "scientific truths." A scientific truth

... belongs to a type that one man or a few can discover and pass on to others. The truth goes forth "to whom it may concern" from discoverers who have no way of knowing the identity of those, if any, who will be concerned. As a bit of objective truth, it carries no stamp of its finder's identity, since it is not about him as an individual, although out of generosity the world may affix his name to it. It says nothing personal to or about the individual who comes upon it afterward, since it is not

about him either, even when it is a truth about his species. Finally, its primary importance is to stand available, whether many or few or none ever look it up (Nielsen 1967:52).

Thus, it is a scientific truth that the ancestors of Homo sapiens evolved as omnivores, depending upon both vegetable and animal foods obtained through gathering and hunting. It is also a scientific truth that this subsistence pattern changed only 11,000 years ago, with the invention of plant domestication. Strictly speaking, Homo sapiens was and is a predator at least part of the time.

But the methods of natural science do not work in the realm of Socratic truths, which are of two types (Nielsen 1967:52-53). The first is beliefs and values that we hold personally and express in our attitudes and behavior toward ourselves and others. It is a truth of social science, for instance, that American blacks have not held the same civil rights as American whites. It is a truth about myself that I do or do not care about the situation, and that truth holds regardless of anyone else's opinion (like scientific truths), but someone else cannot discover this truth and pass it on to me, and since it is a truth about me, it is meaningless separated from me. The second type of Socratic truth is a matter of faith--beliefs about ultimate origins and values, and the future that come from authorities outside the individual, e.g., Jesus or Buddha ... or Robert Ardrey.

The mischief occurs when someone tries to transform a Socratic truth into a scientific one, or a scientific truth into a Socratic one. For example, it appears that Professor William Shockley of Stanford University believes that American blacks should not have the same civil and social rights and benefits as American whites because they are biologically incapable of equivalent intellectual achievement. He tries to transform a moral truth about himself into a scientific one and thus put it beyond the realm of moral challenge. Ardrey performs a similar piece of legerdemain: he tries to remove acts of human violence against other human beings from the moral realm into the scientific one, and he accuses anyone who challenges his claim of being a poor scientist or a liar involved in a conspiracy against Truth! Transformation also can move in the other direction. Since modern astronomy has discovered that our galaxy is only one of millions and our sun is only one of trillions in a universe without apparent end, some might claim that an individual human life therefore is insignificant and any belief to the contrary is pathetic (Nielsen 1967:55). A classic confusion of scientific and Socratic truths is the perennial confrontation of Darwin's theory of evolution by some bearing the Book of Genesis. Creationists are unable to see that there is no contradiction, that evolution is scientific, and Creation is Socratic as a question of faith and not a "theory" that the Almighty offers to the public. Possibly some Creationists

are caught up in the mystique of science in Western culture and cannot be content with faith alone but must find "proof" to buttress their faith. I have encountered religious students who refuse to talk about faith altogether; they argue that their explanation of the world is factual.

As a scientific truth, killing fellow human beings may be due to the fact that, unlike other animals, we have not evolved inhibitions to avoid violent conflict (Gorer 1968). In a fight between two wolves, if one wolf bellies up and bares his throat, the other one is instinctually inhibited from striking a death blow and the fight is over. It is assumed that, if wolves did otherwise, they would soon be extinct. Humans have a variety of submissive and surrender gestures, but unlike the wolf they may not inhibit further aggression. Why should this be so? A possible, and untestable, explanation is that the human capacity to create and manipulate symbols through language has kept the absence of such instincts from being lethal to the species. That is, talking to avoid or conclude fighting has been successful often enough to keep ritualization or extinction from occurring through natural selection. Selection has given a slight edge to talking, yet not enough to eliminate lethal fights. There is a problem with mutually unintelligible languages, yet how often do total strangers, without any previous knowledge of each other, go to war? This argument is plausible, especially when based on the premiss that language is the

human quality and that it has probably developed, as Holloway contends (1969), in direct association with tool-making.²

While Ardrey contends that mutual hostility is the normal human condition, Montagu argues that cooperation and love are the normal condition, that violence and destruction are due to learned responses, frustration, and faulty values in a competitive, overcrowded, and dangerous world. War is of recent invention in the whole of human history: 12,000 years ago in the Neolithic, people became farmers, settled in large dense populations, and began to find things to be more important to them than other people (Montagu: pers. comm.). Montagu, Boulding, and the others think, furthermore, that Lorenz' and Ardrey's views are dangerous. They justify the status quo:

What we are unwilling to acknowledge as essentially of our own making, the consequence of our own disordering in the man-made environment, we saddle upon Nature, upon "phylogenetically programmed" or "innate" factors. It is very comforting, and if, somehow, one can connect it all with findings on greylag goslings, studied for their "releaser mechanisms," and relate the findings on fish, birds, and other animals to man, it makes everything all the easier to understand and to accept (Montagu 1968:16);

Nothing could more effectively prolong man's fighting behavior than a belief that aggression is in our genes. An unwelcome cultural inheritance can be eradicated fairly quickly and easily, but the incentive to do it is lacking while people believe that aggression is innate and instinctive with us... (Carrighar 1968:50);

... Idealism of some sort is necessary for justification and legitimation, for no line of policy can be pursued for very long without self-justification. A line of argument like that of Ardrey's, therefore, seems to legitimate our present morality, in regarding the threat system as dominant at all costs, by reference to our

biological ancestors. If the names of both antiquity and of science can be drawn upon to legitimate our behavior, the moral uneasiness about ... Vietnam may be assuaged (Boulding 1968:89).

Ardrey and Lorenz want to strip culture from man and say, This is what natural man is like. Montagu and the others point out that this is impossible: culture is the essential characteristic of the nature of man. But we cannot know what natural man would be like, first because culture has been around for so long and is the crucial factor in human biological evolution; and second, because human infants cannot survive without other human beings to take care of them--in ways they learned from their parents. The influence of culture begins at the moment of birth, and it can be argued that it begins even before, since the unborn child perceives light and dark, sound, and motion. War is not natural, but a cultural invention that we expect to resolve our conflicts. It is an idea that spreads from society to society and has an apparently irresistible quality: those people that do not accept the idea must flee or probably be eliminated culturally and even physically although the effectiveness of non-violence has rarely been tested. The usual alternative is to borrow the idea, i.e., fight back.

The dispute over nurture versus nature continues to rage and probably will never be put to rest. On the contrary, more books and articles appear every month using the ethological or biological paradigm. The following

quotations represent what the nature movement started out to be, and the illogical extreme that it has become.

When ethology first began to make an impact in England and America it was like a breath of fresh air blowing through the laboratories where rats turned treadmills, threaded mazes, and pressed levers, and psychologists constructed theories of human behaviour therefrom. We (we non-biologists, that is) learnt from the ethologists that it was unscientific to generalize even from one breed of rat to another, let alone from rats to human beings; and we began to get an inkling of what the perceptual and sensory world of other species than our own might be like, and how anthropomorphic it could be to talk of animals "thinking" this or "deciding" to do that (Anon. 1972).

From such measured science, the mad rush to the side of nature is epitomized in the following, a letter by a geographer to the editor of Natural History on Marvin Harris' (1972) explanation for warfare, discussed later. While it may confuse the reader a bit, I include the letter here as the most appropriate spot.

Although I find Marvin Harris's ideas on the origin and function of warfare ("Warfare Old and New," March, 1972) fascinating, I also find them highly speculative. Harris's conclusion--that warfare is not associated with instinct--is unfounded. Certainly, he fails to "prove" his conclusion. Since Harris objects to the view that we go to war because of our aggressive animal instincts, I recommend he delete the word animal so that it reads "because of our aggressive instincts." He may also wish to delete the term aggressive because humans also become involved in warfare for protective reasons.

However, the "urge" to protect ourselves, our possessions, interests, territories, and beliefs is something we are born with; it is purely instinctive in origin. Because it is, we place so much value on protection (or security) that we become aggressive about it and employ just about every cultural means--including bombing raids halfway around the globe--in an effort to guarantee it. Not only warfare, but probably everything we do--biting our nails, mowing the lawn, or holding up a bank--is at

least to some degree determined by, or based on, our instincts. Careful observation of children's behavior, for example, suggests that each and every activity (normal) people engage in serves the purpose of satisfying one or more of our basic needs: the needs for identity, security, and stimulation (to use Robert Ardrey's terminology).

Because our instincts are an integral part of us ..., we cannot detach ourselves, our thinking, or our behavior from them. No matter how often we are told to "love thy neighbor," the overwhelming majority of humans will remain ego-centric, selfish, and narrow-minded, and groups will remain ethno-centric. Sermons and philosophies cannot change human nature. For this reason, I contend that if it seems that some of our actions are not associated with our instincts, the likely reason is that we fail to see the obscure connections that (must) exist between our hidden, innate tendencies and our behavior (Reitsma 1972:4-6).

While the letter speaks for itself, permit me to comment on the obvious: Professor Reitsma demands "proof" from Harris, yet any "proof" for his own position is strictly a priori, that of the Compleat Determinist, and not scientific at all.

2. It is in our minds.

Frustration leads to aggression.--This theory states that when one's hopes, expectations, and needs are not met or are thwarted, one becomes angry and tries to remove obstacles through the use of violence; therefore, one is also trying to adapt or to cope with the problem at hand. Factors that may influence the aggressive response are: the nature of the frustration itself, previous experience, maturity, and feelings of the one being frustrated, and available alternatives to aggressive behavior (Gilula and Daniels 1969:398). In

order to reduce violence, frustrations must be removed, e.g., violations of civil rights, economic deprivation, social stress, and constructive expressions of aggression learned [hitting a Bozo doll, competing in sports, building buildings and dams] (Gilula and Daniels 1969:398).

There is another version of this theory which holds that the socialization process by its very nature is frustrating, inevitably producing internal conflict and aggression (Holloway 1968). Since aggression is inevitable, it must be directed into useful enterprises: sports, public works, exploration, and hostility channeled into literature and the arts. William James called such activities "moral equivalents to war."

Both renditions of the frustration-aggression theory suffer from an ailment common to most psychological theories: unfalsifiability. Every instance of violence is explained as the result of some frustration, delineated through the open-ended attributes given above. This is not to say that we will not become angry, enraged, or hit or kill when we cannot have something we want; or that people will not try to free themselves from deprivation, exploitation, and despair through violence; or that frustration does not illuminate a particular case. It is to say, however, that a theory which explains everything--we cannot ask what it would be like not to be frustrated--explains nothing, contrary to the belief of many behavioral scientists that the more inclusive their

theory is, the better it is (Popper 1965:37 ff.). Like those who use Freud's theory of sexuality as a unitary theory, or Jung's theory of the unconscious, those who apply frustration-therefore-aggression across the board are arguing from an unassailable position.

The psychiatrist stresses the similarities, draws the analogies, presents the pictures in a new light with new association, as a rhetorical device to focus attention on the patient's problems. But his tendency is then to treat his rhetoric as a theory of human development. The plausibility and success of his rhetoric leads him to think that his hypotheses of human development have been proved. The theory is surrounded by a cloud of logical operations and protected from the need for verification which, as a learning theory, it would require (Louch 1966:221).

Karl Popper, in 1919, invented an extremely useful principle, guideline, rule-of-thumb to judge the significance of theories labeled as "scientific." At the time, he was struck by what seemed to be an essential difference between Einstein's theory of gravity on the one hand, and Marx's theory of history, Freud's theory of sexuality, and Adler's theory of the will to power on the other hand, all considered to be scientific. He determined the essential difference to be that Einstein's theory could be tested, as indeed it was by the astronomical observations made by Eddington during a solar eclipse--which proved that heavy bodies (here the sun) do indeed attract light (of the stars) as well as material bodies (the planets of our solar system). Had Eddington found no differences in distances between stars during solar day and solar night, the new theory of gravity would have had

to be reworked or scrapped. But Freud, Marx, and Adler could find no cases in which their theories did not apply. Furthermore, Freud and Adler could and did give radically different yet internally consistent analyses of the same event. Everywhere they looked, they found confirmation for their theories. Therefore, Popper concludes, these theories are non-scientific. He did not mean, however, that because these theories could not be tested they had no meaning. He was not concerned with meaning at this time.

Popper's verifiability principle was adopted and altered by those in a philosophical specialty, logical positivism, that began in the early 1920's in Vienna with philosophers Moritz Schlick, Rudolf Carnap, Herbert Fiegl, and scientists and mathematicians. The concern of the Vienna Circle, and what they held to be the proper concern of philosophy, was to distinguish between statements that are nonsense and those that are sense, while science attends to the business of deciding if statements that make sense are true or false. To determine sense-nonsense, they borrowed Popper's verifiability principle, which he called a "criterion of demarcation," but instead of using it to determine the scientific or non-scientific status of a statement, they interpreted it so that scientific statements were meaningful and therefore worthy of discussion, while non-scientific statements were metaphysical and therefore meaningless pseudoproblems incapable of solution because they offer nothing to be

solved. Popper did not intend, however, that all statements and theories that could not be tested should be junked, or that the line of demarcation in many cases was not blurry. He was trying to provide some measure of scientific status (Popper 1965:39-40).

Above I said that the frustration-aggression theory cannot be tested, i.e., falsified. Psychologists are quite aware of some of the problems with the theory and have tried refinements of the 2 basic assumptions: that all aggression presupposes frustration, and that all frustration leads to (instigations of) aggression. Tests of frustration often include administering electric shocks to people and then observing what they do when it is their turn to administer shocks; or tests may be run on pigeons and rats. In spite of the artificial testing situations (outside the psych lab one would probably be arrested for shocking people electrically), and psychologists' admissions "that the level of aggression shown by the frustrated subjects was not very great relative to the level possible in the situation" (Berkowitz 1969:9), it appears to me that attempts to refine and test continue to be made against a background of unfalsifiable assumptions. For example, what kind of risk is Berkowitz taking in testing when he calls inhibitions masks of otherwise aggressive reaction to thwarting, and a non-aggressive response to frustration a habit? Still, even if they only confirm intuitions or common sense, and granting the large assumption that

producing frustration and anger in the laboratory through the application of electric shock is analogous to frustrations in everyday life in our society, or that the shocks do indeed produce frustration, the psychologists' experiments are interesting. Berkowitz (1968) has carried out such experiments, in one case on the effect the presence or absence of a "stimulus to aggression," e.g., a gun, has on the expression of violent aggression.

As we suspected, the presence of the guns affected both the number of shocks the students gave their partners and how long they held the key down for each shock ... from a statistical point of view, our most significant finding was that the angry men who saw the guns gave more shocks than any other group (1968)

Berkowitz has tested a corollary hypothesis that violence will not erupt until a third factor, in addition to strong frustration and presence of cues, is present: low inhibitions. To what extent is violence tolerated or encouraged within a society? He has found that rather than having a cathartic effect by sublimating violence and providing an outlet for "the spontaneous accumulation of some excitation or substance in neural centers" (Lorenz 1966), the observation of and participation in violence encourages and legitimizes greater violence. In his tests, after a group of students saw a movie in which they thought the violence justified, they administered more shocks to partners who had previously shocked them (and thereby frustrated them) than any other group.

The social implications of the research I have described are clear, though they are much easier to recite than to act on. A society that wants fewer violent outbreaks should reduce frustration, leave inhibitions intact and remove immediate cues that can set off aggressive acts (Berkowitz 1968)

People learn how to be aggressive.--Social learning theory states that aggressive behavior is learned, by watching and imitation, and does not require frustration for its expression. Reward or punishment of such behavior reflects the basic values and "adaptive behaviors" of the group.

In American culture, where achievement, self-reliance, and individual self-interest are valued highly, we also find a relatively high emphasis on military glory, a relatively high incidence of personal crime, and a society characterized by a relatively high degree of bellicosity.... From this theory we infer that as long as a nation values and accepts violence as an effective coping strategy, violent behavior will continue (Gilula and Daniels 1969:398).

In contrast, the Semai of Malaya conceive of themselves as simply not the sort of people who would hurt each other; they see themselves as nonviolent. This is not merely an ideal to strive for. They do not say "Anger is bad" or "It is forbidden to hit people." They say, "We do not get angry," and "We do not hit people." The Semai do get angry and quarrel, and they do hit, but not often. They show their anger not in violence but in mutual avoidance and by spreading rumors behind each other's backs. Any more direct expression of aggression than name calling or throwing a few objects around is very rare. They believe it is wrong to frustrate someone's desires or force a child to do something it does not

want to do. They have a horror of physical violence; one adult would never hit another because "suppose he hit you back?" Nor should one hit a child because, "How would you feel if it died?" (Dentan 1968:55-58). We can identify with Berkowitz in this case and say that the Semai experience little frustration, that they encourage inhibitions against violence, and that they do not live with aggressive cues.

While Americans are ambivalent about violence and devote considerable effort to explaining and justifying it, the Yanomamo of southern Venezuela do not. Instead, they have elaborated waiteri (ferocity) into an ideological complex--a male complex.

The socialization process selects for and encourages ferocity. Masculinity and aggressiveness are instilled in small children from an early age. It is common to see parents tease a small boy to strike at his tormentors, rewarding his anger with approving laughter. Girls, on the other hand, are taught to acquiesce timidly to the punishment they receive from their brothers, so that by the time children are six or seven years old, the boys have already learned that it is appropriate to bully the girls and spend a great deal of time at mischievous pranks calculated to intimidate them.

Boys ... are encouraged to be fierce fighters. They have numerous opportunities to participate in fights.... They are pressed into the fighting by their adult superiors, but are given privileged positions in raiding parties until they acquire the necessary skills and experience.... Usually a boy does not take an active role in raiding until he is seventeen years old, and even then he may be so frightened that he will fake illness and return home before the enemy village has been reached.

Yanomamo boys, like all boys, fear pain and personal danger. They must be forced to tolerate and learn to accept ferocity as a way of life (Chagnon 1968a:130).

That way of life includes giving and accepting challenges, temper tantrums, wife-beating, beating contests with other males, and war expeditions.

Yanomamo enculturation of bellicosity is one of the best documented events in recent ethnography. It helps to support the following generalization by Andreski.

In every warlike polity ... there are elaborate social arrangements which stimulate martial ardour by playing upon vanity, fear of contempt, sexual desire, filial and fraternal attachment, loyalty to the group and other sentiments. It seems reasonable to suppose that if there was an innate propensity to war-making, such a stimulation would be unnecessary. If human beings were in fact endowed with an innate proclivity for war, it would not be necessary to indoctrinate them with warlike virtues; and the mere fact that in so many societies past and present so much time has been devoted to such an indoctrination proves that there is no instinct for war (1968:187).

This forms part of the basis for certain questions for which data have been collected in this study. Again, I am not willing to make a direct causal connection between the presence of certain practices and values related to warfare and the frequency of warfare experienced by a particular society. But the presence or absence of these values I consider to be vitally important with regard to those reasons, intentions, and purposes of an individual anonymous warrior. One certainly does not want to commit the fallacy that "the Yanomamo go to war because they are warlike," nor to be merely offering a trivial truth that people will seek to achieve what they value. Indeed, the latter may not be so trivial after all for those of us who have grown up in a culture that the

psychologists say values violence but whose politicians in every other breath use the words peace, reconciliation, generosity, mercy. The problem may lie in what constitutes violence to the members of a particular society. Unfortunately, the questionnaire question for this study which attempted to get at the way violence is valued in a particular society neglected (or I was not able to invent) to control for an etic-emic distinction. Answers to that particular question were coded impressionistically in most cases; occasionally an ethnographer would help by giving clear evidence for a particular answer. While specific details of questionnaire construction and rationale properly belong in Chapter II, in a crude way this study will end up testing many, if not all, of the explanations offered for the occurrence of warfare discussed in this essay. In any case, I do have one piece of evidence that there exists, at least in our own culture, dramatic variation--according to subculture--of what defines or identifies an act as violent.

The Institute for Social Research of the University of Michigan conducted interviews in the summer of 1969 with a nation-wide representative sample of 1,374 American men, collecting attitudinal data on what acts they considered as violent, what they thought causes those acts, and how they thought violence should be dealt with. The results were illuminating. I do have a major objection to the study: why, especially since the project was headed by a woman

psychiatrist, didn't the research workers collect comparable data for 1374 American women at the same time? In terms of the sociology of knowledge, it looks as though the research workers were guided by an implicit value: women are not relevant to violence, or that a violent act is a male act.

Anthropology of psychology aside, "... one of the key findings of the study was that what people mean by violence has a great deal of significance for other attitudes. And the more one labeled an act as violence, the more force one was willing to unleash to combat it (ISR 1971:4). As support for my own philosophy of language position on definitions, the study found that generally the men all knew what violence was: "... something bad, worthless, fierce, strong, and unnecessary. Blacks were the only exception to this; they tended to define violence as weaker, less bad, and less fierce than other groups" (1971:4). Yet 645 men of the sample did not agree on which acts are violent acts. In a gross breakdown of events considered to be violent (Table 1), looting was mentioned by 85 per cent of the informants, burglary by 65 per cent, draft-card burning by 58 per cent, police beating students by 56 per cent, police shooting looters by 32 per cent (ISR 1971:4). In answer to the question of how burglary, a crime against property, and draft-card burning could be considered similarly violent,

The study directors believe that one answer lies with the illegitimacy of the acts. Men tended to put under the rubric of violence actions they categorized as illegitimate, and the majority of American men believed

TABLE 1. WHO CALLS WHAT VIOLENCE*

	College Students	White Union Members	Blacks
POLICE			
Police beating students is violence	79%	45%	82%
Police shooting looters is violence	43	23	59
Police frisking is violence	16	10	34
BURGLARY			
Looting is violence	76	91	74
Burglary is violence	47	67	70
DISSENT			
Student protest is violence	18	43	23
Sit-ins are violence	4	24	15
Draft-card burning is violence	26	63	51
Denial of civil rights is violence	54	40	70
N	(63)	(279)	(303)

*ISR 1971:4.

that the state has a monopoly on legitimate force (ISR 1971:5).

The study found that the men were more willing to use force against individuals or groups they did not like, and that they did not consider force to be violence. So, police action against a disliked group, no matter how much physical damage they inflicted, would not be considered as violence. The research workers tie these findings into in group--out group behavior, discussed elsewhere in this essay under the emic statement, "It is wrong to kill a human being but all right to kill an enemy." The research workers also interviewed the informants about 5 values related to the legitimacy of force: retributive justice, self-defense, person versus property, liberalism versus conservatism, and kindness.

They found that retributive justice--"an eye for an eye"--and self-defense were the most potent values among American men generally in determining attitudes toward violence--kindness was the least potent: "One is tempted to say that the values that justify violence are more important in the determination of attitudes than the values that oppose it."

The study found that the more justifiable a man found the use of violence by police to control hoodlums, students, and rioting blacks, the more he believed in the value of self-defense, retributive justice, material over humanistic values, property over persons, and the less he believed in kindness (ISR 1971:5-6).

However, the more a Black American held the values of self-defense and retributive justice to be important, the more he believed that the goals of student demonstrations and protests would make a better world, that police were not

trustworthy, and that police acts of shooting looters, beating students, and frisking were ones of violence.

A more hopeful finding was that the more education a man had, the less he approved of police violence for social control, the more he was able to identify with members of an out-group, and--of special interest to me--the less he valued retributive justice (ISR 1971:6).

Obviously, the attitudes toward violence documented for our own society break down significantly into sub-cultures: those of college students of 1969, white union members of 1969, and Blacks in 1969. With even finer distinctions, other interesting percentages might have turned up, but the sample size would dwindle. In any case, what these psychologists were looking at were values shared by and probably learned within a particular group of people.

Maybury-Lewis (1967:305-307) suggests a promising hypothesis that I was not able to collect data for in this project but is probably, I think, very significant in matters of attitude, bellicosity, and war frequency. In comparing Gê-speaking groups in South America--the Shavante, Kayapó, Sherente, and Timbira--3 were troubled with severe factionalism, frequent killings within and between communities, and break-up of communities. Factionalism is less severe or absent among the Eastern Timbira. All 4 societies have age-set systems, age-moieties for log races, and neither the Timbira nor the Kayapó have clans or lineages (potential

units of competition). But while the Timbira are internally harmonious, the others have continual internecine warfare. The one institution found in the fractious 3 but not among the Timbira is the men's house. Rather than being a means to achieve harmony or Tiger's stable backbone of society, the existence of a men's house has just the opposite effect. In the men's houses of the Central Gê, boys are taught not only ceremonial and fraternal duties, but the values of manliness. One of the ways manliness is expressed is in bellicosity, expressed ritually by initiated males against uninitiated ones, and in the most important initiation ritual of all, ceremonial gang rape of select (and terrified) women, the ultimate outsiders. Bellicosity is not confined to ritual situations but spills over into secular life, Maybury-Lewis argues, stimulating political factionalism, often violent, within communities and, as communities fission, between communities. The Timbira not only do not have a men's house, they also do not have a violent or possessive attitude toward women: sex is easy and fun. There are several all-male corporate groups but also many community activities with opposite-sex participation. The arts of diplomacy, compromise, peace-making, and generosity are those valued by the Timbira. Maybury-Lewis' speculation about the influence of the men's house should be readily testable. The Yanomamo do not have a men's house, but the village is so constructed that it could be described as a single huge round house with

compartments for individual families, each compartment open to the center plaza and largely open to its neighbors. The point is, men can readily gather in any spot for their drug-taking or palavers, and their behavior is constantly in the public eye. Males learn from and teach other males. Their education and achievement are continually assessed by other men.

3. Cultural determinism and war.

I do not know what is true. I do not know the meaning of the universe. But in the midst of doubt, in the collapse of creeds, there is one thing I do not doubt, that no man who lives in the same world with most of us can doubt, and that is that the faith is true and adorable which leads a soldier to throw away his life in obedience to a blindly accepted duty, in a cause which he little understands, in a plan of campaign of which he has no notion, under tactics of which he does not see the use.

--Oliver Wendell Holmes, Memorial
Day Address, Harvard, 1895

One of the more recent and faster spreading schools of explanation in regard to warfare is that of economics and ecology, and for the following discussion I am concerned with the positions and contributions of White, Sahlins, Harris, and Divale. Previous discussion has dealt with the individual--in biology, evolutionary history, and psychology. But reducing explanations of a group activity (warfare) to the level of the individual is highly disapproved, risking charges of "reductionism" or, even worse, "psychologism." What Harris is calling for is a revival, more sophisticated,

of course, of the 19th century search for laws in the socio-cultural history of mankind. It is a back swing of a pendulum, the antithesis of the historical (particularist), structural-functional, and diffusionist schools of anthropology that developed during the first half of this century. They were not only not concerned with the discovery of social laws but thought such discovery and laws impossible. Their concern was with the individual person, the individual culture, and the varieties rather than the uniformity of pattern. They were interested in the historical development of individual societies and historical relations between societies, and in the non-historical structure and working of society, rather than determinism and Laws of History. Most of the really good descriptions of warfare among the Indians of North America were done by the American historicalists, and those for Africa by the British structural-functionalists. Anthropologists have never actually lost their interest in regularities, contrary to Harris' charge, and the interest is even stronger, threatened as we are with inundation by our accumulated data. But Harris is critical that scientism is not more widely followed, specifically that our search for regularities is not concerned with causality and origins.

Various stratagems have been introduced that avoid statements of causality, while conveying the impression that an explanation is being offered.... We have so-called functional explanations; we have correlations in which it is not known how the causal arrow points, and we have "accounting" for in terms of paradigmatic cognitive frames, which are accepted as givens, although nothing is

known about how long they have existed (Harris 1968:2). Harris' primary reason for writing The Rise of Anthropological Theory is "to assert the methodological priority of the search for the laws of history in the science of man" (1968:3). He regards middle-range theories as eclectic, chaotic, useless, and based upon the unrealistic expectation that eventually it will all make sense. He contends that anthropology needs a general theory of history in order to make decisions about how research funds are to be spent, and decisions in social engineering, especially of international development programs. The ultima ratio of social usefulness for a scientific paradigm is a crucial issue in both professional ethics and the philosophy of social science. Its philosophical implications will be taken up later.

Etics, emics, and laws.---The basic analytic scaffold for the following discussion is the etic-emic disjunction. On the one hand, it is useful as heuristic to cope with the twists and turns of sociocultural theory. On the other hand, it is a specific issue in the philosophy of anthropology, its formulation indigenous to the discipline. Within cultural anthropology, linguistics has often been lauded as the most rigorous, theoretically fruitful, and therefore successful of the specialties. As a consequence, investigators of sociocultural phenomena have tried to emulate this success by borrowing the basic analytic framework of descriptive

linguistics and applying it to human behavior. Many problems have arisen in this transfer, and Pelto (1970) declares that the debate between emicists and eticists is fundamental. Briefly, what is going on?

Etics in linguistics is specific and limited. It refers to a universal grid describing the production of speech sounds so that the sounds of any language recorded by any linguist can be reproduced on sight by any other linguist. It isolates discrete units of sound in the speech stream and describes the units with varying fineness of detail in standard symbols. Theoretically, the grid should include all possible sounds that the human speech mechanism can make. Mechanism is an appropriate term; phonetic description is mechanical. Nevertheless, it is subject to the vagaries of the linguist's ear and the informant's competence. Disagreements between investigators occur at even the lowest level of analysis. Once the grid has been used to isolate and describe sounds, analysis shifts diametrically from that of a universal open system to that of a specific closed system--emics. One concludes with a statement about the significant structural features within a language. Yet one cannot understand one side of the disjunction without the other: one cannot make mutually exclusive categories of sounds without the sounds themselves; sounds left in isolation are meaningless. The principle of opposition is operating: dark cannot be perceived unless you know what

light is; truth cannot be recognized unless you know what falsehood is.³

In the transfer to sociocultural phenomena, the etic-emic distinction is similar to Robert Merton's concept of real and ideal cultural patterns. The etic is how people actually behave in the judgment of the anthropologist; the emic is how the people themselves conceive of their behavior--their reasons, intentions, and explanations. Etic and emic descriptions are often contradictory. The anthropologist's and informant's explanations for a single event may be completely different and each deny the validity of the other's explanation. But Harris insists that the etic is prior:

There is no error more common or devastating than to confuse what people say, wish, dream, and believe they do with what they actually do (1971:149; italics added).

The anthropologist is to lay an "etic grid" or macro-theory--in Harris' case that of cultural materialism--upon the culture under investigation and isolate the discrete units of behavior, analogous to phones, which subsequently are organized into emic categories. But somewhere along the way what is heuristic in linguistics becomes the real world in culture, illustrated by Harris' emphasis on what people actually do quoted above. How transferable is the linguistic paradigm? An etic grid says nothing about the relationships among sounds--that comes with internal emic analysis. But Harris' analogous "etic" units--basic resources, tools,

techniques of production, energy, supplies, and control--are inextricably interrelated. Furthermore, etic grids are not causal, but cultural materialism is. White, Sahlins, Kaplan, and Harris represent the etic point of view and place themselves in opposition to those anthropologists, specifically Boas and his students, who stand on the emic side. Their etic grid is that of cultural materialism and evolution.

Before dealing directly with these theorists, however, I want to digress a bit and talk about Lévi-Strauss' use of the etic-emic disjunction in analyzing sociocultural phenomena, because his more literal transfer of the paradigm provides comparison and contrast for what the others have done. Lévi-Strauss' paper, "Structural Analysis in Linguistics and Anthropology" (1945), is an extraordinary attempt to apply linguistic method to the analysis of social relationships, to kinship systems in particular, in order to provide explanations as to how and implicitly why these systems can be arbitrarily derived and yet function with regularity and "effectiveness" in maintaining society. As linguists are no longer interested in treating terms within a language as independent but as elements in relationships that form a system to be subsumed under general laws, so the anthropologist, Lévi-Strauss argues, should view what we call culture traits as elements within a social system. Kinship terms are analogous to phonemes, and their arrangement analogous to phonemic systems. Lévi-Strauss means something more than analogy,

however. The structure of language is evidence of inherent structures of the human mind. Therefore, since kinship systems are another manifestation of that mind, they must have structure also. Indeed, they must have the same structure as language.

At the time he was writing, attempts to apply structural linguistic method to social data were apparently unsatisfactory; the results of such analysis were more complex and less elegant and explanatory than the raw data on which they were based. Why? Because kin terms were treated as words, between which there is no necessary relationship, rather than broken down into "phonemes." To obtain a structural law, the linguist analyzes phonemes into differential elements or distinctive features (etics) which he then organizes into "pairs of oppositions" (emics). These elements and their relations exist independent of psychological, natural, and physical factors.

To not only demonstrate but to prove his thesis, Lévi-Strauss applies formal structural linguistic analysis to the social relationship between mother's brother and sister's son and presents us with a general law: The relation between maternal uncle and nephew is to the relation between brother and sister as the relation between father and son is to that between husband and wife; if we know 1 pair of relations, we can infer/predict the others, since in each of the 2 generations there is always 1 positive relationship and 1 negative

one. The premises from which this law is derived include a brother-sister-sister's son basic unit of kinship, the incest taboo, and alliance theory.

But these premises consist of unfalsifiable assumptions. Lévi-Strauss, unlike the descriptive linguist, persists in going beyond form--items and their arrangement--into meaning and function. Although recognizing that form does not correlate predictably with meaning, that the symbols or kin terms are arbitrary, he still presents what he considers a predictive model that integrates form, meaning, and function. In this paper, and throughout most of his work, he relies upon case illustration to establish a general law, a practice derived from Durkheim, who believed that 1 good case supported a universal explanation. Quantification is not considered necessary because the structure of the human mind is a universal, like Freud's structure of the psyche.

From what one may call historical idealism, or a version thereof, let us move to the etic application of historical materialism. The argument of the cultural evolutionists that material conditions are the cause for certain things happening in human history is powerful. We are probably more likely to nod our heads in agreement while reading analyses of events in these terms than when reading Lévi-Strauss' interpretations, although both are seductive albeit mutually exclusive arguments. Let us look more closely at this

feeling of agreement, and then look at actual cases in warfare studies.

In their concern with methodological rigor, anthropologists often insist on drawing tight distinctions between hypotheses, theories, and laws, especially those anthropologists such as Manners and Kaplan (1968), influenced by the logical positivists. Nielsen (1967), a philosopher of another school, offers an alternative that is no less demanding but is more in accord, I think, with the ways in which human beings ask questions and seek answers. What hypotheses and theories do is to make sense out of a phenomenon: a hypothesis "closes up a gap of puzzlement"; a theory is a wider sort of explanation which "enables men to set their minds at ease about a broad range of diverse facts...." (Nielsen 1967:32). While it is difficult to make a distinction in all cases, a law

... does not explain anything but expresses the regular mode of action to be expected from a certain class of phenomena or the regular mode of action of a force (Nielsen 1967:32).

With the exception of a reference to Lotka in White (1959), we do not refer to Darwin's theory as a law. Instead, Darwin's theory has "set our minds at ease" about facts from geology, paleontology, embryology, and biology. That is, fossil and species variability is made intelligible--we can look at these phenomena without feeling that they are inexplicable. Thus,

... a theory is an idea or a manageable handful of ideas by reference to which men can throw light on a large class of physical facts (Nielsen 1967:32).

In comparing 2 specific theories, Einstein's theory of gravitation and Darwin's theory of evolution, there is a striking and significant difference between the two: the former is predictive and testable, the latter is not. Had Einstein's theory not met the test of predictability, as it did with Eddington's findings, it would have faced refutation and radical revision (Popper 1965:36). But, as Nielsen points out, theory in biology and the social sciences is more often explanatory than predictive. It is not because our standards or methods are somehow faulty, but because physical phenomena and phenomena involving living forms are different scientific subjects. Darwin (and all of us) did make predictions, e.g., that foxes who were unacquainted with and therefore unafraid of men when first discovered would soon become extinct--which in fact happened. But this prediction was not deduced from his theory; instead, he made it by appraising the circumstances or context with the eyes of years of experience. Had the foxes not become extinct, Darwin's theory would not have been refuted.

Theory-construction proceeds in a variety of valid, useful, and important ways, depending upon the kinds of facts under study and the kinds of questions men want answered. In the light of these distinctions it would seem that the dream of axiomatizing biology, psychology, and their sister sciences has been entertained without full awareness of the variety of problems the sciences consider (Nielsen 1967:38).

The primary goal is intelligibility, and biological evolutionary theory offers a matrix for explanations of particular events. When we nod our heads in agreement, this means that what we are dealing with is intelligible. But the etic anthropologists are seeking more than intelligibility, or they require that intelligibility be validated through prediction. And if one demands predictive power, one must talk in causal terms.

While anthropology has been concerned with the problem of cultural evolution for over a century now, individual theorists vary in the degree of emphasis and explication. Leslie White has grounded his theory of culture in physics, and his conception of the evolutionary process in general is the most deterministic and the least concerned with the individual. I will go into it in some detail, since it is the most extreme expression of a theoretical position influential in warfare studies. White (1959) offers the unitary theory that everything in the universe can be explained in terms of energy, which is the basic and universal concept of science.

The physical ground for the cultural theory is the Second Law of Thermodynamics, which states that within the closed system of the universe, order is breaking down into disorder and chaos, differences are leveling, towards the final state of equilibrium, or maximum entropy. But living things, because they are open systems linked to their surroundings, for a while at least can overcome the entropy

produced in the process of living because they draw on free energy outside themselves. This energy or negative entropy is used not only to maintain life and balance positive entropy, but any excess is utilized to develop structures of greater organization, complexity, and efficiency. Thus, species evolve. Individuals, however, eventually are overcome by positive entropy as their structures simply wear out from resistances inside and outside themselves, e.g., aging, chronic disease, mortal wounds.

Thus life and death alike receive their most profound and illuminating definitions in terms of thermodynamics. The maintenance of life is a continuous balancing of positive entropy with negative entropy. Dying is the losing battle to overcome positive entropy. Death is the state of maximum entropy, of thermodynamical equilibrium (White 1959:35).

The primary source of energy for all living things is the sun, and since solar energy is relatively boundless the expansion of living things is limited only by the capacity of the earth to "accommodate" them. This expansion is both quantitative (through reproduction) and qualitative (through the development of higher forms of life). White specifies "higher" as meaning greater structural organization and more concentrated energy, e.g., animals are more highly developed thermodynamic systems than plants; mammals, than reptiles. In the struggle to exist and survive, an organism adjusts to its habitat in terms of such factors as temperature, humidity, radiation, and food, and competes with other organisms for habitat and food. Those organisms with the most

efficient "energy-capturing devices" have the advantage in this competition and will hold it. The tendency of the life process itself is to increase the mass of the organic systems, and the rate of circulation of matter through the system and matter's transformation into energy (White 1959:37), regardless of whether the energy is used quantitatively or qualitatively. Yet this process can continue only so long as there is free matter and energy available.⁴

Man is an organism, therefore he must adjust to his habitat, compete for that habitat (White uses the phrase "defense from enemies"), and reproduce. To do this, man must capture and utilize energy, which he accomplishes through his body and through culture. Culture is both a means by which we capture and use energy, and the product of that use.

"Culture" is but the name of the form in which the life forces of man as a human being find expression. It is an organization of energy transformations that is dependent upon symboling (White 1959:38).

As the fundamental process of the human organism is to capture and utilize free energy, so the function of culture is "the harnessing of energy and putting it to work in the service of man." Culture cannot exist without man, and it is always found with man.

But from the standpoint of scientific explanation of cultural diversities and processes of change (but not of the nature of culture in general), culture may be treated as if it had an existence of its own, independently of the human species. [Italics added.] The "as if" factor does not render explanations made on the basis of this assumption fictitious or nonscientific. The science of linguistics proceeds upon this assumption, and it is the closest

approximation to a mature science that we have on the level of human affairs. Man, the human species or human organism, is irrelevant to the science of linguistics. He, or it, is likewise irrelevant to the science of culture (White 1959:15-16).

Since culture may be treated logically "as a distinct and autonomous kind of system," and since cultural systems are material systems,

... we may interpret the evolution of culture in terms of the same principles of thermodynamics that are applicable to biological systems (White 1959:39).

Subsequently, White sees cultural systems using the energy they capture to extend themselves quantitatively (population growth, group fission) and qualitatively (higher forms of organization and greater concentrations of energy).

As the amount of energy harnessed by sociocultural systems increases per capita per year, the systems not only increase in size, but become more highly evolved, i.e., they become more differentiated structurally and more specialized functionally (White 1959:40).

In "discovering" cultural structure and function, one does not need to consider the environment at all; such consideration belongs to the particular case:

... the law of falling bodies is valuable precisely because it ignores the influences of atmosphere and the composition and structure of the falling body. In exactly the same way, the culturologist is trying to formulate laws of behavior of cultural systems. Like the physicist, he wants valid universals. If one wishes to deal with particulars, with particular cultures or particular falling bodies, then allowance must of course be made for particular conditions in each instance (White 1959:52).

How does warfare fit into this theoretical matrix?

Contending in his earlier work, The Science of Culture

(1949), that one cannot understand warfare by looking at the psychological motivations of individuals, White insists:

Wars are fought between societies, between sociocultural systems, between tribes and nations. It is the culture of any given situation that determines whether warfare shall be engaged in or not, and if so how, with whom and for what (1949:131-32).

His discussion of the problem is brief, with the conviction that the individuals involved are simply not relevant. In 1959, while his discussion of war is again brief, White gives a more sympathetic picture of man as victim, especially of the agricultural revolution, the development of social stratification, and the concept of personal property.

An elaboration and direction application of White's theoretical position is contained in two papers by Newcomb dealing specifically with warfare. The first (1950) is a re-examination of the causes of warfare on the Great Plains analyzing warfare as influenced by the horse and gun complex, the fur trade, and the dislocations and migrations of native peoples as the result of white intrusion. The analysis appears sound, and was in fact anticipated by Oscar Lewis' study of the effects of the fur trade on Blackfoot culture in 1942 of which apparently Newcomb was not aware. The analysis is in terms of the Chippewa doing something, the Dakota doing something in response, and so on. For example:

By 1650 the Chippewa and associated tribes had come into the territory west of Lake Huron, and by the middle of the next century were west of Lake Superior. It was the misfortune of the Sioux, who had been in possession of the Minnesota territory, to come into violent collision

with these invaders equipped with the steel knife and musket. After a war which lasted many decades, the Sioux were for the most part crowded west of the Mississippi. The Ojibway, however, had had to eject the Sioux from Minnesota because they themselves were being harassed by the oncoming white settlers (1950:322).

It appears that he is describing human actions, reasons, purposes, and so forth. In conclusion, he gives the causes of Plains warfare: migration onto the Plains from east and west, competition for the horse, competition over hunting territories and decreasing game, competition for guns, and European political machinations of playing tribes off against each other (1950:327-28). Newcomb concludes:

In broad terms we may say that from the introduction of the horse until the extinction of the bison herds the Plains peoples were making continual and successive adjustments to the forces of European culture (1950:328).

While this theoretical generalization produces a mild let down after the previous ad hoc explanation, the real difficulty comes in trying to plug this account of Plains history into the proposition that

... war will be treated as a type of armed conflict that takes place between societies, meeting in competition for anything that is valued by the groups involved, usually consisting of territory or certain products of this territory, such as good hunting grounds, oil-producing or agricultural lands.

It must be emphasized that this definition says nothing about individuals, for in terms of this analysis warfare is held to be a function of socio-cultural systems, and individuals are regarded as being no more than the means through which these systems attain their ends (1950:317; italics added).

Or that

The motivation of the individual is not the cause of warfare, it is rather the method by which a cultural irritation or need is satisfied (1950:320; italics added).

Or finally that

Plains tribes did not habitually engage in war because individual men were "warlike." Individual men were warlike because their socio-cultural systems obliged them to be. The individual attitude of war was an expression of the socio-cultural process, by no means its cause or initiator. Whether individual men will fight for obscure economic reasons, which benefit the society and the individual only in passing, is unknown. The fact of the matter is that most individuals everywhere, in all warring cultures, fight because of immediate, personal reasons. They fight for glory, for social prestige, to escape civilian frustration, or for other individual reasons; not primarily, or perhaps even knowingly for impersonal, broadly cultural reasons. It seems probable that men everywhere fight better if they are fighting for personal reasons. Broad cultural ends are more quickly attained by the psychological provision of adequate motivation. Yet it does not matter for what reason the individual thinks he is fighting and dying, as long as he is satisfying the needs and imperatives of his culture (1950:329; italics added).

While it is laudable that Newcomb should criticize and offer alternatives for the previous superficial and tautological explanations offered for Plains warfare, the ethnographies contain accounts showing individuals, in varying degrees to be sure, quite aware of so-called economic or cultural imperatives for warfare. The emic is in accord with the etic.

In a second article (1960) Newcomb discusses the culturological view of warfare in general and this time offers a typology rather than a case analysis. While the paper is also an expansion of background arguments, especially against the utility of psychological explanations, in considering the

"entire known spectrum of human cultures" Newcomb finds four types of conflict, each associated with and determined by particular technologies. Type 1 includes food gatherers living in isolated (e.g., Australia) or unproductive (e.g., Great Basin) regions, who tend to be peaceful because there is little of economic value for the small scattered groups to fight over--no one can accumulate surpluses, control large territories, profit from captive labor, spare much time from the food quest, and organize into large military units.

Infringement upon another band's territory is perhaps the most important source of conflict. But conflicts are generally so infrequent, brief, unorganized, and involve so few individuals that they must be considered a distinctive form of warfare (1960:327).

Type 2 is what we have in mind in referring to primitive warfare. People subsist primarily as food-collectors who hunt, or garden part or even most of the time. Their mode of production cannot support cities, social stratification, or states. Conflicts tend to be uneconomic, crude, sport-like, unorganized, and brief (1960:328). They do not have, in Malinowski's phrase, "culturally constructive significance," i.e., they are not means to enlarge the economic base because one's neighbors do not have much wealth. "Primitive warfare is best comprehended as a transitional type of conflict--transitional between an ordinarily peaceful state of affairs and serious, deadly, competitive strife" (Newcomb 1960:328). People carrying out primitive warfare cannot raise or control large fighting groups, and those groups that

are raised cannot stay in the field for very long. Nevertheless,

... the technological level of these cultures is not so restrictive in its effect as to preclude war. They are probably more frequently in conflict over favored hunting and fishing grounds, and other natural resources than are the technologically most primitive cultures (1960:329).

Cultures in Type 3 are based on food production, which can support large dense populations, specialization, cities, and states. Property replaces kinship as the basis of socioeconomic organization. Consequently, war is highly profitable, and the end of increasing movable and immovable wealth is accomplished by means of mobilizing large sectors of dense populations.

Rich hunting grounds, favored fishing sites, and the like, are always apt to be objects of contention between even simpler societies, but for agricultural civilizations the basis from which their cultural blessings flow--fertile river valleys and other types of productive lands, mines and supply routes--must be controlled at all costs. And those who are defeated can look forward only to exploitation, slavery, serfdom, and perhaps cultural annihilation. Thus, war becomes more deadly and serious, its consequences greater; there is more to fight about, more to gain or lose (1960:329-30).

This type is "true" warfare. The reader will recall my discussion of such distinctions in the section dealing with definitions.

Type 4 warfare includes world war, which "may be regarded as a consequence of the industrial revolution" (1960:330). Coalitions of nations fight each other with huge armies supported by well-organized civilian populations, for

long periods of time. The "causes" for these wars are still those of Type 3 food-producers, with the addition of urgent industrial imperatives to have access to raw materials and markets for finished goods (1960:330).

To these four categories, one could add two more: atomic warfare and one of its consequences, limited warfare--total war but only to a certain point, i.e., the use of nuclear weapons.

Newcomb notes that wars between societies of different technological levels may take place because they are different--the poor want what the rich have, and the rich look upon the poor as fair game, e.g., Indians of North America versus European settlers. Yet Newcomb concludes without comment that whether or not a "culture goes to war" and uses its technological capability in war, depends upon a very wide range of other variables, which I shall discuss in a moment.

Finally, this typology has nothing to do with individuals, since culturologists conceive of culture as "a superorganic entity, obeying its own laws and moving in accordance with its own principles" (Newcomb 1960:332).

When it is realized that cultures do have their own independent integrity, their own direction and force, and an existence apart from individuals, the distinction between the reasons why individuals fight in wars and the causes of their culture's wars comes clearly into focus.... In fact, a nation may be able to produce more strongly motivated fighting men if they are ignorant of the real causes of conflict. How many American youths could be enlisted to fight the battles of the oil companies, or for the markets of southeast Asia? How many, on the other hand, could be enlisted to prevent the rape of

their mothers, or to "see the world" ... or for other personal reasons? (1960:332)

How do we account for this inconsistency between the etic and the emic? "All cultures present themselves" as moral, good, and true to their members, but relationships between cultures "have always been governed by the law of the jungle, by sheer power," because "competitive conflict" is the basic cause of war (Newcomb 1960:332-33). The exercise of force majeure may lead to intricate rationalizations of behavior.

Yet the student who views culture superorganically is relieved from being upset by the fact that "peace-loving" nations are frequently at war, or that (as the journalistic world assures us) the man in the street, whether it be Red Square or Main Street does not want war although it is this man who will become his nation's soldier. The superorganicist realizes that the wishes and hopes of the individual are the result of the interplay of cultural forces which are affecting him, and that it is not the other way around (Newcomb 1960:333; italics added).

In assessing the culturological contribution to explanations of warfare, the good points stand out, especially when considered in the context of the intellectual history of anthropology. The emphasis on technology and its effects upon other cultural phenomena are not to be disputed; nor the perhaps pivotal position in human life of materialism--economics may indeed make the world go 'round--and the emphasis on the consequences of the agricultural revolution; nor the drive for a bird's-eye view of history; nor the invention of heuristics to make that view communicable and applicable. The culturological theory is partially true--these theorists are not fools--but false in generality, as I will try to show.

My primary objection is the loss somewhere along the way of the "as if" in the conception of culture having an existence of its own, independent of human beings. Even though White, Newcomb, and, later on, Sahlins and Harris repeatedly state that culture cannot exist without man and that where man is, one will also find culture, they continue to ask a question that, I contend, is unanswerable. As the biological determinists ask, "What is man without culture like?," the cultural determinists ask, "What is culture without man like?," investigating the flip side of the nature-culture disjunction. No longer do sociocultural phenomena occur "as if" they were doing so in accord with certain principles and laws, but they really do. The model becomes the thing modeled: a metaphor of the human puppet responding to the tugs of the superorganic puppeteer ceases to be a metaphor. Bidney (1953) discusses the problem of the culturological "as if" in detail, and concludes that the culturological fallacy incorporates a new animism of the superorganic, which has explanatory power as do all myths--and even perhaps a kernel of truth, as most myths--but can hardly be called scientific, analogous to theoretical physics.

Perhaps one should qualify this by saying that it can hardly be called scientific, yet. Popper states that historically nearly all scientific theories originated from or were anticipated by myths.⁵ Therefore, a theory found to be non-scientific, i.e., untestable, may still be important,

"but it cannot claim to be backed by empirical evidence in the scientific sense--although it may easily be, in some genetic sense, the 'result of observation'" (1968:38).

Popper identifies Marx's theory of history as non-scientific or pseudo-scientific because it does not take predictive risks, i.e., be tested for situations in which it may not apply. Popper accuses Marxists of adjusting the theory so that it will always be demonstrated, like the astrologer or soothsayer making open-ended, adjustable predictions so he will not lose clients by being proven wrong. Since White has been so strongly influenced by Marxist theory, Popper's criticisms also apply, not analogously, but literally to him. The criticisms are especially relevant when one recalls that White's justification for "as if" statements in the first place is in the name of scientific explanation and a "science" of culture. Attempting to be what he conceives of as scientific, he creates myths instead. In paraphrasing the culturological view of war, then, when men join together through choice or conscription, ride or march off to ambush or engage an enemy, carrying spears or M16's, and give as reasons for doing so righting a wrong, getting rich, gaining respect and prestige, doing a job, or following an order, their actions, beliefs, and materiel are simply expressions of cultural laws, as falling rocks and feathers express the Law of Gravitation.

Ordinarily, one does not expect laws to explain why cultural systems behave in such a manner, or why things fall--but to simply state that they do so, regularly, and that this regularity is testable. But testable under what conditions? The law of freely falling bodies operates in a vacuum; the disruptive factor of atmospheric context is removed. Rocks dropped from the Tower of Pisa or the Empire State Building will reach the ground before feathers; a coin will fall almost as fast as a cannonball, but in a vacuum all will fall at exactly the same rate. We can drop feathers, rocks, chairs, and flowers thousands of times and they will behave as the law states. If they did not, the law would have to be altered or thrown out, its new form subject to similarly risky tests.⁶

Let us set up these "laws," one physical, one cultural, side by side. We should be able to do this if, as Manners and Kaplan contend, there is no "logical or ontological gap" between the physical and the social sciences (1968: 10). For the first case, I shall continue examining the law of freely falling bodies for several reasons. White uses it as a standard of explanatory power; it is relatively simple; it is important in the foundation of classical mechanics and classical physics; and it is deterministic--characteristics making it more analogous than say quantum physics to attempts at macro-theory construction in anthropology. The law states that the velocity of a falling body is proportional

to the time of its fall and that the distance covered increases as the square of time, presented in the formula:

$$s = \frac{1}{2} a t^2$$

where s equals the distance covered, t equals time, and a equals acceleration, a constant. (The acceleration of free fall is 386.2 inches per second squared, or 386.2 inches per second per second, varying slightly with latitude and altitude (Gamow 1962:33).) Simple, predictive, mathematical, and the basis for Newton's Law of Universal Gravity⁷ and Einstein's theory of gravitation as the curvature of the space-time continuum. There is no need for an "as if" qualification; such a qualification would be in error.

For the second case, consider Kaplan's Law of Cultural Dominance, which at first looks like a law, i.e., it states a regular mode of action of a class of phenomena, but it is a direct homolog to the Darwinian theory in which the principle of natural selection is not a law. Kaplan's "law" states that the

... cultural system which more effectively exploits the energy resources of a given environment will tend to spread in that environment as the expense of less effective systems. ... a cultural system will tend to be found precisely in those environments in which it yields a higher energy return per unit of human labor than any alternative system available (Kaplan 1960:75-76).

Then Kaplan proceeds to explain that he realizes that as yet anthropology has not devised a measure of thermodynamic effectiveness, other than the extent to which a culture in a

particular environment dominates or is dominated.

In the present state of evolutionary theory we are placed in somewhat the same embarrassing position as the biologists who account for the survival of certain organisms in terms of their better adaptability and then turn about and assert that the reason they know one organism is better adapted to its environment than another is that one survives and the other does not (Kaplan 1960:76).

One should certainly not think that such embarrassment is an indication that adaptation is a worthless concept, because it is not. Its utility has been demonstrated repeatedly.

Indeed, we would not be able to talk about the processes of biological evolution without it. But the important point is that Kaplan apparently calls this trend, or proposition, or even principle or theory a law because of the implicit assumption that while it cannot qualify for law status now, in the future we will be able to fill in the holes. On the basis of its potential, call it a law. Using such reasoning, plus an unconditional criterion of regularity, we could elevate any number of statements to the status of law and thus by fiat be scientific. Why not have the Law of Oedipus, the Law of Frustration and Aggression, the Law of Maximization, the Surplus Law, the Law of Male Dominance, and the Law of Matrilateral Cross-Cousin Marriage?

Harris (1971:203 ff.) does offer a mathematical measure of technoenvironmental efficiency, which could be considered a direct measure of adaption. His formula states:

$$E = \underline{m} \times \underline{t} \times \underline{r} \times \underline{e}$$

where E equals food energy or the number of calories produced per year; m equals the number of food producers; t equals hours of work for each food producer; r equals calories expended per hour; and e equals the average number of calories produced for each calorie expended. The value of e is derived by:

$$\frac{\text{total calories of food produced per day}}{\text{the number of workers} \times \text{average hours of work per worker} \times 150 \text{ calories (that which each worker expends each hour above basal metabolism)}}$$

Therefore, 7.4 Kung Bushmen of the Kalahari Desert working 6 hours each and expending 150 calories each per hour, collect 64,200 calories worth of food, giving them an index of 9.6. If the index were less than 1.0, one would assume that the people were starving. Including this index in the larger formula, one is not solving for E . Instead, E is estimated at $365 \times 64,200$ (assuming of course that this is a daily average) equals 23,433,000. The value of m is given as 20, the number of adult individuals who worked at food getting over a certain period. The formula is completed by solving for t , which is 605 hours per food collector per year. One hundred forty-six Tsembaga Maring, swidden agriculturalists in New Guinea, produce 130 million calories in vegetable foods annually, each working 380 hours per year, and having an efficiency ratio estimated at 18.0, and 18 million calories in animal food (pigs), but which produces a much lower efficiency rating of 2.1 (1971:210-13). Five million farm

workers in the United States in 1964 produced 260 trillion calories, each working an average of 1714 hours, with an efficiency index of 210.0 (1971:217). Harris specifically omits energy expended in food preparation (1971:206).

By interpolation with Kaplan's Law, we could expect that the society with the highest efficiency rating in a particular environment will tend to spread in that environment and in most cases by force or the threat of force: military conquest, extermination, dispossession, colonization:

... an advanced cultural system can marshall a greater and more powerfully equipped military force,⁸ enabling it to take, and hold against encroachment or revolt, areas where its exploitative techniques are more effective than rival systems (Kaplan 1960:88; note added).

Or a dominant type may spread by adoption, as a threatened society adapts to the system of the threat rather than fighting it. Harris admits to the problems of inexactitude in what one could call cynically a nutritional theory of history:

Although this formula is constructed from several "guess-estimates," correspondences with the data from other societies increase our confidence in its basic accuracy. The most problematical factor is the value of 150 calories per hour for r. It is very difficult to measure calorie expenditure per time unit under natural field conditions (Harris 1971:205).

One would also have to know the caloric values of native foods, the caloric intake of each worker, the exact amount of time spent working, and seasonal variations. Unlike taking a pulse by counting for 15 seconds and multiplying by 4, in measuring highly variable work, as opposed to a position on

an assembly line, it does not seem legitimate to measure for 3 months and multiply by 4 for an annual figure. One should know these things beyond "guesstimates" if the mathematical rigor of the food-energy formula is to even approach that of the Law of Falling Bodies. What might be empirical implementation of the Law of Cultural Dominance outwardly wears the trappings of physical law but upon closer inspection it is filled with unknowns. There are 2 possible courses to take in dealing with these unknowns. The first is to invent means, expend effort, and demand rigor to fill in those unknowns as Koebben (1967), Hempel (1959), and Harris insist on the assumption that the task is possible.

The second is to examine the possibility that at the most such formulae are estimates and will remain so because of the nature of human life, in this case "work." Putting it another way the unknowns can never be filled in exactly, like the rate of acceleration on earth, because human action is open ended, not because we have not yet found the limits. For example, what about time and caloric expenditures for events like thinking and talking about food-getting, and preparations for food-getting activities? We cannot, contrary to White's belief, put human action in a vacuum, nor is it legitimate to operate "as if" we could.

Many an earnest psychologist or sociologist apologizes for the fact that his science is not, or is not yet, a science in the Newtonian sense, that is, a small set of formulas and principles from which individual behavior can be predicted in much the same way that astronomers

predict an eclipse. But are apologies called for? In Newton, as in Einstein later, the fullness of time brought together a genius for theorizing and a domain in which certain kinds of cyclical or otherwise repeatable events lend themselves to mathematical treatment. However, not every scientific subject exhibits that initial kind of order (Nielsen 1967:34; italics added).

Yet Service (1960) lauds the culture evolutionary point of view as good because it is predictive and therefore relevant to modern life in forecasting the future and making improvements in the world. Again, useful engineering is the ultima ratio. Before examining the predictive and explanatory claims of the Law of Culture Dominance--

The law of cultural dominance, which is derived from examination of the process of the rise and spread of dominant culture types, not only underlies the distribution of culture and the historic movements of peoples and societies, but also explains why some cultural systems have been able to spread at the expense of others and some have not (Kaplan 1960:92; italics added)

--I want to look at another comparative case, from the social sciences this time.

Formal economics, commonly thought of as the most "scientific" of the social sciences, makes a strong engineering claim, but when looked at closely its ideal postulates operating in a social vacuum do not explain or predict well in the real world. We might even call formal economic theory "super-etic."

The world as depicted by conventional economics is a highly "idealized" world. It is a world in which individuals act with complete information and foresight; in which all action issues from economically rational decisions and is directed toward ends that are always maximized; in which there are no cultural or psychological restraints on translating decision into immediate

action; and in which all individuals make choices and act wholly independently of one another. Within this idealized world, economists have been able to move with logical consistency, deductive certainty and, frequently, mathematical elegance. In responding to criticisms that this idealized world seems to bear little relationship to any concrete empirical system, economists have replied that this is the way of science (Kaplan 1968:237).

In his elegant criticism of this view, Kaplan points out that economic rationality is not a limiting condition of human behavior in the same sense that a vacuum is a limiting condition of physical behavior. The buyers, sellers, consumers, and entrepreneurs of microeconomic theory are not real people but idealizations and abstractions, assigned certain properties within the theory. Even if one does not reject formal theory a priori and plugs substantive data into it, it only explains and predicts in market economics. Yet there is serious question as to its usefulness in market economics, specifically the inability to move from ideal cases to real ones.

So long as economists have remained in their purely formal-hypothetical world they have been able to explain and predict with some measure of success. When they have tried, however, to make the transition to any concrete economic state of affairs they have encountered the same methodological problems that other social scientists confront (i.e., non-closed systems, a multitude of variables, etc.). Their predictive successes seem not to have been appreciably greater than those of other social scientists.... (Kaplan 1968:241)

But is evolutionary theory, biological or cultural, predictive? Recall that Nielsen has stated Darwin's theory to be nonpredictive, i.e., it cannot tell when or in what direction a species will evolve or under what specific

conditions, or even that evolution will continue to occur. It does explain what has taken place in the past and, if change occurs in the future, how it came about. Yet the theory even explains only in a general way.

Darwin's propositions do not directly explain any of the facts that prompted him to formulate them.... Instead, a connection between the theory and a particular fact is made by talking through to the fact in a manner suggested and permitted by the theory. This talking (or writing) is not to be confused with any form of [mathematical] calculating.... (Nielsen 1967:33-36)

For example, in looking at a collection of fossil hominid skulls, we notice considerable variation in dentition and facial skeleton. Indeed, through seriation, supported by absolute and relative chronological evidence, the specimens are arranged in what is thought to be a developmental sequence. The observed variations through time are explained in evolutionary terms as due to adaptation to and selection for both vegetable and meat eating. But we cannot deduce this explanation in tight logical form from the principle of natural selection which states that those individual organisms best able to get along in a particular environment will tend to live longer and produce more offspring than those who are less well able. Instead, we work from the theory to the fact in what Nielsen calls a "discursive language" using our knowledge of physiology, genetics, and culture, e.g., the relationship between the development of the hand and tools and both the reduction of big grinding molars and big canines. Predictions about future hominid dentition are not impossible,

but they cannot be deduced from the theory. For instance, similar to Darwin's prediction about the extinction of tame foxes in the face of human intrusion, in appraising Western middle-class dietary and dental hygiene habits plus increasing longevity, we can predict a continuation of periodontal disease and tooth loss despite our enormous dental care industry. We cannot predict, however, what hominid dentition will be like 10 generations from now.

In an article analyzing the expansion of the Tiv and the Nuer against their respective neighbors, Sahlins (1961) uses an etic grid over an emic case with results similar to but more sophisticated than Newcomb's analysis of Plains warfare. Sahlins argues that the crucial factor in the expansionary success of these 2 societies is not sufficiently their environment, technology, and economics but the ways in which people are organized in social groups in order to adapt to certain conditions.

The Tiv and Nuer are both expanding against populations whose subsistence base is the same as their own, but while the Tiv are the larger population (800,000, the largest group in northern Nigeria), 200,000 Nuer have intruded successfully into the pasture lands of 900,000 Dinka. The significant factor apparently is the segmentary lineage system by means of which people are able to mobilize and through which they make claim to the right of land use. Among the Tiv, "... every compound headman within the minimal segment

holds a right against the world to sufficient farming land" (Sahlins 1961:337). The world consists preferably of foreigners, but for those lineages towards the center of Tiv-land, one moves against those neighboring lineages most distantly related to one's own. As internal lineages grow in population and satisfy their felt right to land by taking land from distantly related lineages, eventually those kin groups at the borders are forced to replace their lands expropriated by other Tiv by moving against foreigners. Such action leads to a "long and bitter war" (Bohannon 1954:7). The residents of the border villages may be moving against their will. "The lineage is simply crowded out as the Tiv side of its land is consumed by the appetites of other Tiv" (Bohannon 1954:7; italics added).

The Nuer, whom Sahlins considers as "perhaps an outstanding instance of the Law of Cultural Dominance," apparently intruded into land held by the Dinka and successfully pushed back, split up, and absorbed many of the autonomous subtribes characteristic of Dinka social organization. As in the Tiv case this success is due, Sahlins argues, to Nuer segmentary lineage organization. The Dinka, even with a population 4.5 times as great, are unable to mobilize against continued Nuer expansion because of the absence of segmentary lineages, and the Dinka, even in the face of great pressure, have apparently not invented an alternative. In Sahlins' words,

The Dinka lack the thermostatic mechanism for massing against the outside, a deficiency that has been fatal (1961:340).

The reason for this difference between 2 cultures that are otherwise alike is apparently because the Dinka arrived in the region first and, facing no opposition, dispersed into small autonomous groups with little need for cooperation [quantitative utilization of negative entropy].

These circumstances favor fission but select against complementary opposition or fusion, and long term occupation will eventually fix this structure making it comparatively inflexible (Sahlins 1961:342; italics added).

The Nuer, however, were intruders into an already occupied area and faced opposition. "This selective circumstance placed a premium on the ability to fuse as well as to segment, on complementary opposition" (Sahlins 1961:340). If the Nuer developed segmentary lineages as an "adaptive response" to their intruding, is it not reasonable to ask why the Dinka did not develop an adaptive response to intrusion? On the one hand, we can accept the proposition that inflexibility because of age means they could not and therefore were pushed around and threatened with extinction--they were too specialized. On the other hand, one could interpret Dinka capitulation and absorption as an adaptation, i.e., an alternative to being killed, but be subject to the adaptation tautology.

Why did the Tiv and Nuer move against their neighbors at all? Sahlins cites overpopulation as the reason given by

both Evans-Pritchard and the Nuer themselves. Yet among the Tiv, while part of their land is overused in the south, rapid expansion has recently occurred in the north, where population density is less than half the average of 64 persons per square mile. The significant factor seems to be the idea and belief of the people themselves that they need land.

Sahlins concludes:

... it seems to us that a certain relativity is required in assessing land hunger among societies competing for occupation of a specific habitat. Because the success of one contestant is necessarily to the detriment of the other, neither has enough land until the other has been eliminated. The need for "living-space" is built in: it becomes a cultural attitude and theory, particularly in that society which has the decisive competitive advantage. Among the invaders a natural increase of population beyond the carrying capacity of present resources will be taken for granted, and at least for them land hunger exists--the idea is adaptively advantageous--even if, by objective standards, there is enough land to support the present population.... From an adaptive point of view this is no paradox (Sahlins 1961:341).

Sahlins' explanation does not seem to follow, especially the statement "Because the success of one contestant is necessarily to the detriment of the other, neither has enough land until the other has been eliminated," and the statement, "From an adaptive point of view there is no paradox." The former if...then statement does not logically follow, even though taken separately the premise and conclusion may be true; the latter adaptive statement is by nature non-paradoxical and unfalsifiable.

On the one hand the Tiv are not making a real adaptive response because there is plenty of land; on the other hand,

because they think they need living room and more land, moving against others is an adaptive response to Tiv and Nuer ideas of native conceptions of overpopulation and land need. One is left with a "so what" feeling in regard to the explanatory power of adaptation but with quite a different reaction to the explanatory power of the reasons people give for doing certain things. With some hesitation Sahlins does consider such information significant--and Harris does not, as we shall see--and that ideas can be "adaptively advantageous," in the sense here that people are motivated to continually increase their land holdings.

Harris wants to supply a macro-theory of sociocultural evolution, the basic principle of which is not a law like Newton's Laws of Motion or the laws of quantum mechanics, but a "law" analogous to the principle of natural selection in Darwinian evolutionary theory, "... a basic research strategy, from the application of which there is an expectation that a nomothetic causal understanding of sociocultural phenomena may be achieved." Harris' sociocultural analog to the principle of natural selection is the principle of techno-environmental and techno-economic determinism.

This principle holds that similar technologies applied to similar environments tend to produce similar arrangements of labor in production and distribution, and that these in turn call forth similar kinds of social groups, which justify and coordinate their activities by means of similar systems of values and beliefs (1968:4).

At this point one could utter Louch's "so what's new" response. But there is more to it. As differential reproduction is to natural selection, cultural materialism is to techno-environmental and techno-economic determinism. Based on Marx's The Critique of Political Economy,

This strategy [or "law" of cultural evolution] states that the explanation for cultural differences and similarities is to be found in the techno-economic processes responsible for the production of the material requirements of biosocial survival ... that the techno-economic parameters of sociocultural systems exert selective pressures in favor of certain types of organizational structures and upon the survival and spread of definite types of ideological complexes ... that in principle, all of the major problems of sociocultural differences and similarities can be solved by identifying the precise nature of the selective parameters; yet as a general principle, it does not commit itself to the explanation of any specific sociocultural types or any specific set of institutions (Harris 1969:241).

If "... the explanation of biological transformations is to be found in the adaptative advantages (measured in terms of reproductive success), which particular innovations⁹ confer upon the organism and its lineage" (Harris 1968:241), then the explanation of cultural transformations is to be found in the adaptative advantages (measured in terms of thermodynamic efficiency) which particular inventions confer upon a society [the Law of Cultural Dominance]. Harris then proceeds to offer a materialist explanation of the difference between race relations in Brazil and the United States, as opposed to an idealist one based on Portuguese national character and Catholicism versus Anglo-Saxon racism and Protestantism, that is based first on ecology, then on migratory

patterns from Europe (labor), demography, industrialization, politics, militarism, and cognition. Perhaps he would consider explaining national character and Anglo-Saxon racism in techno-environmental terms. I doubt that anyone would argue that an explanation based on national character or racism could account for the historical data nearly as well as Harris' "material" explanation. Nor do I expect denial of the great importance of ecology and economics in human history.

But what are the logical entailments of using cultural materialism as a unitary theory or the true explanation? One thing one must not do is pay attention to the reasons people give you for their actions--that is emic and does not tell us what people actually do. Cultural materialism, Durkheimian sociology, British social anthropology, Freudian psychological anthropology, and French structuralism--especially that of Lévi-Strauss--

... are predicated upon the assumption that the actual participants in social life are incapable of an objective description of their own behavior or of a scientifically valid explanation of that behavior. All of these approaches thus share a common commitment to clearing away the errors of autoanalysis, the facade of ideology, the rationalized appearances of things, in order to penetrate into the deeper levels of both thought and action. ... The announced goal is to explain social facts in terms of social facts, rather than ideas in terms of other ideas.... The hypothesis that causal explanations reside in the material conditions of life enjoins an attitude of extreme skepticism toward the relevance of the manifest meanings of all verbal events (Harris 1968: 234).

The consequences of this assumption are enormous and far-reaching. Surely, if Marx, Engels, and Harris asked a Cheyenne why he spent so much time fletching an arrow with a certain kind, amount, and positioning of the feathers, they would not doubt his reply that not to do so would mean the arrow would not fly straight, he could not hunt game as well, and his family might go hungry. But what if they asked him why he and his fellow Cheyenne perform an annual ceremony to renew the Sacred Arrow bundle? Would they doubt his statement that not to do so would put the whole tribe in spiritual jeopardy, so their arrows will not fly true and their families will go hungry? Probably. They might reply that the real reason is that Cheyenne technology (arrows) and economy (hunting) have exerted "selective pressures ... upon the survival and spread of definite types of ideological complexes" (Harris 1968:241).

How does one determine which statements to doubt and which not to doubt? If one subscribes to the hypothesis "that causal explanations reside in the material conditions of life," then a materialist reply given by an informant is accepted and others rejected as being superficial idiosyncratic rationalizations. But he may be wrong about his materialist explanation; after all, he is deluded about why he performs the religious ceremony. We know he is deluded because the ideas or values of religion and spiritual jeopardy and sacred arrows do not explain the facts of

success or failure in hunting--in the materialist paradigm. At this point we are entering the realm of yet another disjunction, fact-value, an especially important issue in the social sciences. While I cannot take up the issue here for the attention it deserves, let me note that anthropologists of whatever theoretical persuasion by and large are deeply attached to this disjunction and accept and teach it as enlightened doctrine, which in the face of absolutism I suppose it is.

On the specific issue of warfare, Harris offers an explanation that appears to have 2 interrelated components: (1) competition for resources to support growing populations; (2) an adaptation to maintain populations below "techno-environmental carrying capacity," the absence of which would mean population control through malnutrition and disease. Both of these components are best understood in evolutionary terms, specifically principles analogous to that of natural selection--discussed to some extent above. The first component is straightforward and consistent with the economic explanations discussed below. The second is more difficult and its implications less clear.

Harris sees warfare as the inevitable result of competition for natural resources to support food production, which in turn leads to increased population, i.e., the enlarged food base precedes a rise in population. Subsequently, smaller and therefore weaker populations, in order

not to be eliminated by larger stronger ones, must increase their own numbers, which leads to intensified friction between populations and eventually to warfare. Food-collectors, because of their small dispersed groups with fluctuating membership and network of marriage alliances, have conflicts but not "true" warfare. Food-producers, however, experience intensified warfare, in competing for crop lands.

In-group identities increase and as a result whole village communities become each other's "enemies." The frequent occurrence of full-scale wars of annihilation by one low-energy agricultural community against another cannot be denied. These wars differ from modern wars only in scale, in effectiveness of the weapons of homicide, in degree of the organization of military exploits. If primitive wars were less genocidal or less brutal than our own, it was simply for lack of technique and technology (Harris 1971:226-27).

He assumes that the expansion of "high-energy" societies (those with higher productivity) is determined, and that preindustrial

... sociocultural systems are under constant ideological and political pressure to expand to the limit of their technoenvironmental carrying capacity. The larger the group, the more secure it is against attack. Hence in a given region each group tends to maximize its strength by getting as close as possible to its local carrying capacity (Harris 1971:225).

These assumptions, that expansion and attack are inevitable, are questionable: some sociocultural systems do, some do not. Furthermore, I am not clear whether or not maximizing population is supposed to be "unconscious" or conscious. There are examples in modern history where large families and

ideologies of "filling up empty space" are consciously planned and carried out, e.g., the settlement of frontiers by Euro-Americans, Germany in the 20th century, and Russia after World War II, but I am not clear on comparative cases for non-Western societies. The determination of "carrying capacity" itself is a difficult one, with problems similar to those of determining technoenvironmental efficiency and economic surplus.

Harris' second component is less clear, because he views warfare as an ecological adaptation of population control analogous to natural selection for bipedalism and the capacity to make tools in biological evolution, interlaced with conscious human motives such as contraception. "Since primitive peoples lack effective chemical or mechanical contraceptives, all primitive systems of population control alternative to war also involve suffering, deprivation, and the reduction of human welfare" (Harris 1971:229). Malnutrition and disease as a "system" of population control

... [are] ecologically even more dangerous than primitive warfare since the entire environment may become degraded through overuse, resulting in a permanent and irreversible decline toward extinction. Another alternative to war among primitives is to practice infanticide. To control population by killing one's own children, however, can scarcely be regarded as a major improvement over warfare from the point of view of human well-being (Harris 1971:229).

While contraception and infanticide are conscious means of controlling population (e.g., the mother who smothers a newborn because it will mean too-early weaning and weakened

health for the previous child gives this as the reason for her heartbreaking act), reducing population is not given as the reason for going to war except in rare instances such as genocide. Obviously, a group would not go to war to reduce its own population. Harris realizes that the public evidence for warfare as a population control mechanism is virtually nonexistent.

In a general sense, we can attribute the underlying causes of primitive warfare to population pressure in conformity with the theory [given above]. It must be admitted, however, that the evidence supporting this theory has been obscured because the motives that the belligerents themselves cite for going to war rarely indicate any awareness of population pressure (Harris 1971:227; italics added).

Harris explains the lack of fit between the etic and emic: the reasons people do give--revenge for homicide, trespassing, poaching, witchcraft, adultery, and woman-stealing--are manifestations of social and physical distress as the population saturates its technoenvironmental carrying capacity, and that the acts provoking such revenge can "consciously or unconsciously ... express the need of a group for more territory" (Harris 1971:227).

It may seem strange that the people who lose their lives in armed combat seldom accurately understand why they do so. But the masking of deeper causes by superficial psychological motives is advantageous for groups locked into a system of population control that depends on war. To understand the causes of war is to relieve the enemy of the onus of guilt, making it impossible to mobilize the adrenalin and other hormones necessary for effective hand-to-hand combat. The primitive group that is burdened by doubts is subject to annihilation (Harris 1971:227).

Thus, Harris has tied up the loose ends. Not only is warfare adaptive, but not knowing the real reason why one goes to war is also adaptive, otherwise one could not fight and would be eliminated. But recall in the discussion of Sahlins that the Tiv and Nuer go to war because they think they need land primarily, even though there may be no real need. I cannot conceive of individual human beings "locked into a system of population control that depends on war." I can conceive of them influenced by beliefs and values, varying with individuals, especially the belief where one life and one wrong must be paid for with another life and retribution.¹⁰ Finally, is it illuminating to treat the causal effects of war--reduction of population--as causes, which people's reasons, purposes, and intentions merely "mask"?

But Harris (1972) in later publications has clarified his theory of primitive warfare and population control, largely because of the influence of research by Divale (1970; 1971), who in turn has based his theory on Chagnon's Yanomamo work. I will discuss Divale's findings in a moment, but first a restatement of Harris' theory of primitive warfare as a population control device.

Whereas in 1971, Harris rejected infanticide as the primary means of population control, in 1972 he has changed his explanation so that female infanticide is the initial response to population pressure, and warfare is a consequence of this initial condition. Thus, when a given population

begins to experience pressure on resources, i.e., it is reaching technoenvironmental carrying capacity, in the absence of effective contraceptive and abortive techniques, infanticide, whether as a conscious act or through neglect, is the easiest way to control population.

Simple neglect of babies is perhaps the most common form of population control. This will begin to take effect at a point well below maximum carrying capacity as mothers, burdened by extra work, become less responsive to the demands of their children. The babies cry unattended for longer periods and the mothers nurse them less effectively or less often. In ecological perspective, the line separating infant neglect from infanticide is extremely thin. In few primitive cultures will the members admit that the murder of children is common. But unconscious deprivations can exert as much influence on infant mortality as deliberate infanticide (Harris 1972: 18).

This proposition would appear to be readily testable, if not with primitive societies, then with the large number of populations in the third world that have reached or already passed the carrying capacity of their environment.¹¹ Perhaps such child-care studies already exist; I am not familiar with the literature. Research on such a problem should be carried out by a woman in the field who should probably work solely with women informants--as evidence for trustworthiness at the very least: there are many important things that members of one sex simply do not want members of the opposite sex to know about.

The significant qualification to the practice of neglect or infanticide is that it is directed against female babies. Etically, Harris explains this as an ecological

condition of limiting brood stock--boys do not get pregnant. Emically, the decision to kill or the inclination to neglect female rather than male babies is an effect, in feed-back fashion, of the consequences of the initial case of female infanticide. That is, due to female infanticide there will be a shortage of brides for the males of that age group when they want to marry, so they raid other people to capture women, an act of war. Whether out of revenge or the desire to replace those lost women, the offended group will retaliate in kind. Once the practice of warfare becomes established, female infanticide will persist, but now due to pressure to produce male warriors. Thus, a woman-shortage will persist and, as a consequence, so will war. But Harris frames this chain of events in functionalist terms:

Thus the primary function of primitive warfare is not to kill off "surplus" males but to insure the continuation of high levels of female infant mortality. Much of what we nowadays regard as male chauvinism has its roots in this situation. One conclusion that I draw from this is that the whole complex of masculine aggressiveness is a by-product, not a cause of war (Harris 1972:18-20).

With this I must take issue. Primitive warfare does not "function" to "insure" anything. I would say instead that the primary effect of primitive warfare is not a reduction of a surplus male population, but perpetuation (if it ever existed) of female infanticide because of the need for males to fight. Mortality is not all female; males die, too, but as adults, in the pursuit of women, glory, wealth, or whatever.

Unpleasant as it may be, it is difficult to avoid the conclusion that warfare began as part of an ecologically adaptive system of population control. Death through combat strikes us as wasteful; yet for primitive peoples the alternative to war for balancing the adult sex ratios was the expansion of male infant mortality [an unconscionable act in many societies] (Harris 1972:20; italics added).

But what is the evidence for that initial condition of population pressure to which female infanticide is the response? The data upon which Harris bases the above explanation are census data collected by Divale for 112 primitive populations to demonstrate the hypothesis that:

... warfare plays a positive and beneficial role in primitive society and in fact is necessary for cultures at these levels of complexity. Primitive warfare is part of a syndrome which also includes female infanticide, polygyny, and marriage alliances. The almost universal occurrence of this syndrome in primitive cultures plus its important ecological role has led me to conclude that the syndrome constitutes the basic structural framework or template of primitive social organization. The syndrome's purpose is to control excess population and to maintain an equilibrium between a group's population and their available resources given their level of techno-economic ability to exploit those resources (Divale 1970:2).

The stimulus for Divale's hypothesis is Chagnon's study of Yanomamo warfare and his inferences about the relationship between female infanticide and warfare (1968a, 1968b).

Divale's own evidence is inferential, using demographic figures for 112 groups. Of these 112, 91 per cent have sex ratios in which boys outnumber girls in the "young generation," with an overall average of 146 boys:100 girls. He concludes that since sex ratios at birth are almost equal, this discrepancy can be explained only by the presence of

female infanticide (1970:3).¹² In the "adult generation" the ratios are equalized or tipped in favor of females as males die in warfare. For the 112 groups, the average adult ratio is 109:100 (1970:3), within what one could consider as a "normal" range. Unfortunately, the limits of that range are not specified. How many more males than females should one require before the practice of infanticide can be inferred legitimately, if it is not mentioned in the ethnographies? While I do not deny the existence or potential significance of female infanticide, I would like to see some consideration taken of alternative explanations of unbalanced sex ratios. For instance, among Orthodox Jews there are many more boys born than girls. Recent research on conception and sex determination have explained this particular case: for the Orthodox sexual intercourse is forbidden until 14 days after the onset of a woman's menstrual period. In a normal 28-day cycle, the 14th day marks the onset of ovulation, and the usually acid vaginal secretions become alkaline. Spermatozoa carrying a Y chromosome (which would determine male sex) are lighter in weight, faster moving, and more sensitive to un hospitable acid conditions than those bearing an X chromosome. Thus, if intercourse takes place on that optimum 14th day, the embryo is more likely to be male. Individual physiological idiosyncracies must also be taken into consideration, and when they are, that theoretical 1:1 sex ratio of live births seems unusual.¹³

Divale readily concedes that there are serious problems with the demographic data, but of another sort. First, a large part of his census data was collected long after colonial governments suppressed native warfare, so there are more adult males alive than he assumes would be under aboriginal conditions. Secondly, the evidence for infanticide cannot be supported by ethnographic accounts but must be inferred from the figures alone. After contact in an unspecified number of cases,

... the young generation ... sex ratios continue to show high proportions of boys over girls long after government control and prolonged missionary contact because infanticide is much harder to detect and because most westerners --anthropologists included--do not look for its practice. ... Primitive peoples quickly learn that government authorities and missionaries frown upon and try to suppress infanticide so upon being questioned by even an anthropologist they will often deny the practice. One such example are the Tsembaga of highland New Guinea who denied using infanticide except against twins when questioned by Rappaport (1968:15). However, their sex ratio for those under 15 years is 148 boys per 100 girls which can only be explained by the use of female infanticide (Divale 1970:5).

Undoubtedly, these would be difficult data to collect, particularly if the anthropologists also happened to be male. Recall that infanticide immediately after birth is carried out by women, not men.

As for statistical data on male deaths in warfare, Divale cites 2 examples: 24 per cent for the Yanomamo (Chagnon 1968a:140; 1968b:20), and 28 per cent for the Murngin (Warner 1930:481-82). His best case comes from demographic data on 5 Fiji Island tribes: in 1880 the total

population of the tribes was 18,028 and the average childhood sex ratio was 130:100. In 4 of the 5 tribes, the adult sex ratio was 96:100, but for the fifth tribe, males continued to outnumber females 125:100. Divale's explanation for the difference is that the fifth tribe had been under missionary control and without war for 40 years, while the other 4 tribes still carried on warfare (1970:7; Fison and Howitt 1880)174-75). Therefore, following Divale's reasoning, wherever population figures indicate a ratio of more males than females in childhood, against a base line of approximately 105 males to 100 females, one can infer the existence of female infanticide; wherever the figures indicate a ratio of fewer males to females in adulthood, one can infer (perhaps unnecessarily for this variable) the prevalence of warfare or, on the contrary, its absence if there remain more males than females.

Up to this point, I grant that Divale's hypothesis is attractive, but its testability has severe limitations, both in the nature of the data and in Divale's method, limitations that I shall continue to discuss in a moment. First, consider the problem of percentages. Chagnon (1968a:140) collected data on the causes of 240 adult Yanomamo deaths. Of these, 39, or 16.2 per cent, of all deaths were due to war and club fights, and of these, 33, or 23.9 per cent, were males. Contrast war fatalities with those of the biggest killers, malaria and epidemics (mostly malarial): 130, or

54.2 per cent of the adult population, of whom 58 (42 per cent of all male deaths) were men and 72 (70 per cent of all female deaths) were women. Only 3 out of 102 Yanomamo women died in childbirth. I suspect that this particular statistic is deflated because of the complications that malaria can cause in a pregnant woman. Furthermore, Chagnon notes that his mortality figures are probably underestimates:

The Yanomamo have very strong proscriptions on discussing the dead by name, and statistically adequate data requires complete genealogical information which is particularly difficult to obtain with respect to individuals killed in warfare. Their anguish at the mention of killed kinsmen precludes intensive questioning on this topic (1968a:140).

One Yanomamo village of 200 people was raided about 25 times between November, 1964, and February, 1966; 10 people (sex not specified) were killed, which is 5 per cent of the total population. From a treacherous attack during a feast in 1950, 15 people or 13 per cent (again, sex not specified) out of a population of 115 were killed (Chagnon 1968a:141).

These figures indicate that primitive war is often what it is not supposed to be--lethal. Divale's statistical reasoning in this vein may not be sound, however. In an attempt at comparable statistical statements while criticizing anthropologists for underestimating the deadliness of primitive warfare, he says:

... primitive populations are small and the male population is being controlled on a generational basis. For example, it would be a large village that had 100 adult males but even so, it would only take one killing a year over a 25 year period for one-fourth of those males to

perish in warfare. Thus the evidence indicated by the decline of males in the adult sex ratios plus the specifically observed adult male death rates from warfare of about 25 percent leads to the conclusion that primitive warfare was indeed effective and functioned to regulate the excess male population (1970:8).

Following this reasoning, if there were only 1 death per year over a 100-year period, 100 per cent of the adult males would have died in war. A death rate of 1 per cent per annum cannot be compounded into 25 per cent per 25 years and have the same meaning; the basis of the ratio is distorted.

Before leaving the problem of the deadliness of primitive war, let us compare Chagnon's Yanomamo figures to some from World War II, certainly a "lethal" "modern" "war" by anyone's criteria. Germany, with a population of 71 million, mobilized over 10 million soldiers, of whom 3,250,000 died; that is, 14 per cent of the population was mobilized, of which 31 per cent was killed outright or died of wounds. The U.S.S.R., with a population of 175 million, mobilized 22 million, and lost 7.5 million soldiers plus 7.5 million civilians; that is, 13 per cent of her population was mobilized and of this 34 per cent died; 8.6 per cent of her total population was killed or died in the war. Other horrendous figures include: Hungary lost 42 per cent of her 350,000 troops; Rumania lost 46 per cent of her 1,136,000 troops. For the Axis powers as a whole, 22 per cent of their 25.5 million troops, representing 11 per cent of a total population of 221 million, died. Out of the total Axis

population, 3.4 per cent died. For the Allied or United Nations powers, while Russia sustained the greatest losses, the United States with a total population of 135 million mobilized 12 per cent and of those troops lost a mere 2 per cent. The United Kingdom, with a total population of 48 million, also mobilized 12 per cent, but lost 9.4 per cent of them. Out of a total Allied population of 1.5 billion, only 5 per cent of the population were mobilized and, of those troops, 14 per cent were killed. Of the entire Allied population, 2.9 per cent were killed or died in war (Wright 1965: 1542).

Returning to the issue of inferring female infanticide and subsequent warfare from sex ratio data, I must first point out that it is misleading to refer to the demographic data from 112 populations as a "sample." Divale declares:

The sample is unbiased in the sense that it contains every age-sex ratio I was able to obtain in a year's search of the literature on primitive cultures. Of the 112 societies, 91 per cent have sex ratios in the young generation where boys outnumber girls: the average for the 112 groups is 146 boys per 100 girls (1970:3).

Geographic stratification of this sample is as follows:

North America, 34 societies; South America, 6 societies; Africa, 5 societies; Australia, 11 societies; Melanesia, 56 societies. These societies would be better identified as populations and the whole collection as almost a caricature of the Tylor-Galton problem. I do not wish to disparage Divale's theoretical contribution to understanding primitive warfare, or to undervalue the individual pieces of

demographic data he offers, but he cannot claim this study to be a quantified test of his theory. For example, Africa is represented solely by 5 Nigerian populations: 1 represents Edo-speaking peoples, the other 4 are all villages of Ibo-speaking people. As for Melanesia, 20 cases are from New Guinea; 13 are from the Bismarck Archipelago; 2 are from the Admiralty Islands; 5 are from the Fiji Islands; 5 are from the Solomon Islands; 1 is from Tikopia; and 10 are from Great Andaman Island! Of the 6 cases from South America, 2 belong to the Yumo of the Columbia culture area, and the remaining 4 are from Amazonia--3 of the 4 are Cashinawa villages.

Divale's statistical inferences are limited to one:

Of the 50 groups who were censused [sic] while in an unacculturated state, 90 per cent have adult sex ratios of less than 110 males per 100 females. While on the other hand, of the troupes who were censused some time after their warfare period, 63% have adult sex ratios of over 110 males per 100 females. A chi square test indicates over a 99 percent probability that the correlation between the adult sex ratios and the presence or absence of warfare is not due to chance alone. A Q test and a Phi test also show positive associations and correlations (1970:4).

The correlations are $X^2 = 33.67$; $Q = .879$; $\phi = .542$ (1970: 11). Divale provides no table showing distributions of cell frequencies, nor does he specify which are the unacculturated and acculturated societies or what constitutes "young" or "adult." Because of sampling bias, I must conclude that Divale's statistical correlations are spurious.

Nonetheless, disregarding the methodological problems that invalidate Divale's work in terms of quantification, and the potential of actual skewing because of indefinite demarcation between age groups, what do the population ratios as Divale has used them show? Table 2 gives selected examples of Divale's data, in the format he uses.

This demographic material is interesting, when taken case by case. Following Divale's reasoning, where males outnumber females in the Young Age Group, by inference, female infanticide must be practiced; when the males are outnumbered by females in the Adult Age Group, by inference warfare must be present; where males continue to outnumber females in the Adult Age Group, warfare is absent, probably due to European pacification. Looking at these figures, we know that the Yanomamo are the type specimen; ethnographic data support inferences made from the demographic data about the existence of female infanticide.

The Yanomamo also practice male infanticide, but because of the preference to have a male as their first child, they unknowingly kill more females than males. The Yanomamo have only three numbers: one, two, and more-than-two. They are, accordingly, poor statisticians. They are quite unaware of the fact that they do kill more female babies, and every time I questioned them about it, they insisted that they killed both kinds--"more-than-two" of both kinds (Chagnon 1968b:74).

It is not clear at all, however, how many more females than males are killed; Chagnon's evidence is not conclusive, and he says that his demographic data must show considerably less female infanticide than is actually practiced.

TABLE 2. DIVALE'S SEX RATIO DATA*

Sample Number	Group	Year of Census	Total Population	Males per 100 Females	
				Young Age Group	Adult Age Group
6	Cherokee	1715	-	94	98
8	Cheyenne and Arapaho	1880	-	114	112
14	Cree (Alberta, Saskatchewan, Montana)	1894	1,000	106	76
	Cree	1899	1,000	95	96
	Cree	1904	1,000	100	87
	Cree	1919	1,000	101	88
	Cree	1934	1,000	93	93
16	Crow Agency	1880	-	115	88
17	Eskimo, Alaskan	1839	6,560	103	106
18	Eskimo, Caribou	1929	-	145	80
23	Kiowa, Comanche	1880	-	106	72
34	Zuni	1935	2,036	123	136
Average, 34 North American Groups				118	86
	Cashinahua (3 villages)		206		
35	Balta	1966		136	70
36	Samuel	1966		237	92
37	Sika	1966		110	86
38	Yanomamo	1968	400	129	102
Average, 6 South American Groups				890	165
47	New South Wales	1846		118	155
53	Murngin	1930	3,000	143	70
55	Tiwi	1928	109	84	84
Average, 11 Australian Groups				10,258	131
59	Kapauku	1955	181	146	81
73	Maring (Tsembaga village)	1963	204	148	115
Average, 17 New Guinean Groups				423,348	174

TABLE 2 (Continued)

Sample Number	Group	Year of Census	Total Population	Males per 100 Females	
				Young Age Group	Adult Age Group
77	Gazelle Peninsula and adjacent islands	1914	27,853	124	114
80	Vitu Islands	1914	2,523	149	109
81	New Ireland (Namatanai District)	1914	11,739	114	100
82	(Kavieng District)	1914	14,749	126	151
87	Lihir Islands	1914	2,818	147	141
88	Tanga Islands	1914	1,241	126	121
Average, 13 Bismarck Archipelago Groups			98,399	124	124
Fiji Islands					
92	Viti-Levu District	1880	7,236	122	98
93	Tribe "A"	1880	1,381	138	98
94	Tribe "B"	1880	984	127	90
95	Wainimala	1880	1,719	134	99
96	"Mission" Lau	1880	6,708	129	125
Average, 5 Fiji Groups			18,028	130	102
102	Tikopia	1929	1,278	136	104
	Tikopia	1952	1,753	118	105
Great Andaman Island					
103	Aka-Cari	1901	39	300	106
104	Aka-Kora	1901	96	73	96
105	Aka-Bo	1901	48	70	93
106	Aka-Jeru	1901	218	185	122
107	Aka-Kede	1901	59	150	80
108	Aka-Kol	1901	11	300	300
109	Okō-Juwoi	1901	48	700	110
110	A-Kucikwar	1901	50	66	221
111	Akar-Bale	1901	19	300	50
112	Aka-Bea	1901	37	75	87
Average, 10 Andaman Groups			625	132	112

* Extracted from Divale (1970:16-23).

The Kapauku are the second type specimen, and the ethnography supports the existence of warfare but does not mention infanticide practices. Yet the Kapauku sex ratios show a more dramatic shift than those of the Yanomamo. The Zuni in 1935 were certainly not engaged in warfare, and there are many more males than females in both age groups. Thus, we are supposed to infer the practice of infanticide but the absence or discontinuation of warfare. Discontinuation is supported by frequent Zuni warfare reported for the 1880's. The Tiwi ratios show no change, and we know that female infanticide was unthinkable for them, given the high value placed on females even before they were born in marriage, alliance, and labor. We also know that the Tiwi fought, and frequently, yet male mortality was low in these encounters. Here, neither female infanticide nor warfare is having any demographic effect. Yet we would not know that they did have war on the basis of sex ratios alone. Looking at the ratios for Tikopia, it would appear that they practiced female infanticide and warfare in 1929, or something was happening to those excess males in the Young Age Group, or that in 1952 female infanticide was practiced less often. But we know from Firth's ethnographies that the Tikopia did not engage in warfare aboriginally. We know that the Andamanese frequently fought each other but not anyone else. But the ratios alone do not tell us much because they are extremely variable from group to group, probably because of the small

population size upon which the ratios are based. The addition or subtraction of just a few members of one sex to these populations would dramatically change the ratios. My point is, how trustworthy are the inferences made from demographic figures alone? For some cases the ethnographies corroborate the inferences, for others they do not and may even contradict the inferences. How are we to know which figures reflect life accurately and which do not, without checking out every ratio ethnographically? Problems of interpretation and intervening variables, in addition to those concerning the reliability of raw demographic data from primitive societies, would appear to make the demographic approach, however desirable, difficult indeed.

There are 2 other components in Divale's "warfare syndrome": polygyny and alliance through marriage. Where practiced, polygyny "worked as a built-in source of conflict as a result of an unequal distribution of women to insure a continuation of warfare" (Divale 1971:iv). But political alliances initiated and maintained through the exchange of women "worked as [a] regulatory device in limiting the intensity of primitive warfare" (Divale 1971:iv).

I have found that marriage rules of primitive societies tend to be explainable in terms of their particular warfare patterns. For example, Yanomamo and Kapauku marriage rules favor more concentrated flows of women and as a result build closer bonds between allied groups.... Conversely, Ibo marriage rules almost prohibit the establishment of strong alliance patterns by preventing Ego's generation from duplicating the marriage patterns of the previous one.... A tentative conclusion is that alliance

and marriage patterns are interrelated with the intensity of the warfare; that is, groups with weak alliances have more intensive warfare since no village or group has really strong bonds with another while conversely, groups with strong alliances rarely fight amongst themselves. These conclusions are very tentative however and do not contain the degree of supportive evidence that the infanticide, feuding, and polygyny components of the syndrome have (Divale 1970:9-10; italics added).

He is wise to be cautious. While the dictum "marry out or die out" is perhaps generally true, there are various ways of satisfying that necessity, with varying degrees of success in alliance formation. At one extreme, Sahlins (1968:60) describes the conditions arising from the most "inbred" alliance form, that of sister-exchange. The political community as a whole would be splintered into endogamous pairs who exchange women with and thus are allied only to each other. Or, what is more likely, the political community would consist of two halves (exogamous moieties), which form a self-sufficient whole. "Certain Amazonian tribes display just this form of dual organization, with each village a union of two intermarrying halves" (Sahlins 1968:60). At the other extreme, such reciprocal exchange is prohibited and lineages--especially segmentary lineages--become more important than marriage in political alliances.

What side do you take when your relatives by marriage dispute with your relatives by lineage? ... The contradiction is endemic in lineage systems, but it can be minimized by a "prohibition of parallel marriage"; that is, a bar on repeated unions between the same lineages (Sahlins 1968:62).

Consequently, a lineage has affinal ties to several other lineages, but while the absence of intense reciprocal exchange may create a wider network of marital alliances, the ties are weaker, and allegiance to lineage is preeminent.

Nonparallel marriage lends itself to a local consensus of belligerence toward everyone else. In case of dispute with another lineage, at best only one of the local families, directly connected by marriage with that group, might have reservations about the propriety of finishing them off (Sahlins 1968:62).

With these extremes in mind, let me reconsider Divale's assertion that Yanomamo and Kapauku are cases in which the patterns of woman-exchange create stronger alliances than among the Ibo. First, note that all 3 engage in frequent or continual internal warfare. Second, the Yanomamo case, Divale's paradigm case, is rather complex in patterns of social and political organization. Yanomamo villages are theoretically composed of 2 unnamed, exogamous patrilineages, and marriage is based on brother-sister exchange. Members of a lineage call all other members of the same sex "brother" or "sister," and males of one lineage call all males of the other lineage "brother-in-law" and all females "wife." Therefore, all "brothers" are in competition with each other for the "wives." If a third lineage should join a village, whichever of the existing lineages in the original dual organization establishes brother-sister exchange will find its ties to the other original lineage correspondingly weakened. Because of these shifts, and because of the

competition among "brothers" manifested in club fights over possession of women, villages readily fission. Once this happens, even though the males remain agnates, violence reigns.

The most bitter fighting, in fact, takes place between members of different villages who are related to each other agnatically. It is not uncommon for men to kill their classificatory brothers in the ensuing raids (Chagnon 1968b:66).

Residential units continue to fission as generations pass and population grows to the maximum tolerable level of about 200 people, or men fight over adultery, rights to marriage, and murder.

Although affinal ties (reinforced through brother-sister exchange) seem to be stronger in the long run than agnatic ties, matters of political alliance do not proceed smoothly, either. Yanomamo villages do form political alliances with other villages, "negotiating" through 3 stages: trading, feasting, and finally the exchange of women. The Yanomamo tend not to attack those villages with whom they trade and feast, but they will do so, especially if "some specific incident, such as the abduction of a woman, provokes them" (Chagnon 1968b:98). If revenge is taken for abduction, then abduction itself must be carried out in the first place by one member of the embryonic alliance. At any point in the stages of alliance formation, hostilities may break out. While the third stage, the exchange of women, does link villages together through affinal relationships, it

appears to me that what may hold these groups together is not completed exchanges but "the obligation to each other to continue to exchange women" (Chagnon 1968b:98; italics added). Parties to an alliance are very reluctant to enter into the third stage of obligation: weak villages may be afraid that they will not receive as many women as they give; strong villages force weaker ones into unequal exchange and do receive more women than they cede.

The weak, therefore, are compelled to exaggerate their strength by bluff and intimidation and by attempting in general to appear to be stronger, militarily, than they really are, thereby hoping to convince their partners that they are equals, capable of independent existence. By so doing, they also inform their partners that any attempt to coerce them out of women will be met with the appropriate reaction, such as a chest-pounding duel or club fight [which can escalate into war] (Chagnon 1968b: 98-99).

While this may be the ideal pattern of alliance formation, Chagnon notes the following discrepancies.

Rarely does [the ideal pattern] develop far enough to reach the stage where women change hands, particularly if the two villages concerned are of approximately the same military strength. Fights and arguments over women or food develop, and the principals withdraw temporarily on semi-hostile terms, perhaps attempting a rapprochement sometime in the future. Or, if the principals are obviously different military potential, the stronger of the two will coerce its weaker partner into ceding women early in the alliance development, taking advantage of its own military strength, thus altering the course of alliance development in the opposite direction (Chagnon 1968b:99).

That is, hostilities will begin again. Naroll, in his cross-cultural quantitative study of the deterrence hypothesis, discussed more fully in Chapter II, based on his small

sample found that the exchange of women did not reduce the possibility of hostilities. Unfortunately, nowhere does Naroll provide the frequency distributions and statistical correlations for his conclusions in regard to the hypothesis that cultural exchange manifested in subsidies, trade, and women reduce the likelihood of war.

Women--whether they are purchased, wooed, or raped--become part of their husband's household; the exchange of women, therefore, represents the most complete form of cultural contact. In many primitive groups men seek wives in communities which are also their potential military foes.

Our comparisons, regrettably, show no significant relationships at all between the frequency of war and these three measures of peaceful intercourse (Naroll 1966:20).

The exchange of women certainly has not lessened the likelihood of fissioning and conflict among Akwe-Shavante groups, either (Maybury-Lewis 1967). Nor do marital ties prevent the breaking of alliances among the Dugum Dani of New Guinea (Heider 1971). There is great conflict between moieties in each Shavante community and factionalism is not prevented by sodalities, although warrior age sets exist and are important in Shavante life.

So, where alliance theoretically should be strongest due to reciprocal exchange of women, there are a number of conditions to prevent or nullify such a harmonious relationship. Why are Yanomamo (and Shavante) alliances so fragile? Why are political relationships within and between Yanomamo villages so subject to disarray? Do we have a case of

natural man, a Hobbesian creature of every man's hand against every other man? What do the Yanomamo value more than alliance? Well-documented by Chagnon, it is bellicosity: the image and awareness they have of themselves. They would rather avoid full commitment to alliance because it goes against their self-image of individualistic self-sufficient bellicosity. Instead of accepting Divale's contention that warfare is a consequence of weak alliances, plus other conditions, one could argue equally well that weak alliances are a consequence of warfare--its frequency and the expectations and attitudes the participants have towards it.

Elsewhere, Divale (1971) gives a general "description" of primitive warfare, in which he states that "... primitive societies throughout the world lived more or less in a state of perpetual warfare," and "... since primitive warfare occurred almost universally, it is an indication that warfare served an important function in the cultural-ecological adaptation of primitive cultures," and it was so important that it "... pervaded almost every aspect of social and individual behavior.... This is to say that warfare was a normal condition in primitive culture and did not represent disequilibrium" (1971:iii). That is, warfare is a normal condition where bands fight other bands and tribes fight other tribes. But where chiefdoms, say, fight tribes,

... primitive warfare in such an instance was the result of the disequilibrium that resulted from the clashing of cultures of varying complexities. This type of warfare

could not go on indefinitely and would end when a new equilibrium was established; when, for example, the retreat of the tribals into a remote area that is not ecologically exploitable by the more dominant chiefdom (1971:iii).

At this point, I share Aberle's dismay at the use of the equilibrium model: when is equilibrium disequilibrium and when is it equilibrium? It would depend on whose side you were on. Surely for the "tribals" to be saved from further losses only because they retreat into lands the winners have no use for would place them on the short end of an "equilibrium." As Aberle puts the matter, "Conceivably all disequilibrating relationships between two social organizations can be shown to be equilibrating devices for one of the two parties in the conflict, but it seems doubtful that they can be seen to be equilibrating for both parties" (1968:99). A similar confusion exists in modern cold war politics, where one side recognizes the existence of a "balance" of power only if it has the edge in military superiority.

Without his specifically mentioning it, it appears that Divale's generalizations to the universe of primitive societies in his 1971 essay are again based on "type specimens": the Kapauku, the Yanomamo, the Central Eskimo, and Plains Indians.¹⁴ For example, the following generalization is obviously based on the Yanomamo material, whose "standard procedure" for treachery I find to be unusual, even in South America.

As with all warfare, treachery was an acceptable strategy of primitive societies. A standard procedure was for one group to invite another to its village or camp for a feast. In primitive politics inter-group or inter-village feasting was a process by which alliances were built. Even if the group invited to the feast suspected a double-cross, they would usually accept the invitation because to refuse might imply fear, which in tribal politics was an open invitation to be attacked. A third group in treacherous complicity with the host village would wait in hiding, to attack when the visitors were drunk or sleeping or about to return home. Sometimes the hosts themselves would suddenly turn on their unsuspecting guests. Treachery usually resulted in tremendous slaughter because the victims were unaware and in close physical proximity to their killers. It was not uncommon for most of the visiting men to be murdered and most of the women to be stolen (Divale 1971:vi).

In a footnote (1971:vi), Divale supplies 3 accounts to support this generalization: The Yanomamo, the Roman rape of the Sabine women, and the story in Genesis of Simeon and Levi, sons of Jacob, who took revenge on the non-Hebrew seducer of their sister, Dinah (Genesis 34:1-31).

Divale sees the "purpose" of primitive warfare to be revenge, mostly for adultery and wife-stealing, but at the same time he declares that "... it was rare for a raid to be conducted solely to steal women" (1971:viii). He further associates types of fighting (feuding, raiding, pitched battles) with population densities (bands, low-density tribes, and high-density tribes, and some chiefdoms, respectively). These associations are already familiar to the reader, but based on the variable of economics (Newcomb 1951, 1960). Divale specifically rejects economics as significant in primitive warfare.

Primitive warfare was conducted for purposes of blood revenge and not for economic motives. ... disputes over women were the major cause of primitive wars (1971:viii).

Many of Divale's (undocumented) generalizations are familiar and testable, but Divale is a functionalist and determinist and offers us, in the end, a unitary theory of robot man.

Several Anthropologists [sic] report that charges of witchcraft and sorcery were also the cause of many primitive wars. This notion, however, probably confuses cause and effect. As many primitive societies do not believe that death can occur from natural causes ..., whenever a death of this type occurs it is usually charged to witchcraft and sorcery. The kinsmen of the dead person naturally ask "who was the witch?" and the response, almost invariably, is an enemy of the dead man. The relevant point is that charges of witchcraft and sorcery were usually directed against individuals or groups where prior disputes were present (1971:viii).

Well enough and good. There is a vast body of data to support this straightforward conclusion, both on the part of the people and on the part of Divale. But Divale then suggests

"... that witchcraft be viewed, in respect to warfare, as a mechanism for maintaining group solidarity and hate for the enemy, rather than as a cause of primitive warfare" (1971:viii), at which point I throw up my hands in despair at the futility and dehumanization of functionalist-ecological "explanations." Divale's restatement of Ardery/Sumner/Hobbes' amity-enmity complex simply substitutes witchcraft for warfare as the means to achieve social cohesion, with even less success than Murphy's use (1957) of that hypothesis to explain Mundurucu warfare.

4. War and the exercise of power.

War is an evil, is a proposition so familiar to everyone that it would be tedious to develop. No one is forced to engage in it by ignorance or kept out of it by fear, if he fancies there is anything to be gained by it. I suppose that no one will dispute that we went to war at first in order to serve our several interests; that we are now, in view of the same interests, debating how we can make peace; and that if we separate without having as we think our rights, we shall go to war again.

--Thucydides

"Now what I have thought," said Arthur, "is this. Why can't you harness Might so that it works for Right? I know it sounds nonsense, but, I mean, you can't just say there is no such thing. The Might is there, in the bad half of people, and you can't neglect it. You can't cut it out, but you might be able to direct it, if you see what I mean, so that it was useful instead of bad."

--T. H. White, The Once and Future King

Matters of conflict and violence have commonly been treated as belonging to the political sphere: law reigns within society and war rages between societies. War is the absence of law. Yet there is a definitional oddity here. Repeatedly, writers refer to the rules of war; repeatedly, they separate primitive warfare from modern warfare by describing the former as game-like or sportive and, as with all games, subject to rules and, as with all games, not the real thing. Note the distinction we make in our language between war games, which the Pentagon or the Rand Corporation play, and war. The former is an enactment of war subject to certain rules, but in the long run, just a game. Indeed, if real casualties were to result from war games, a legal

investigation would ensue to determine negligence, malicious intent, or accident on the part of participants in the "game," because one of the rules is that no one on either side is supposed to get killed for real. Like chess. But there are also rules for real war, which is not supposed to be a game. On the one hand, "... in many societies of the world, the rules of warfare are so pronounced as to make the warfare seem a game," while on the other hand, "Modern twentieth-century warfare is, if not unique, at least rare in the sense that it involves the total society and an absolute minimum of rules commonly understood by both sides--the game element is minimal in 'total war'" (Bohannon 1963:304; italics added). So we have what seems to be a continuum: the more rules there are, the more game-like and less true is the warfare; conversely, the fewer the rules, the less game-like and more true the warfare. But there are still rules. The combatants may totally ignore them, but never publicly. Fighting by the rules, the Geneva Conventions for example, is part and parcel of making claim to the justice of one's cause. Breaking or disregarding those rules (and being caught at it) makes one theoretically subject to punishment. The Plains Indian war leader who lost any men on a raid is held responsible and must atone in mourning and compensation to the families of the dead warriors. Within the modern military hierarchy, each officer is held responsible for the conduct of his command, and the government as a whole discharges its debt to

the widows and orphans with a decent burial and compensation. That these examples are not strictly comparable may be attributed to differences in the centralization of political power and the assumption of responsibility for the death--it is now the enemy's fault that a soldier died, not that of his commanding officer who followed the rules. There are rules of warfare in both cases. What else should one be aware of in studying warfare? Bohannon (1963:305) asserts the following.

The necessary requirement in understanding warfare is to note that war is a type of relationship leading to a given mode of the multicentric system of political organization--the mode based on violence and minimal communication. There are many types of war, as there are many types of counteracting institutions within the unicentric systems. There are some places in which the war is decided by warriors in contest, and the major parts of the societies are not concerned. There are wars fought by whole communities--mobs in violence. There are wars fought by specialist bodies, called armies. Most North American Indian societies were typified by the first kind of fighting: the exploits of warriors were considered a dominant value in the culture; fighting usually involved only a few people, and consisted in rather loosely organized raids in which young men are given a chance to shine. They then came home, counted coup, and were rewarded with the best women of the tribe. ... their purpose was never to aim at a "just peace" under "our control."¹⁵

As a consequence, Bohannon continues,

... it may be unwise to call this sort of situation [raiding, head-hunting] warfare; it is merely a violent social relationship with what seems to us and probably to them an antisocial sort of expression (1963:305).

To which I can only add, yes--war is at the very least an "antisocial sort of expression."

But, although there are rules of warfare, we cannot call them laws. The extension of the rule of law proportionately diminishes the likelihood of war, as the elaboration of public law within society replaces private law--blood feuds, vigilantism. The solution to the problem of war offered time and again by anthropologists in the 1940's and 1950's was to establish a single political unit, a single in-group. Indeed, the term in-group would be meaningless because there would no longer be an out-group. Here I am speaking strictly of political structures and not of humanistic principles. In any case, external war may simply be replaced by internal or civil war, rebellions, and revolutions. Somewhere between the poles of law and war exists the perilous land of diplomacy, subject to neither the explicit rule of law or the implicit rules of war. Using Bohannon's (1963:305) conflict model, the diplomat within a critical but fluid period of time after a breach of norm must first establish commonality with the potential adversary and then convey this understanding to his own group. Unfortunately, if you do not care for war, too often the latter task is more difficult than the former.

Diplomacy is therefore a thankless job. Even when the cultures of the two [political] centers are very close together or even all but identical, diplomacy is a thankless task because it gets involved with the egocentric, ethnocentric in-group desires and cupidities of each of the centers, as a unicentric group. Indeed, they may understand each other only too well (Bohannon 1963:303).

If diplomatic efforts do fail for whichever of a great variety of reasons and war does take place, Bohannon sees the catastrophe as a means of re-establishing relationships on the old basis, leaving one with the chicken-or-the-egg dilemma, or on a new basis, thereby illustrating a dialectical process.

Bohannon defines war as "a contest having as its aim a peace in which the balance of power is shifted." New Guinean and North American raiders do not qualify because relations of enmity with their neighbors are permanent and continued enmity, not peace, is the end to be maintained (1963:305).

"True warfare," if we may call it such, is a somewhat different business. It has as its end peaceful settlement with new political conditions, not continuation of fighting. And obviously, when we find it, we are going to find societies in which it is carried out by specialist bodies called "armies" and others in which it is carried out by whole bodies of citizens. In fact, there is here something of the same difference as that between the moot and the court. It is, in fact, the difference between a lynching mob and an army. A lynching mob is a mode of a community; an army is a specialist organization within a state (Bohannon 1963:306).

Once again we are faced with trying to figure out into which definitional pigeonhole to place the group violence of stateless societies: if whole bodies of citizens in a stateless society fight to achieve peace, they are waging true war. As for the others, they could be either amateurs or professionals, but they are interested in war for its own sake. It is highly likely that my analysis of Bohannon's criteria is

prejudiced by the Orwellian paradox of waging war to achieve peace, a paradox whose expression in our own time has almost achieved perfection, perhaps best of all in The Report from Iron Mountain, a hoax taken seriously for too long a time, which drafted secret contingency plans for the unthinkable--that "true" peace should break out. I can impressionistically conceive of a developmental continuum of human conflict: In the beginning, people may fight for defense, then for the sake of war, then for the sake of peace (in Bohannon's terms), but then again for the sake of war--wars not as a means to peace (under different conditions, of course) but as a means to more wars. If such is the case, so much for "true warfare" in modern times: instead of war being an instrument of foreign policy, foreign policy becomes the instrument of war--or its specialists.

In careful appraisal of this intuitive curve, one must consider first the base upon which political power rests within a society. I accept, with some important qualifications, the paradigm of Harris, Fried, White, and Sahlin, i.e., access to basic economic resources. Using Fried's (1967) types of egalitarian, ranked, and stratified societies, and speaking at least typologically if not evolutionarily, band societies are egalitarian in the sense that everyone has equal right to tools and natural resources. If they are poor, they are equally poor and may not necessarily be so (cf. "What Hunters Do For a Living, or How to Make Out

On Scarce Resources," by Richard Lee, 1968). Order is maintained in such societies through the absence of superior fire power, through the option of moving away from potential conflict, through the threat of adverse public opinion and thus ostracism or death, and through the need for cooperation in all vital human activities. Of these, public opinion--the requirement of being accepted as a "regular guy" by one's fellows--is fascinatingly effective, even into our own time, whether the non-conformer be called "witch" or "communist" or "libber." The accused heretic is everywhere damned, although in complex societies this trivial truth may be obscured by the sheer weight of law and its administration. Everywhere, the prosecutors of non-conformers--whether shaman, diviner, priest, policeman, or judge--wield formidable political power, albeit sometimes unofficially. As for official political leaders of egalitarian societies, Harris caricatures their power:

Both headmanship and egalitarian chieftainship are likely to be frustrating and irksome positions. The cumulative impression conjured by egalitarian chieftainship among Brazilian Indian groups is that of an overzealous scout-master on an overnight cookout. The first one up in the morning, the headman/chief tries to rouse his companions by standing in the middle of the village plaza and shouting, "Everybody up for the fish-poisoning expedition! Let's get those women into the manioc gardens! Anyone for roof-thatching?" The egalitarian chieftain seems to cajole, harangue, and plead from morning to night. If a task needs to be done, it is the headman who starts doing it; and it is the headman who works at it harder than anyone else. Moreover, not only must the egalitarian chief or headman set an example for hard work, but noblesse oblige, he must also set an example for generosity (Harris 1971:385).

This headman has no means to physically force anyone to do anything, although he might use sanctions of public opinion and threats of witchcraft. If he does not behave as a good headman should, people will simply ignore him and there is not a thing he can do about it. Political power is diffused throughout the social group.

For such societies, Fried (1967:101) contends that even if ethnographers have described them as warlike, the time involved in preparations, battle, and ceremonials seems little. Bands do not build fortifications, do not stockpile food and materiel, do not provide any special military training for warriors, and do not possess tools specifically used in killing men as distinguished from those used in hunting.

The typical action is a raid involving few attackers; the appropriate word for what takes place seems to be clash--there is a sudden violent set-to and most of the participants return hoarse from screaming threats and insults but are otherwise unscathed. Again, it is tempting to compare this behavior with that of the other primates [those same primates whom, the reader will recall, Ardrey considers to be "evolutionary failures"] who, we are told, expend most of their energies in combat situations carrying out threatening behavior rather than actual onslaughts. This is not to say that warfare on the egalitarian-band level is devoid of casualties (Fried 1967: 102).

Fried suggests that the Siriono are possibly close to the norm of warlikeness of simple egalitarian societies: they do not fight with each other and they retreat from rather than fight outside groups. Other classics of peaceful bands include Shoshonean groups, the Tiwi, the Copper Eskimo, the Mbuti Pygmies, and the Yahgan. Yet the case is not clear.

Admittedly, the problem is a tough one. There are indications, for example, that the Vikings feared people whom they knew as "Skraelings," who seem to have been Eskimos, for these people paddled right out to the Viking ships and attacked the Norsemen fiercely. This also reminds us of the awesome reputation of some of the Andamanese who are reported to have destroyed many crews of wrecked vessels over the centuries.

This problem is worthy of much more serious attention than it has been given because the nature of warfare throws much light upon many problems of comparative sociology. The final revelations may be surprising. Some simple societies of the ethnographic present may be discovered through combined archaeological and ethnohistorical techniques to have previously been more complex, with the more complex structures having been among the casualties of massive contact (Fried 1967:104).

Note that the assumption remains that simple egalitarian societies are peaceful because they have neither the technological means, nor organizational principles of command, nor the tactics, nor the casualties to qualify as having war. Fried (1967:105) also comments on the absence of war leaders, i.e., "leadership of military affairs," as extremely significant: "... every man stands and fights or runs away by himself." The context of his comment is refutation of the Spencerian notion that centralization of political power is derived from war chiefs who will not give up their authority once the war is over, an important point to which I shall return shortly. The problem of the moment is what is meant by "war leader"--what does he do, when does he do it, and do the followers obey? In almost every society surveyed for the cross-cultural research I have done, if a people engage in war, there is someone designated as "war

leader." An exception are the Trumai, a depopulated society in a state of collapse, whose 43 members were no longer able to organize for anything, even subsistence activities (Murphy and Quain 1955). Furthermore, a war leader may lead in at least one and possibly two spheres: (1) the planning, preparation, and initiation of fighting; (2) giving command during the actual fighting. The crucial distinction is to what extent is his leadership one of authority, i.e., backed by force. Do warriors obey or do they not? Although our questions were phrased somewhat differently, Otterbein (1970) and I have searched the data for answers. Sometimes we are not in agreement. For example, Otterbein codes the Papago as having a high degree of military subordination, i.e., "warriors obey orders given by leaders," as opposed to "warriors frequently do not obey orders given by leaders" (1970:144), while I have coded the Papago as having a low degree of subordination, i.e., "an informal leader whom people obeyed because of respect but who had no means to force warriors to obey." Possibly Otterbein and I are asking different questions of the data, in which case the comparison is irrelevant. The variable I am trying to measure, as discussed further on in Chapter II, is the extent to which men voluntarily participate in fighting activity.

One point that does seem to be clear is that these simple egalitarian societies, when described by ethnographers, existed at the ends of the earth or in refuge areas.

Most of the egalitarian societies known to ethnography have been located in areas remote from the centers of complex cultural development over the past 5,000 or 6,000 years or more. Particularly as we approach our own time, we find egalitarian societies in relatively impoverished habitats, frequently extreme in climatic conditions and poor in natural food resources (Fried 1967:110).

Do they exist in such regions because they have been pushed into them by more aggressive societies? What is involved in the "pushing" or "being pushed"?

There are ...a few societies where men seem to find no pleasure in dominating over, hurting, or killing the members of other societies, where all they ask is to be at peace and to be left in peace. These societies are, of course, small, weak, technologically backward, and living in inaccessible country; only so could they survive the power-seeking of their uninhibited neighbors. ... As far as the history of these small tribes can be reconstructed, they have always chosen to retreat into ever more inaccessible country rather than stand their ground and fight with invaders. There is no reason to suppose that their psychological or physiological potentialities are different from those of their more aggressive neighbors, but their values certainly are; for them peace and the absence of quarreling and jealousy are far more important than a reputation for bravery and virility (Gorer 1968: 34-35; italics added).

Out of such reputations are prestige statuses made, which one encounters in rank societies, Fried's second political type.

These societies are characterized by equal access to tools and natural resources necessary to sustain life, but also by differential access to positions of prestige, positions "... somehow limited so that not all those of sufficient talent to occupy such statuses actually achieve them" (Fried 1967:109). Unlike the simple egalitarian societies, "... rank societies have managed to come down into our own historical period still in occupation, if not control, of

some of the most desirable stretches of the earth's surface" (Fried 1967:110). Rank societies are usually food-producers (with the exception of the Northwest Coast Indians), have larger and denser populations collected in permanent, autonomous, and significantly exogamous villages organized according to formal kinship ties of descent--not necessarily unilineal--where redistribution of goods is more important than reciprocity in economic integration and the role of village redistributor carries prestige and political status but not political authority (Fried 1967:110-19). Kinship is of primary importance because it is the organizing principle of a core group whose residence patterns are male-dominated (virilavuncu-patrilocal) lineages and clans.

All the sources of interpersonal conflict found in egalitarian society persist in rank society, as indeed they persist in all subsequently evolved types of society. Certain kinds of irritation not present in egalitarian society make their appearance in rank societies, although their expression may still be relatively subdued. For example, while access to basic resources within the corporate unit is not significantly altered, there tends to be much more consumer's property in rank society. Patterns of reciprocal exchange do operate to keep these things in circulation, but there is a qualitative break with egalitarian societies as accumulation of nonstrategic values is often the basis or means of validation of rank distinctions (Fried 1967:141-42).

That is, conflict can arise over who has right to a limited resource of non-vital but prestige value. Who has the right to care for and wear certain masks and religious paraphernalia; how much is one entitled to in an exchange with respect to one's status, in gift-giving, in damages?

Obviously, when people are concerned with rank and status, they are at least as concerned with lack of or threat to that rank as they are with recognition: receiving one's just due passes without notice, but anything less breeds trouble.

A line of demarcation between egalitarian and ranked societies is difficult to draw, as in all classifications, and Fried readily admits this. He sees ranking as analogous to biological preadaptation: "... after stratification sets in, ranking emerges in the form of a socio-economic class system. As such its significance is impossible to overestimate" (1967:154). Actually, ranking is significant enough in differential access to prestige resources. Fried does not have to explain backwards from stratification to ranking. As for warfare generally, Fried states that

... rank societies tend to be combative, ... many of them exist in what may be seen as a chronic state of war, and ... terror and psychological warfare are common means of maintaining group integrity in the face of competition for survival (1967:178).

There are some problems with Fried's identification of his third type, stratified societies, because as he admits, it is almost impossible to find stratified societies that are not states. Nevertheless, he assumes that most if not all states must have (rapidly) passed through a stage of stratification in their evolution towards the state. Fried defines a stratified society as "... one in which members of the same sex and equivalent age status do not have equal access to the basic resources that sustain life" (1967:186). The specifics

of those basic resources are variable, of course, according to geography, technology, and subsistence ideology--"the historically determined perception of the exploitable environment," in Fried's words. In simpler language a minority control access to basic sources of food and the majority must sell their labor to the minority in order to live. This labor can be used in several ways. The most obvious is in further food production; the most interesting for our concerns is as instruments of force--police and soldiers.

Why should people "give up" political control over their own lives in the transitions from egalitarian to state societies?

The question has long been a favorite one: Why have people permitted themselves to be seduced, bilked, murdered, or otherwise conned into relinquishing a condition of egalitarianism for one of inequality? The question, of course, is loaded, not only politically but culturally. Apart from being biased, however, the question is wrong in its implication that individuals faced a conscious choice and selected the alternative of rank society. I believe that the evidence is quite to the contrary and that events conceived retrospectively as cataclysmic actually passed without notice until they were fully accomplished. Rank society grew out of egalitarian society without the conscious awareness of the members of the society in which it occurred; I believe that stratified society and the state emerged in the same quiet way and were institutionally fully present before anyone fumbled for a word by which to designate them (Fried 1967:182-83).

Thus, according to Fried, transition from one type to another "just happens" at the very least, or more fairly due to specific "initiating conditions" in the environment:

... population pressure; shifts in customary postmarital residence patterns; contraction or sharp natural alteration of basic resources; shifts in subsistence patterns arising from such factors as technological change or the impingement of the market system; development of managerial roles as an aspect of maturation of social and ceremonial system. I explicitly reject warfare and slavery as initiating conditions (Fried 1967:196; italics added).

But how did the initiating conditions that exert what we must assume to be selective pressures in Fried's evolutionary paradigm themselves come about?

While I do not wish to recount Fried's explanations for each one of these conditions, I do want to consider population pressure for 2 important reasons: it has been a basic premiss in the previous discussion of Harris' and Divale's hypotheses, and it is a variable intercorrelated in my study. Fried is faced with the difficult task of reconstructing events in the original stratified societies in the Old and New World centers of subsequent urbanization, on evidence that is virtually nonexistent. Acknowledging this problem, he then analyzes stratification where it has occurred as a consequence of contact with more complex (and usually Euro-American) societies. His example is Tikopia, under contact conditions in which indigenous population controls--late marriage, limited births, and warfare--were eliminated by Christian missions so that population between 1929 and 1952 increased by 35 per cent and into famine (Fried 1967:199; Firth 1959:53). As a consequence, access to land, the basic resource, became circumscribed by more rules based upon

membership in small agnatic lineages, and something like rent had to be paid by non-members for land usage (Fried 1967: 200-201). Formulated as a generalization, then, land scarcity develops due to population pressure, land tenure rules become more rigid, although the rules usually delineate right to use rather than right to own. Fried does not explain what happens to the have-nots, or how delineation of land-holding rules becomes hierarchical, which, after all, is what stratification is all about.

It might be interesting to reorder Fried's initiating conditions in accordance with Driver's and Massey's (1957) well-tested evolutionary order of change: division of labor, residence, land tenure, descent, and cousin terms, but controlling for geographical region and language family. As Driver (1966), Jorgensen (1966), Chaney and Revilla (1969) have shown, in the long run geography and history are more significant than universal psyches and functions. Tentatively, I offer a revised scheme that integrates Fried's initiating conditions of stratification with Driver's and Massey's sequence of change and supplies some missing links: [basic resources]; "contraction or sharp natural alteration of basic resources"; [technology]; (division of labor); "shifts in subsistence patterns arising from such factors as technological change or the impingement of the market system"; (residence); "shifts in customary postmarital residence patterns"; "population pressure";¹⁶ (land tenure); (descent);

(cousin terms); [kinship-based stratification]; "development of managerial roles as an aspect of maturation of social and ceremonial system"; [property-based replacing kin-based stratification]. Following Fried's reasoning, the controlling minority of a stratified society would subsequently use force as a means to solidify their gains, maintain internal stratification, and enhance their property, position, and power.

It is a truism that many, indeed most, people do not realize a change has taken place until it is upon them, and that possibly under human conditions of short life expectancy, non-literacy, the habit of custom ("we have always done it that way"), conservatism, and even fatalism, the question of choice is alien. Nevertheless, some people do choose. Marvin Harris, despite all his emphasis on etics and the laws of history, offers a hypothetical reconstruction of events that makes the reasons, intentions, and purposes of the to-be-ruling minority very clear. This is what might happen. In redistributive economic systems, the redistributor (headman, "big man") maintains his prestige--remember he has no power--by demonstrating his generosity repeatedly in giving feasts, the produce for which he and his kinsmen have worked long and hard. Each feast must be more lavish than the last if the headman is to secure his position, which he obviously must want or why go to all that trouble? In the redistributive pattern, guests also bring food to a feast and

the banquet pot, sweetened by the headman, is divided up equally among the guests with special portions going to kinsmen who worked to provide for the feast in the first place. A larger and larger portion of the kitty is held back for the headman and his kinsmen. Feasts recur and guests bring their gifts, their attendance perhaps encouraged through the use of public opinion or the threat of witchcraft. More and more is held back until the host's share becomes a royal treasury used to purchase force whereby gifts to the headman become taxes to the king, non-payment of which is a crime against the state, a state headed by a royal lineage and administered through a bureaucracy of specialists (Harris 1971:392-403). There are numerous variations on this theme, but it is inconceivable that at least some of the people do not know exactly what they are doing and continually make specific decisions to achieve certain ends. As the discrepancy between the haves and the have-nots widens, proportionately more revenues must be used to maintain law and order.

Even when the state objectively provides the mass of citizens with a measure of security and well-being superior to that of egalitarian peoples, the expropriation of the peasant's output, the sealing off of habitats, and the demand for obedience to authority places the governing class in an essentially unstable and vulnerable position. The evolutionary viability of the state rests in large measure on the perfection of institutional structures that protect the ruling class from confrontation with coalitions of alienated commoners. These structures fall into two basic categories: (1) institutions that control the content of ideology; and (2) institutions that physically suppress the subversive, rebellious, and revolutionary actions of alienated individuals and groups (Harris 1971:406).

Harris (1971:406 ff.) goes on to list the ideologues of pre-industrial states such as the Inca, Aztec, ancient Egyptian, Roman, Medieval European, Oriental despotisms, and modern tyrannies, a background against which the United States of 1974 is an anomaly, an aberration, a relatively open society within which one can see more easily the maintenance of political power of the state in operation. He does not mention 2 states for which we have evidence that the rulers knew what they were doing to achieve their goal of centralized power and empire, Tlacaelel of the Aztecs and Shaka of the Zulus.

Tlacaelel, half-brother of Moctezuma I and equivalent of prime minister, elevated the god of war Huitzilopochtli to the position of most important deity in the Aztec pantheon. He decreed that a great temple be built to this god, and that the surrounding city-states become markets from which the Aztec could obtain food for their god, humans for sacrifice taken in war (Leon-Portilla 1963). One man isolated a minor religious cult and generated a mystical militarist pattern that grew steadily in scale and intensity until the Spanish Conquest in 1517. Three hundred years later, on the other side of the world, Shaka assumed a pattern of tyrannical government and military expansion begun by an uncle and out of it generated an empire. Although it is not clear that he was not influenced by European contact, he invented military tactics that quickly decimated or incorporated non-Zulu

populations and were successful even against the British for a time. His control was absolute and maintained by terror: any transgression, actual or potential, was a capital crime. Yet his subjects did not rebel, probably because of fear, but also because of material gains and the gratifications of success in an expanding empire (Walter 1969). These 2 men were innovators, people who designed and executed plans to achieve the ends for which they are remembered. The Aztec peasants and perhaps even the warriors may not have realized what those goals were. The Zulu common persons may not have understood the source and structure of their ruler's power. Perhaps those who did, died. In any case, it makes events more intelligible to believe that at least some of the people activating institutions knew what they were doing as they planned for and initiated both change and maintenance of the status quo. The determinist alternative is to believe that as History rolls on, human beings unknowingly fall into waiting roles and speak but do not understand lines that It has written.

There remains 1 more theoretical variation to consider here. Carneiro (1972) offers a deterministic theory for the origin of the state in which warfare is neither the effect of stratification (as Fried would have it), nor the cause of stratification (as Andreski would have it), but a necessary although not sufficient condition. Carneiro's rather elegant theory is based on the idea of circumscription, whether

environmental or social, which stimulates warfare that in turn stimulates a progression toward political coalescence.

Thus,

... there is little question that, in one way or another, war played a decisive role in the rise of the state. Historical or archeological evidence of war is found in the early stages of state formation in Mesopotamia, Egypt, India, China, Japan, Greece, Rome, northern Europe, central Africa, Polynesia, Middle America, Peru, and Colombia, to name only the most prominent examples (1972:426).

Carneiro sees warfare as a necessary but not sufficient condition. The sufficient condition is circumscription. For the environmental type, land used for food-production is bounded by geographical barriers: mountain ranges, deserts, or oceans. His examples for comparison are the coastal valleys of Peru and the Amazon basin. In the latter, as horticultural populations grew, there were vast forested areas into which land-hungry people could spill, rather than compete with their neighbors over a limited land resource.

Warfare was certainly frequent in Amazonia, but it was waged for reasons of revenge, the taking of women, the gaining of personal prestige, and motives of a similar sort. There being no shortage of land, there was, by and large, no warfare over land (Carneiro 1972:427).

The defeated, rather than becoming subject or tributary to the victors, could simply move away and open new gardens in uninhabited areas of the forest. Thus, Carneiro concludes, warfare stimulated the spread of horticultural tribes throughout the Basin, who continued to live in autonomous dispersed villages. Nevertheless, he infers that there may

have been situations of social circumscription operating in Amazonia, basing his argument on the now familiar Yanomamo case. While the Yanomamo live in uncircumscribed rain forest, population density and consequent strife are most intense towards the center of Yanomamo territory and diminish as villages are located closer to the periphery. War is most intense in the center because those who are land-hungry can satisfy the demands of population pressure only by taking land away from other Yanomamo, and those who are in danger of losing have no place to escape to. As a consequence, we have a situation in which Yanomamo fight other Yanomamo, the intensity of such warfare decreasing as one moves outward from the center, with those Yanomamo on the periphery not fighting any non-Yanomamo. Contrast the expansive effects of such pressure with the Tiv case, discussed at length earlier, where those on the periphery are forced to fight non-Tiv to replace lands lost to other Tiv. While subsistence base is somewhat comparable between the 2 cases, the concerns of lineage organization are different. Yanomamo lineages focus on the exchange of women in marriage; Tiv lineages focus on internal solidarity and land tenure.

An explanation based upon environmental conscription is more straightforward. In Carneiro's Peruvian example, he presents an inferential reconstruction of political development from autonomous agricultural villages to imperial status as follows. Agricultural land was limited to 78 small

valleys, "... backed by the mountains, fronted by the sea, and flanked on either side by desert as dry as any in the world" (1972:427). As autonomous villages grew in size, they fissioned, and as available virgin farm land decreased, intensity of cultivation increased and marginal lands were made productive through terracing and irrigation. But at some point [doubtless below Harris' ceiling on the techno-environmental carrying capacity of the Peruvian valleys] physical struggle specifically over land within a valley began.

With increasing pressure of human population on the land ... the major incentive for war changed from a desire for revenge to a need to acquire land. And, as the causes of war became predominantly economic, the frequency, intensity, and importance of war increased (Carneiro 1972: 428).

The outcomes of these wars were very different from those waged in Amazonia. Losers had no place to escape into, and if they were not annihilated, they became politically subordinate tributaries to the victors, resulting in supra-village political units, i.e., chiefdoms. As population continued to rise, putting even greater pressure on arable land, competition took place between chiefdoms, with political subordination once again the outcome of war, and the larger, more complex victor was then a kingdom, and a valley embraced a state. The last and final step was conquest of valley-kingdom by valley-kingdom, the winner of this Peruvian tournament to be the international grandmaster. The contest was

won by the Incas, who controlled a single empire. The aggregation of larger political units was accompanied by increasing internal complexity, specifically the development of an elite to administer conquered political units.

And it was the individuals who had distinguished themselves in war who were generally appointed to political office and assigned the task of carrying out this administration (Carneiro 1972:429).

Administrative duties were of the sort that, in my estimation, increase in efficiency in proportion to the threat or exercise of force: maintaining law and order, collecting taxes, mobilizing labor for public works. So we have, in Carneiro's scheme, an upper class composed of a military elite, the ruler, and his kinsmen supported through taxation of conquered food-producers; a lower class of prisoners of war becoming servants and slaves; and a "middle-class" of those dispossessed from their land by war but not themselves enslaved who moved into larger communities and sold their labor as workers or artisans to the upper class and received tax goods as payment.

Areas of circumscribed agricultural land elsewhere in the world, such as the Valley of Mexico, Mesopotamia, the Nile Valley, and the Indus Valley, saw the process occur in much the same way and for essentially the same reasons. In these areas, too, autonomous neolithic villages were succeeded by chiefdoms, chiefdoms by kingdoms, and kingdoms by empires. The last stage of this development was, of course, the most impressive. ... But, in a sense, empires were merely the logical culmination of the process. The really fundamental step, the one that had triggered the entire train of events that led to empires, was the change from village autonomy to supravillage integration. This step was a change in kind: everything that followed was, in a way, only a change in degree (Carneiro 1972:429; italics added).

And apparently this quantum political leap did not take place until about 5000 B.C.

Under pressure probably of the mystique of unitary theory, in order to account for exceptions Carneiro extends the range of his theory to include chiefdoms that existed along the Amazon, arguing that they developed because of resource circumscription. Carneiro uses the term "resource concentration" and says that it "amounted almost to a kind of circumscription" (1972:430), whether the resource be the basis of a food-collecting (such as the Northwest Coast tribes) or food-producing economy. In summary, Carneiro's theory simply states that political centralization comes about through warfare relative to the scarcity of resources due to geographic or social conditions of circumscription.

If Otterbein's 1970 cross-cultural results are reliable, they seem to lend support more to Andreski and Carneiro than to Fried: a centralized polity cannot persist without the backing of a sophisticated military force, but a society can be efficient militarily without political centralization (1970:70-76). In his Foreword to Otterbein (1970), Carneiro is most impressed by Otterbein's observation that

... in war the test of fitness is applied, not just to military practices but to societies themselves.

And, Carneiro continues:

The ultimate test of fitness, of course, is survival. And no matter how well adapted a society may be in other

respects, if it proves unable to cope with its enemies it has failed in its over-all adaptation and must give way.

...

When societies fight, the cultural equivalent of natural selection comes into play. ... As societies compete, the less well adapted tend to fall by the wayside, leaving outstanding those best able to withstand the competition.

From the point of view of the traits involved, ... more efficient traits survive and spread, while less efficient ones decline and disappear. ...

Cultural selection, which operates even on traits of little or no adaptive value, acts with special intensity on traits directly concerned with survival. And since there is generally no greater challenge to a society's existence than war, it is here that we find selection operating most rigorously. ...

Coldly viewed, warfare has enormous ecological significance. It is concerned, after all, with a most vital aspect of a society's environment--its enemies. If waged successfully, war means the preservation of a society's integrity and independence, and the defense or even increase of its territory and resources. If waged unsuccessfully, warfare may mean defeat, subjection, or even extermination. This is ecology in spades! (Otterbein 1970).

Once again, and even more significantly since what is subject to evolutionary processes here is the very political survival of a society, we must conclude, including Otterbein's 1970 findings, about which more later, the following:

Those organisms best adapted to a given environment will tend to live longer and produce more offspring than those organisms less well adapted. Those cultures with the most efficient energy-capturing systems will tend to increase and spread at the expense of those cultures with less efficient energy-capturing systems. Those political communities that can capture, maim, and kill their enemies most

efficiently will tend to expand into the territories of those political communities with less military efficiency. And we know which organisms, cultures, and political communities are best adapted, most efficient, and most successful because they reproduce more, spread farther, and kill better. Like one lost in the woods, the eco-functionalists have led us around in a circle.

Answers

"Have you guessed the riddle yet?" the Hatter said, turning to Alice again.

"No, I give it up," Alice replied. "What's the answer?"

"I haven't the slightest idea," said the Hatter.

"Nor I," said the March Hare.

Alice sighed wearily. "I think you might do something better with the time," she said, "than wasting it in asking riddles that have no answers."

--Alice in Wonderland

Like Alice, I am weary of looking for an answer to a riddle that may not have one. At least, there may be no single answer, no one testable generalization that will cover all cases. In order to be consistent with the principles of theoretical validity set forth earlier, it is highly unlikely that there is any single set of generalizations that can be subsumed under a law-like statement when we are dealing with human behavior. At the same time, if I follow my conception of human action to its logical conclusion, there is the danger of ending with a description or map on the scale of 1

to 1, a condition of coming around full circle and duplicating the world, at the very least a physical impossibility. So somewhere between hither and yon, now and then, micro and macro, light and dark, there is a land of meaningful understanding. Perhaps Harris and others like him would identify such an area as that of "middle-range" theories and mushy ones at that, but I contend that this interstice holds explanatory power that may be closer to real human beings acting in a real world. This interstice has been variously labeled, and often dismissed, as "semantics" and "cognition." Chaney (n.d.:18), in his concern to return destiny to human control, makes a similar claim for meaning:

Both the unique and universal in the space-time-mode-meaning-significance continuum of human existence are to be explained in terms of the emergent quality of man's pan-human meaning-mediated existence. What we call a culture, society, or "periodization" in human history is better conceptualized as an artificial, historical (spatiotemporal) clustering of local distortions in the space-time-mode-meaning-significance continuum of human existence.

This view appears to be consistent with that of Bidney, that because culture is an open system subject to human self-awareness, it is indeterminate (1967:17-18). I must take issue, however, with Chaney's description of all cultures as artificial and distorted, because one does not know what would subsequently be real and undistorted.

Patterns do exist in human actions. As individuals we tend to behave with some measure of consistency. We share with our neighbors and our children similar but not identical

ways of accomplishing daily tasks in everything from when to introduce solid foods to an infant to how to govern our communities. Our lives are guided by norms and standards against which we continually measure ourselves, neighbors, children ... and strangers. Chaney (n.d.:18) conceives of sociocultural patterns as

... holding in terms of the continuity of a culture (habituation and conditioning of human, artificial paradigms for expectation and action differentially reinforced through the satisfaction of primary and derived "urges"), rather than in terms of something inherent in sociocultural phenomena analogous to "cosmic glue."

Thus, we learn to behave in certain acceptable ways and are taught by other human beings. I have some doubt that teaching and learning are suitably explained in behavioristic terminology, but that is not an issue here. Again, Chaney's view appears to be consistent with Bidney's (1967:17-18), "that some degree of cultural determinism characterizes human life, and that cultural anthropologists may study comparatively the significant correlations between culture, personality, and society." If the reader will recall from my initial discussion of determinism, it is not at all clear that culture is indeterminate but that a human within a culture is determined. Yet Chaney's distinction must be observed: it is only what people learn from other people in a particular group that can be spoken of in some measure of determinism, not that classes of sociocultural phenomena inherently and therefore universally hang together, like magnetism or

gravity. I must object, however, to the notion of "artificial paradigms" and instead offer Spradley's and McCurdy's (1971:4) conception of culture or its paradigms as "arbitrary," i.e., there are many cultural solutions to a problem of human life.¹⁷ This term avoids the logical pitfalls of both "artificiality" and relativism. I speak of interstices, Chaney (n.d.:19) speaks of transcendence:

I wish to stress that the present discussion attempts to transcend disjunctions such as absolute-relative, idealistic-materialistic, rational-empirical, free will-determinism which erode into (1) searches for the direction of the causal arrows or (2) anything goes. Rather, the stress here is that human beings exist in variously interrelated conceptual plots which mediate their existence.

So in the end we again face the old ultimatum of either order or chaos, find both lacking, and seek a third alternative. At this juncture, however, we take different paths: Chaney describes conceptual plots as mediating existence, that is, standing between a human and the world; I contend that those plots are the world.

Let me try to make this distinction clearer by discussing how anthropologists have talked about cognition and semantics. The great contribution of transformational grammar has been to come to grips with the problem of meaning in language, something descriptive linguistics is unable to handle. But the theory is full of difficulties and at bottom rests upon a deterministic position of inherent linguistic structures in the human brain. Cognition studies have become

enormously popular, but they too rest upon a view that sees culture as some sort of screen, sieve, or map through which experience filters, giving only a distorted view of the world and separating us eternally from knowing the world "as it is." Since cognition and semantics deal primarily with language (we do field work by talking to informants--we learn about their way of life to the extent that we can understand what they say to us), what have those working in ethnographic semantics accomplished? Judging from Colby's (1966) survey article, results in terms of "the semantic codes of a particular speech community" are disappointingly slight, especially for those convinced of the significance of the actor's explanation--which some have supplied with the generic name of ethnoscience. Yet we do know a great deal about the languages, values, concepts, and Gestalten of other cultures. Anthropologists have been able to collect such data by learning the language, talking with people, and participating in their lives. The difficulty, I think, hinges on the search for those codes: efforts are bent towards boiling down words and word-making to first principles from which alone can be generated statements that are linguistically or conceptually acceptable to native speakers. This is determinism with a vengeance. Unlike descriptive linguistics, "Ethnography still lacks anything comparable to the phonemic principle" (Colby 1966:16), i.e., a classification of meta-meanings whose distribution can account for all "semes" in a culture.

Colby's use of "lacks" and continued discussion in his article assumes that the discovery of such a principle is possible, that meaning like sounds can be reconstituted, like orange juice. Another revealing illustration of the mental set of those searching for order in meaning is the work of Katz and Fodor (1963) on semantic rules. Colby (1966:10) describes their contribution as

... a semantic metatheory describing constituents, objectives, and constraints of a workable semantic theory. According to the authors, a semantic theory accounts, without recourse to the context (either linguistic or non-linguistic) for the speaker's ability to interpret a sentence of his language. A theory which accounts for contextual influences on the interpretation of an utterance would have to represent all the speaker's knowledge of the world, a requirement which the authors consider unrealistic (*italics added*).

But, while dropping context because its inclusion would impede the development of a generative theory, Katz and Fodor state that a

... semantic theory should account for sentence interpretation through (1) determining the different readings possible, (2) detecting semantic anomalies, and (3) deciding on paraphrase relations between sentences (Colby 1966:10),

goals that seem impossible to even explain without reference to context! Dreyfus (n.d.) argues that it will be impossible to program computers to use language (other than programmed translation "tricks") because language is context-dependent even for computers, and you cannot program the machine for all possibilities. Exclusion of context, moreover, means that artificial intelligence is "unrealistic."

The article of faith sustaining anthropologists, as opposed to linguists, interested in contextual factors in semantic theory is that non-linguistic contexts are limited by commonality. Colby (1966:13) presents this view:

Actually, there is no reason to assume that the non-linguistic context of speech, as conceptualized by the speakers, is so unique and rooted in particularity of time and setting that it cannot be characterized relatively parsimoniously. The anthropological faith that non-linguistic context need not be treated encyclopedically--that somehow it can be made more simple--is reflected in frequent reference to "cognitive structures" and related concepts (*italics in the original*).

The results, e.g., two classics, Frake's "The Diagnosis of Disease Among the Subanum of Mindanao" and Conklin's "Hanunoo Color Categories," are obsessed with category, system, and structure, immobilizing meaning and thus human life in plexiglass. Once again the search for order is triumphant, every pin and needle accounted for, the cultural audit complete.

But all is not well in cognitive theory. D'Andrade (n.d.) presents data that he thinks are evidence that social scientists construct cultural reality falsely. Three sets of observations have been collected of what D'Andrade assumes is a single event: lengthy discussion among groups of 3 undergraduates each, observed through a 1-way mirror by an investigator. The first set of observations consists of "immediate recording" by the investigator of the social behavior of each subject. The second consists of each subject's rating of the social behavior of the other 2 members of his group after the discussion event. The third consists of the

investigator's ratings from memory of the behavior of each subject. The epistemological assumption behind the test was that if the results of any set of data did not agree with the other 2, or if all 3 disagreed, the data were therefore invalid.

With these three different types of data it is possible to carry out the same kind of analysis of each type of data, and then to compare the results. Such a comparison gives a general indication of the validity of the techniques, since two techniques which yield non-similar results cannot be valid measures of the same thing (D'Andrade n.d.:15).

According to the findings, ratings of behavior recorded during the event by the observer are dissimilar from either set of ratings made by actors or observer after the event-- "long term memory." D'Andrade concludes that "memory drift" of how other people behave moves "in the direction of the rater's conception of 'what is like what'; that is, in the direction of the rater's construction of reality." D'Andrade's conclusion is that we are unable to pry culture off the world, and that this is a deficiency.¹⁸

As a result of this type of memory drift, any attempt to discover how human behavior is organized into multi-behavior units, such as dimensions or clusters, which is based on data consisting of judgments based on long term memory will result in conclusions which reflect primarily the subjects' construction of reality, not the world as it is (n.d.:18-19; italics added).

Therefore, reconstruction of social behavior (which includes most ethnography) is invalid.

If the argument presented here is correct then a large number of social science studies are brought into question. Studies which are based on correlations from

memory based check-lists, ratings, or interviews are obviously placed in the category of dubious findings. Studies in which the correlations are based on questionnaires in which the respondent answers on the basis of his recollections also are placed in doubt (n.d.:20).

D'Andrade does not discuss solutions or provide a measure of what constitutes long term memory. The logical implications of his reasoning seem to be that (1) since actors probably cannot interact and analyze their interactions simultaneously, any emic observations cannot tell us what is "really happening"; (2) any after-the-fact judgments of the observer's are also invalid; (3) the only way for the social scientist to get at the "world as it is" is to describe events as they happen, which implies further that (4) an observer cannot be a participant or in any way be present in the event, otherwise the indeterminacy principle comes into play; and lastly (5), it would seem that the only way anthropologists can do their work is to watch people as George Schaller watched lions--from behind a blind, or a 1-way mirror; one certainly could not talk to them. The culture here-world there view can become ridiculous in addition to being wrong.

Generative grammars, rewrite rules, semantic theories of all sorts are trying to achieve one thing: to reduce language and meaning to a basic recipe with directions for making everything else out of it on the assumption that language acquisition and use, and therefore linguistic understanding, proceed in certain orderly, rigid, predictive ways. Whether it is emic order or, in the long run, etic order, the

predication of sifting out generative/predictive order is probably based on the reality that we can use language to fit correctly an infinity of situations--we do not speak randomly or accidentally. So, by extension, everyone is busily trying to nail those patterns down in what he understands to be a scientific manner. But despite Chomsky's dismissal of an "encyclopedic theory" (Colby 1966:13), we cannot understand a part without the whole--in this case the sociocultural contexts in which language is used--and if we can never encompass the whole, which bits do we include and which do we leave out? If you put back together the parts isolated in your analysis, have you reconstituted the whole itself? a simplified version? a primitive version? something that has little resemblance to the original? When you peel layers off an onion, do you still have an onion? Can you put the layers back together again and restore your onion? So much for the mediation of experience.

Yet there is pattern, accumulated experience, and expectations. How are conceptual plots the world? Perhaps a primitive analogy will help to understand the nature of the interstice.

We have a picture puzzle of 600 pieces. If one tried to put the puzzle together randomly, trying to match every projection to every notch, assuming 2 nubs and 2 notches per piece--and not adjusting for edge pieces--there is 1 chance in 1200 of joining the first 2 pieces; then 1 in 1198 for the

next match, 1 in 1196, 1 in 1194, and so forth. The puzzle would take a very long time to put together. The supreme challenge would be an all-white, circular puzzle. It is a gimmick because it is not an ordinary picture puzzle. But, disregarding gimmicks, what about a puzzle with a picture? How to begin? We can sort the pieces according to edges and corners, color, or lines. Doing a puzzle without a helpful picture of the completed puzzle by your elbow would be possible, but it would still take a long time. To put the puzzle together according to the picture is to duplicate the picture. Without that helpful picture, can one generate the picture out of the sorted categories? Only superficially. For example, this puzzle has a lot of blue (sky?) and green (trees?), and a small pile of something red. Only when the piles are put together does one realize that the blue is probably sky (see that bit of cloud) and the green is a grove of trees. Yet that does not tell us much about the whole. There comes a point at which the puzzled person ceases to be puzzled and says, perhaps, "I've got it!" or "Now it's coming!" He sees what the completed picture will look like, and this seeing can occur with the fitting of a single piece. The remaining pieces make sense through reference to the whole, which has become--built up piece by piece but dependent upon relationships between pieces--the context. If the picture were of something out of the ordinary, for instance blue trees and pink sky, the task would be more difficult

because the puzzle would not refer to the real world. If the puzzle were of a Jackson Pollock painting, the moment of seeing would be delayed for an even longer time, but not so long as with our gimmicky all-white puzzle.

We use language in a roughly similar fashion, although to maintain an analogy between words and puzzle pieces, we would have to limit analysis of our language user to an average middle-class American $2\frac{1}{2}$ year old child, and yet that child could not possibly even begin a picture puzzle of 600 pieces. The point about context--whose inclusion the current crop of cognitive anthropologists dismiss as "unrealistic"--is the crucial one, and the sense in which I am talking here about semantics and cognition. As J. Nammour (1973) has shown, it is impossible to separate language from the world we learn to know and talk about through language, and since language is an essential part of culture, on a larger canvas we cannot separate culture from the world, or know the world in any way other than in culture. Puzzle pieces make sense only against the larger context that they in the end form. Puzzle pieces are the context. Words make sense only in the context of human experience. Language is experience. Non-linguistic human behavior makes sense only in the context of human experience. Culture is experience. Violent human actions are intelligible in the context of human interests. Reasons, intentions, and purposes are interests.

The significance of such interests may be becoming apparent to some anthropologists doing research on warfare, but they still retain allegiance to the older order. By 1969, Vayda had modified his eco-functional position to question determinism, stressing the confusion of causal-effect with first cause, and admitting the significance of human thoughts and feelings, and of history. He counters White's culturological view of cultural evolution with a view that sees cultural evolution as less predictable, more random, and more fortuitous in relationships between environment and technology.

What do these contrasting views imply for the interpretation of warfare among the Borneo tribes? Newcomb concluded his study of Plains warfare by saying that the "Plains cultures were warlike because they had to be," [1950:239] and we may suppose that Iban culture could, likewise, in a manner consistent with White's view of cultural evolution, be interpreted as warlike because it had to be. No such imputation of necessity or inevitability is entailed by the other view of cultural evolution [which] ... allows us to note, where data are available, the particular conditions under which the Iban pattern of fighting arose and to note what we may call ... the role of "historical accidents" in its origination. Thus we can cite evidence on the recency of the custom of head-hunting among the Ibans; the indications that the Ibans took it over from other tribes among which it had been established since an earlier time, although in a different ritual context; and the indications that the Ibans were encouraged in head-hunting during the first part of the nineteenth century by the Malays who made a practice of rewarding their Iban associates in piracy (prior to its suppression by Rajah James Brooke) with the victims' heads, as well as sometimes with a share of the plunder and slaves.... What needs to be emphasized about these various circumstances ... is that in the early years of Iban head-hunting the thoughts and feelings of Iban individuals can be argued to have indeed mattered [*Italics added.*]. If, as I think is the case, we cannot insist that the adaptive effects conducive to a

continuation and spread of Iban warfare were operating from the outset, then whatever it was--whether Malay influence or something else--that made the Ibans in the beginning think that head-hunting was a worthwhile thing to do and the very fact that they did think so may be important. It can be argued that had they thought otherwise and had they not taken up the practice--and it is possible that such would have been the case under only very slightly different historical circumstances--then the later development of Iban warfare, which we have attributed in part to its adaptive effects, might have not taken place (Vayda 1969:219).

Yet Vayda is cautious and denies that his remarks are "an abjuring of determinism." Instead, he suggests that "there may be determination without predetermination and that there may be orderliness in cultural evolution ... and yet only a limited predictability" (1969:219-20).

With special reference to Iban warfare, we can say that at those points in its evolution when it was still something new to the Ibans themselves the thoughts and feelings of the Ibans about its desirability may have been significant determinants of whether or not it would then become established among them (Vayda 1969:220, italics added).

On the one hand, note that Vayda still couches the effects of thoughts and feelings in the language of determinism (and thus escapes the charge of unscientific heretic?); assumes that once an innovation is no longer new people do not have thoughts and feelings about its merits any more (some do, some do not); continues to think of human action in terms of linear cause-and-effect. Note that he allows the existence of order without predictability, an understanding of evolution more consistent with the findings of biological evolution, and that once warfare is accepted its effects can cause

the continuation of war and are thus adaptive:

The circular chain of cause and effect is readily seen: warfare has effects conducive to the survival and increase of the Ibans; the survival and increase of the Ibans are conducive to the maintenance and spread of Iban warfare (Vayda 1969:217).

This kind of circularity is not a tautology, unlike the Law of Cultural Dominance. As Ibans increase reproductively, they increase the number of people who share similar customs, beliefs, reasons, intentions, and purposes. In the long run, however, Vayda gives more weight to the "adaptive effects" of warfare, e.g., if our neighbors are warring against us, pushing us out of hunting and gardening lands, stealing our women and killing our men, we respond by borrowing their war practices and fighting back. In so doing, we are no longer a peaceful people but a warring one. "A cause of the warfare by us may then be said to be the fact that the warfare by our enemies had adaptive effects for them" (Vayda 1969:218).

This is adaptation once removed. Although Vayda suggests that the Iban might have chosen not to take up warfare, he does not explore possible alternatives. Consequently, I perceive that the implicit adaptive advantage for us (and adaptation is always seen as advantageous) is that if we do not borrow the institutions of our tormentors we will die out. While common sense may prompt us to such a response--fight back or die out--it may be our own cultural belief that violence stops violence. This belief has been found erroneous time again, yet probably a majority of our population clings

lovingly to the deterrence hypothesis. For instance, prior to the intervention of a British military peace-keeping force in Northern Ireland, 80 people had died in 3 years of troubles. Since the arrival of the peace-keepers and their threat of violent retaliation to acts of violence, nearly 800 people have died in 4 years (Prasad 1973). While there are serious methodological problems in his studies, Naroll (1966, 1969) offers quantitative findings for both primitive and historic warfare that at least do not support the deterrence hypothesis. (See Chapter II for further discussion of Naroll's work.) Although there is a very strong probability that in the long run the use of nonviolent force will result in far fewer casualties (Deming n.d.), the attitude in our culture as expressed by one of my students who finds nonviolence unthinkable is, "I would rather go down fighting!"

Disregarding his insistence on adaptive function, Vayda is on to something that I consider to be the most fruitful explanatory heuristic available at this time for understanding warfare of whatever type: a circular chain. I have played with chains; Harris expresses his principle of technoenvironmental determinism as a linear chain; except for its initial condition, the Harris-Divale theory of primitive warfare is a circular chain; the Carneiro theory of the origin of the state posits a chain of events. Yet all of these are deterministic: an element can have only 1 position in a sequence, and because it does is in the natural order of

things and outside human volition. The economist Gunnar Myrdal talks about chains in a different way. There is pattern but not predetermined order; there is movement, but not along a line from here to infinity, or around in a flat circle, or in a simple feed back model, or towards a stable equilibrium. Furthermore, unlike Vayda, Myrdal does abjure determinism and its laissez-faire, ex post facto consequents.

What is wrong with the stable equilibrium assumption as applied to social reality is the very idea that a social process follows a direction--though it might move toward it in a circuitous way--toward a position which in some sense or other can be described as a state of equilibrium between forces. Behind this idea is another and still more basic assumption, namely, that a change will regularly call forth a reaction in the system in the form of changes which on the whole go in the contrary direction to the first change.

The idea I want to expound ... is that, on the contrary, in the normal case there is no such tendency towards automatic self-stabilization in the social system. The system is by itself not moving toward any sort of balance between forces but is constantly on the move away from such a situation. In the normal case a change does not call forth countervailing changes but, instead, supporting changes, which move the system in the same direction as the first change but much further. Because of such circular causation a social process tends to become cumulative and often to gather speed at an accelerating rate [e.g., inflation, militarism] (Myrdal 1971:13).

Myrdal's notion of circular causation moves in a spiral, away from an equilibrium; the effects of phenomena can be to intensify themselves.¹⁹ Let me borrow Myrdal's illustration (1971:11-12). Because a man is poor he cannot afford proper food; because he cannot eat well his health is poor; because his health is poor he cannot work; because he cannot work he is poor. This chain, or vicious circle, of the circumstances

of poverty can never spiral upward out of poverty unless someone introduces at least one new factor at one or more of several possible points, for example, a guaranteed annual income or nationalized health care. The task is complicated, however, because there is no predominant factor, e.g., the "economic factor," that one can readily identify and adjust by recipe to alter the direction of the spiral. Furthermore, it is crucial to know how change in one factor will affect the other factors before one begins to tinker (Myrdal 1971:19-20). Yet without the introduction of viable changes, the chain will spiral downward and intensify into Oscar Lewis' culture of poverty and the widening abyss in the modern world between the have and have-not peoples and nations. In the latter instance the effect is compounded because wealth is concurrently generating an upward spiral. Once a man has money to spare that, when put to work making more money by controlling tools and resources, can provide him with a living and a bit left over, he is no longer poor and in fact becomes wealthier (Cf. Lundberg 1968).

Do the spirals of cumulative causation have inherent limits? A downward spiral does, in the manifestation of starvation and death. Any Malthusian lower limit is blurred, however, where modern medicine lowers the death rate through medical treatment of diseases that are symptomatic of starvation. Any upper limit is even harder to define, but it exists. Myrdal (1971:35-36) gives as examples (1) the

accumulation of old capital equipment that slows down production and is overtaken by developing industrial areas using new equipment. England was displaced by the United States and, later, Germany as the world's industrial leader largely because of this limit. (2) If industry and population become too concentrated, the burden of public expenditure may slow down or reverse the spiral of prosperity. The present condition of American industrial centers such as Detroit and New York, where the inner city has come close to bankruptcy and death, is a manifestation of such a limit. (3) If wages rise so high that the product workers produce is no longer competitive in a larger market, the spiral rapidly reverses. In the past decade, products produced by the highly paid American worker cannot compete with less expensive products of higher quality made with cheaper foreign labor. American factories close, and prosperity is no more--for the workers, that is. The upward spiral is still in motion for multinational corporations that can readily move capital to wherever costs are lowest without any risks to their prosperity. Thus the American worker is on a downward spiral but American capital continues upward carrying with it--for the time being--the Japanese worker.

Although Myrdal's hypothesis is nearly two decades old, its address to the equilibrium economics of the 1950's is not only still relevant in economics, it is a refreshing draught in anthropology where so many are still "discovering"

equilibrium models. Indeed, Myrdal intended that his "principle of interlocking circular interdependence within a process of cumulative causation" be valid for all social relations (1971:23). In a burst of irritation at Vayda's earlier (1968) attempt to explain primitive warfare as an equilibrating response to disequilibrium between contenders, Aberle retorts:

With respect to the topic at hand--warfare--it seems to me, as a starting point, valuable to assume that the aim of much warfare ... is not to equalize inequalities, as is suggested in one of Professor Vayda's hypotheses, but to increase them. Conceivably an equilibrium model might have some possibilities for application when the warfare ... occurs between groups at a similar level of organization and with similar technologies in similar environments--groups that are adjusting to random fluctuations of population and/or resources. But when war occurs between groups with different technologies (military, productive, or both) or different levels of organization, or both, it is typically an expansive operation for one of the systems in question; it aims at a continuous disturbance of inter-societal equilibrium. ...

In several parts of the globe we find very large continuous areas occupied by language families whose members have diverged from one another relatively recently in human history. This would be true ... for Bantu, Semitic, Indo-European and Sinitic. Many of these linguistic expansions can be shown to have swallowed up, shoved aside, or pushed into refuge areas groups of different linguistic background that earlier occupied the area. These distributions suggest that expansion of peoples "on the prod" is not a product of modern times or even of the historic period. ...

I suggest, then, that in terms of scientific yield we are likely to get further, faster with concepts like competition, expansion, and domination, than with concepts like function, equilibrium, homeostasis, and reduction of inequalities (1968:99-100).

It is not clear to me, however, that equilibrium theory is any more applicable to warfare between similar societies than

it is to dissimilar ones. Why one type but not the other? Why either? We run the risk of being caught in the futile trap of adaptive versus maladaptive effects: if one breaks even or wins, warfare is described as adaptive; if one loses, it is maladaptive. Harris (1971) identifies Yanomamo warfare, the primary or type case for the Harris-Divale hypothesis, as maladaptive, and these people fight only each other! Lastly, couldn't one also describe warfare between dissimilar societies as an adjustment "to random fluctuations of population and/or resources"?

Where is the place of human volition in Myrdal's spiral? Can the poor man choose to be healthy? Can he will food into existence? In the period of transition between the capitalist and welfare states, the good liberal winces at conservative charges that the poor are lazy and no-good and it is all their own fault. The good liberal goes to considerable lengths to show that the poor man is a victim of circumstances beyond his control. Yet, while the poor may be victims, they are not blind or completely helpless. They can, and have, protest and revolt against those who perpetuate economic inequalities, and their rebellion need not be violent. The poor man is poor in a larger context, one that includes the rich man. Even for those who are born into wealthy families, we do not argue that the man's wealth is beyond his control, that he is a victim of circumstances, that he cannot stop his money from making more money.

Volition is probably the most important factor in being rich--otherwise you might become unrich very quickly. At the heart of all welfare legislation and projects to eradicate underdevelopment and inequality is the necessity for the rich to choose to help the poor, to choose to redistribute income and access to resources. The state, in Myrdal's estimation, has always owed some measure of its existence to the popular will (1971:44-48). Even at least some of the great reigns of terror persisted because of popular support, based upon a variety of reasons, e.g., Tlacaelel and Moctezuma, Shaka, the French Revolution, Hitler. Those few modern states that have achieved an egalitarian economy based upon redistribution have interfered with the spirals and introduced new factors because of the wills, wishes, ideals, and reasons of that segment of the population that had the power to do so, thus intensifying and spreading egalitarianism, and recreating the most ancient form of human society.

Although equilibrium theory does not appear to work in economics or anthropology, it is part of the cultural context of both our economic activity and our warfare. The whole arms race, for instance, is predicated on the assumption that if an equilibrium in armaments is achieved, war will not occur. This assumption is a corollary of the deterrence hypothesis. But, speaking etically, balance of power politics has a catch to it: a balance exists when we have the edge. Since all other contenders hold a similar belief,

there is no stable equilibrium but an accelerating spiral of cumulative causation that already has blown many people to kingdom come. So much for equilibrium between societies of similar technologies and organization. It is what the actors in our time believe and want that powers the spiral, and no natural law of equilibrium can possibly become manifest. In transferring the heuristic from economics to warfare--and it is intended to be transferable--the spiral has a vital characteristic. Wars and events leading up to them seem to possess a dynamism that few other sociocultural phenomena have. There is human volition, decision, and implementation. Even when we speak of events taking on a momentum of their own at some point, a closer look may reveal a human choice such as an ultimatum. Ultimatums by their nature and definition set a point of no return. Yet it is human notions of bluff, or honor, or impatience, or manipulation of events to provoke an excuse that direct the issue of an ultimatum and a response to its acceptance or rejection. The spiral of actions and reactions sometimes seems to tighten and increase in momentum--the Indochina War is a good example--unless another factor is introduced to alter the spiral, such as a change in foreign and military policy. And human beings make such policy.

This is the end of my critical examination of current theories prevalent in anthropological thinking that are determinist and unitary in design. I have tried to show what

some of the consequences are of philosophical determinism and of unitary theory in social science. In particular, I have argued that upon close investigation the theories not only compete with each other but, when taken singly, appear to create more problems of understanding than they solve. I have concluded that the qualities of order assumed by philosophical determinism are incongruous with the human actions they are meant to explain. Since warfare occurs in a variety of cultural patterns that are not necessarily or sufficiently predictable, understanding human action recognized as war must be in terms of the context in which it occurs and from the human beings who experience it. In the end, as psychological and functional explanations prove inadequate, one must rely on geography and history to trace out the cultural patterns.

In the second half of this thesis, I shall try to reconcile the Trukese to the European ethnographer in a quantitative study that includes as variables a few human reasons, intentions, and purposes. As Berreman hopefully declares

I would prefer that we Trukese ethnographers not give up our Trukese methodology, but that instead we define, explicate and thereby improve it. For I think it is possible to demonstrate that it works--that it gets us there--when well and properly done. What we have tried to do is find out how one goes about doing it well and properly. And there may be many different ways of doing it; or a few principles which can be applied in different ways, or many techniques which work in various combinations. Perhaps some new techniques need to be added--Trukese navigators, after all, have by now acquired

compasses for use in emergencies. European ethnographers, meanwhile, might unbend a bit--try coming out of the chartroom and observe more of the world around them. Each approach has something to say to the other. Time will tell what is useful and what is not (1972:231).

Yet I would rather rely on something other than the judgment of history to assay whether or not one is wasting one's time. As it is not wise to declare a priori what the lives of humans are about, neither is it just to trust only a posteriori.

CHAPTER II

METHODOLOGY

The Logic of Cross-Cultural Studies1. What are we looking for?

In the first chapter I discussed some of the salient theoretical issues that directly and indirectly influence the substantive outcome of warfare studies. The next consideration is of the research methodology I have used in the present study.

The primary value of the cross-cultural comparative method is to provide empirical testing of an hypothesis on a large number of cases with findings presented in a mathematical statement. The determination of "large" is difficult, in addition to the more obvious problem of the independence of cases (the Tylor-Galton problem). There is a variety of other problems in cross-cultural research that may call the method's utility into question.²⁰ Anthropologists who consider themselves humanists find the method rude, crude, and dehumanizing. The new wave of scientism finds it necessary but not sufficient and deplores the inability of synchronic comparisons to produce cause-and-effect statements:

Statistical cross-cultural surveys can, indeed must, be used to supplement other modes of generating and testing

hypotheses, but they cannot be used alone or even as the primary sources of nomothetic statements (Harris 1968: 618).

The source for such statements is "... detailed diachronic and synchronic causal-functional analysis of specific cases" (Harris 1968:633), that is, the analytic and creative powers of individual intelligences.

There seem to be just three ways to go. (1) An individual may make a universal generalization about human behavior through analysis of one or a few cases. This fallacious hasty generalization in the name of science is highly disapproved of and has been labeled case illustration (McEwen 1963). It also suffers from circularity: the illustration is used as proof of the theory. An ad absurdum exercise in this method is Lionel Tiger's acceptance of William Golding's fictional illustration of human bestiality as proof for biological determinism and innate depravity. Less exciting examples are Divale's (1971) use of 6 societies upon which to base a theory of warfare applicable to all primitive societies, and Colin Turnbull's (1973) account of the dreadful Ik, a disrupted culture turned parasitic and cancerous, for which Turnbull recommends radical surgery before the social disease spreads and corrupts other societies. His conclusions will delight Ayn Rand and her followers, who will probably parade the Ik case as evidence that humans are by nature primarily selfish and that subsequently her theory of objectivism is a fact.

(2) Or one can put a large number of societies coded for a variety of variables into a computer and see what variables come out sticking to each other. Harris (1968:632) suitably but disapprovingly calls this "the-throw-it-against-the-wall-and-see-if-it-sticks" technique. Yet such pure induction is logically impossible, since facts do not speak for themselves (Hanson 1958; Chaney 1973), and data are invariably coded in accordance with at least tacit assumptions. The cross-cultural comparisons contained in this dissertation appear to be of the throw-it-against-the-wall type, but as discussed in detail below the selection of variables and their coding have been done in accordance with a theory, albeit amorphous, of human action.

(3) Or one can do as Harris advises, which is initially similar to (1) above: Derive a generalization/hypothesis through analysis of a few cases (Hanson's retroduction); look for antecedent variables in a large collection of cases; put the data into the computer and see what variables come out not sticking to each other (association) but preceding and following each other (cause and effect).

If we wish to achieve an orderly picture of history, we must begin by assuming that there are certain orderly principles which are at work. And we must use these principles to order and classify the data. ... we must process the data, question it, classify it, and code it in relation to the expectations of our major premises. Then and only then can we accept a failure to stick to the wall as evidence against the major premise. At the same time, it is only from such a procedure that the particular correlation on each trial may be seen as

connected and governed by the general principles of sociocultural evolution (Harris 1968:632-33).

Those principles, of course, are to be those of cultural evolution and cultural materialist causality. Beneath the appearance of scientific rigor, I think one shall find the nemesis of unfalsifiability. On the one hand, how could the major premises be found in error if the minor premises (data) are selected according to the major premises? On the other hand, an argument can be valid although the premises are false as long as there is internal consistency. The point in question is the truth or falsity of those premises--and we have the right to inquire after truth since these macro-theories purport to account for as profound a matter as order in history. If I understand Harris correctly, while he allows for the possibility that properly done (style 3) studies could admit disproof, exceptions to regularities, and unsuspected relationships, he seems to view the absence of relationships as indicating that the research is not any good rather than entertain the possibility that there is no pattern or regularity of the sort he assumes to be the case.

Let us admit that positive correlations established in the face of the hazards of coding and the unreliability of the sources may nonetheless, barring systematic bias, merit our confidence.... There remains, however, the problem of the noncorrelations which may arise from poor ethnography, biased analysis, faulty coding, and improperly phrased hypothesis (Harris 1968:632).

This explanation of noncorrelation is identical with that of Köbben (1967), who insists that where exceptions to a

correlation occur, it is not due to the nature of the exception but to faults such as those Harris lists. While I certainly do not deny that human error may be a significant explanation for some exceptions--much cross-cultural work is appallingly sloppy--we cannot assume that it must account for all exceptions. The presumption of ubiquitous order is patently a priori. Chaney (1972) has dealt with the possibility of little or no regularity and, unlike almost all anthropologists who have written on philosophical issues, does not find such absence threatening:

... it behooves social scientists to realize that since the degree and nature of regularity in sociocultural phenomena is still an open question, to state that no regularities have been found is just as important as to find regularities. If one does not know one way or the other, it is just as significant to implicate the extent of no regularities as to explicate the extent of regularities (1972:13).

Taking users of the comparative method as a whole, we are looking for 2 things: the presence and absence of joint occurrences of variables and explanations for the origin and perpetuation of these presences or absences as due to either fortuitous circumstances (geography and history) or natural order (psychology and functionalism). Research workers in anthropology heavily emphasize the presence of joint occurrences and prefer to explain their existence in psychofunctional terms. This emphasis has been traced, illustrated, and discussed throughout this paper. Even when history is reluctantly admitted as significant, the natural

order of things is invoked to explain the adoption and perpetuation of a known trait. When Driver (1966) explains the distribution of kin avoidance behaviors in terms of geography and history, some commentators admit the possibility of diffusion to account for the origin of a cultural trait but assume that its perpetuation must be due to psycho-functional factors, i.e.,

... that the information in question (a) meets certain socio-psychological needs and/or (b) functions to maintain some variable critical to the survival and well-being of the population (Collins 1966:149).

Therefore, what exists must exist of necessity. One must remark that such facile explanations are post hoc and imply a belief in the natural benevolence of social institutions; neither explains situations in society where human beings are deprived and miserable. Another reply to Driver argues that

... the variables traditionally associated with avoidance of kin are here shown to be less important functional correlates than culture area and language. If this be true, we are still left with the question, why? Diffusion is no answer. Most anthropologists since the time of Patterns of Culture have agreed that a trait is "accepted" if it "fit-in-with" the other major elements of the "taking" culture. The final explanation must therefore be a functional one (Freilich 1966:153).

Thus we have anthropology by consensus and acceptance entailing purpose. The assumption of function is perhaps as deeply imbedded in the language of anthropology as is adaptation, although the case for its necessity is by no means as clear. Yet even Driver assumes some kind of psycho-functional motivation, although it may be a necessary but not sufficient

condition for the diffusion of a trait:

Probably all the psycho-functional "causes" of kin avoidances advocated by Tylor, Frazer, Freud, Lowie, Murdock, and Stephens and D'Andrade have had some influence on the origin, maintenance, and dispersal of these behaviors. Even the most extreme geographical-historical enthusiast needs a package of psycho-functional "causes" to get the avoidance behavior started. Once such behavior has become firmly established, however, it seems to diffuse by intertribal marriage to peoples who lack some or even most of the "causes" discovered so far. It also fails to occur among some peoples who possess most of the "causes" (Driver 1966:147).

But what is going on in that package is simply not clear. Barnett (1953) manages not to develop any such package in his elegant and finely detailed work on the origin and acceptance of innovations, in which he argues that needs are as likely to be generated as satisfied by innovations; that well-being is meaningful only to the individual; that people, not cultures or societies, accept or reject innovations in the light of many considerations; and that what "fits-in-with" the life and expectations of one individual may be anathema to her neighbor. We do know that patterns exist, that human social life and cultural experience are not random or chaotic, although they are very untidy (Douglas 1966). For instance Driver and Massey (1957) have made a strong case that change in gross sociocultural phenomena tends to progress in a certain sequence: division of labor, residence, land tenure, descent, and cousin terminology. Eggan (1966), apparently independently and from a different theoretical position, reconstructs a similar progression in change among several

groups of North American Indians. Nevertheless, I am largely in agreement with Chaney, who argues that "the 'patterns' of sociocultural phenomena are best conceived of as holding in terms of the continuity of a culture ... rather than in terms of something inherent in sociocultural phenomena analogous to 'cosmic glue'" (1973:1370). Thus, we must deal with specific groups of people and the specific behaviors and beliefs that people who live together share. Even at this level one must be careful not to make the patterns more homogeneous than they are.

In the end, the only point we can be clear about is that the tables, numbers, and coefficients cannot reveal the truth: "... statistical manipulation of sociocultural data can only summarize the data. It can not yield direct information as to functional and/or causal factors" (Chaney 1973: 1368). What statistical manipulation can show is how often variables that I select seem to occur together, and whether or not there is variation in this frequency from one geographical region to another. It is entirely up to me to make the associations and their variability intelligible.

2. How is it done?

The biggest initial stumbling block and source of error in comparative work is the famous Tylor-Galton problem: how to be certain that each member of the sample is an independent case and not a duplication, through diffusion, of

another, which would inflate the strengths of the associations. It is also referred to as "controlling for history," i.e., eliminating those cases in which an institution or behavior is borrowed rather than originating indigenously. Here is an apparent contradiction. One is concerned with the laws of history but not with "mere" history. Laws of history should be translated into psycho-functional relationships. Considerable effort has been expended in inventing ways to draw a pristine sample through statistics rather than rules of thumb. Naroll (1961; 1964), Naroll and D'Andrade (1963), and Driver and Chaney (1970) have invented various sophisticated techniques, 4 of which Murdock and White (1969) used to develop their Standard Cross-Cultural Sample, the one I have used here. This Sample, culminating over a decade of work, has been developed so that research workers devoting energies to any problem would turn out comparable findings. Prior to this, everyone developed his own sample with highly variable standards and no confidence that the sample at hand was comparable to any other. Apparently oblivious to Murdock's effort, Naroll (1970) has developed his own standard sample, and eventually we shall be faced with a choice between the 2 and a revival of the continuity problem. In any event, Murdock and White have carefully pinpointed the societies in their sample as to exact geographical location and specific date to which the largest body of ethnographic data pertains. To solve the Tylor-Galton problem and allow for geographical

stratification, Murdock and White have gone through one last step in classifying cultures according to similarity, proceeding beyond clusters (Murdock 1966, 1967) and sampling provinces (1968) to selecting 1 society from almost all of the 200 sampling provinces. Given the demands of pinpointing 2 provinces are unrepresented; 2 are further divided into 2 again and a representative chosen from each of the 4; 14 are unrepresented because further judgment has been made that they should be lumped without other provinces. The revised provinces or "distinctive world areas" number 186, 1 society representing each. Of the 186 societies, 141 have food-producing economies (56 having intensive agriculture, 19 horticulture, 51 swidden; 15 pastoral) and 45 food-collecting economies (13 primarily gathering, 14 hunting, 18 fishing). Politically and geographically, the sample is stratified as follows (Murdock and White 1969:340).

Major Region	Stateless Societies	Minimal States	Small States	Large States	Total
Sub-Saharan Africa	8	9	6	5	28 [27]
Circum-Mediterranean	1	8	5	14	28 [29]
East Eurasia	11	10	2	11	34
Insular Pacific	13	11	6	1	31
North America	23	7	2	1	33
South America	23	5	2	2	32
Total	79	50	23	34	186

The pinpointed time periods have the following distribution (Murdock and White 1969:341).

Time Period	A	C-M	EE	IP	NA	SA	Total
1750-1 B.C.	0	2 [3]	0	0	0	0	2 [3]
A.D. 1-1500	0	1	1	0	0	0	2
A.D. 1501-1600	0	0	0	0	1	2 [3]	3 [4]
A.D. 1601-1700	0	0	0	0	2	1	3
A.D. 1701-1800	0	0	0	1	3	1	5
A.D. 1801-1900	9	7	11	4	16	5	52
A.D. 1901-1950	19	13	17 [16]	16	7	19 [18]	91 [89]
A.D. 1950-1965	0	4 [3]	5 [6]	6	0	2	17
Total	28 [27]	28 [29]	34	31	33	32	186

Deviations from the Standard Sample in this study with regard to these classifications are indicated in brackets. Explanations for such deviations are given below.

Murdock and White designed the Standard Sample so that a research worker need not use all 186 societies but could include every second or third society as "... either being equally representative of the world's known and well described cultures as well as exhibiting slightly less contamination from historical influences" (1969:352). I decided against this option because of the uneven quality of warfare data: any way I chose, too many of the best-documented societies were left out. Furthermore, for statistical validity I consider any fewer than 100 cases to be unreliable, and for the correlations within each geographical area, I do not want the Ns to drop below 20. Lastly, one of the contributions of this study is to compare the results done on small samples with findings from a large sample. While I have worked with the entire 186 societies, because of the unevenness of the data I have to exercise another option stipulated by Murdock and White, and that is "... substitute other societies from the same distinctive areas or sampling provinces without sacrificing any of the advantages except possibly that of intercorrelation with the results of other studies" (1969:352). I have noted the 10 substitutions as they occur in the list of Standard Sample societies (Appendix A).

Sample size as a serious consideration has been generally avoided in anthropology and relegated to last place after problems in sample selection, bias, coding, and the ethnographies (e.g., Rohner and Pelto 1970). Indeed, in

response to Chaney and Ruiz Revilla's (1969) findings that small samples of 48, 60, 110, and 112 societies produce correlations that can be significantly different from those based upon large samples, there may be a minor movement afoot to justify earlier work based on such small non-stratified samples with the claim that they are mathematically superior to large stratified samples! Rohner and Pelto, reviving an argument by Benfer (1968:950) that "... all things being equal, the larger the sample size, the greater the probability of the particular test rejecting the [null hypothesis] ..." and therefore anthropologists should use small samples, without bothering to give any indication of what they mean by "large" and "small," and bolstered by Meehl's (1967) findings that any intercorrelation using a sample of 55,000 will be significant, argue that "researchers need not hesitate to use samples as small as forty-eight or sixty societies" (1970: 1455). Yet Rohner and Pelto are not consistent in their recommendations. After dismissing Chaney and Ruiz Revilla's findings (1969) as not proving anything because the 7 samples were drawn by different procedures, which is just the effect that Chaney and Ruiz Revilla were testing (Chaney 1970:1458), attributing variation among samples as due to "... a variety of extraneous factors rather than sample size itself" (1970: 1454) but not specifying what these factors might be, and urging researchers to go ahead and use samples as small as 48, in the end they recommend samples of "at least" 100 to

allow for contamination and intervening variables. "If one's sample contains only 50 or 60 cases ... subsample operations may become meaningless because of the small number of cases involved" (1970:1455). The ultimate justification for using small samples is that the user "... is seldom in danger of erroneously drawing positive conclusions regarding the association between variables" (1970:1455), that is, Rohner and Pelto are claiming, on the basis of Chaney and Ruiz Revilla's work, that if one comes up with a significant correlation using a sample of 48, one can assume that the correlation would be even stronger using ever larger samples. They do hint at the possibility of samples being too small. Similar reasoning seems to be used by Divale (1973), who argues that using data from different time periods and different communities of one society--in contrast to Murdock and White's (1969) insistence on using data only from a pinpointed time and community--is perfectly all right because such distortion produces only random error which lowers correlations. "Thus, if significant associations occur, the actual correlation is even higher than the one found, and the danger of spurious correlations can be ignored" (Divale 1973:46). Following the recommendations of these gentlemen, we would be exercising scientific virtue by using small samples whose data is taken from any old time or place identified as "Cheyenne" or "Maori." The seriousness of sample size becomes even more important when one observes that many cross-cultural workers

either claim or assume that their findings apply not just to the sample from which they are derived but to the universe of all human cultures.

The most difficult aspect of quantitative research is coding: making hundreds of decisions as to how to classify gross data. One is constantly plagued by doubt about one's judgment. Since coders are actually further classifying the ethnographer's classifications, error can be compounded. Often the very possibility of cross-cultural quantitative research is questioned because of the numerous possible and actual instances of error. Why even begin if the ethnographies are not reliable? Such reasoning has probably stopped many potential research workers cold. The only alternative, if one is going to do research in anthropology, is to go out and become another ethnographer (but a reliable one, of course). Rechecking field accounts is a rare practice, for some obvious reasons--the people are dead, the culture is gone, it is thought better to go where no other anthropologist has gone before--and one not so obvious. The rare restudies, for instance Oscar Lewis' work in Tepoztlán following that of Robert Redfield, can be in agreement or not. Do Lewis' findings, which frequently are at odds with those of Redfield, mean that Redfield's descriptions are wrong, that he did not see correctly? Or that Lewis is wrong?

If ethnologists are expected to analyze their own descriptions (field data) and such work is acceptable, why

then scorn the sources of the comparative research worker? Much of the goodness of ethnographies must be taken on faith--indeed, we do so all the time when we teach--but there are other philosophical justifications for doing just that. Cody (1967) contends that the distinction between description (what the ethnographer observes) and action (what the people observed are doing) is a false one. For those of us trained so assiduously in relativism, such a thought is nearly unthinkable. For instance, in examining the classic witness experiment--someone rushes into a classroom, assaults the teacher, leaves, and 20 astonished students are then asked to write down what they saw--we can make 1 of the following conclusions as to why no 2 of the descriptions are identical. Some are in error (the students did not really see what happened) and some are not, but which ones? If we take all the points in common to the 20 descriptions, we might have an account of what actually happened, a composite or summary. Or we could say that there is no way of even talking intelligibly about what really happened, because our only evidence of what really happened is in the 20 different descriptions. That is, logically and really, there are 20 different events.

Yet, bowing to pressures for conformity in the data, more than one coder can go over the ethnographies independently, the coding decisions compared, and discrepancies accounted for in more traditional ways, perhaps in most instances by having more coders go over the same data until

some congruence is achieved. Possibly the only real benefit of reliability coding is to spread around the responsibility for decision-making. It makes the research worker feel better. There is the continual temptation to code in accordance with implicit or explicit hypotheses, and to infer carelessly. What is considered to be adequate coding?

The purists among cross-cultural researchers tend to throw out any case in which the trait in question is not specifically reported as either present or absent. Others have devised ways (often quite ingenious) to get around this problem. A frequent solution has been to examine each case within a larger whole--either the context of the entire ethnography or the totality of what the researcher already knows of the society or of the culture area of which it is a part. If in the light of this contextual examination the researcher concludes that a given trait is either present or absent, regardless of whether the ethnographer specifically says so, he will code it as such--normally using a special device to indicate a lesser degree of reliability. In addition, a content analysis of the ethnographic text may reveal that the ethnographer carefully and fully covered the context, e.g., puberty and religion, in which the trait, e.g., circumcision rites, would be expected to occur; in the absence of any mention of circumcision rites, the rater may feel justified in rating the trait as absent for that particular society (Lebar 1970:716).

The latter technique runs the great risk, I think, of designing the data to fit the hypotheses. I have used inference sparingly. I have found myself confident of my own coding only through reading entire ethnographies even when the ethnographers are explicit. I am uncomfortable with the Human Relations Area Files' sorting of warfare data and find it necessary to put a culture back together before taking it apart. Lebar (1970:716) suggests an acceptable reliability figure of 80 per cent agreement between any 2 coders. The

less "objective" and the more "judgmental" variables are, of course, the lower the reliability figure will be. In this study, for instance, there are 2 attitudinal warfare variables that could have profited from better operationalization. One, however, was not used in quantification, and agreement among coders for the other was very close.

After all the tedious work of sample selection and coding, and the perennial frustration in warfare research of scanty data, one reaches the point of it all--making those mathematical statements. Even though there are relatively few statistics invented for nominal data as compared to ordinal data, research workers in anthropology seem to be partial only to phi (and a relative, r point-biserial) and chi-square, respectively a measure of two-way association between 2 dichotomized variables and a measure of such an association's significance. Sometimes only chi-square and a probability value are given. Beyond dichotomous subclassification of dichotomized variables and performance of phi and chi-square upon them, and an occasional scalogram, there is no further statistical manipulation by and large in quantitative warfare research.

The greatest amount of effort in such studies is expended in interpretation of the coefficients, explaining their relationships in causal terms. Consistently, coefficients of association are not treated as summaries of the data, as Chaney defines their utility, but as licenses to

build chains of cause and effect to stand universally for all cultures throughout time and space. Refinements of this interpretive manner are demonstrated by those research workers thinking in the evolutionary paradigm who arrange the pairs of associations in a developmental sequence, also to stand universally.

In the following section, which deals with specific authors and their contributions to quantitative warfare research, I shall look closely at individual studies in terms of methodology, hypotheses, and interpretation of results.

3. Who has found what?

In this section I discuss in varying detail the contributions of 8 authors to quantitative warfare studies in anthropology. It is a nearly exhaustive review of what has been offered as significant statistical statements about the nature of warfare. Such a detailed review is necessary not only to establish the precedents for my own work but to fill in the background against which my research can be better judged. I deal with the authors in a certain order, primarily the date of their work and secondarily vague themes in orientation, from general omni-variable studies (Wright, Broch and Galtung) to problem-oriented studies with particular points of view: political (Naroll, Otterbein, Ember and Ember), psychocultural (Russell), culture patterning (Sipes), and the involvement of women in war (Nammour).

Quincy Wright was the first to engage in quantitative and comparative warfare studies. His Study of War (1942; second edition 1965) is a massive compendium of theories, discussion, historical detail, and descriptive statistics about war throughout human time and space. The bulk of this work is devoted to historic warfare, but his attention to primitive war, although brief and dated, is serious. He has coded 650 primitive societies for the following variables: region; character of war (defensive, social, economic, or political); climate (cold, temperate, hot annual mean temperatures); habitat (forest, mountain, seashore, desert, grassland); climatic energy (low, medium, high--based on variations in temperature and humidity); race (Pygmy, Australoid, Negroid, Hamitoid, Red, Yellow, Brown, White); subrace (20 in all); culture (hunters, pastorals, agriculturalists); subculture (lower hunters, higher hunters, dependent hunters, etc., as used by Hobhouse, Wheeler, and Ginsburg); political organization (clan, village, tribe, state); social organization (sex and age, professional, caste); intercultural relations (isolated, moderate contact, close contact--the contacting culture is a civilization) (Wright 1965:544-51). Wright's determination of "warlikeness" rests upon the reasons for the warfare of a particular group, reasons ranked in a hierarchy of rising expectations, ambition, and inclinations to engage in war. Thus, war for defense is at the lowest level, then war for revenge, sport, and prestige, superceded by war for

economic gain--slaves, livestock, land, culminating in war to maintain or extend the power of a ruling class.

It seems appropriate to regard people employing this type of war as the most warlike of all, not only because of their peculiarly favorable attitude toward war but also because they receive and inflict the greatest losses of population from war of any primitive people. The high morale which armies developed by people of this type customarily display enables them to endure more mutual slaughter than can the less-disciplined warriors involved in other types of primitive warfare. Furthermore, the tactics and weapons used by people of this class are more efficient for purposes of slaughter (Wright 1965:561).

Wright then proceeds to tabulate the incidence of each type of war with all the other variables. I have included one of his tables, very slightly modified, which is still usable in the anthropology of the current decade (Table 3).

Using the 1942 edition of Wright, Broch and Galtung (1966) perform "trivariate analysis," cross-tabulating all possible pairs of Wright's variables of world region, climate, habitat, race, culture, subculture, political and social organization, and intercultural relations with the dependent variable "belligerence"--the presence of war for economic or political reasons. They have found that the white state in close contact with other societies is 100 per cent belligerent; that there is rising belligerence as one increases in political complexity from clan to village to tribe to state; that pastoral cultures are highly belligerent; that grassland in North America "disposes particularly to belligerence"; that South American states are low in

TABLE 3. RELATION BETWEEN CONTINENTS AND WARLIKENESS*

Continent	Defensive War 1.	Social War 2.	Economic War 3.	Political War 4.	Total	Mean Average Warlikeness**
No. of Primitive Peoples in Each Continent Practicing Each Type of Warfare						
(Percentage of Primitive Peoples in Each Continent Practicing Each Type of Warfare)						
Asia and Indonesia	18 (12)	91 (59)	42 (27)	4 (2)	155	2.21
North America	7 (6)	67 (55)	44 (36)	4 (3)	122	2.37
South America	4 (6)	48 (69)	17 (25)	0 (0)	69	2.19
Africa	1 (1)	28 (23)	67 (54)	28 (22)	124	2.99
Australia	0 (0)	75 (100)	0 (0)	0 (0)	75	2.00
Oceania	0 (0)	37 (82)	4 (9)	4 (9)	45	2.27
Total...	30 (5)	346 (59)	174 (29)	40 (7)	590	2.38

* Adapted from Wright 1965:551.

** This figure was obtained by multiplying each figure by the number at the head of the column, adding the products in the row, and dividing this sum by the total at the end of the row.

belligerence (1966:35-36). The authors have designed an "index of primitivity" (or civilization) based upon the assignment of so many points for type of culture, subculture, political organization, and social organization that, when added, will give a score to a particular society ranging from a low of 0 (hunting "in its lowest form," clans, and age-sex stratification) to a high of 6 (the highest form of agriculture with state organization of specialists or castes) (1966:37). Cross-tabulating the index with types of war, Broch and Galtung have gotten "the impression that belligerence is an concomitant of increasing civilization" (1966:37; italics in the original). They posit two-way causation: "increased civilization leads to increased belligerence, which in turn leads to increased civilization in others (because of 'survival of the fittest,' homology, diffusion and simply eradication of the more primitive)" (1966:41). In conclusion, they hypothesize that since increasing similarity between societies increases belligerence due to direct competition, such societies will fight unless some form of integration between them occurs (1966:42). In their study, Broch and Galtung deal only with Wright's crude variables and in percentage statements of the cross-tabulations. They use no inferential statistics.

Naroll has 2 studies in print to date, dealing quantitatively with warfare and using inferential statistics, in which he tests the deterrence hypothesis versus the arms race

hypothesis. It is held by advocates of the former that increasing weaponry decreases the possibility of war because war becomes too costly, while advocates of the latter argue that increasing weaponry increases the likelihood of war--rivalry is interpreted as intent to wage war.

In an arms race each side strives without limit to military superiority. Neither can be satisfied with simple parity, because an underestimated rival may at any time achieve a technological breakthrough that will give superiority.... "We are simply protecting ourselves in case of attack, but they are arming for war" (Naroll 1966:14).

The advocates of deterrence point out that there is no defense against a nuclear war, and consequently our best hope is for a stable, credible deterrent. For a deterrent to be stable it must be invulnerable so that increased effort against it would be futile. Mutual invulnerability would, from this point of view, stabilize the arms race in a balance of terror. This balance would then, presumably, allow conflicts to shift to limited wars, and to an eventual stabilization and finally to reduction of war (Naroll 1966:15).

The first study used a sample of 48 (unidentified) primitive societies.²¹ Naroll's variables are the frequency of war (frequent or infrequent) and the presence or absence of several traits seen as indicators of an orientation toward war. These traits include (1) fire-and-movement tactics: firing at the enemy from a distance and then moving close in for hand-to-hand combat, combined tactics that require more coordination than 1 used alone and that produce more casualties; (2) flexible surprise tactics--recognizing that surprise is the optimum but not the only tactic, that if an impending surprise attack is discovered, to persist in more

formal battle array; (3) multiple expectations: the order of rising military expectations Naroll has found to be revenge and defense, plunder, prestige, and political control, and he concludes that the more expectations a society has, the more likely it will be to go to war; (4) many potential enemies: "The larger the number of potential foes, the more likely it is that there will be trouble with at least one of them" (1966:18); (5) military readiness: measured by the presence of reconnaissance missions, posted sentinels, or mobilization points; (6) fortifications: forts, fences, man traps; (7) Western technology: specifically the presence of guns; (8) repressed hostility: the absence of malicious gossip, quarreling, or public ridicule (Naroll 1966:17-18). Naroll does not give cell frequencies or statistical statements of the findings of his intercorrelations, only identifications as "strong positive," "moderate positive," or "no relationship." He concludes that his study gives no support to the deterrence hypothesis, that the only significant correlations are between multiple expectations, military readiness, fortifications, repressed hostility, and frequent wars. His strongest relationship is between frequent war and military expectations. In summary, then,

... societies that expect more kinds of satisfactions from successful warfare tend to be societies which fight more frequently and which make more preparation for war. They also tend to be societies with large numbers of potential enemies, that use fire-and-movement tactics (1966:19).

Yet Naroll is not clear in his own mind whether the presence of these variables is the cause or the result of frequent warfare (1966:19). In any case, Naroll finds that war orientation, specifically the possession of guns, has a strong positive correlation with territorial growth.

Thus societies whose territories increase are characterized by Western technology and, to a lesser extent, military readiness. The most plausible explanation is that military preparedness tends to make for territorial expansion. The correlations also show a strong tie between territorial instability and military expectations. Societies which hope for a great deal from warfare are societies whose boundaries are likely to change, one way or another. It is tempting to explain this as a three-link chain of influence, with war frequency leading to increased expectations, and increased expectations leading to instability by making tribal land the stakes of warfare. The results here are equivocal, but they do demonstrate that warfare is an agent of cultural selectivity, and that the notion that we can best preserve our way of life by throwing away our arms is dangerously naive (1966:20).

That temptation must also lead us to conclude that the way of life one is apparently maintaining is a militaristic one. It is also dangerously naive to assume that one's "way of life" is somehow a separate entity from one's military activities.

Naroll's second test of the deterrence hypothesis is published in more detail, as a pilot study "intended to try out the cross-cultural survey method of anthropology on a comparative study of history" (1969:150).²² He tests 30 hypotheses based upon 32 variables on a sample of 20 societies (or states) spaced through time in the randomly chosen decade of 76 to 85, although the specific century in which the decade is pinpointed depended upon availability of data.

All variables have been coded for a target date, a target state within that time period, and its specific relationship with a target rival. Frequency of war, territorial gain, and territorial instability are dependent variables, operationalized as Z scores. Sample members are:

	<u>Europe</u>	<u>State</u>	<u>Rival</u>
1.	225 B.C.	Rome	Carthage
2.	25 B.C.	Rome	Parthia
3.	176 A.D.	Rome	Marcomanni-Quadi
4.	376	Rome	Visigoths
5.	576	Byzantines	Persia
6.	1276	France	England
7.	1376	England	France
8.	1576	Spain	Netherlands
9.	1676	France	Netherlands
10.	1776	England	France
	<u>Switzerland</u>		
11.	1376	Swiss Confederacy	Kiburg
12.	1476	Swiss Confederacy	Burgundy
13.	1576	Swiss Confederacy	Swiss Catholic
	<u>Russia</u>		
14.	1476	Muscovy	Novgorod
	<u>Saracens</u>		
15.	776	Abbasids	Byzantines
	<u>China</u>		
16.	125 B.C.	Former Han Dynasty	Huns
17.	25 B.C.	Former Han Dynasty	Huns
18.	776 A.D.	T'ang Dynasty	Tibetans
19.	1076	Sung Dynasty	Tanguts
20.	1376	Ming Dynasty	Yunnanese Mongols

(from Naroll 1969:154).

Although Naroll argues that he has controlled for Galton's problem in this sample by using the linked-pair method for both time and space, I cannot help but be alarmed by the inclusion of 14 of the 20 cases as European, despite their spread over 2000 years. The cultural continuity of European history is real, not an artifact, and thus I do not see how one can reasonably control for history or diffusion. Naroll feels otherwise:

No tendency was evinced for successive periods of time in the same cultural tradition to resemble each other in the frequency of warfare involving the conspicuous state; nor was there any tendency for contemporaneous neighboring cultural traditions to resemble each other in this respect (1969:158-59).

No tendency was evinced for successive periods of time in the same cultural tradition to resemble each other in territorial gain, nor was there any tendency for contemporaneous neighboring cultural traditions to resemble each other in this respect (1969:160).

It is entirely possible that there is a basic disagreement between Naroll and me as to what constitutes cultural likeness and dissimilarity and thus solutions to Galton's problem.

In any event, based upon coefficients of correlation product moment (quantitative variables, i.e., $\bar{Z}$ scores), point biserial ($\bar{Z}$ scores x qualitative variables), and phi (qualitative variables), Naroll found no support for the deterrence hypothesis, or for the notion that a defensive stance---implicitly contrasted with an aggressive stance---is less likely to get a state involved in war ($r_{pb} = -.03$). Naroll interprets the latter: "It follows that peace-loving

nations are no less likely to be involved in war than warlike nations. Hence, one must conclude that it takes only one nation to make a war, not two nations, a conclusion offering no comfort whatever to advocates of unilateral disarmament" (1969:152). Yet, since arms make war more likely, bilateral disarmament does "receive some modest empirical support" (1969:153). One would hope so; if neither side has armaments, war is a common-sense impossibility. Naroll continues to be ambivalent in interpreting the direction of what he talks about as the causal relationship between war frequency and military preparations, arguing that the latter causes the former and supporting this with examples that appear to illustrate that the former causes the latter (1969:153).

Some of Naroll's other positive findings are: the more active are diplomats, the more frequent is war ($r_{pb} = .29$); ultimatums do not deter war ($r_{pb} = -.21$). Naroll interprets diplomacy and ultimatums as "presumably" symptoms "of serious trouble" (1969:158). An interesting cluster of positive correlations includes findings that rulers over 45 years old are more likely to be involved in war than younger rulers ($r_{pb} = -.29$), hereditary rulers more likely than elective or self-appointed ones ($r_{pb} = .30$), and states with greater political centralization more likely than those with less centralization ($r_{pb} = .34$). Naroll explains this phenomenon as that of the ruler who is more conscious of his status, as contrasted with his role, and is

thus more likely to become involved in war (1969:158). Yet rulers with more than 9 years of experience tend to gain or lose less territory than those with less experience ($r_{pb} = .62$). Thus, we have a statistical portrait of a middle-aged hereditary ruler with a great deal of political power going to war often, but for small stakes if he is an experienced prince, perhaps very concerned with his image as a vigorous ruler.

Consistent with the 1966 study, Naroll finds a positive correlation between territorial growth and the quality of military preparedness and technology ($r_{pb} = .39$), but that civil war within the state and active trade between the state and its rival are associated with territorial loss sustained by the state ($r_{pb} = -.44$; $r_{pb} = -.33$). Ambivalence enters once again, however, for while trade is associated with loss, cultural exchanges (undefined here) are associated with territorial gain ($r_{pb} = .31$).

There are a few other correlations that Naroll finds significant that I shall not present here. I simply wish to make the comment that Naroll seems to be under considerable pressure to interpret multivariate relationships despite the limitations of bivariate analysis using a sample of only 20 cases. The problems inherent in this miniscule sample make those of Otterbein, to be discussed next, look like nothing at all. Out of several hundred intercorrelations, Naroll has

made a handful of statistical statements that appear to be applicable largely to a handful of European states.

Otterbein, as co-author and alone, has published the most material in cross-cultural warfare studies. He works with samples of 50 societies, selected according to various randomizing procedures but in the long run according to the availability of data. Otterbein and Otterbein (1965) tested hypotheses to explain the presence or absence of feuding, i.e., the occurrence of blood revenge following a homicide, and they concluded that feuding occurs when fraternal interest groups, indicated by the presence of polygyny (which would establish a residence group of half-brothers) and patrilocality, are present, but that it is "suppressed" in societies with a high level of political integration that engage in warfare continually. The variables, their sub-classifications, and cell frequencies for the 50 societies look like this:

	Level of Political Integration					
	Low			High		
	Feuding			Feuding		
	Present	Absent		Present	Absent	
War--Occasional or Never						
Other	1	5	6	1	1	2
Patrilocal	1*	4*	5	4*	0	4
War--Continual						
Other	2	6	8	3	6	9
Patrilocal	8*	0	8	2*	6*	8
	12	15	27	10	13	23

Otterbein and Otterbein 1965:1479.

This is a small sample; the cell frequencies, especially when so finely classified, are very small. The addition of 1, 2, or 3 cases to any class is going to make a difference mathematically. For instance, out of 25 patrilocal societies, 15 have frequent or infrequent feuding; that is 60 percent. Out of the other 25 societies, that do not have patrilocality, 18, or 72 per cent, have no feuding. The addition of 3 cases raises a descriptive figure 12 percentage points. The reclassification of any 1 case in a sample of 50 societies dichotomized in a 2 x 2 table means a shift of 2 percentage points. This is a great deal of potential error. The geographical representativeness of the sample is Africa, 8 societies, Circum-Mediterranean 2, East Eurasia 7, Insular Pacific 11, North America 12, and South America 10. Of these societies, the Bemba and the Yao belong to the same

World Sampling Province, as do the Murngin and Tiwi, and the Abipon and Mataco (Murdoch 1968). All but the Tiwi are coded as having continual warfare and thus constitute 5 of the total of 28 societies so coded. It would have been useful if Otterbein and Otterbein had enumerated the specific societies in the crucial 6 positions marked by an asterisk in the table above, to allow further checking.

Otterbein (1968) continued his quantitative research, extending his interest from relationships between feuding and general warfare to concentrate on the nature of internal war, that is, warfare between culturally similar but politically autonomous communities.

It is demonstrated that fraternal interest groups and unauthorized raiding parties influence the frequency of internal war in uncentralized political systems, but not in centralized ones. It is also shown that the frequency of external war (warfare between culturally different political communities) does not influence the frequency of internal war (Otterbein 1968:277).

That is, where there are groups of related males, these males will act in concert to take revenge for a homicide (feuding) and to raid other political communities as small-scale military organizations. Individual warriors will get a raiding party together. As for the organizing principle for such groups, Otterbein found correlations between patrilocality, the obvious principle, and continual or frequent internal war to be insignificant with a very weak phi correlation coefficient of .14. Surprisingly, polygyny has a higher correlation with internal war, with a modest phi of .31:

Internal War			
	Continual or Frequent	Infrequent	
Polygyny			
Absent	13	11	.24
Present	15	3	18
	28	14	42

$$X^2 = 3.93 \quad p < 0.05$$

(Otterbein 1968:281).

Yet fraternal interest groups as indicated by polygyny are no better a predictor of the frequency of internal war than the variable Initiating Party dichotomized into "Anyone" and "Official." For uncentralized political systems, either fraternal interest groups or anyone as initiator may be associated with internal war, but in any case, centralized political systems with officials initiating war have more frequent internal war.

In centralized political systems, "officials" rather than "anyone" initiate warfare. The intercorrelation is high, but Otterbein interprets the intercorrelation as supporting the hypothesis "that the higher the level of political complexity, the less likelihood of war being initiated by anyone in the political community. ... Apparently centralized political systems are able to prevent unauthorized parties, which would include fraternal interest groups, from engaging in war" (Otterbein 1968:282). But it turns out that

internal warfare is more, not less, frequent in centralized polities. The only conclusion Otterbein draws from this is that groups engaged in internal war must be authorized in centralized polities. This apparent increase in internal war with centralization seems to go against the case Otterbein seems to be arguing--that centralization brings internal peace by controlling inherently violent male associations--yet he persists with a non sequitur:

I have ... shown that uncentralized political systems in and of themselves are not prone to more internal war than are centralized political systems; in fact, they are prone to less. Thus it seems that it is the existence of unauthorized raiding parties, rather than the absence of a centralized political system, that leads to internal war (Otterbein 1968:283).

Since he means by "unauthorized," "anyone" or nonofficials, and uncentralized political systems by definition mean absence of officials, the hypothesis appears tautologous. Anyhow, the test of the hypothesis "that societies in which anyone can initiate war are more likely to have internal war than societies in which an official initiates war" (1968:283) produced indications to the contrary:

... it appears that centralized political systems in which an official is the only one who can initiate war are more likely to engage in internal war than are centralized political systems in which anyone can initiate war. The data seemingly provide support for the interpretation that in uncentralized political systems wars are initiated by unauthorized parties and officials are the parties who try to maintain peace, while in centralized political systems wars are initiated by officials (Otterbein 1968:284).

To explain further intercorrelations that appear to indicate that centralized political systems without fraternal interest groups, indicated by the absence of polygyny, are "more prone to internal war," Otterbein asserts that

A possible interpretation is that since societies without fraternal interest groups lack conflict--specifically feuding--within the political community, their officials are more willing and able to engage in internal war (1968:284).

There is certainly no evidence in the intercorrelations to warrant such an interpretation, other than transformation of the Sumnerian hypothesis of an inverse relationship between internal and external violence.

Indeed, Otterbein (1968:285) goes on to test the amity-enmity complex, that the more frequently political communities fight those who are culturally dissimilar, the less likely they are to fight political communities that are culturally similar to themselves. As scaled, not only are the correlations not significant, they cannot support interpretation as tendencies, e.g., phi coefficients of .05, .11, .13, .14, .17, and .29. Nor does supplying an intervening variable of centralization tease out any significant inverse relationship between internal and external war. One interesting hypothesis that does have statistical support is given:

The assumption made by many writers that revenge and retaliation play a prominent role in war can be submitted to empirical test if it is formulated as follows: the more frequently the political communities of a cultural

unit are attacked, the more frequently they will attack other societies (Otterbein 1968:285).

Distribution of cases looks like this:

Frequency of being attacked	Frequency of Attacking		
	Infrequent	Continual or Frequent	
Continual/Frequent	5	18	23
Infrequent	15	9	24
	20	27	47

$$\phi = 0.41 \quad X^2 = 7.98 \quad p < 0.01$$

(Otterbein 1968:286).

It is not clear, however, which is the dependent and which is the independent variable. Does being attacked lead to attacking or vice versa or both? If one were to accept the statistical validity of this study, one could perhaps as credibly interpret the results as showing that the fewer the number of people authorized to initiate war, the more likely that both internal and external war will occur, and that violence escalates into more violence.

Furthermore, Otterbein's conclusions are confusing. Reiterating the findings of the 1965 study about fraternal interest groups, he says (1968:287), "In both studies it was found that the level of political complexity had no significant influence upon either feuding or internal war. That is, centralized political systems are just as likely to be

characterized by feuding and internal war as uncentralized political systems." This is patently a contradiction of what he says about his findings in 1965:

Not only is there a strong relationship between war and the absence of feuding in high level societies [centralized polities], but in low level societies [uncentralized polities] war and feuding are positively correlated. These results indicate that a society which has a high level of political integration is indeed capable of preventing the internal conflict which would be detrimental to its welfare. Although it would seem judicious for any society engaging in war with its neighbors to have internal cohesion, societies with only a low level of political authority apparently cannot control the feuding which is engendered by the presence of fraternal interest groups; in these societies war and feuding go hand-in-hand (Otterbein and Otterbein 1965:1478).

The rest of Otterbein's 1968 conclusions are riddled with contradictions:

To summarize, in uncentralized political systems fraternal interest groups are a determinant of both feuding and internal war, whereas in centralized political systems fraternal interest groups are a determinant of feuding but not internal war. It has been argued in both studies that officials in uncentralized political systems are unable to prevent fraternal interest groups from engaging in either feuding or internal war; on the other hand, it is difficult to understand why officials in centralized political systems--who apparently can prevent unauthorized raiding parties, including fraternal interest groups, from engaging in internal war--would permit fraternal interest groups to engage in feuding (Otterbein 1968:287).

Contrast this interpretation of his findings with the following, found in the paragraph immediately succeeding that quoted above and after statements that there is no relationship between war and feuding, or internal and external war.

However, when the relationship between war and feuding was controlled for level of political complexity, a strong relationship between war and the absence of

feuding was found in centralized political systems, but in uncentralized political systems war and feuding were positively correlated. Apparently officials in centralized political systems intervene to prevent development of feuding only when the society is engaged in war. On the other hand, controlling for level of political complexity does not affect the relationship between external and internal war. In other words, both uncentralized and centralized political systems within a cultural unit engage in internal war with the same frequency as they do external war. Seemingly officials in centralized political systems do not unite, and thereby eliminate internal war, when engaged in external war (Otterbein 1968: 287-88).

Otterbein includes the last interpretation in his conclusions after testing it as a hypothesis and finding no support, even tendencies, in his coefficients of association (1968:286-87).

In the end, Otterbein falls back on case illustration, non quantification, as evidence for his hypotheses, specifically the Yoruba wars of the 19th century. In my estimation, the only way his hypothesis about the amity-enmity complex could be demonstrated would be to deal only with cases of political centralization where political unit and cultural unit are identical. Then, by definition, any war that would erupt internally would be revolution or civil war. By a deft stroke, increasing internal peace and increasing centralization would coincide and thus remain consistent with mid-twentieth century democratic political philosophy that the state is the panacea for conflict, and that anarchy (in its pure sense) allows the natural inclination of male groups to fight to flourish. This judgment may be harsh, but Otterbein refuses to admit the seriousness of non-significance in his

correlations, which he must do to play the numbers game fairly, and persists in representing his hypotheses as proven, although indirectly and with a good deal of now-you-see-it-now-you-don't in his writing.

With regard to the mechanics of this study, the sample is again 50 cases, distributed geographically: Africa, 10 societies, Circum-Mediterranean 4, East Eurasia 8, Insular Pacific 9, North America 10, South America 9. This time, none of the 50 seem to be cultural duplicates. Because of uneven data, most of the N's for specific intercorrelations are lower than 50; they are usually 36 or 42 and, once, 47. With decreasing sample size, the mathematical effect of a single case increases, and with finer scaling, the number of cases in each cell diminishes and may even disappear. Inspect the following example.

	Uncentralized Political Systems			Centralized Political Systems		
	Internal War			Internal War		
	Continual or Frequent	Infrequent		Continual or Frequent	Infrequent	
Initiating party:						
Official	3	6	9	9	2	11
Anyone	12	2	14	1	1	2
Totals	15	8	23	10	3	13
Grand Total	36					

$$\begin{aligned}\phi &= .54 \\ \chi^2 &= 6.63 \\ p &= .01\end{aligned}$$

$$\begin{aligned}\phi &= -.27 \\ \chi^2 &= .97 \\ &\text{n.s.}\end{aligned}$$

(Otterbein 1968:283).

For these intercorrelations, Otterbein is working with sample sizes of 23 and 13. We are certainly faced with the dramatic effect of a single case.

Otterbein's research culminates in a study that tests relationships between political centralization and military activity:

All the above theories [about the evolution of the state and war] have in common the notion that societies become socially and politically more developed through time. As societies evolve, they come to wage war in more efficient ways. Sometimes war is seen as producing the evolution of societies; sometimes it is the political level of the societies which is seen as being responsible for the type of war waged. In either instance, level of political centralization and degree of military efficiency are viewed as being functionally related. This study tests a series of hypotheses which relate level of political centralization to various aspects of warfare. In most instances it will be shown that more efficient military

practices are associated with centralized political systems. Support is also provided for the general hypothesis that as societies become politically more centralized, they wage war in a more efficient manner. The study also demonstrates that societies which wage war efficiently are likely to be militarily successful (Otterbein 1970:2; italics added).

Military efficiency is measured by the presence of specific military practices that, in Otterbein's estimation, improve a fighting force's chances of defeating its opponent, e.g., shock weapons over projectile weapons (bombs over bullets) and "a high degree of subordination" over a low degree of subordination in organization of personnel (generals over warrior chiefs). Seventy-five per cent of the societies in the 1970 sample have a high degree of subordination. Each of the more efficient military practices is associated with centralized political systems, since such systems are assumed to be higher on the evolutionary scale. The presence or absence or the types of these and other traits--military organization, initiating party, diplomatic negotiations, tactical systems, protection, field fortifications, cavalry, fortified villages, causes of war--are coalesced into a military sophistication scale, and an efficiency rating is derived by dividing the total presences of the 11 military practices considered to be more efficient by the number of absences. Thus, if a society has a professional military organization with a high degree of subordination but no cavalry, it gets 2 points for presences and 1 for absences. Sophisticated societies are those that have a rating of .50 or higher;

unsophisticated ones, .49 or lower. Out of a total of 46 societies, 27 rated as being militarily unsophisticated and 19 as sophisticated (Otterbein 1970:70-74).

The sample of 50 societies this time is drawn with respect to Murdock's 6 geographical regions of the world, 1 each of the 60 culture areas. In the long run, admitting such limitations as availability of data in the English language in Lawrence, Kansas, during the spring of 1965, an availability sample of 50 societies was eventually drawn. No representative for 10 culture areas was drawn on any criteria. For the final sample, the geographical distribution is: Africa 10 societies, Circum-Mediterranean 4, East Eurasia 8, Insular Pacific 9, North America 10, South America 9. Of the 50, 4--the Copper Eskimo, Dorobo, Tikopia, and Toda--did not have any kind of military organization, due to isolation, whether indigenous or refuge (Otterbein 1970: 12-14, 20).

Let me set forth Otterbein's string of hypotheses and their statistical test results. (1) "The higher the level of political centralization the higher the degree of military sophistication" (Otterbein 1970:75). For the intercorrelations, the variable political systems are dichotomized into centralized and uncentralized; the variable military sophistication is dichotomized into low and high. Statistics used are point biserial with a (1-tailed?) t test worked on the efficiency ratings for every society in each cell, and phi.

	Military Sophistication			
	High	Low		
Political Systems				
Centralized	13	3	16	
Uncentralized	6	24	30	N = 46
$r_{pb} = .64$ $\varphi = .59$ $X^2 = 16.15$ $p = .001$ (Otterbein 1970:74).				

(2) "The higher the degree of military sophistication, the higher the casualty rates" [because those weapons that kill the most enemies also bring the user closer to the fighting, giving the enemy greater opportunity to inflict damage on personnel; the Zulu adoption of the short stabbing sword and the concomitant rise in casualties is an example]. Casualty rates are dichotomized into high (1/3 or more of the society's combatants killed) and low.

		Mortality Rates			
		High	Low		
Military Sophistication	High	8	7	15	
	Low	5	13	18	N = 33
$r_{pb} = .48$ $t = 3.04$ $p < .01$ $\varphi = .26$ $X^2 = 2.24$ n.s. (Otterbein 1970:83).					

(3) "The higher the degree of military sophistication, the more likely that the political communities of a cultural unit

will engage in frequent or continual internal war [warfare between political communities of the same cultural unit]."

This hypothesis is justified on the assumption that military sophistication must come about through frequent wars, i.e., multiple opportunities where weapons are tested and retained or discarded. There is a curious twist here: in no way are we to conclude that military sophistication leads to frequent wars, but instead the causal arrow points in the other direction. Military sophistication, as Otterbein presents it, is the dependent variable. The intercorrelation based upon 40 cases was found non-significant, so this hypothesis is discarded (Otterbein 1970:85-86). (4) "The higher the degree of military sophistication, the more likely that the political communities of a cultural unit will engage in frequent or continual offensive external war" (Otterbein 1970:88). The difference between this hypothesis and (3) is that external war is warfare between political communities of different cultural units, and here the society in question is the aggressor. This intercorrelation was found to be non-significant with a phi of .17, but the point biserial correlation of .32 was significant at $p < .05$, with unspecified degrees of freedom. (5) A third variation, "The higher the degree of military sophistication, the less likely that the political communities of a cultural unit will be attacked" (Otterbein 1970:90), turned out to be nonsignificant on all counts, thus providing non-confirmation of the deterrence

hypothesis, favorite of most cold war politicians, military personnel, and munitions manufacturers. Thus the only even faintly positive correlation is that societies with efficient means for killing people are likely to be the attackers and in the subsequent war lose a great many fighting men.

The final variable of concern to Otterbein is military success--what is it and does military sophistication bring it about? As a standard measure of success that is not based directly upon casualty rates, success in individual battles, or motives, Otterbein uses territorial boundaries of a society: do they expand, remain unchanged, alter but retain the same area, or contract? Military success is measured by territorial expansion.

It is difficult to imagine that a political community which is being defeated by its neighbors could be expanding territorially, unless it is being driven into marginal, uninhabited land (Vayda 1961). The four societies in this study which do not have military organizations--Copper Eskimo, Dorobo, Tikopia, and Toda--were apparently driven from more desirable lands (Otterbein 1970:93).

Of 39 societies in the sample, 13 are expanding territorially, 24 have remained unchanged, 3 are shifting, and 3 are contracting. Thus, (6) is generated: "The higher the degree of military sophistication, the more likely that the political communities of a cultural unit will be militarily successful" (Otterbein 1970:94). By lumping constant and shifting boundaries with contracting ones, the intercorrelation table looks like this.

		Military Success		
		Territory Expanding	Territory Not Expanding	
Military Sophistication	High	10	9	19
	Low	3	17	20

N = 39

$$\phi = .40 \quad X^2 = 6.21 \quad .01 < p < .02$$

$$r_{pb} = .44 \quad t = 2.96 \quad p < .01$$

(Otterbein 1970:95).

Apparently testing the traditional assumption that political centralization and military success go hand in hand (bands and tribes by definition do not have "true war," but chiefdoms and states do, therefore primitive societies cannot do as well as states), Otterbein formulates (7): "The higher the level of political centralization, the more likely that the political communities of a cultural unit will be militarily successful" (1970:97). The results were quite a surprise: the phi value was only .12. Further subclassification of the variables (1970:100-101) failed to alter the findings that only the efficiency of a group's techniques for killing is associated with expanding territorial domination. Thus in the end it is armaments and tactics that influence the outcome. While in the test of (1) above, there is a strong correlation between military sophistication and political centralization, and between (6) military sophistication and military success, there is no correlation between political centralization and military success. That is, while

Otterbein may identify centralized political organization as a more evolved form than uncentralized, and efficient methods of killing as more highly evolved than inefficient ones, they do not necessarily evolve together.

If it is true that a political community will probably defeat any political community which wages war in a manner less sophisticated than its own, then it is to be expected that some uncentralized political communities will be able to defeat some centralized political communities.

Although this study, because of the method used in drawing the sample does not provide direct evidence that there are uncentralized political communities with high military sophistication scale scores which have defeated centralized political communities with low military sophistication scale scores, it does provide indirect evidence that they may occur. The fact that a cultural unit is composed of centralized political communities does not ensure it of military success (Otterbein 1970: 107).

Thus the hypotheses offered as proven in the introduction to his study (1970:2) are not, in fact, linked in the way they appear to be. It looks to me as though military sophistication may be the independent variable. Is it possible that the relatively small number of people who make decisions in centralized political systems lead their people into frequent or continual wars, lose great numbers of them, and lose in the long run? Can we tentatively explain the fall of states, the eventual demise of every empire that has existed, so often falling before the "Barbarians who ride out of the North"? Military sophistication also may respond to a "Law of History": Increasing sophistication does not deter war, it generates it; but without efficient force of arms, a

society cannot expand and thus is faced with military failure, since Otterbein uses only expansion as an indicator of success. Otterbein does not offer these interpretations or indicate pursuing them, possibly because they go against traditional political and evolutionary theory, which sees whole complexes of social institutions sticking together, and which places a high value on social specialization and political centralization, without exploring the possibility that the evolution of higher biological forms is predicated on the existence of variation, plasticity, and non-specialization. That is, cultural specialization and concomitant centralization are to human beings what monstrous antlers were to the Irish Elk. This may be the human paradox, generated by dearly held beliefs.

In conclusion, then, Otterbein has found positive statistical correlations between efficiency in killing the enemy and territorial expansion. Adhering to these variables are frequent war and high mortality of combatants. That is it. That is the nature of any evolution of war. Otterbein offers his findings with a different cast:

It can be argued that this study provides evidence for the evolution of war, if the following assumption is made: namely that for any pair of alternative military practices, the more efficient practice is more evolved than the less efficient practice. The basis for making this assumption is that the more efficient military practice confers a survival advantage upon a political community by increasing the likelihood that its military organization will defeat the military organizations of other political communities, thus also increasing the likelihood that the political community will be the survivor in intersocietal struggles (1970:105).

I may remind the reader that the rhetoric of fight or die out in this case equates no territorial expansion with non-survival. Let me again quote Naroll in his Foreword to Otterbein's study:

Of the many observations Otterbein makes on the subject, the one that most impresses me is his assertion that in war the test of fitness is applied, not just to military practices, but to societies themselves. The ultimate test of fitness, of course, is survival. And no matter how well adapted a society may be in other respects, if it proves unable to cope with its enemies it has failed in its overall adaptation and must give way. Here lies the real impetus to evolution in warfare and in all aspects of culture that are connected with war. ...

Coldly viewed, warfare has enormous ecological significance. It is concerned, after all, with a most vital aspect of a society's environment--its enemies. If waged successfully, war means the preservation of a society's integrity and independence, and the defense or even increase of its territory and resources. If waged unsuccessfully, warfare may mean defeat, subjection, or even extermination. This is ecology in spades! (Otterbein 1970:iv-v).

It is also Social Darwinism in spades!

Otterbein performs one other interesting exercise with his sample: a scale of the "causes of war" or what Naroll (1966) calls military expectations, i.e., the reasons why a society goes to war. While Naroll identifies military expectations as those given by the people themselves and disavows the possibility of talking meaningfully about causes, Otterbein equates causes with "goals," which may confuse cause with causal effect, and he allows his coders to decide what the goals of a society are (1970:64). Otterbein concludes that his replication of Naroll's 1966 scale finds the same

results, and that both refute Wright's 1942 scale, where he determined that the order of rising expectations in war was defense, social, economic, and political. The order of the scales follows, with Otterbein's lumping of categories to gain comparability with Naroll's indicated.

<u>Wright's Scale</u>	<u>Naroll's Scale</u>	<u>Otterbein's Scale</u>
political	political control	subjugation and tribute
economic	prestige	trophies and honors
social	plunder	land
defense	revenge and defense	plunder
		revenge
		defense

Otterbein's positioning of defense and revenge as primary did not come out of his coding of the data:

... although I recorded what I thought were all the important causes of war, only 29 out of 46 societies fought for either revenge or defense. Apparently if I found several more sophisticated reasons for war, I did not seek and record mentions of revenge and defense. Therefore, I assume that all the societies in my sample (except those without military organizations) fought either for revenge or defense (1970:65).

Otterbein's justification for this inference is apparently that defense and revenge-and-defense appeared first in Wright's and Naroll's scales, an odd practice if one is supposed to be replicating and therefore testing just those scales. I am guilty of the same practice in my first cross-cultural warfare study, with 35 societies; I made the

assumption that revenge and defense had to be there, even if not mentioned specifically in the data--after all, everyone fights back and wants to get even, right? But not necessarily so. The interpretation of Naroll's and Otterbein's scales, then, is that either revenge or defense is always present in war; where there are reasons of prestige, there are also always economic reasons; where there are goals of political control, there are also social, economic, and revenge-or-defense goals (Otterbein 1970:65-67).

This concludes my discussion of Otterbein's contributions to warfare studies. I may only point out again that while the 1970 study is more sophisticated and discriminating in selection of sample, doubtless under the influence of Naroll, we are still confronted with the problem of small sample size and the mathematical effect of single cases. May I also point out for the last time that even if Otterbein's correlations are statistically valid, they are subject to various interpretation, and Otterbein has not pointed out the instances where they seem to refute orthodox political and simplistic evolutionary theory.

Ember and Ember (1971) perform a series of tests of explanations as to why matrilocal residence occurs rather than the patrilocal mode, specifically the traditional explanation in anthropology that sexual division of labor is the precondition for post-marital residence. They refine Murdock's division of labor variables in the Ethnographic

Atlas to these attributes: women do more than men overall in the subsistence activities; neither sex predominates; men do more than women. They lump the residence coding into 3 attributes: females localized, neither sex localized, males localized. They have found no significant relationships in the crucial cells of the 3 x 3 table on a world-wide sample of 288 societies. In retesting Driver and Massey's findings for North America (1957), one of the original cases upon which division of labor was found to be the significant condition for post-marital residence, Ember and Ember's results duplicate those of Driver and Massey but only for North America. The relationship between division of labor and residence does not hold up on a world-wide sample, and it varies widely in the other geographic regions of the world. The 2 examples the Embers give are a nonsignificant relationship in South America, and a nearly significant negative relationship in Oceania (1971:575).

In looking for other possible determiners of residence--specifically conditions that give one sex greater social status than the other--Ember and Ember find polygyny, herding, slavery, and multilocal political organization to be significantly associated with male localization on the large sample, but only herding and multilocal political organization are good predictors of male localization. The Embers use the term "predictors" although their statistics are only Phi and Fisher's Exact for probability levels. They do not

use a coefficient of predictability. A last variable, warfare, could not be based on the Ethnographic Atlas since it does not code that variable, so Ember and Ember use Otterbein and Otterbein's 1965 sample and find no relationship between continual warfare and male localization. Ember and Ember conclude that an explanation of residence that is based upon sexual social status is not very strong and that, in effect, there may be unidentified intervening variables (1971:576-77). Using the Otterbeins' 1965 sample, they proceed to identify continual warfare as the intervening variable in the association between division of labor and residence: where warfare is continual but men do most of the subsistence work, residence is usually patrilocal; where warfare is continual but women do most of the work, residence is usually matrilocal; where there is no continual warfare, residence still tends to be patrilocal regardless of which sex does most of the subsistence work. The task remaining is to identify what circumstances in warfare influence whether or not the men could keep up with their subsistence work. They make an interesting [and erroneous] assumption that the normal division of subsistence labor is patridominant because women are taking care of the children and "other universally ascribed duties around the home" (1971:578). This assumption gets them into some difficulty later in their interpretations, specifically trying to explain the cases in which women do as much as men without recent warfare as due to "culture lag" (1971:581).

On their own warfare sample of 22 societies (Africa 10, Circum-Mediterranean 1, East Eurasia 0, Insular Pacific 3, North America 6, South America 2), Ember and Ember test their first hypothesis that "men will do more than women in subsistence unless warfare prevents them from doing so" (1971:579). Circumstances seen as preventive are those where

... (1) warfare occurs frequently all year round; (2) warfare occurs at least once every two years but potentially can occur at any time (as indicated by year-round sentries or fortified villages); (3) warfare occurs frequently only at certain times of the year and work in the dominant subsistence activity has to be done at that time ...; (4) warfare occurs at least once a year and is at least sometimes offensive, the travel to and/or the fighting itself is at least sometimes long in duration (that is, more than a day), and in addition work in the dominant subsistence activity has to be done while fighting takes place outside the community (if warfare occurs only within the confines of the community, it will disrupt all subsistence labor and hence will not prevent the men from doing more than the women) (1971:578).

Under all other conditions of warfare, men will assume their normal dominance in subsistence labor. Ember's and Ember's predictions of the division of labor according to these criteria intercorrelated with the actual division of labor for the 22 societies are marginally significant ($FE = .074$).

They next test the influence of residence upon warfare, with the intervening variable assumed to be descent, in the second hypothesis that matrilineal communities will have wars only with other societies (external warfare) because the males in the matrilineal community will be fighting consanguineal kin in any internal warfare, but patrilineal communities will fight either other societies or other communities

of their own culture, because the consanguineal ties of the men are all in their home community. This relationship was highly significant--on a sample of 18 ($\varphi = .79$; $FE = < .002$). The geographical representation of this sample is 7 Africa societies, Insular Pacific 2, North America 5, South America 4. Uneasy, Ember and Ember replicated this intercorrelation on a larger sample of 33, and again found the relationship to be significant ($\varphi = .65$; $FE < .01$). The geographical distribution of this sample is Africa 12, Circum-Mediterranean 1, Insular Pacific 5, North America 9, South America 6.

At this point Ember and Ember have 2 intercorrelations and 3 variables: war, division of labor, and post-marital residence. The second and the third variables do not intercorrelate significantly, or they did not in the original large sample. Therefore, the authors proceed to interpolate these variables through verbal, rather than statistical, reasoning. I quote their exegesis at length because it is interesting in argument and inference and because some of my own intercorrelations are referable to those of the Embers.

The initial state is the situation where warfare is present. (Judging from our sample data, this is the statistically normal situation: most societies as of the time of description either were still fighting or had only recently stopped fighting, the latter being the case usually because of pacification by a colonial or internally conquering power.) If the warfare is at least sometimes internal, there is only one possible resultant state for residence but two possible resultant states for division of labor. The one possible state for residence, when warfare is present and at least sometimes internal, is patrilocal. Again judging from our data, the fact that warfare is at least sometimes internal appears to require

patrilineally related males to be localized after their marriages. Or, in other words, if fighting occurs between neighboring communities, families would want to keep their fighters at home for protection. As for division of labor, the two possible states are produced by the interaction between the internal warfare and the nature of subsistence work. If subsistence work has to be done while fighting occurs, the normally patridominant division of labor will be disrupted and females will come to predominate (or at least contribute equally to subsistence). ... On the other hand, if work does not have to be done while fighting occurs, the males will continue to predominate in the division of labor.

Turning now to what happens when a purely external pattern of warfare emerges, there are two possible resultant states for division of labor and two for residence. If subsistence work does not have to be done while the purely external warfare occurs, the normally patridominant division of labor will be disrupted such that the females will come to predominate (or at least contribute equally to subsistence), and the pattern of residence will change to matrilocal. This latter change will occur because, in the absence of local fighting (internal warfare), patrilineally related males do not have to be localized; that is, there would be no need to keep the men at home after marriage for protection. Thus, because the females support the kin group, they rather than the males would be kept at home after marriage (1971:584-85).

Note the confusion of cause and causal effect and the evidence given to support it--war is primary because most societies were experiencing it at time of contact; note also the disregard for geographical variation, which made the first part of the research a significant contribution. The absence of this variable and the distribution by inspection of the societies makes one wonder if not controlling for geography was a tactical decision or based upon the assumption that if a correlation appears to hold on a world-wide sample, geographical variation is irrelevant. The initial reply, of course, is to

question whether or not the world-wide sample is really "world-wide."

In summary, Ember and Ember have integrated the effects of the 3 variables into a "model":

... patrilocal residence is avored by the presence of at least some internal warfare (that is, warfare within the society), whether or not such warfare interferes with a patridominant division of labor; and matrilocal residence is avored by purely external warfare if such warfare compels the division of labor to become matridominant (1971:503; italics added).

Note that the Embers' use of the phrase "avored by" is a translation of coefficients into the idea that warfare is one independent variable, patridominant labor and patrilocal residence are two other independent variables, and matridominant labor and matrilocal residence are variables dependent upon warfare. There is some inconsistency: "internal warfare" and "external warfare" are variables dependent upon the form of residence. So is warfare independent or dependent?

As a change in point of view from the strictly political toward broader influences of culture upon the expression of human aggression, Russell (1972) performs the purely inductive technique of factor analysis upon a large group of characteristics drawn from Textor's A Cross-Cultural Summary (1967) that have to do, in Russell's estimation, with aggression. His task is "to isolate the primary groups or patterns of cultural characteristics related to warfare " (1972:279), actually to produce a psychocultural explanation of war. He has made up a matrix of Phi coefficients of the correlations

between Textor's measures--bellicosity, emphasis on military glory, emphasis on killing and torturing enemies, amount of warfare--and another group of measures which, although they do not have a direct relationship with the core warfare variables, do have strong relationships with other variables that are related to the warfare variables or that may indicate new leads, e.g., 5 geographic areas, which subsequently do not become part of any factor. Russell uses 78 variables altogether, for 400 cultures. What he has found are clumps of variables sticking to each other. Military glory, bellicosity, torturing and killing of the enemy, and the amount of warfare clump, along with murder, assault, and theft.

Thus, all measures of every type of aggression that have been examined load on this one factor, and apparently the more violent the aggression the stronger the loading. This factor, then appears to represent not just warfare but all forms of cultural aggression, internal as well as external to a society, which means in turn that all forms of aggression tend to be strongly related to each other. Evidently cultures tend to vary not only in their tendency to be warlike but also in their general level of hostility, and this hostility will take any and all available forms (Russell 1972:291).

Through the shifting and sifting of factor analysis, Russell has also found that narcissism, indicated by "boastfulness," "sensitivity to insult," and "invidious display of wealth," is closely related to warfare (1972:292). He also isolates a third clump of characteristics--achievement motivation: "pressure to achieve," "anxiety over not achieving," "religion supports achievement," "invidious display of wealth," and "existence of entrepreneurs," commenting that

entrepreneurial activity may be approved and sublimated cultural hostility, i.e., a mirror image of theft. Clinging to the achievement clump, Russell has found that only 2 of 12 anxiety measures (ironically, "aggression anxiety" and "anxiety about being self-reliant" [as contrasted to anxiety about not being self-reliant]) were not positively related to warfare.

This is strong evidence of the great amount of underlying anxiety and insecurity found in warlike cultures. Thus, it is possible that high levels of hostility represent as much attempts to compensate or defend against feelings of insecurity and anxiety as an expression of intrinsic aggression; of course, both could be involved. In any case, this factor indicates that members of a warlike culture are not only more hostile but also more narcissistic and insecure than members of peaceful society (1972:295).

Russell's other psychocultural findings that have positive relationships with the warfare factor are "infant aggression satisfaction," "extramarital sexual relations are punished," "premarital sexual relations are punished," and "sex anxiety in adult is high" (1972:296).

In summary, when all these results are examined together, a rather consistent picture of the psychocultural aspects of warlike cultures appears. It is evident that warlikeness is only one form that hostility in a culture may take, and that the level of hostility in a culture varies as a whole from culture to culture. This hostility may appear in the form of bellicosity, personal crime, theft, or emphasis on achievement, wealth, and entrepreneurial activities. In regard to dynamics, the level of hostility is positively related to the amount of restrictiveness of punitiveness that the culture places on its members at all age levels. In adolescence and adulthood, the punishment of extramarital and premarital sexual relations is particularly evident. The result of this punitiveness is evident not only in cultural hostility but also in a deep sense of anxiety and insecurity, which appears in narcissistic attitudes, such as boasting and

sensitivity to insult or an exhibitionistic display of wealth, and also in the many measures of anxiety used in this study.

... The members of such cultures are certainly neither happy nor "mentally healthy." Thus, the ramifications of warlikeness extend far beyond bellicose behavior itself. However, it should be noted that this effect is a general trend among cultures and that any single culture may not exhibit this pattern (Russell 1972:297).

Russell goes on to argue that formal characteristics of culture, such as descent and complexity, are probably only weakly related to warfare, and that the clusterings of psychocultural variables are not only strongly related to war but are also strongly related to each other and thus are a manifestation of universal features. Therefore, while materialist interpretations of differences among cultures and of history are important, for some phenomena such as warfare the psychocultural interpretation is much more important--political and economic causes "rest on a general level of hostility and anxiety. A high level of hostility probably potentiates certain political actions as well as making other actions impossible" (1972:304). Moreover, if psychocultural variables are primary, not formal or material ones, " ... highly complex cultures could be as peaceful as simple cultures if they had the right psychocultural patterns" (1972:303). The "right" patterns would be satisfaction of needs and reduction of social anxiety. In the face of Naroll's (1966; 1969) and Otterbein's findings (1970) that warlike cultures tend to expand and the implication that therefore

changing warlike attitudes would amount to "psychological disarmament," Russell concludes with the observation that Western societies can afford considerable change: "... with the high level of hostility that now exists in this society a great amount of change could occur without placing us in danger of being overrun; rather it would probably enable us to take a more positive role in creating world peace" (1972: 306-307).

While Russell's claims are a refreshing change from those of the kill-or-be-killed school, one is still left with the problem of inherent conditions--imperatives of the psyche this time. Nevertheless, as a possible insight into why patterns of aggression are perpetuated generation after generation and may fall upon especially receptive ground in diffusion, an explanation formulated in terms of deprivation and anxiety, despite persistent vagueness and circularity, speaks in more life-like terms despite its genetic relationship to the frustration-aggression hypothesis. Russell includes 5 geographical areas in his variables--Africa, East Eurasia, Pacific, North America, South America--but only mentions geography once in his findings in a passing comment about possible loading in Africa. History and diffusion once again are dropped out, which is to be expected, I suppose, in most psychological approaches. I also would like to note that insecurity and anxiety may not generate hostility and thereby warlikeness but can arise out of trying to live up to the

cultural ideals of a warrior society. Yanomamo warriors commonly get stomach aches and sore feet on the way to a raid, which excuses them from further action. Modern businessmen wear out their hearts, stomachs, and nerves trying to conform to cultural standards of acceptable behavior, an achievement that most can state they want.

Sipes (1973) tests 2 familiar competing explanations for relationships among aggression, sports, and war--the Drive Discharge Model and the Culture Pattern Model. The former claims that aggression, whether innate or acquired, builds up in an individual and can be drained off through sports, as an alternative to war (cathartic discharge; functional or moral equivalents). Thus, those societies with more war will have less combative sports activity, and those with less will have greater sports activity. The second model claims that aggressive behavior is primarily learned and is therefore cultural, and that since behavior and values associated with war are also associated with sports, the 2 phenomena reinforce rather than neutralize each other and their relative presence or absence will be found to vary together (cultural consistency; themes; configurations) (Sipes 1973:64-67).

Sipes selects 10 relatively warlike societies from Otterbein's 1968 sample that had adequate data on sports, but since he is unable to match them with 10 relatively peaceful societies from the same source, because Otterbein's sample

could supply only 4, he adds 6 of his own choice. The 10 societies having frequent or continual war that Sipes uses are Tibet, Thai, Sema Naga, Ila, Comox, Aztec, Tehuelche, Abipon, Timbira, and Mundurucu. The 10 peaceful societies are the Toda, Dorobo, Copper Eskimo, Tikopia, Semang, Bhil, Hutterite, Lapp, Kung Bushman, and Naskapi-Montagnais.

Sipes' results for the 20 primitive societies are:

		Combative Sports		
		Yes	No	
Warlike	Yes	9	1	10
	No	2	8	10
		11	9	

$$\phi = .6035 \quad FE < .0028$$

(1973:71).

Obviously, he concludes that the Culture Pattern Model is valid while the Drive Discharge Model is not (1973:71).

Sipes performs a second test of the 2 competing theories upon a single case over time, the United States of America from World War II through the Korean Conflict and the Indochina War. He has broken down the dependent variable of sports into 20 spectator/participant and combative/non-combative types, selecting 4 as type-specimens: football (combative-spectator), hunting (combative-participant), baseball (non-combative-spectator), and race betting (non-combative-participant). Using quantitative data as indices

of relative interest in the sports--hunting licenses issued, horse racing revenue, number of spectators at National League baseball games and National Football League games--and inter-correlating and graphing these with the percentage of adult males in the military from 1920 to 1970, Sipes again has found support for the Culture Pattern Model. Hunting, football, and race betting show a continuous, steep, upward trend over the half-century, while baseball (the non-combative, non-participant sport) has experienced an overall slight decline in interest, with sharp drops during all 3 wars.

As a final observation, Sipes notes that since the Drive Discharge Model is so much a part of Western science, there should be further testing of the drive versus the patterning model on other expressions of violence, such as "suicide, murder, punishment of deviants, drug use, physical assault on family or other community members, gossip, psychogenic illnesses, and malevolent magic" (1973:80). Despite the methodological rashness of using a sample of merely 20 societies--a practice apparently not discouraged by Naroll and Otterbein, whom Sipes credits with critical review (1973:80)--I am impressed by Sipes' willingness to test a dearly held belief and broadcast its failing to the world. Given the limited variables, there should not be much difficulty in expanding the sample size to one more reliable. I hope that Sipes does just that.

My preliminary cross-cultural warfare study in 1968 (Nammour n.d.) is concerned with the participation of women in warfare, testing 7 specific hypotheses on an availability sample of 35 societies sprinkled around the world: New World 13, Africa and the Circum-Mediterranean 11, Eastern Europe and the Insular Pacific 11. Each of these cultures belongs to only 1 of Murdock's (1967) culture clusters. Some control for diffusion is achieved by using Murdock's criterion that cultures be separated by 3° of latitude or longitude in the tropical zone, $4-5^{\circ}$ in the temperate zone 35° from the equator, and 6° in the frigid zone. The hypotheses tested are: (1) The participation of women in military activities is associated with the frequency of warfare. (2) The participation of women is more strongly associated with military expectations of revenge, defense, and prestige than with expectations of territorial gain. (3) The level of participation of women in warfare is dependent upon the political complexity of a society. The relationship is obverse: if political complexity is high, participation of women will be low. (4) The participation of women in warfare decreases as the dominant subsistence activity becomes more complex. (5) If women are dominant in the division of labor in the dominant subsistence activity, but war is frequent, female participation will be low because they must devote their energies to subsistence activity in order to support the military complex. (6) Where warfare is frequent, residence will be

patrilocal, but where participation of women is high, residence will not be patrilocal, on the assumption that a woman residing with her consanguineal kin will be more involved in warring activities than a woman residing with her husband's family. (7) Since descent has been demonstrated to succeed residence (Driver and Massey 1957), the relationships among descent, frequency of warfare, and participation of women are expected to show stronger relationships between respective attributes but in the same direction as correlations for residence. Only hypotheses (1), (2), and (4) produced statistically significant results, despite elaborate partialling of variables. Including tendencies indicated by Yule's Q and the phi coefficients, although they are not significant, in 1968 I interpreted the intercorrelations as hinting at the following relationships. If a tribe is involved in frequent warfare, women will participate vigorously, especially in ritual activity. Women participate in war for prestige, but not for revenge, defense, and plunder. When the level of political complexity is high, i.e., when 2 or more jurisdictional levels beyond the local level exist, women do not participate in warfare. Women will be most involved in warfare in societies having hunting and fishing economies, for which the highest military expectation is prestige. While warfare may be frequent in those societies with matri-dominant labor, women do not seem to participate because they are active in providing enough to eat through farming while the men are

off to war. Frequency of war is associated with patrilocal residence but more strongly associated with patrilineal descent. This hint is interesting in light of Otterbein's findings (1965:1968) that patrilocality is not an indicator that men will act in concert, while polygyny is. Perhaps mere residence is not enough; jural claims for cooperation, whether through patrilineality or polygyny, must exist. War does not occur often with matrilocal residence and less often with matrilineal descent. Participation of women in war is less active with patrilocal residence than with patrilineal descent and does not occur with matrilocal residence or matrilineal descent.

Methodologically, this work is fatally weak in sample size, and any findings are offered only tentatively and as things to look for using a larger sample. Other than sample, the mechanics of my original warfare study are sound.

This concludes my presentation and discussion of precedents in quantitative warfare research in anthropology. The next section presents the coding and statistical procedures that I have used in the quantitative research at hand.

Coding and Statistical Procedures

1. The data.

In my research effort I used 2 kinds of data: material analyzed and compiled in the Ethnographic Atlas (Murdock 1967) and its successor in refinement for the Standard Sample, the "Cross-Cultural Codes" (Murdock and Darrow 1970; Murdock and Wilson 1972; Tuden and Marshall 1972), and published ethnographic sources that I have coded for the warfare variables. Appendix E contains the bibliographic references used, listed separately by society. Reliability of the published codes I have assumed to be adequate, and the newer "Cross-Cultural Codes" include information on reliability checks. In any kind of categorizing there is bound to be some disagreement between coders and disagreement by anthropologists using the codes with the decisions made by their architects. At the same time, occasional disagreement does not seem to be adequate grounds for dismissing the codes out of hand as inevitably wrong and therefore useless. In my experience, it seems that some of those who attack quantification ardently are themselves engaged in continuous cross-cultural comparison, albeit on a non-mathematical basis. In gathering and coding information on warfare, two other persons also have done some coding, and a portion of their work I repeated independently as an informal reliability check. Its informality exists because I have not mathematically

measured the percentages of disagreement between myself and other coders. The check does have a formal aspect in that I scrupulously did not look at others' decisions before I made my own. The major difference between the other coders' work and my own is that I have felt compelled in most cases to read comprehensively on the culture before making coding decisions--in a sense putting the culture back together before I could take it apart, again. My note-taking to support coding decisions tended to be more copious. Doing such contextual work does increase one's irritation with the injustices of classification; there is so much that must be cut off around the edges. Occasionally I have used another coding technique, one used by Ember and Ember (1971), in which I worked together with a second coder making cooperative decisions. The cultural representatives of Japan and China are 2 examples of societies dealt with in this manner. Below I present each variable, its attributes, and the coding procedure.

2. Statistical manipulation of the data.

When I wrote the proposal for this project, I had planned to use the Stanford Statistical Package (Nie, Bent, and Hull 1972) for the computer work on the intercorrelations, but at the time California State University, Sacramento, unlike the University of Oregon, did not own the compiler for that program, and none of their existing canned

programs satisfied my needs. Therefore, I had a program written to intercorrelate every variable with every other variable in fourfold tables and to perform 8 statistics on each correlation, to exhaust the mathematical possibilities of a limited format using qualitative variables.

The statistics include Yule's Q, phi, chi-square, Pearson's C, Goodman and Kruskal's Tau on columns and rows, and Goodman and Kruskal's Lambda on columns and rows. As gross indicators of geographical variation in correlation coefficients, I used Pearson's r . Yule's Q is a measure of 1-way association and has virtually no utility by itself, but when used with phi, which is a measure of 2-way association, the coefficients complement each other and can be used only on 2 x 2 tables. For instance, if 2-way association is very weak in a particular case, there may still be a strong 1-way association between variables which certainly should be salvaged and may be quite important in detecting the direction of attraction between variables. Chi-square, of course, is the measure used to derive probability values. Since chi-square raw values are not comparable, Pearson's C is a normed value derived from chi-square that is comparable to other Cs. Using fourfold tables, phi and C are identical or close in value for lower coefficients, but since C can reach unity and phi cannot, higher values manifest increasing discrepancy. For instance, in a perfect correlation, C will be 1.0 while phi will be .707, with either a positive or negative sign.

Carrying a sign is the virtue that ϕ has to counterbalance C 's ability to reach unity but without a sign. If I relied upon C s alone, I would not know whether a relationship was a perfect positive correlation or a perfect negative correlation. Lambda and tau are measures of association that give predictive values: how often does knowledge of the independent variable allow one to predict its association with a particular dependent variable. The beauty of both these measures is that they are directional. Knowledge of variable A may not give any capacity to predict A 's association with B , but knowledge of B may allow one to predict the presence of A to a specified degree. While lambda and tau do the same sort of thing, they are mathematically derived in different ways. By and large in my work lambda has been more useful than tau, but there are instances in which lambda may be 0 but tau may be greater than 0. By using both measures, I hoped to extract all possible information from the relationships between qualitative variables. Since Q and ϕ are not predictive measures, lambda and tau are powerful tools. It is possible to have high ϕ values and highly significant chi-square values but no predictive capability at all--lambda and tau may be 0. In such a case, the non-predictive measures of association simply summarize how often in one's sample the variables occur together but give no insight into whether or not the variables mathematically generate indications of an inherent relationship. The absence of such

indications severely limits the interpretation of statistical results, especially for those workers inclined toward universal theorizing.

The computer program has performed one last service. Each society in the Standard Sample has an identity number. Each member of every cell in the tables is identified by its number, so that I not only have cell frequencies but also know who is where. This is especially important for my basic hypothesis, of significant geographical variation in the configurations of the variables.

Each variable has been intercorrelated with every other variable in the interests of program efficiency, first for all the societies within each of the 6 geographical regions and then for all of them together forming the world-wide sample. Further statistical analysis of relationships I have performed by hand. The interpretation of all the mathematical bits and pieces is by inspection. This task can be likened to creating a 4-dimensional scene (including time) out of 2-dimensional puzzle pieces.

The Variables

Because I have used such a large number of variables in my research, the most straightforward way to present them is to list each one, giving for each the source of the data, how it has been coded, and how I have categorized the attributes for statistical manipulation. The first group of

variables are familiar sociocultural ones that I thought pertinent to warfare at the time I selected them. The second group consists of variables dealing with the practice of and attitudes toward warfare. Each variable has a number assigned for the computer program and retained for all tables included here. A separate number is used for each variation of a basic variable. Refer to Appendix B for the code sheets and to Appendix C for the variable codings for each society in the sample.

1. Regional identification. I have used the identifications from the Ethnographic Atlas (Murdock 1967), hereafter referred to as the EA, of Africa, Circum-Mediterranean, East Eurasia, Insular Pacific, North America, and South America without breaking these regions down into finer units, since the Ns would then be too small for any statistical analysis. In the computer run on the world sample, I have treated each geographical region as a variable, to be intercorrelated with all other variables but not with each other.

2. Polygyny. This form of the family has been coded present if it has an incidence of at least 20 per cent for a given society and absent if less. Originally I used the codings of the EA but then revised them to conform to the revisions of "Cross-Cultural Codes 3" (Murdock and Wilson 1972).

3, 4. Marital residence. The attributes of this variable have been scaled 2 ways and each correlated with all the

other variables. The first dichotomizes residence into patrilocal or virilocal on one hand and matrilocal, avunculocal, or uxoriocal on the other. The second dichotomizes residence into patrilocal, virilocal, matrilocal, avunculocal, or uxoriocal, in contrast to ambilocal or neolocal. The source for the codings is the EA.

5. Community organization. Attributes for this variable as coded in the EA have been lumped, perhaps awkwardly, into dichotomies identified as endogamy prevalent (demes, segmented communities without local exogamy, agamous communities) and exogamy prevalent (exogamous communities, segmented communities with local exogamy, clan-communities). The source is the EA, although CCC3 has a better variable, "intercommunity marriage," that more suitably describes these marriage patterns. My later judgment is that I should not have used the EA data. I attempted to use the CCC3 material to revise the EA codings and was somewhat successful, but the 2 variables are not the same and the result is patchwork.

6, 7, 8. Settlement pattern. The attributes of the variables have been scaled 3 ways and each intercorrelated with all of the other variables. The first lumps nomadic bands, seminomadic communities, and semisedentary communities in contrast to all other types; the second lumps compact but impermanent settlements, neighborhoods of dispersed family homesteads, and separated hamlets; the third does the same with compact, relatively permanent settlements and complex

settlements. The source of data is the EA.

9, 10, 11. Mean size of local community. This variable is scaled 3 ways, opposing each focal cluster of attributes to all the others in the dichotomy. The first lumps communities with fewer than 50 persons, 50-99 persons, 100-199 persons, and 200-399 persons. The second sifts out the smallest communities--fewer than 50 persons and 50-99 persons--while the third in turn pulls communities out having 100-399 persons. In all 3 scalings, the attributes consistently placed in opposition in the dichotomy are communities of 400-1000 persons, 1000 without any town of more than 5000, 1 or more towns of 5000-50,000, and 1 or more cities of more than 50,000 persons. I have placed so much emphasis on the smaller groups, trying to find some population floor beneath which a community cannot support intensive warfare. The code source was initially the EA, corrected by the revisions in community size coded in CCC 3 (Murdock and Wilson 1972).

Population density per square mile. Fortunately, "Cross-Cultural Code 3" (Murdock and Wilson 1972) also includes coding for this variable. Coding it independently would have been difficult. Murdock and Wilson concentrated on "... the density of population in the area exploited or controlled by the focal or typical community" (1972: 257). The authors developed 7 attributes, which I have scaled in several ways: each one against all of the others, and then dichotomies that successively move one attribute at a time from one side to

the other. This variable, vital as it is, was not included in the full computer run. Preliminary hand-computations are discussed in Chapter III.

12, 13, 14. Political complexity. This variable is based upon Murdock's variable "Jurisdictional Hierarchy" in the EA, which is coded for both the number of local jurisdictional levels and the number beyond the local community. "Cross-Cultural Codes 3 and 4" also include variables on political organization, but I found them to be either too detailed and tedious or too broad to be of use to me at this time. I did use the CCCs as a check against my interpretation of the original rough EA classifications. This variable remains a crude indicator, possibly because political organization and the distribution of power exists in myriad forms. Local complexity is dichotomized into 0-2 against 3 or 4 levels. Complexity beyond the local community is scaled 2 ways: 2-4 levels in contrast to 0 or 1 level, and 1-4 levels against the absence of any superordinate jurisdiction. In the terminology of CCC 4 (Tuden and Marshall 1972), from 2 to 4 jurisdictional levels would correspond to petty paramount chiefdoms, small states, and large states.

15. Subsistence. The dominant subsistence activity of each society has been intercorrelated 2 ways: a dichotomy between food-collectors (gathering, fishing, hunting) and food-producers (pastoralism, incipient agriculture, extensive agriculture, intensive agriculture), and each subsistence

technique taken separately but intercorrelated with only a few selected warfare variables. The source for the subsistence coding is primarily the EA, supplemented by "Cross-Cultural Codes 1" (Murdock and Morrow 1970) for those Standard Sample societies not included in the EA.

16, 17. Descent. The attributes for this variable have been scaled 2 ways. The first constitutes a dichotomy between patrilineality and matrilineality only, the second poses unilineal against duolateral, bilateral, and ambilineal organization. The coding source is the EA.

18. Division of labor. With this variable I wanted to test for any relationship between the warfare variables and which sex was doing most of the work in the dominant subsistence activity, following up an idea by Ember and Ember (1971). The attributes of the variable are dichotomized only 1 way: males alone or almost alone and males appreciably more contrasted to a lumping of equal participation, females appreciably more, females alone or almost alone, and sex irrelevant. The source of the codings is the EA. I have made a mistake in not including as variables the second most important subsistence activity of a society and which sex does most of the work in that activity. I am suspicious that the assumption that if men do most of the work in an activity therefore it must be the dominant one has influenced judgments in coding these variables. If this is so, then possibly even with such data the effect of women in subsistence may be teased out of

coding activities judged not to be the most important. While the Cross-Cultural Codes include refined coding on subsistence, they do not include division of labor for any activity.

19. Class stratification. The attributes of this variable from the EA are simply dichotomized into one cluster that includes an elite based on control of resources, an hereditary aristocracy, and social classes contrasted with a second made up of the absence of class stratification among freemen or distinctions of wealth only. The source is the EA.

20. Succession of local headman. The attributes of this variable are dichotomized into hereditary succession, regardless of the rule of inheritance, and nonhereditary succession, whether by appointment, seniority, influence, election, or informal consensus. The source is the EA.

The remaining variables are designed to deal with the issue at hand, warfare in human society. I shall continue to list them, giving the dichotomies of the attributes, the rationale for inclusion of the variable, and sources for those used elsewhere. Following the precedent set by Ember and Ember (1971), if warfare does not exist at the time pinpointed by Murdock and White (1969), but it has existed within 50 years prior to that date, I have coded for it as though it were still going on. In such cases what usually has happened is European contact and pacification. The effect of this practice is to recategorize some societies in

the time slots identified by Murdock and White, in particular putting ones pinpointed in the first half of the 20th century back into the late 19th century.

21. Frequency of war between political communities of the same cultural unit. This is what Otterbein calls "internal war." Otterbein (1970:3) uses Naroll's definition of a "territorial team," but renames it a political community, as a "group of people whose membership is defined in terms of occupancy of a common territory and who have an official with the special function of announcing group decisions--a function exercised at least once a year" (Naroll 1964:286). Otterbein does not use Naroll's complementary concept of a "cultunit" but chooses instead Malinowski's (1941) concept of cultural unit.

Contiguous political communities which are culturally similar comprise a cultural unit. In most instances, the cultural unit is the same as a society, which is the unit used in the universe from which the sample is drawn. Although a single political community may be coterminous with a cultural unit, a cultural unit usually consists of more than one political community (Otterbein 1970:3).

These definitions are largely academic, however, because Otterbein uses cultural units as defined by Murdock in the Ethnographic Atlas, and I have used the target communities specified by Murdock and White (1969) for the Standard Sample.

Some estimate of occurrence is obviously basic in warfare studies. The attributes are continual (perpetual, constant), frequent (common, intensive), and infrequent or never

(occasional, sporadic, rare). The dichotomy is between continual and frequent war, and infrequent war. The form of this variable closely follows that of Otterbein (1970:143). Selecting the most appropriate coding slot is based upon terms used by the ethnographers themselves (e.g., "continual," "common") or, where contextual description does not appear to support the ethnographer's adjective, a revised assessment by the coder supported by notes on the pertinent information. This variable does not distinguish between the target community and other communities as to who is attacking and who is being attacked. This is a weakness in the variable, but it probably cannot be helped because the target societies in the Standard Sample may be either a single community or the whole culture, whose member communities one discovers do fight with each other. The distinction between feud and warfare within a cultural unit is of course observed, usually relying upon the identification of the ethnographer. Doubtless many cases of such identification will be disputed by other anthropologists, leading to all the definitional contortions discussed at the beginning of Chapter I.

22. Frequency of attack by political communities of the cultural unit upon members of other cultural units. This variable and the next are what Otterbein calls "external war." The same indicators for coding decisions are used as in the previous variable and the dichotomies remain the same,

but the task of distinguishing between feud and war is obviously much easier. The literature often fails at this point, however, by mentioning the frequency of warfare between specific parties but ignoring the difference between the incidence of attacking and being attacked.

23. Frequency of being attacked by non-members of the cultural unit. The discussion of the previous variable is applicable here, also. Both variables closely follow Otterbein (1970:143-44).

24. Mobilization. Otterbein's (1970:144) variable consists of 4 attributes, making fine distinctions between non-professionals and professionals and combinations thereof. I emphasize the organizational units and base the dichotomy on the attributes of age-grades, military societies, and standing armies, as opposed to organization based instead on friendship or kinship, or the absence of organization altogether. This variable has been one of the easier ones to code, with usually adequate ethnographic description. I am interested in the form of mobilization not as a military tactic, which is Otterbein's concern, but as the basis for the armed group with the common goal, that is, one of the 3 necessary conditions for warfare. Furthermore, I am interested in organizational principles that bring males together on a regular basis, especially where militarism may be a principle upon which the organization is built, which in turn may perpetuate military attitudes and exercises. The

difference between the 2 types of grouping is self-evident: age-grades, and particularly military societies and standing armies, exist in primary relationship to military activities; friendship and kinship groups may include military activity as merely another of a variety of activities and obligations.

25. Decision to go to war. This variable is identical with that of Otterbein (1970:144) and consists of only 2 attributes: the decision to go to war is made by an official or council of the political community, or anybody can decide on his own without reference to a higher authority. Again, Otterbein includes this variable as a military tactic--an official decision is more highly evolved socioculturally than an individual one. My concern is entirely different. For instance, as I pointed out in my discussion of Otterbein's work, he sees higher political authority as bringing inherent individual violence under control. In contraposition, I see decision-making authority vested in the few as likely to affect many more persons with violence than if they acted as individuals.

Dissenting opinions. I had hoped to be able to gather ethnographic material on how members of a society receive dissenting opinions in regard to war, i.e., protestors, pacifists, diplomats, and if any attempts are made to stop one's group from going to war. This variable obviously is based upon our own political history. Since ours is only 1 culture among hundreds, I wished to discover if in other cultures

attempts were made to dissuade fellow citizens from waging war, particularly an offensive one. Sadly, the information simply does not exist except in a few superlative cases. I can conclude only that the ethnographers have not asked the question. This variable is not included in the quantitative analysis.

26. The beginning of war. This is another variable that I have borrowed from Otterbein (1970:144). While his is again tactical, my concern is with the circumstances under which warfare is carried out openly and with some measure of agreement between the parties. The 3 attributes of the variable are dichotomized by distinguishing between war begun by announcement or mutual agreement and war begun by surprise attack.

27. The ending of war. I have borrowed this variable from Otterbein (1970:144), with very slight modification. The dichotomy contrasts conclusion by negotiation with conclusion by simply stopping or not concluding at all.

28. Peace ceremony. I think that a peace ceremony usually gives overt sociocultural recognition of the costs of war and the intentions of participants in the future. While it may be a sham, the existence of the notion of fraud in such an event also attests to its positive importance. Occasionally in the literature one comes across comments by anthropologists to the effect that a people engage in perpetual war because they have no institutional means to end it.

Negotiation, where it exists, is a means to such an end, and the peace ceremony is the ritual that sanctions the end. It is not necessarily the case, however, that negotiation inevitably includes a ceremony. This variable has been coded simply as either present or absent.

29, 30; 40-47; 49-52. Military expectations. Recall that the concept of military expectations was originated by Naroll (1966), who argues that while it is extremely tenuous to talk about the objective cause of a particular war, one can readily collect information from the actors as to their reasons for going to war. Further recall my repeated argument that objective cause does not tell us much anyhow, because people act in accordance with what they believe to be the case--that the military expectations of the people are not second-best data but the data we should concern ourselves with first and foremost.

This variable is initially based upon Naroll, with modifications by Otterbein (1970), and augmented by 1 new attribute. Naroll's original attributes, listed in order of rising expectations, were:

Revenge and defense. Tribes fight to gain satisfaction for injuries (like murder or witchcraft spells), or to expel a foe from their territory. All warring tribes have this expectation.

Revenge, defense, and plunder. Tribes may also fight ... for booty of some economic value--cattle, wives, slaves, land, cannibal victims (... consumed for food).

Revenge, defense, plunder, and prestige. In addition to both expectations above, warriors will go to battle to

prove their military prowess--for instance, to acquire scalps or victims for ceremonial cannibalism.

Revenge, defense, plunder, prestige, and political control. The incorporation of the defeated enemy into the political system of the victor becomes an additional goal of warfare (Naroll 1966:17-18).

I used these attributes in an earlier study (Nammour n.d.) with one modification: I judged the last attribute, political control, to be present if a tribe went to war for territorial control. For example, the Nootka (who are not included in the Standard Sample) go to war to wrest fishing and gathering territory from other peoples, but they also practice war of attrition tempered by slave taking. In any case, at that time, if territory were mentioned by the ethnographers as a goal in warfare, I considered the fourth attribute to be present. Nevertheless, control of territory and its natural resources and incorporation of a defeated people into the victor's political system are not necessarily the same thing. Subsequently, I have borrowed Otterbein's (1970:146) attribute of land--fields, hunting territories, fishing territories, pastures--as a distinct expectation from either subjugation of a people or plunder. Furthermore, plunder and tribute are different sorts of economic gain. The Aztecs exacted tribute from defeated cities and thus dominated surrounding peoples, but they did not hold dominion over them. Their collection of tribute is an entirely different political act from others' raids for horses and camels. Otterbein prudently separated Naroll's attribute of

revenge and defense, a lumping which obscures a critical difference between defending oneself when attacked and seeking revenge for a felt wrong. Moreover, not all peoples, "warring" or otherwise, defend themselves when attacked or seek revenge thereafter. The assumption that they do is just that--an assumption. While theoretically a society would not exist for long if it did not at least defend itself, any deterrent effects of revenge may not be felt immediately. A people may defend themselves but do not subsequently seek revenge, e.g., the Lepcha, or they apparently may do neither in a certain cultural context, e.g., the Semai. I must admit that Naroll would not identify these 2 peoples as "warring," even though they have experienced warfare. There is a second type of "defense": defending oneself by attacking first, if it is thought that an enemy is planning to attack. While this notion of defense is familiar in modern international relations--the "preemptive strike," "protective reaction"--it is also found in tribal societies such as the Caingang and perhaps has much wider occurrence than is indicated in the ethnographies. An alternative would be to code such action as revenge, for an anticipated act in this case, but this would obscure an interesting distinction.

Thus, while Naroll has 4 attributes and Otterbein has 6, I have used 8 here: subjugation of territory and people; collection of tribute; land--fields, hunting territories, fishing territories, pastures; plunder (including captives

for food, slaves, hostages, adoption, and marriage); trophies and honors (including captives for sacrifice); revenge; defense; aggressive defense (defending oneself by attacking first). I have handled these attributes 4 ways: each as a separate variable intercorrelated with all other variables but not other military expectations; lumped into 4 larger categories; and scaled in 2 dichotomies. Both dichotomies are separated into high military expectations contrasted to low military expectations. In the first scaling, if subjugation, tribute, land, or trophies and honors are present for a given society, that society is rated as having high military expectations. In the second scaling, only if subjugation, tribute, or land were present is a society given the high rating. Since in previous scalograms prestige sorted out as a higher expectation than plunder, revenge, and defense, I wanted to see what effect if any including trophies and honors would have on patterns of frequency in warfare. My ethnographic cases of inspiration are the mounted Indians of North America and pastoralists of Africa, the Circum-Mediterranean, and East Eurasia.

In coding these attributes, I counted 1 as present if its presence was specifically mentioned by the ethnographer. If it was not mentioned, I counted an expectation as absent rather than "no data." Occasionally the ethnographers also mention the reasons people do not go to war. The coding of defense was generally difficult because so few authors

specifically mentioned it. Following the rule, I marked it as absent. I simply do not know where it was truly absent and where the ethnographer assumed that any ninny would know that it would exist. Otterbein (1970) also had difficulty coding this attribute because of such an assumption. Therefore, I do not consider my quantitative results on defense to be reliable. Otterbein made one useful distinction that I could not implement satisfactorily. He coded the military expectations in order of importance--first, second, third. There were too many cases where I could not make such a judgment, even if the data on expectations per se were good.

31. Estimated casualty rate. I modified this variable from one of Otterbein's, in which he was interested in casualties sustained by a society using its major form of fighting as a measure of efficiency. I am interested in casualty rates regardless of military modes. This has turned out to be a difficult variable to code; information does not exist in most cases. The dichotomy adds further complications: Otterbein (1970:81, 146) rated as "high" the deaths of over one-third of the combatants and lower losses as "low." There is irony here along with difficulty. Even in the state wars of the 19th and 20th centuries, casualty rates of such magnitude that include the wounded and missing as well as the dead are unusual. During World War II, when losses were 2 to 4 times greater than in World War I, while Germany lost 3.35 million of 10.20 million mobilized,

Japan lost 1.51 million of 9.9 million mobilized, a surprisingly small overall figure given the low survival rate of Japanese soldiers in specific battles. In World War II, with total losses estimated at 60 million persons, 17 million were military and the remainder were civilians dying mostly in military action but also as a result of war-induced epidemics (Wright 1965:1542-43). Loss of one-third of the combatants must be rare indeed. Otterbein encountered difficulty in coding this variable, also. Of the 50 societies in his 1970 sample, he coded casualty rates for 33, and of those 13 are high. I would disagree with several of those decisions, applying Otterbein's dichotomy strictly. Where the information existed, I coded for the casualty rate of all members of a party to war, making no distinction between combatant and non-combatant. Even so, I was able to code casualty rates for only 79 of the 186 societies in the Standard Sample.

32. Command. While Otterbein (1970:23-28, 144) designed a variable around the "degree of subordination" in a society's military organization, again with military efficiency in mind, I am more interested in organization to exact compliance--do persons behave differently under conditions of individual autonomy than they do under conditions of superordinate authority? The wording of our variables I think reflects well the difference in theoretical orientation between Otterbein and me:

What is the degree of subordination within the military organization? ...

1. high--warriors obey orders given by leaders
2. low--warriors frequently do not obey orders given by leaders

(Otterbein 1970:144)

Who gives commands during battle?

1. an official who can back up his decisions by force
2. an informal leader whom people obey because of respect, but who has no means to force warriors to obey
3. everyone is on his own.

When I drew up this variable, I neglected to allow for those cases where a leader has an official position--appointed war chief, hereditary war chief--and is very aware of the honor and respect of this office but still has no means to force followers to obey. His formality may be hollow if the people choose to disregard his direction.

33. Prestige of a warrior in the community. I have designed this variable to code attitudes with regard to the social status of a warrior or soldier, assuming that high prestige will act as a stimulus to participate in military activities and thus strengthen the militarism of a given society. There is a certain ambiguity in the first attribute--"a great deal; import for every male"--that is partially resolved by the other attitudinal variables. For instance, Plains Indian warriors accrued enormous prestige through military exploits, carried out with nonauthoritarian leaders. In modern Western states, it is important for every male to do his military service if conscripted and he will be severely punished if he does not, but the personal prestige

of soldiers is relatively low.

34. Cowardice. This variable is a failure, but for an interesting reason. With very few exceptions, even including professionally trained ethnographers, observers simply do not state what action brings down an accusation of cowardice. Cowardice is often mentioned, but not what it is seen to be. It must be a universal by assumption and not subject to relativist scrutiny.

35. Rewards for warriors. This variable augments variable 33. I am interested in the extent to which members of the community publicly reward the military behavior of individuals. I ask, "Were there special gifts, praises, or ceremonies (not including ritual purification) for a man who has killed an enemy in battle or otherwise shown skill in war?" The attributes were yes, usually or always (with the implication of elaborate ritual); sometimes (with the implication of perfunctoriness and simplicity); rarely or never (with the implication of minimal, occasional, or total absence of such rewards). As in variable 33 I hoped to collect data to support the argument that rich rewards are an incentive to participate in militarist activity. The ambiguity of the state enters here, also. Soldiers in state armies may receive recognition of personal actions (ribbons, medals, citations, booty), but it is against the larger background of the mass of soldiers performing their social duty. The individual soldier is only occasionally singled out.

Ritual purification is an entirely different matter from rewards. In retrospect, I should have included a separate variable for the presence or absence of such rituals, since I have a hunch that the more seriously members of a culture regard the taking of human life as undesirable, the more elaborate will be rituals cleansing a person from carrying out the most heinous act. Conversely, the less seriously a community regards such a transgression, the less elaborate will be the rituals. In the Indochina War, the American military and civilian community omitted such social purification altogether, with misery for the men as a consequence. Perhaps such an omission is consistent with our modern disregard for human life, even within our community.

36. Expectations of violence. I wanted to somehow test Gordon Allport's notion, discussed in Chapter I, part 2, that if people expect violence to solve their problems, they will use it. My operationalization of this variable breaks all the methodological rules and subsequently is vulnerable to attack. The attributes are simply yes, no, and ambivalent or no evidence, and the coder has used his assessment of the material overall. But let me defend myself a little. Someone has suggested that I could use policing and legal institutions as a measure of expectations, but we only have to look at the United States today to find objections to such a measure. With all our institutions for social control and legal redress, our cultural violence is legendary. And as

other cultural patterns diffuse, so has violence, to Great Britain for instance. A country like Lebanon, on the other hand, while it has a system of Roman law inherited from the French that Americans would find oppressive, exists in a condition that we would call anarchy. Yet the incidence of personal and institutional violence is very low. The army prior to 1972 was a national joke. The police are interested primarily in equal sectarian representation on the force. I think the variable is enormously important, and eventually I hope to build some rigor (but not mortis) into it.

37. The value of violence. This variable complements but does not duplicate, or resolve, variable 36. The question is, "Is violence/war against non-members of the group ... (a) enjoyed and considered to have high value; (b) considered to be a necessary evil; (c) consistently avoided, denounced, not engaged in." The 3 attributes are dichotomized by contrasting (a) to (b) lumped with (c). Clumsy as this variable may seem, it was quite easy to code, largely due to clear observations by the ethnographer as to the value of war. The evidence may be comments by the observer, texts of poems and songs, or statements by the actors.

38, 39. Military success. I borrowed this variable from Otterbein. Until going over his material I had not considered including such a variable at all, but after the fact its necessity seems obvious even if only to make tests of Otterbein's conclusions possible. I modified Otterbein's wording

and included population as well as territory as the entities to be adjudged as expanding, remaining stationary, equalizing, or shrinking, while retaining territory as the primary measure. Otterbein dichotomizes the variable only one way, as I have discussed in another section, assuming that any society that is not actually expanding territorially is a military failure. I have used the same dichotomy but added a second scaling, assuming that any society that was not actually shrinking territorially was a military success. The second scaling has proven to be of little use, since in a fourfold table the c cell as well as the a cell is loaded up by such a reclassification of attributes, lowering the correlation coefficients.

This concludes my presentation of the basic variables and their variations that I have included in this quantitative study. An exhaustive inventory of corresponding hypotheses is not necessary, in my estimation, because of the stated purpose of this study. There are several things happening here. Avowedly this study began as a broad undertaking, using inductive techniques to discover patterns or the absence of patterns among the numerous sociocultural and warfare variables--Harris' throw-it-against-the-wall-and-see-if-it-sticks technique. Throughout the first chapter I have presented and criticized many hypotheses and theories of warfare, some of which are testable inductively. I have done

detailed criticism of other quantitative warfare studies, emphasizing their weaknesses but pointing out strengths as well. Since several of the variables in this study duplicate or approximate those used in the smaller studies, one aspect of my work is to retest selected variables on a larger, more rigorous sample. I have attempted to explore any relationship between economic patterns and warfare, a dry hole in Otterbein's 1970 work. I have looked at community organization and population density, basic demographic variables. I have tried to isolate basic sociocultural variables as the background against which warfare can be understood, holding as a possibility that there may be some configuration in which warfare is the independent, not the dependent, variable. I have tried to do more on a bigger canvas than anyone else has done on warfare in anthropology.

There are, moreover, 2 additional hypotheses that I am clearly testing: the existence of significant geographical differences obviating a universal explanation, and the association of attitudes and values held about warfare by the actors themselves with the other variables. I will admit again that the attitude variables are wanting in operationalization, and that I should have included more and better variables. At the same time, I will defend their inclusion as going in a better direction than more traditional, psychological variables, and that a preliminary test may hint at future improvements. I may be criticized that the attitude

variables are loaded--that I am bound to get what I want inductively because of the coding which is biased in favor of my argument. There may be considerable truth to such a criticism on the basis of the overt crudity of the variables. The broadness of the variables is partly due to 1 other factor. Reading and coding warfare data on the societies in the Standard Sample was already well along before I began to develop an interest in and argument for the significance of beliefs and values in war. I had already changed the variables twice and simply could not expend the effort required to begin again and do elaborate attitudinal research, even if the information existed in the literature, which it usually does not.

In this chapter I have presented the justification, precedents, methodology, and substance of the original research in this dissertation. In the third and last chapter, I shall report my findings.

CHAPTER III

FINDINGS: DISCOVERIES OF THE ODYSSEY

In a whimsical moment, the computer program for this research was christened ODYSSEY. After a time the name seemed more and more appropriate--better than AGONY, or BLOOD, or GORE. So, if the readers will continue to be tolerant of my poetic indulgences, they may relieve the inevitable tedium of statistical discussion. I will first present the quantitative results of the 'round the world inspection of war, and then look closely at each of the 6 geographical regions. The model I am using is an article by Driver and Schuessler (1967) in which they intercorrelate and factor analyze 30 sociocultural variables--various subsistence activities, settlement patterns, forms of marital residence and descent, kin terms, political integration and succession--coded by Murdock in his World Ethnographic Sample (1957) for 565 ethnic units taken all together and then within each geographic region.

A View of the World

Before looking more closely at the significant associations without regard to relative geographical distribution, I would like to discuss the results of geography as a variable. The C values for these intercorrelations are in

Table 4. In the interest of avoiding repetitious wording, I shall inventory the variables that are significant at the .05 level or higher with each geographical region and their predictive direction. When I state that a variable is present or absent at a certain significance, this does not mean that it is always present or always absent for every society. It means that certain attributes of a variable are absent often enough to mathematically dominate the number of times they are present, that the absences occur more often than if by chance. The mathematical statements are simply summaries. The lambda values are generally lower in the world-wide intercorrelations than in the internal regional ones.

1. Africa.

Polygyny is present (.001) and the predictive direction of lambda is the presence of polygyny from knowledge of "Africa" 21 per cent of the time but no ability to predict Africa from polygyny. Compact but impermanent settlements, neighborhoods of dispersed family homesteads, and separated hamlets are present (.05) and patrilineal descent is present (.05), but neither attribute possesses a predictive direction. Unilineality is present (.05). In the dominant subsistence activity, division of labor in which men do all or most of the work is absent (.02) but it is not predictable. No other basic sociocultural variables associate significantly with Africa.

TABLE 4. COMPARISON OF $\underline{C}$ COEFFICIENTS: GEOGRAPHIC REGION AS A VARIABLE

Variable	Africa	Cir-Med	Eurasia	Pacific	N Amer	S Amer
2	(.335)*	.042	(.239)*	.071	.052	.020
3	.116	(.235)*	.005	.007	.125	(.198)
4	.032	.100	.089	.006	.046	.006
5	.132	.111	.134	.084	.124	(.154)
6	.066	.076	.012	(.221)*	(.311)*	.039
7	(.166)	.028	.103	.094	(.198)*	.085
8	.074	.094	.094	.137	(.145)	.106
9	.029	(.259)*	.024	.086	.070	.101
10	.124	.083	.051	.068	.040	(.164)
11	.090	(.167)	.026	.142	.026	.062
12	.072	.031	.088	.102	.055	.127
13	.048	(.350)*	.117	.056	(.226)*	(.214)*
14	.131	(.296)*	.113	.031	(.225)*	(.328)*
15	.107	(.249)*	.062	.103	(.366)*	.114
16	(.201)	.176	.095	.136	(.318)*	.098
17	(.144)	.051	.140	.010	(.177)	(.165)
18	(.179)	(.182)	.024	(.152)	(.204)*	.050
19	.068	(.245)*	.045	.034	(.155)	(.223)*
20	.090	.007	.040	.025	.067	.009
21	.074	.044	.001	(.199)*	(.312)*	.123
22	.132	(.155)	.076	(.178)	.089	.050
23	.090	.071	.118	.099	.005	.051
24	.078	(.209)*	.058	(.176)	.071	.089
25	.018	.125	.069	.060	(.227)*	.027
26	.015	.130	.080	(.247)*	.148	.148
27	.053	.027	.074	.075	.057	(.225)*
28	0	.067	0	(.298)*	.081	.205
29	.003	(.186)	.008	.003	.022	(.194)*
30	.021	(.285)*	.091	.030	.107	(.217)*
31	(.216)	(.290)*	.091	.072	.047	.056
32	.042	.118	(.185)	.025	(.294)*	.010
33	(.200)*	.033	(.212)*	.001	.079	.094
34	.165	.152	.238	.075	.168	
35	.133	.065	(.206)	(.189)	.077	.035
36	.076	(.178)	(.187)	.091	.002	.010
37	.065	(.201)	(.168)	.065	.068	.080
38	.123	.113	.107	.106	.093	.084
39	.057	.013	(.158)	(.185)	.001	.056
40	.056	(.307)*	.067	.040	(.208)*	.061
41	.062	(.204)*	.076	.055	.104	.020
42	.042	(.116)	(.574)*	.056	.044	(.236)*
43	(.162)	.012	(.255)*	.027	.129	.010
44	.048	.134	(.185)	.050	(.282)*	.045
45	.120	(.170)	(.226)*	.087	(.314)*	.106
46	.005	.113	.041	.014	.106	(.182)
47	.056	.061	.137	.050	.004	.092

($p = .05$) ($p = .01$)*

Among the warfare variables, the presence of external war-attacking is non-significant ($< .10 > .05$). High casualties are significantly absent (.05) but the absence is not predictable. High prestige for warriors is present (.01) and is predictable from "Africa" 10.8 per cent of the time. Plunder as a military expectation is present (.05) but not predictably.

2. Circum-Mediterranean.

This geographical region and North America have the largest numbers of significant associations with the other variables. Patrilocality is present (.01) but not predictably. A mean community size of over 400 persons is present (.001), predictable from the Circum-Mediterranean (.077). Two or more levels of jurisdictional hierarchy beyond the local community are present (.001) and knowledge of this is predictable from knowledge of the Circum-Mediterranean (.196). Food-producers are present (.001) but not predictably. This is odd because food-production is the only subsistence mode in the region. Men doing most or all of the work in the dominant subsistence activity is present (.02), and it is predictable from knowledge of the region (.112). Class stratification is present (.001) and predictable in the Circum-Mediterranean (.102).

This region has numerous significant warfare associations. While the lambdas remain weak, they are stronger

here than in associations with other regions. External war-attacking is present (.05) but not predictable. Mobilization organized through age-sets, military societies, or armies is present (.01) and predictable (.095). High military expectations defined by subjugation, tribute, or land are present (.001) and predictable (.207). High casualties are present (.01) and predictable (.194). Violence is expected to solve problems (.05) but its presence is not predictable. Violence is highly valued (.02) and predictably so (.147). The following military expectations are present: subjugation (.001; $\lambda = .027$), tribute (.01; not predictable), and land (.05; not predictable). Revenge as a military expectation is absent (.05) and the absence predictable (.047). There is, however, no association with any form of military success.

3. Eastern Eurasia.

This region produced significant associations that are strikingly different from those of the Circum-Mediterranean. There is only 1 sociocultural variable: polygyny is absent (.001) but not predictably so.

Among the warfare variables, authoritative command is present (.05) and predictable (.119). High prestige for warriors and soldiers is absent (.01) but not predictably. Elaborate public rewards for warriors are absent (.05) predictably (.095). Violence is not expected to solve problems (.05), and its absence is predictable (.129). War is

regarded as a necessary evil or denounced and avoided (.05), predictably (.093). Military success defined as expanding, unchanging, or equalizing is absent (.05) but not predictably. However, there is no significant association between military success defined as expansion alone and Eastern Eurasia. The following military expectations are absent: plunder (.001; $\lambda_c = .147$), trophies and honors (.02; $\lambda = 0$), and revenge (.01; $\lambda_c = .094$).

4. Insular Pacific.

Nomadic, semi-nomadic, and semi-sedentary settlements are absent in this region (.01) but not predictably. Women do as much work as the men, or more, in the dominant subsistence activity (.05), but not predictably.

Among the warfare variables, internal war is present (.01) and predictably (.132). External war-attacking is absent (.02), with a small predictive value (.088). Military organization in age-sets, military societies, and standing armies is absent (.02) but not predictably. War begun by agreement or announcement is present (.01); a peace ceremony is present (.01); rewards for warriors are present (.05); military success defined as expanding, unchanging, or equalizing is present (.02). None of these 4 significant correlations is predictable in either direction.

5. North America.

Nomadic, semi-nomadic, and semi-sedentary settlements are present (.001) and predictable from knowledge of "North America" (.177). Homesteads and scattered hamlets are absent (.01) but not predictably. Compact and complex settlements are also absent (.05) and the absence is weakly predictable (.024). Any levels of jurisdictionary hierarchy beyond the local community are significantly absent (.001) with a relatively strong predictive value (.232). Food-collectors are present (.001) and predictably (.143). Matrilineal descent, as opposed to only patrilineal descent, is significantly present (.001) and predictably (.147). Bilateral descent, as opposed to unilineal descent, is present (.02) and predictably (.125). Men do all or most of the work in the dominant subsistence activity (.01), predictably if they are North American (.138). Class stratification is absent (.05) but not predictably. This pattern of significant presences and absences of specific variables is in strong contrast to the configurations in the Circum-Mediterranean region.

The warfare variables show another distinctive pattern, actually confirmation of the common sense conclusions one develops from simply reading the ethnographies. The numbers show that internal war is rare enough to be significantly absent in North America (.001), and predictably (.118). Official decision-making for going to war is absent (.01),

while beginning a war by surprise attack is present (.05), but the association of neither variable is predictable. Warriors follow an informal leader or are on their own in battle (.001) and this relationship is predictable (.09). The following military expectations have a significant association with North America. Trophies and honors are present (.001) predictably (.100). Revenge is present (.001). Subjugation of territory and people is absent ($< .01$). The last 2 associations are not predictable.

6. South America.

Matrilocal residence is present ($< .02$) when opposed only to patrilocal residence in the correlation with this geographical region, but there is no predictive direction. Community endogamy is present ($< .05$), not predictably. The average size of a typical community of between 0 and 99 persons is present (.05) with a very weak predictability value (.036). There are usually no levels of jurisdictional hierarchy beyond the local community (.001) and this is predictable 23 per cent of the time. Unilineal descent is absent ($< .05$) predictably (.103). Class stratification is also absent.

Among the associations between South America and the warfare variables, conclusion by negotiation is absent ($< .01$) with a small predictive value (.0469). Low military expectations--indexed by the presence of defense, revenge,

plunder--are present (.01), and when trophies and honors are also included as a low military expectation, prediction is .099. The specific military expectations of land and defense are significantly absent ($< .01$ and $< .02$ respectively) but only defense generates predictive direction (.105).

Thus, by using geographical region as a variable in the world-wide intercorrelations, disparate inventories of variable incidences exist among the regions. Many of them are familiar, for instance polygyny and women in subsistence work in Africa; food-production, class stratification, patri-locality, patri-dominant labor, and the state in the Circum-Mediterranean; small, relatively transitory communities, the absence of complex political organization, food-collectors, and bilaterality in North America.

As for the warfare variables, the configuration in the Circum-Mediterranean is impressive: external war-attacking takes place with formal military organization, going after high military expectations (subjugation of people and territory, the collection of tribute, and land), sustaining high casualties, and is seen as a solution to problems and highly valued. In Eastern Eurasia, however, while there is authoritative command in war, there are no elaborate rewards or high prestige for warriors, and violence and war are not adjudged as solutions to problems but seen as necessary evils or avoided. In yet another variation, in the Insular Pacific internal war is the mode, carried out by agreement by the

parties, concluded with a peace ceremony and rewards for the warriors. In this region cultures are militarily successful in the sense that they are not losing territory. Probably the island environments of so many of the cultural units in this region account for this warfare pattern.

World Configurations

Looking next at the world sample as a whole, every variable has been intercorrelated with every other variable but not with its own variation, e.g., the 2 scalings of the marital residence attributes are not correlated with each other. The Pearson's C coefficients for each comparison are arranged in Table 5. Those values significant at the 5 per cent level or higher are in parentheses. Those significant at the 1 per cent level or higher are additionally marked by an asterisk. Out of a total of 987 intercorrelations, 308 or 31.5 per cent are significant at the 5 per cent level or higher. Two hundred six or 20.8 per cent are significant at the 1 per cent level or higher. In contrast, out of Driver and Schuessler's 435 C coefficients, a mere 7.8 per cent are significant at the 5 per cent level or higher, a surprisingly low figure. Furthermore,

No one single variable is consistently correlated with all of the rest, or even with a small subset of them. Except to hint at possible clusters of traits, these categories would have to be combined into broader groupings

TABLE 5. C COEFFICIENTS FOR THE WORLD: EACH VARIABLE WITH EVERY OTHER VARIABLE

Variable	2	3	4	5	6	7	8	9	10	11	12	13	14	15
2														
3	(.171)													
4	(.143)													
5	.110	(.259)*	.111											
6	.030	.041	.009	.019										
7	.092	.033	.083	.011										
8	.103	.013	.059	.027										
9	.012	.060	.037	(.179)	(.259)*	.040	(.217)*							
10	.039	.010	.087	.106	(.317)*	.063	(.256)*							
11	.025	.065	.047	.065	.064	.022	.042							
12	(.161)	.053	(.248)*	.015	(.192)*	.044	(.149)	.104	(.234)*	.130				
13	.014	.107	.047	.125	(.304)*	.130	(.380)*	(.322)*	(.277)*	.037				
14	.118	(.172)	(.150)	.091	(.287)*	(.167)	(.390)*	(.320)*	(.386)*	.086				
15	.086	.125	.050	.025	(.522)*	(.265)*	(.336)*	(.334)*	(.343)*	.016	(.203)*	(.334)*	(.425)*	
16	(.297)*	(.665)*	(.203)	(.270)*	.077	.004	.071	.175	.136	.048	.020	.140	(.245)	.180
17	(.165)	.024	(.269)*	(.237)*	(.148)	.109	.053	.042	(.163)	.115	.271	.055	(.238)*	(.278)*
18	(.151)	.119	.038	.071	.105	.064	.049	.083	.066	.014	.103	.032	.127	(.222)*
19	.086	.113	.022	.063	(.318)*	.101	(.377)*	(.257)*	(.288)*	.046	.037	(.493)*	(.467)*	(.305)*
20	.103	.005	.046	.032	.034	.033	.059	.071	.147	(.197)	.098	.088	(.154)	.070
21	(.157)	.069	.114	.036	.127	.110	.034	.085	.034	.047	.114	.001	.107	.117
22	(.220)*	.019	.029	.046	.047	.084	.111	.138	(.192)	.058	(.157)	(.225)*	(.266)*	(.154)
23	.081	.042	.027	.049	.080	.019	.062	.115	.137	.025	.078	.038	(.181)	.136
24	.026	.010	.036	.034	(.276)*	.149	(.365)*	(.329)*	(.232)*	.103	.049	(.423)*	(.291)*	(.286)*
25	.111	(.180)	.018	.095	(.264)*	.034	(.267)*	(.185)	(.246)*	.040	.055	(.226)*	(.265)*	(.251)*
26	.039	(.176)	.024	.022	.101	.070	.041	.075	.139	.058	.031	.157	(.179)	.116
27	.143	.009	.011	.027	.044	.126	.060	.126	.142	.009	.050	.059	.014	.149
28	.083	.007	.037	.049	.061	.128	.055	.008	.040	.042	.010	(.263)	.043	.016
29	.111	.018	0	.061	(.205)*	.110	(.275)*	(.232)*	(.270)*	.035	.035	(.249)*	(.400)*	(.253)*
30	.014	.069	.107	.003	(.188)	(.175)	(.307)*	(.247)*	(.228)*	.021	.013	(.328)*	(.377)*	(.228)*

(p = .05) (p = .01)*

TABLE 5--Continued

Variable	2	3	4	5	6	7	8	9	10	11	12	13	14	15
31	.030	.026	.070	.092	.128	(.273)*	(.331)*	.204	.069	.131	.076	(.322)*	(.239)	.139
32	(.222)*	.122	.097	.145	(.387)*	.043	(.396)*	(.258)*	(.280)*	.011	.005	(.471)*	(.411)*	(.368)*
33	(.259)*	.128	.008	.040	.056	.018	.069	.004	.127	.114	.026	.061	(.204)*	.151
34	.094	.102	.030	(.290)	.033	.001	.030	.077	.060	.112	.157	.025	.172	.232
35	(.243)*	.035	.169	.083	.126	.070	.062	.130	(.214)	.072	.061	.066	(.251)*	(.262)*
36	(.266)*	.062	.088	.158	.132	.056	(.171)	(.280)	.129	.070	.163	(.219)*	(.210)	.047
37	(.305)*	.114	.115	.089	.114	.082	(.172)	.037	.148	.104	.142	(.244)*	(.307)*	(.164)
38	(.173)	.009	.136	.052	(.178)	.027	(.190)*	.116	(.146)	.029	.065	(.284)*	(.171)	.129
39	.011	.064	.058	.075	.098	.007	.099	.002	.045	.044	.051	.031	.009	.077
40	.109	.094	(.165)	.085	(.268)*	.088	(.318)*	(.298)*	(.237)*	.062	(.158)	(.489)*	(.333)*	(.291)*
41	.056	.077	.004	.068	.081	.078	.138	.137	.024	.105	.042	(.350)*	(.222)*	.121
42	(.172)	.110	.004	.033	.015	(.171)	.126	.128	(.172)	.042	.078	.108	(.278)*	.106
43	(.291)*	.010	.009	.071	.093	.089	.016	.084	.020	.060	.045	.039	.103	(.149)
44	(.231)*	.045	.034	.092	.064	.011	.052	.037	.146	(.168)	.101	.051	.126	.071
45	.097	.108	(.169)	.038	(.204)*	.088	(.259)*	.100	.083	.016	.051	(.253)*	(.276)*	(.271)*
46	.043	.024	.142	.060	.013	.181	(.159)	(.147)	.056	.086	.067	.069	(.150)	.110
47	.023	.075	(.234)*	.022	.060	.036	.086	(.150)	.055	(.189)	(.289)*	.045	.101	.016

TABLE 5--Continued

Variable	16	17	18	19	20	21	22	23	24	25	26	27	28	29
2														
3														
4														
5														
6														
7														
8														
9														
10														
11														
12														
13														
14														
15														
16														
17														
18	.110	(.172)												
19	.072	.012	.016											
20	(.217)	(.233)*	.033	(.210)*										
21	.054	(.259)*	.132	.077	.014									
22	.077	.123	.002	(.222)*	.099	(.168)								
23	.068	.116	.084	.045	.094	.131	(.447)*							
24	.074	.011	.064	(.395)*	.138	.083	(.174)	.034						
25	.100	.090	.088	(.342)*	(.308)*	.091	(.247)*	.079	(.230)					
26	.005	.017	.019	(.222)*	(.181)	.053	.140	.114	.084	.044				
27	.045	.061	.088	.015	0	.057	0	.128	.057	.033	.060			
28	.221	.028	(.277)*	.160	.069	(.231)	.028	.060	(.298)*	.087	.103	(.290)*		
29	.065	.008	.091	(.280)*	.004	.044	(.327)*	(.220)*	(.234)*	(.197)*	.091	.089	.042	
30	.111	.006	.081	(.335)*	.075	.022	(.212)*	.100	(.281)*	(.262)*	.052	.009	.069	

TABLE 5--Continued

Variable	16	17	18	19	20	21	22	23	24	25	26	27	28	29
31	.138	.153	.072	(.239)	.101	.035	(.276)	.214	(.230)	.188	.101	.095	.141	(.255)
32	.129	.111	.053	(.449)*	(.204)	.022	(.187)	.007	(.355)*	(.403)*	(.119)	.013	.160	(.287)*
33	.189	.098	.078	.118	.067	(.196)	(.292)*	(.229)*	.032	.057	.059	(.175)	.032	(.293)*
34	.091	.155	(.274)	.107	.019	.092	.088	.058	.031	.185	.224	.032	.086	.152
35	.085	(.249)*	.116	.086	.076	(.288)*	(.449)*	(.369)*	.126	.172	.022	.020	(.255)	(.309)*
36	.081	.106	.029	(.246)*	.046	(.261)*	(.414)*	(.212)	.085	.079	.011	.162	.163	(.333)*
37	.182	.028	.088	(.277)*	.103	(.173)	(.413)*	(.204)	.100	.024	.111	(.250)*	.086	(.282)*
38	.085	.041	.007	(.201)*	.049	.098	(.373)*	.091	(.178)*	(.238)*	.054	.085	.075	(.336)*
39	.095	.088	.033	.011	.018	.030	.039	(.239)*	.019	.006	.100	.081	.020	(.154)
40	.057	.101	.138	(.395)*	.040	.056	.086	.049	(.436)*	(.224)*	(.172)	.023	.181	(.389)*
41	.142	.003	.055	(.301)*	.145	.005	(.201)*	.079	(.223)*	(.179)	.134	.117	.087	(.274)*
42	.178	.167	.061	.148	.041	.045	(.233)*	(.180)	.001	.078	.067	.020	.075	(.456)*
43	.021	.118	.021	.059	(.198)	(.162)	(.353)*	(.264)*	.107	.070	.079	(.176)	.163	(.157)
44	.062	.059	.057	.004	.072	.031	(.249)*	(.226)*	.025	.022	.027	.003	.028	(.451)*
45	(.234)	.120	.083	(.240)*	.070	(.167)	.003	.095	(.325)*	(.264)*	.033	.001	.016	.097
46	.024	.144	.005	.110	.043	.070	.065	.143	(.224)*	.091	.050	.038	.126	.080
47	.047	.084	.055	.024	.059	.073	.100	.095	.121	.057	.042	.129	0	.034

TABLE 5--Continued

Variable	30	31	32	33	34	35	36	37	38	39
31	(.260)*									
32	(.265)*	(.249)								
33	.120	.070	.010							
34	.220	.284	.022	.091						
35	.092	.105	.132	(.469)*	.101					
36	.154	(.285)	.149	(.437)*	.114	(.331)*				
37	.076	.117	.106	(.449)*	.153	(.354)*	(.556)*			
38	(.322)*	.177	(.194)	(.283)*	.218	.003	(.302)*	(.250)*		
39	(.149)	.072	.066	(.193)	.110	.041	(.211)	.108		
40	(.483)*	(.364)*	(.428)*	.002	.077	.079	(.177)	.126	(.294)*	(.150)
41	(.348)*	.169	(.253)*	.096	.019	.155	(.232)*	(.169)	(.311)*	.106
42	(.575)*	.061	.004	(.161)	.118	.091	.077	.084	(.231)*	.031
43	.045	.110	.083	(.321)*	.116	(.286)*	(.204)	(.354)*	(.243)*	.068
44	.115	.038	.030	(.304)*	.178	(.323)*	(.243)*	(.228)*	(.211)*	.101
45	(.167)	(.311)*	(.373)*	.054	.081	.117	.081	.009	.073	.107
46	.130	(.291)*	.024	.079	.192	.044	.040	.142	.118	(.161)
47	.049	.128	.078	.031	.018	.087	.130	.069	.098	.019

if relations among traits are to be detected (Driver and Schuessler 1967:333).

Subsequently, the authors relied upon the purely inductive statistical technique of factor analysis to float out clusters of variables. I do not seem to have that problem here. By inspection of the C values, the following "factors" or concentrations of significant associations call for attention: Polygyny; permanent or complex settlements; patricentered organization; centralized political organization and stratification; external war-attacking; decision-making, mobilization, and command in time of war; attitudes and values with regard to violence and war; military success; high military expectations and the specific expectations of subjugation, plunder, and revenge.

Looking first at polygyny and its association with warfare, if any, one finds the presence of polygyny to associate significantly with the presence of continual or frequent internal war at the .05 level, and internal war is explained by polygyny 9 per cent of the time. Otterbein (1968) has used polygyny as an indicator for the existence of fraternal interest groups and also found a positive correlation with internal war at the .05 level. But the larger sample reveals something that Otterbein's smaller one cannot. Otterbein's table (1968:281) looks like this:

	Internal War		
	Continual or Frequent	Infrequent	
Polygyny present	15	3	
Polygyny absent	13	11	42
$\phi = .31 \quad X^2 = 3.93 \quad p = .05$			

Compare this to the results based on the Standard Sample shown in Table 6. Note the geographical distribution of the societies in the 4 cells summarized as follows.

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	12	8	2	4
Circum-Mediterranean	7	3	3	12
East Eurasia	1	2	13	15
Insular Pacific	4	4	15	5
North America	2	8	2	21
South America	8	2	7	8

Looking even more closely at the identities of the societies, one sees that those with both polygyny and frequent internal war are African and South American, and Circum-Mediterranean societies that are either located on the African continent or are Islamic or both. There are a larger number of societies with frequent internal war but without polygyny, and they are concentrated in Eastern Eurasia and the Insular Pacific. The environmental circumstances of war in the Pacific have already been touched upon.

Otterbein considers polygyny only as a basis for military organization. Divale entertains polygyny as a catalyst

TABLE 6. INTERCORRELATION OF POLYGYNY AND INTERNAL WAR

Continual/Frequent Internal War		Infrequent Internal War			
Polygyny Present	Ila	Teda	Thonga	Bellacoola	
	Nyakyusa	Gheg	Lozi	Yokuts	
	Banen	Kazak	Mbundu	Hidatsa	
	Tiv	Tiwi	Kikuyu	Pawnee	
	Ibo	Kapauku	Ganda	Natchez	
	Ashanti	Ajie	Nkundo	Comanche	
	Mende	Maori	Fon	Chiricahua Apache	
	Tallensi	Aleut	Bambara	Papago	
	Azande	Yurok	Fulani	Saramacca	
	Nuba	Goajiro	Hebrews	Siriono	
	Shilluk	Yanomamo	Rwala		
	Masai	Jivaro	Gond		
	Wolof	Tupinamba	Chukchee		
	Hausa	Botocudo	Aranda		
	Kanuri	Shavante	Kwoma		
	Konso	Aweikoma	Tikopia		
Somali	Mapuche	Marquesans			
	34		27		
Polygyny Absent	Nama	Palauans	Kung	Yukaghir	Inca
	Kongo	Ifugao	Luguru	Javanese	Trumai
	Tuareg		Mbuti	Badjau	Timbira
	Lapps		Ingassana	Alorese	Lengua
	Kurd		Fur	Manus	
	Basseri		Amhara	Gilbertese	
	Khalka		Nubians	Ingalik	
	Lolo		Egyptians	Cp. Eskimo	
	Garó		Babylonians	Montagnais	
	Sema Naga		Turks	Micmac	
	Burmese		Romans	Saulteaux	
	Vietnamese		Basques	Slave	
	Siamese		Irish	Kaska	
	Andamanese		Lapps	Twana	
	Negri Sembilan		Russians	E. Pomo	
	Chinese		Armenians	Paiute	
	Ainu		Punjabi	Klamath	
	Gilyak		Toda	Kutenai	
	Balinese		Santal	Omaha	
	Iban		U. Pradesh	Huron	
	Orokaiva		Lepcha	Creek	
	Kimam		Palaung	Zuni	
	Lesu		Khmer	Havasupai	
	Trobrianders		Semai	Huichol	
	Siuai		Nicobarese	Aztec	
	Pentecost		Vedda	Popoluca	
	Mbau Fijians		Tanala	Miskito	
	Samoans		Manchu	Warrau	
	Majuro		Koreans	Carib	
	Yapese		Japanese	Mundurucu	
		42			65

$$Q = .322 \quad \phi = .159 \quad X^2 = 4.262 \quad p < .05 \quad \lambda_c = .09 \quad C = .157$$

in his warfare syndrome and raiding for women to offset a wife shortage as the cause of primitive warfare. One bit of information from the ODYSSEY is that of the 34 polygynist societies that carry on internal war 28, or 82 per cent, go to war for plunder. Taking captives for slaves, adoption, or wives has been defined as one type of "plunder." I have not tabulated the number of cases where captured women are wed, although I have tried to systematically collect such data. However, 26 (62 per cent) of the 42 non-polygynist societies that wage frequent internal war also fight for plunder; thus, the particular information on women is necessary to test this out.

There is another, broader perspective from which the associations with polygyny may be viewed. The presence of polygyny is significantly associated with the absence of patri-dominant labor, the absence of authoritative command, and the presence of patrilocality, patrilineality, 3 or 4 levels of local jurisdictional hierarchy, frequent external war-attacking, high prestige for warriors, elaborate rewards for warriors, violence as a solution to problems, violence and war as highly valued, military success as territorial expansion, and the specific military expectations of plunder and trophies and honors. Tentatively I will call this an androcentric configuration, since the common "factor" among these variables seems to be individual male achievement and local sociopolitical dominance. Seventy per cent of those

societies with polygyny and frequent internal war also engage in frequent offensive external war, although the correlation between polygyny and external war-attacking is the stronger one at less than the .01 level but without predictive power. Outside of the 70 per cent of shared cases for common presences, the distribution and identity of societies in the remaining cells of the intercorrelation between polygyny and external war-attacking are quite different (Table 7). The geographical representation of the 4 cells is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	16	2	2	4
Circum-Mediterranean	8	1	12	5
Eurasia	2	1	15	15
Insular Pacific	2	6	5	7
North America	7	3	9	13
South America	8	2	12	9

Africa has an even stronger influence on this intercorrelation, South America's remains the same (although 1 of the 8 contributing societies is different), and North America makes itself felt. Seven of the African societies with polygyny but not frequent internal war do engage in frequent offensive external war. While only 3 Circum-Mediterranean societies without polygyny carry on internal war, they and 9 others do attack outsiders.

The following series of 2 by 2 tables (Table 8 a-1) summarizes the significant associations of polygyny with the other variables in the configuration. I have given counts

TABLE 8. INTERCORRELATIONS OF TWELVE OTHER VARIABLES WITH POLYGyny

(a)

Postmarital Residence

		Patrilocal	Matrilocal
Polygyny	Present	17 Africa 11 Circum-Mediterranean 3 East Eurasia 8 Insular Pacific 6 North America 2 South America 47	1 Africa 3 North America 5 South America 9
	Absent	3 Africa 10 Circum-Mediterranean 20 East Eurasia 11 Insular Pacific 12 North America 12 South America 68	2 Africa 8 East Eurasia 7 Insular Pacific 8 North America 7 South America 32

$$Q = .422 \quad \phi = .174 \quad X^2 = 4.701 \quad p < .05$$

(b)

Descent

		Patrilineal	Matrilineal
Polygyny	Present	16 Africa 9 Circum-Mediterranean 2 East Eurasia 4 Insular Pacific 1 North America 3 South America 35	1 Insular Pacific 4 South America 5
	Absent	2 Africa 5 Circum-Mediterranean 16 East Eurasia 5 Insular Pacific 2 North America 4 South America 34	2 Africa 2 Circum-Mediterranean 5 East Eurasia 6 Insular Pacific 6 North America 3 South America 24

$$Q = .663 \quad \phi = .311 \quad X^2 = 9.476 \quad p < .01$$

$$\lambda_r = .025$$

TABLE 8--Continued

(c)

Levels of Local Jurisdiction

0-2 Levels

3-4 Levels

Polygyny	Present	5 Africa 1 Insular Pacific 2 South America 8	15 Africa 11 Circum-Mediterranean 3 East Eurasia 7 Insular Pacific 10 North America 8 South America 54
	Absent	3 Africa 5 Circum-Mediterranean 5 East Eurasia 3 Insular Pacific 9 North America 8 South America 33	4 Africa 10 Circum-Mediterranean 26 East Eurasia 20 Insular Pacific 14 North America 14 South America 88

$$Q = -.434 \quad \phi = -.163 \quad \chi^2 = 4.869 \quad p < .05$$

$$C = .161$$

(d)

Division of Labor in Dominant Subsistence Activity

Males Alone Or More

Females Equal, Alone, More

Polygyny	Present	3 Africa 4 Circum-Mediterranean 2 East Eurasia 2 Insular Pacific 5 North America 4 South America 21	17 Africa 5 Circum-Mediterranean 1 East Eurasia 6 Insular Pacific 5 North America 6 South America 40
	Absent	4 Africa 12 Circum-Mediterranean 12 East Eurasia 6 Insular Pacific 17 North America 8 South America 59	3 Africa 4 Circum-Mediterranean 18 East Eurasia 15 Insular Pacific 6 North America 12 South America 58

$$Q = -.319 \quad \phi = -.153 \quad \chi^2 = 4.149 \quad p < .05$$

$$C = .151 \quad \lambda_c = .013$$

TABLE 8--Continued

(e)

		Authoritative Command	
		Present	Absent
Polygyny	Present	7 Africa 5 Circum-Mediterranean 1 East Eurasia 1 Insular Pacific 3 South America 17	10 Africa 4 Circum-Mediterranean 1 East Eurasia 7 Insular Pacific 10 North America 7 South America 39
	Absent	2 Africa 9 Circum-Mediterranean 16 East Eurasia 10 Insular Pacific 4 North America 9 South America 50	1 Africa 4 Circum-Mediterranean 8 East Eurasia 8 Insular Pacific 15 North America 7 South America 43

$$Q = -.455 \quad \phi = -.288 \quad \chi^2 = 7.738 \quad p < .01$$

$$C = .222 \quad \lambda_c = .104$$

(f)

		Prestige for Warriors	
		High	Moderate, Low
Polygyny	Present	13 Africa 6 Circum-Mediterranean 1 East Eurasia 4 Insular Pacific 4 North America 4 South America 32	4 Africa 4 Circum-Mediterranean 1 East Eurasia 3 Insular Pacific 6 North America 4 South America 22
	Absent	2 Africa 5 Circum-Mediterranean 4 East Eurasia 7 Insular Pacific 7 North America 8 South America 33	3 Africa 11 Circum-Mediterranean 22 East Eurasia 13 Insular Pacific 16 North America 7 South America 72

$$Q = .521 \quad \phi = .268 \quad \chi^2 = 11.428 \quad p < .001$$

$$C = .259 \quad \lambda_c = .154$$

TABLE 8--Continued

(g)

Elaborate Rewards for Warriors
Present Absent

Polygyny	Present	13 Africa 5 Circum-Mediterranean 4 Insular Pacific 5 North America 10 South America 33	2 Africa 2 Circum-Mediterranean 1 East Eurasia 2 North America 2 South America 9
	Absent	1 Africa 5 Circum-Mediterranean 5 East Eurasia 9 Insular Pacific 10 North America 8 South America 38	2 Africa 6 Circum-Mediterranean 8 East Eurasia 2 Insular Pacific 11 North America 4 South America 33

$$Q = .522 \quad \phi = .251 \quad X^2 = 7.091 \quad p < .01$$

$$C = .243$$

(h)

Violence Believed to Solve Problems
Yes No

Polygyny	Present	9 Africa 8 Circum-Mediterranean 1 East Eurasia 5 Insular Pacific 5 North America 8 South America 36	6 Africa 2 Circum-Mediterranean 2 East Eurasia 1 Insular Pacific 2 North America 1 South America 14
	Absent	9 Circum-Mediterranean 8 East Eurasia 8 Insular Pacific 9 North America 3 South America 37	5 Africa 5 Circum-Mediterranean 14 East Eurasia 6 Insular Pacific 10 North America 8 South America 48

$$Q = .539 \quad \phi = .276 \quad X^2 = 10.275 \quad p < .01$$

$$C = .266 \quad \lambda_c = .177$$

TABLE 8--Continued

(i)

Felt Value of War

		Positive	Negative
Polygyny	Present	13 Africa 8 Circum-Mediterranean 2 East Eurasia 3 Insular Pacific 5 North America 6 South America 37	5 Africa 1 Circum-Mediterranean 3 Insular Pacific 4 North America 2 South America 15
	Absent	10 Circum-Mediterranean 7 East Eurasia 10 Insular Pacific 8 North America 3 South America 38	5 Africa 6 Circum-Mediterranean 20 East Eurasia 7 Insular Pacific 4 North America 12 South America 64

$$Q = .612 \quad \phi = .321 \quad X^2 = 15.842 \quad p < .001$$

$$C = .305 \quad \lambda_c = .293$$

(j)

Military Success

		Expanding	Not Expanding
Polygyny	Present	11 Africa 3 Circum-Mediterranean 1 East Eurasia 1 Insular Pacific 1 North America 4 South America 21	10 Africa 6 Circum-Mediterranean 2 East Eurasia 7 Insular Pacific 8 North America 6 South America 39
	Absent	6 Circum-Mediterranean 4 East Eurasia 3 Insular Pacific 4 North America 6 South America 23	7 Africa 12 Circum-Mediterranean 27 East Eurasia 18 Insular Pacific 19 North America 15 South America 98

$$Q = .393 \quad \phi = .176 \quad X^2 = 5.575 \quad p < .02$$

$$C = .173$$

TABLE 8--Continued

(k)

Military Expectation of Plunder

		Present	Absent
Polygyny	Present	18 Africa 10 Circum-Mediterranean 2 East Eurasia 5 Insular Pacific 7 North America 8 South America 50	2 Africa 1 East Eurasia 3 Insular Pacific 3 North America 2 South America 11
	Absent	3 Africa 7 Circum-Mediterranean 10 East Eurasia 12 Insular Pacific 15 North America 12 South America 59	3 Africa 10 Circum-Mediterranean 21 East Eurasia 9 Insular Pacific 4 North America 10 South America 57

$$Q = .629 \quad \phi = .304 \quad X^2 = 16.349 \quad p < .001$$

$$C = .291$$

(l)

Military Expectation of Trophies and Honors

		Present	Absent
Polygyny	Present	9 Africa 4 Circum-Mediterranean 1 East Eurasia 3 Insular Pacific 7 North America 3 South America 27	11 Africa 6 Circum-Mediterranean 2 East Eurasia 5 Insular Pacific 3 North America 7 South America 34
	Absent	3 Circum-Mediterranean 7 Insular Pacific 10 North America 5 South America 25	6 Africa 17 Circum-Mediterranean 28 East Eurasia 14 Insular Pacific 9 North America 17 South America 91

$$Q = .486 \quad \phi = .237 \quad X^2 = 9.938 \quad p < .01$$

$$C = .231 \quad \lambda_r = .033$$

of the geographical representatives but not specific identifications in most cases. The regions are abbreviated as A (Africa), C (Circum-Mediterranean), E Eastern Eurasia), I (Insular Pacific), N (North America), and S (South America).

By inspection of these numerous tables, one can see that societies from Africa are exerting consistently strong influence upon the correlations, followed by that from South America and thirdly by some influence from the Circum-Mediterranean region, specifically those societies on the African continent. Whatever the relationship of polygyny to the warfare variables, I cannot accept an explanation of its presence as the basis for Otterbein's fraternal interest groups. It seems very tenuous to argue that the men are getting together because of their common relationship to certain women. Patrilocality as a stimulus for organization is not significant; it does not correlate with patri-dominant labor. Its strongest association is with patrilineal descent, and each is predictable from the other 84 per cent of the time. A larger category of "unilocal" residence (patrilocal, virilocal, matrilocal, uxorilocal, avunculocal) does have a negative association with subjugation as a military expectation ($p < .05$) and a positive one with revenge ($p < .05$), although a weak lambda (.047) indicates that unilocal residence is predictable from revenge but not the other way around.

Patrilineal descent, however, when isolated from all other forms of descent, does have a strong positive

relationship with the presence of internal war, significant at the .01 level. Furthermore, patrilineality explains the presence of internal war 18.4 per cent of the time. Patrilineality does not have a significant relationship with external war-attacking. Apparently it is not residence alone that can lead to internal war but the jural rules of corporate descent groups that organize its members into a cooperating unit.

A second world-wide configuration revolves around the food-collecting community. If people depend upon food-collecting (gathering, fishing, or hunting) for their primary means of livelihood, they will be living in nomadic, semi-nomadic, or semi-sedentary settlements (predictable 50 per cent of the time) averaging fewer than 100 persons although the population can be as high as 400 (both significant at higher than the .001 level). Such a community may have up to 2 local jurisdictional levels but no more than 1 jurisdictional level beyond the local community and none at all 24 per cent of the time (both significant at the .001 level). One may predict with accuracy of 18.8 per cent that those communities with up to 400 residents do not have any superordinate political organization, and one's accuracy of prediction rises to 36.2 per cent if the community has no more than 100 persons. While nomadic and semi-sedentary settlements may possess matrilineal kin organization, the smaller communities are predictably bilateral, ambilateral, or

duolateral ($p < .05$; $\lambda = .128$). There is no class stratification in such communities (.001).

As for associations with warfare, only communities of less than 100 persons and food-collectors generate significant negative associations with offensive external war ($< .02$ and $< .05$ respectively). Mobilization through age-sets, military societies, or standing armies is absent (.001) but the absence is predictable only through knowledge of community size as 0-400 persons ($\lambda = .222$). Anyone in the community can make the decision to go to war ($< .01$) but it is not predictable from knowledge of settlement pattern, community size, or food-collecting. Low military expectations are consistently present although there is variation in the significance levels and predictive strength between military expectations and each of the basic variables in the configuration. For ease in presentation, I have arranged the correlations as follows.

Military Expectations I (Low: defense, revenge, plunder) with: nomadic, semi-sedentary settlements, $< .01$, predictive value of .085; community size of 0-400, $< .01$, predictive value 0; community size of 0-100, .001, predictive capability of .162; food-collectors, $< .01$, predictive capability of .155.

Military Expectations II (Low: defense, revenge, plunder, trophies and honor) with: nomadic, semi-sedentary

settlements, $< .02$, with a predictive value of .086;
 community size of 0-400, $< .001$, predictive value .210;
 community size of 0-100, $< .01$, predictive value .136;
 food-collectors, $< .01$, predictive value .110.

There is informal command in war or no command at all ($p < .001$), and its strongest predictor variable is settlement pattern (.299). The smaller communities of food-collectors have no elaborate rewards for their warriors ($p < .02-.01$), and subsistence is the better predictor of such an absence (.119). Communities of 0-400 do not expect violence to solve their problems ($p < .02$) with a low predictive value (.05). Food-collectors consider violence and war a necessary evil or avoid them ($p < .05$), predictably (.107). Nomadic and semi-sedentary communities of 0-100 are not military successes through expanding their territory ($p < .05-.02$). Subjugation as a military expectation is significantly absent ($p < .001$); revenge is present with the mode of settlement ($p < .01$) and with food-collectors (.001). The attribute food-collectors is also positively associated with plunder at the .05 level. Communities of 0-100 persons do not go to war for land ($p < .05$). None of the specific military expectations generate predictive direction.

Out of all this, I have selected a few tables so that the reader may identify the member societies in this configuration, as in the first, and their geographical distribution.

The distribution in the first table (Table 9) is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	4	3	0	20
Circum-Mediterranean	0	7	0	21
Eurasia	7	4	0	23
Insular Pacific	3	0	2	26
North America	16	6	5	6
South America	10	2	2	18

Sixty-five per cent of the mobile food-collectors are in the New World, and nearly two-thirds of those are in North America.

In the second table (Table 10) intercorrelating nomadic or semi-nomadic settlements and a community population range of 0-400 persons, the geographical representation in the cells is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	6	1	11	7
Circum-Mediterranean	4	2	7	14
Eurasia	10	1	15	8
Insular Pacific	3	0	21	6
North America	18	3	7	4
South America	12	0	14	6

This time, 58.5 per cent of the mobile communities of less than 400 population are in the New World and 60 per cent of those are in North America. Those marked with an asterisk have population of 100 or fewer.

Table 11 shows the results of the correlation between subsistence and offensive external war. The results are a significant negative association between food-collectors and

TABLE 9. INTERCORRELATION OF SETTLEMENT PATTERN AND SUBSISTENCE

Food-Collectors			Food-Producers		
Nomadic/Semi-Sedentary Settlements	Nama	Ingalik	Comanche	Lozi	Hidatsa
	Kung	Cp. Eskimo	Chiricahua	Ila	Pawnee
	Hadza	Montagnais	Warrau	Masai	Omaha
	Mbuti	Micmac	Siriono	Fulani	Huron
	Yurak	Saulteaux	Nambicuara	Somali	Havasupai
	Andamanese	Slave	Botocudo	Bisharin	Papago
	Vedda	Kaska	Shavante	Teda	Goajiro
	Ainu	Twana	Aweikoma	Rwala	Timbira
	Gilyak	E. Pomo	Lengua	Lapps	
	Yukaghir	Yokuts	Abipon	Kurd	
	Chukchee	Paiute	Tehuelche	Basseri	
	Badjau	Klamath	Yahgan	Toda	
	Tiwi	Kutenai		Kazak	
	Aranda	Gros Ventre	40	Khalka	22
Compact Impermanent Settlements; Homesteads, Hamlets; Complex Settlements	Manus	Thonga	Gheg	Tobelor	Mundurucu
	Mbau Fijians	Mbundu	Romans	Orokaiva	Cubeo
	Aleut	Kongo	Basques	Kimam	Cayapa
	Eyak	Nyakyusa	Irish	Kapauku	Jivaro
	Haida	Luguru	Russians	Kwoma	Amahuaca
	Bellacoola	Kikuyu	Abkhaz	N. Ireland	Inca
	Yurok	Ganda	Armenians	Trobriand	Aymara
	Miskito	Nkundo	Basseri	Sivai	Trumai
	Callinago	Banen	Gond	Tikopia	Tupinamba
		Tiv	Santal	Pentecost	Cayua
		Ibo	U. Pradesh	Ajie	Mapuche
		Fon	Burusho	Maori	Alorese
		Ashanti	Lolo	Marquesan	
		Mende	Lepcha	Samoans	
		Bambara	Garo	Gilberts	
		Tallensi	Sema Naga	Marshalls	
		Azande	Burmese	Trukese	
		Otoro Nuba	Palaung	Yapese	
		Shilluk	Vietnamese	Palauans	
		Ingassana	Rhade	Ifugao	
		Wolof	Khmer	Itayal	
		Songhai	Siamese	Creek	
		Hausa	Semai	Natchez	
		Kanuri	Nicobarese	Zuni	
		Fur	Tanala	Papago	
		Kafa	N. Sembilan	Aztec	
		Konso	Chinese	Popoluca	
		Amhara	Manchu	Y. Maya	
		Nubians	Koreans	Bribri	
		Riffians	Japanese	Cuna	
		Egyptians	Javanese	Haitians	
		Hebrews	Balinese	Yanomamo	
		Babylonia	Iban	Carib	
		Turks	Toradja	Saramacca	114

$$Q = .917 \quad \phi = .612 \quad X^2 = 69.265 \quad p < .001 \quad C = .522$$

$$\lambda_c = .367 \quad \lambda_r = .500$$

TABLE 10. INTERCORRELATION OF SETTLEMENT PATTERN
AND MEAN COMMUNITY SIZE

Mean Size of 0-400 Persons		Mean Size of 400 Persons or More	
Nomadic/Semi-Sedentary Settlements	Nama	Yukaghir*	Pawnee
	Kung*	Chukchee*	Comanche
	Ila	Badjau	Chiricahua*
	Hadza*	Tiwi	Havasupai
	Mbuti*	Aranda*	Papago
	Masai	Ingalik*	Goajiro*
	Fulani	Cp. Eskimo*	Warrau*
	Teda*	Montagnais	Siriono*
	Rwala*	Micmac*	Nambicuar*
	Lapps*	Slave*	Timbira
	Yurak	Saulteaux	Botocudo*
	Basseri	Kaska*	Shavante
	Toda*	Twana*	Aweikoma*
	Khalka*	E. Pomo	Lengua
	Andaman*	Paiute	Abipon
	Vedda*	Klamath*	Tehuelche
	Ainu*	Kutenai	Yahgan*
	Gilyak*	G. Ventre	
		53	7
Compact Impermanent/Homesteads/Hamlets/Complex	Thonga*	Sema Naga	Trukese
	Mbundu	Palaung*	Yapese
	Nyakyusa	Semai*	Palauans*
	Luguru	Nicobar	Atayal
	Kikuyu	Tanala	Aleut
	Ganda	Manchu	Eyak*
	Nkundo	Koreans	Haida
	Banen	Japanese	Bellacoola*
	Ashanti	Toradja	Yurok*
	Mende	Iban*	Creek
	Azande	Alorese	Huichol
	Wolof	Orokaiva*	Miskito
	Fur	Kapauku	Bribri*
	Kafa*	Kwoma	Callinago
	Amhara	Manus	Yanomamo
	Nubians*	Lesu	Carib*
	Gheg	Trobriand	Saramacca
	Abkhaz	Siuai*	Mundurucu
	Punjabi	Pentecost*	Cubeo
	Gond*	Ajie*	Jivaro*
	Santal	Maori	Amahuaca*
	Burusho	Marquesans	Inca
	Lolo	Samoans	Trumai*
	Lepcha	Gilberts	Cayua*
	Garó	Marshalls	Mapuche*
			75
	Tiv	Khmer	
	Ibo	Siamese	
	Fon	Negri Sembilan	
	Bambara	Chinese	
	Tallensi	Javanese	
	Otoro Nuba	Balinese	
	Shilluk	Kimam	
	Songhai	Tikopia	
	Hausa	Mbau Fijians	
	Kanuri	Ifugao	
	Konso	Natchez	
	Riffians	Zuni	
	Egyptians	Aztec	
	Hebrews	Popoluca	
	Babylonians	Yucatec Maya	
	Turks	Cuna	
	Romans	Haitians	
	Basques	Cayapa	
	Irish	Aymara	
	Russians	Tupinamba	
	Armenians		
	U. Pradesh		
	Burmese		
	Vietnamese		
	Rhade		
			45

$$Q = .639 \quad \phi = .269 \quad X^2 = 12.994 \quad C = .259 \quad p < .001$$

* indicates societies whose average size is 100 or fewer persons.

TABLE 11. INTERCORRELATION OF SUBSISTENCE AND EXTERNAL WAR-ATTACKING

Continual/Frequent External War			Infrequent Internal War		
Food-Collectors	Nama	Miskito	Kung	Slave	
	Yurak Samoyed	Callinago	Mbuti	Kaska	
	Ainu	Nambicuara	Andamanese	Bellacoola	
	Yukaghir	Botocudo	Vedda	Twana	
	Chukchee	Shavante	Gilyak	Yurok	
	Aleut	Aweikoma	Badjau	E. Pomo	
	Micmac	Lengua	Tiwi	Yokuts	
	Eyak	Abipon	Aranda	Paiute	
	Haida	Tehuelche	Manus	Kutenai	
	Klamath		Ingalik	Warrau	
	Gros Ventre		Cp. Eskimo	Siriono	
Comanche		Montagnais	Yahgan		
Chiricahua		Saulteaux			
		22	25		
Food-Producers	Thonga	Romans	Aztec	Kongo	Omaha
	Lozi	Irish	Yucatec	Nyakyusa	Havasupai
	Mbundu	Russians	Maya	Luguru	Huichol
	Ila	Abkhaz	Bribri	Tallensi	Cuna
	Kikuyu	Kurd	Yanomamo	Songhai	Goajiro
	Ganda	Basseri	Saramacca	Nubians	Haitians
	Banen	Punjabi	Mundurucu	Turks	Carib
	Tiv	U. Pradesh	Jivaro	Gheg	Cubeo
	Fon	Burusho	Inca	Basques	Cayapa
	Ashanti	Kazak	Aymara	Lapps	Trumai
	Mende	Khalka	Timbira	Gond	Cayua
	Bambara	Lolo	Tupinamba	Toda	
	Azande	Garo	Mapuche	Santal	
	Otoro Nuba	Sema Naga		Lepcha	
	Shilluk	Vietnamese		Burmese	
	Ingassana	Khmer		Palaung	
	Masai	Siamese		Rhade	
	Wolof	Japanese		Semai	
	Fulani	Javanese		Nicobarese	
	Hausa	Iban		Tanala	
	Kanuri	Orokaiva		Negri Sembilan	
	Fur	Pentecost		Chinese	
	Somali	Ajie		Koreans	
	Amhara	Marquesans		Alorese	
	Bisharin	Trukese		Kapauku	
	Teda	Hidatsa		Kwoma	
	Tuareg	Pawnee		Trobrianders	
	Riffians	Huron		Tikopia	
	Egyptians	Creek		Maori	
	Hebrews	Natchez		Marshallese	
	Babylonians	Zuni		Yapese	
	Rwala	Papago	76	Palauans	43

$$Q = -.335 \quad \phi = -.156 \quad X^2 = 4.053 \quad C = .154 \quad p = .05 \quad .044$$

continual or frequent offensive external war and, conversely, a positive association between food-producers and continual or frequent offensive external war. Prediction is very weak, however; knowledge of subsistence correcting predicts external war only 4.4 per cent of the time. The geographical distribution of the societies by cells is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>	
Africa	1	2	17	4	
Circum-Mediterranean	0	0	20	6	
East Eurasia	4	3	13	13	
Insular Pacific	0	4	7	9	
North America	8	13	8	3	
South America	9	3	11	8	
Cell frequencies	22	25	76	43	166

Note that once again 70 per cent of the 47 food-collecting societies are in the New World and two-thirds of those are in North America. North America has consistently exerted the strongest influence in the food-collecting community configuration. I shall have occasion to discuss the relationships of food-producers below.

In the finest subsistence scaling for food-collectors, significant intercorrelations turned up that did not with the broad distinction between collectors and producers. Gatherers are negatively associated with defensive external war (external war-being attacked) ($< .01$; $\lambda = .094$); they do not go to war for plunder ($< .05$; $\lambda = .059$); they do not go to war for the lumped expectations of land and plunder

($< .10 > .05$). Fishers do not engage in offensive external war ($< .05$; $\lambda = .074$); nor do they go to war to subjugate people (.02). They do go to war for plunder ($< .05$) but not predictably; they do not go to war for the lumped expectations of subjugation and tribute ($< .01$) but not predictably. Hunters do go to war for plunder ($< .01$) but not predictably. Ten of the 13 societies with this association are in the New World (Montagnais, Micmac, Slave, Gros Ventre, Comanche, Siriono, Aweikoma, Lengua, Abipon, and Tehuelche). Recall that the subsistence designation for all the societies is according to the manner in which the people derive most of their food; there is only 1 subsistence identification for each society. Hunters do go to war for revenge ($< .01$) but not predictably.

The third configuration that I have isolated in the world sample has the largest number of variables and is the most elaborate in its relationships. Although there may be some awkwardness of fit, I call this the state configuration including petty paramount chiefdoms as well as small and large "states." Otterbein's variable of "centralized political organization," as contrasted to "uncentralized," would do also. Because the combinations of the intercorrelations are so numerous, I have arranged all those significant at the .05, .01, and .001 levels in Table 12, leaving non-significant intercorrelations blank. Then I have included fourfold tables of several of the more interesting

TABLE 12. SIGNIFICANCE LEVELS OF ASSOCIATIONS IN THE STATE CONFIGURATION

Variable	8	9	12	13	14	15	19	22	23	24	25	30	31	32	36	37	38	33	35	45
8																				
9	.01																			
12	.05																			
13	.001																			
14	.001																			
15	.001	.001		.001																
19	.001	.001		.001	.001	.001														
22			.05	.01	.001	.05	.01													
23					.05			.001												
24		.001		.001	.001	.001	.001	.05												
25	.001	.05		.01	.01	.01	.001	.01		.01										
30	.001	.001		.001	.001	.01	.001	.01		.001	.01									
31	.01			.01	.05		.05	.02		.05		.02								
32	.001	.01		.001	.001	.001	.001	.05		.001	.001	.001	.05							
36	.05	.02		.01	.02		.01	.001	.02									.001		
37	.05			.01	.001	.05	.001	.001	.02						.001			.001	.001	
38	.01			.001	.05		.01	.001		.02	.01	.001			.001	.01		.02		
33					.01			.001	.01											
35					.01	.01		.001	.001									.001		
45	.001			.001	.001	.001	.01			.001	.01	.05	.01	.001						

correlations, to illustrate their geographical representation. I have attempted to bring out the most powerful relationships that have predictive direction. The attributes present in this configuration are: compact permanent and complex settlements; mean size of the typical community of 400 and larger; 3-4 levels in local political organization; 1-4 levels of jurisdictional hierarchy beyond the local community; 2-4 levels beyond the local community; food-production; social stratification; continual or frequent external war-attacking and being attacked; military organization in the form of age-sets military societies, and standing armies; decisions to go to war made by an official or a council; high military expectations of subjugation, tribute, land, and sometimes trophies and honors; high casualties; authoritative command in battle; violence seen as a solution to problems; war given high value; military success defined as territorial expansion; the absence of revenge as a military expectation; prestige for warriors; and rewards for warriors.

Table 13 shows the results of intercorrelating 2-4 jurisdictional levels beyond the local community with the incidence of offensive external war. The following is a breakdown of the societies into geographical region by cells.

TABLE 13. INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY
BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR- ATTACKING

	Continual/Frequent External War			Infrequent External War	
2-4 Levels	Thonga	Amhara	Sema Naga	Nyakyusa	
	Lozi	Tuareg	Vietnamese	Songhai	
	Mbundu	Egyptians	Khmer	Turks	
	Ganda	Hebrews	Khmer	Gheg Albanians	
	Fon	Babylonians	Siamese	Burmese	
	Ashanti	Irish	Japanese	Negri Sembilan	
	Azande	Russians	Javanese	Chinese	
	Wolof	Kurd	Creek	Koreans	
	Hausa	Punjabi	Aztec	Alorese	
	Kanuri	U. Pradesh	Saramacca	Palauans	
	Fur	Kazak	Inca		
	Somali	Khalka			
			34		10
0-1 Level	Nama	Micmac		Kung	Copper Eskimo
	Ila	Eyak		Kongo	Montagnais
	Kikuyu	Haida		Luguru	Saulteaux
	Banen	Klamath		Mbuti	Slave
	Tiv	Gros Ventre		Tallensi	Kaska
	Mende	Hidatsa		Nubians	Bellacoola
	Bambara	Pawnee		Lapps	Twana
	Otoro Nuba	Huron		Gond	Yurok
	Shilluk	Natchez		Toda	E. Pomo
	Ingassana	Comanche		Santal	Yokuts
	Masai	Chiricahua		Lepcha	Paiute
	Fulani	Zuni		Palaung	Kutenai
	Bisharin	Papago		Rhade	Omaha
	Teda	Yucatec Maya		Semai	Havasupai
	Riffians	Miskito		Nicobarese	Huichol
	Rwala	Bribri		Andamanese	Cuna
	Yurak Samoyed			Vedda	Goajiro
	Basseri	Callinago		Tanala	Haitians
	Burusho	Yanomamo		Gilyak	Warrau
	Lolo	Mundurucu		Badjau	Carib
	Garó	Jivaro		Tiwi	Cubeo
	Ainu	Nambicuara		Aranda	Cayapa
	Yukaghir	Timbira		Kapauku	Siriono
	Chukchee	Tupinamba		Kwoma	Trumai
	Iban	Botocudo		Manus	Cayua
	Orokaiva	Shavante		Trobrianders	
	Pentecost	Aweikoma		Tikopia	Yahgan
	Ajie	Lengua		Maori	
	Marquesans	Abipon		Marshallese	
	Trukese	Mapuche		Yapese	
	Aleut	Tehuelche	61	Ingalik	57

$$Q = .521 \quad \phi = .231 \quad X^2 = 8.645 \quad p < .01 \quad C = .225$$

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>	
Africa	7	1	11	5	
Circum-Mediterranean	13	3	5	2	
East Eurasia	9	3	8	12	
Insular Pacific	1	2	6	11	
North America	2	0	14	16	
South America	2	0	31	11	
Cell frequencies	34	10	61	57	162

Note that the members of cell a--their level codings translatable into petty paramount chiefdoms, small states, and large states, and experiencing continual or frequent offensive external war--are in large majority from Africa, the Circum-Mediterranean, and East Eurasia. Thirty-eight per cent of the societies are from the Circum-Mediterranean region and 85 per cent are from the Circum-Mediterranean, Africa, and East Eurasia. African kingdoms and modern states from Eurasia accentuate the influence of ancient and modern states from the Circum-Mediterranean. While the intercorrelation is significant at the .01 level, it generates no predictive direction. Nor do these centralized polities have a significant association with external war-being attacked (Table 14).

Using another scaling from the Ethnographic Atlas, however, strengthens coefficients and produces predictability but I am not certain how to interpret the cost. In this intercorrelation, the political attribute is 1 to 4 levels of jurisdictional hierarchy beyond the local community. It admits such African societies as the Nama, the Ila, the Tiv,

TABLE 14. INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY
BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR-BEING ATTACKED

Continual/Frequent External War			Infrequent External War		
2-4 Levels	Nyakyusa	Kurd	Thonga	Balinese	
	Banda	U. Pradesh	Lozi	Alorese	
	Nkundo Mongo	Khalka	Ashanti	Palauans	
	Fon	Burmese	Hausa		
	Azande	Vietnamese	Kanuri		
	Wolof	Khmer	Amhara		
	Songhai	Chinese	Hebrews		
	Fur	Koreans	Turks		
	Somali	Japanese	Irish		
	Tuareg	Javanese	Russians		
	Egyptians	Creek	Punjabi		
	Babylonians	Aztec	Sema Naga		
	Gheg Albanians	Saramacca	Siamese		
	Armenians	Inca	Negri Sembilan		
	28		17		
0-1 Level	Nama	Ajie	Tupinamba	Kung	Slave
	Ila	Marquesans	Bctocudo	Kongo	Kaska
	Hadza	Marshallese		Mbuti	Twana
	Luguru	Trukese	Aweikoma	Tallensi	Yurok
	Kikuyu	Ifugao	Cayua	Nubians	E. Pomo
	Banen	Aleut	Abipon	Lapps	Yokuts
	Tiv	Montagnais	Tehuelche	Gond	Havasupai
	Ibo	Micmac	Mapuche	Toda	Huichol
	Bambara	Eyak		Santal	Bribri
	Otoro Nuba	Bellacoola		Burusho	Goajiro
	Shilluk	Klamath		Lepcha	Haitians
	Masai	Kutenai		Palaung	Warrau
	Fulani	Hidatsa		Nicobar	Yanomamo
	Konso	Pawnee		Andaman	Carib
	Bisharin	Huron		Vedda	Mundurucu
	Teda	Natchez		Tanala	Cayapa
	Riffians	Comanche		Gilyak	Shavante
	Rwala	Chiricahua		Chukchee	Yahgan
	Basseri	Zuni		Badjau	
	Lolo	Papago		Tiwi	
	Garro	Yucatec Maya		Aranda	
	Rhade	Miskito		Kapauku	
	Semai	Callinago		Kwoma	
	Ainu	Cubeo		Tikopia	
	Yukaghir	Jivaro		Maori	
	Iban	Amahuaca		Yapese	
	Orokaiva	Siriono		Ingalik	
	Trobriand	Trumai		Cp. Eskimo	
	Siuai	Timbira	65	Saulteaux	47

$$Q = .087 \quad \phi = .039 \quad X^2 = .233 \quad \text{n.s.} \quad C = .038$$

and the Masai, and Circum-Mediterranean ones such as the Riffians and the Somali (Table 15). The geographical distribution of this intercorrelation with the presence or absence of frequent external war-attacking by cell is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>	
Africa	15	2	3	4	
Circum-Mediterranean	18	3	0	2	
East Eurasia	13	9	4	7	
Insular Pacific	3	8	4	5	
North America	8	2	8	14	
South America	5	1	14	10	
Cell frequencies	62	25	33	42	162

While the association is now significant at the .001 level, the relative effect of these 3 Old World regions is lessened. Africa accounts for 29 per cent of the societies in cell a, and it, together with the Circum-Mediterranean and East Eurasia, accounts for 74 per cent. However, the correlation possesses predictive direction, although odd in one way: knowledge of the presence of 1-4 jurisdictional levels allows prediction of the presence of external war-attacking 13.4 per cent of the time. However, knowledge of external war allows prediction of these jurisdictional levels 22.7 per cent of the time. Probably the latter lambda is reflecting age-sets and other supra-community organizations that are directly involved in war.

Moreover, this political attribute has a positive association with external war-being attacked, one of the few such in the entire study (Table 16). The probability level

TABLE 15. INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR-ATTACKING

Continual/Frequent External War			Infrequent External War		
1-4 Levels	Nama	Amhara	Khmer	Kongo	Palauans
	Thonga	Bisharin	Siamese	Nyakyusa	Kutenai
	Lozi	Teda	Japanese	Songhai	Omaha
	Mbundu	Tuareg	Ainu	Turks	Cuna
	Ila	Riffians	Javanese	Gheg	
	Ganda	Egyptians	Ajie	Maria Gond	
	Tiv	Hebrews	Marquesans	Toda	
	Fon	Babylonians	Micmac	Santal	
	Ashanti	Rwala	Eyak	Lepcha	
	Mende	Irish	Gros Ventre	Burmese	
	Bambara	Russians	Pawnee	Tanala	
	Azande	Kurd	Huron	Negri Sembilan	
	Otoro Nuba	Basseri	Creek	Chinese	
	Shilluk	Punjabi	Natchez	Koreans	
	Masai	U. Pradesh	Aztec	Alorese	
	Wolof	Burusho	Yuca. Maya	Kapauku	
	Fulani	Kazak	Miskito	Trobrianders	
	Hausa	Khalka	Saramacca	Tikopia	
	Kanuri	Garó	Inca	Maori	
	Fur	Sema Naga	Tupinamba	Marshallese	
	Somali	Vietnamese	62	Yapese	25
0 Levels	Kikuyu	Muncurucu	Kung	Slave	
	Banen	Jivaro	Luguru	Kaska	
	Ingassana	Nambicuara	Mbuti	Bellacoola	
	Yurak Samoyed	Timbira	Tallensi	Twana	
	Lolo	Botocudo	Nubians	Yurok	
	Yukaghir	Shavante	Lapps	E. Pomo	
	Chukchee	Aweikoma	Palaung	Yokuts	
	Iban	Lengua	Rhade	Paiute	
	Orokaiva	Abipon	Semai	Havasupai	
	Pentecost	Mapuche	Nicobarese	Huichol	
	Trukese	Tehuelche	Andamanese	Goajiro	
	Aleut		Vedda	Haitians	
	Haida		Gilyak	Warrau	
	Klamath		Badjau	Carib	
	Hidatsa		Tiwi	Cubeo	
	Comanche		Aranda	Cayapa	
	Chiricahua Apache		Kwoma	Siriono	
	Zuni		Manus	Trumai	
	Papago		Ingalik	Cayua	
	Bribri		Copper Eskimo	Yahgan	
	Callinago		Montagnais		
Yanomamo		Saulteaux		42	

$$Q = .519 \quad \phi = .276 \quad X^2 = 12.345 \quad p < .001 \quad C = .266 \quad \lambda_c = .134$$

$$\lambda_r = .227$$

TABLE 16. INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR-BEING ATTACKED

Continual/Frequent External War			Infrequent External War		
1-4 Levels	Nama	Teda	Javanese	Thonga	Negri Sembilan
	Ila	Tuareg	Trobriand	Lozi	Balinese
	Nyakyusa	Riffians	Ajie	Kongo	Alorese
	Ganda	Egyptians	Marquesans	Ashanti	Kapauku
	Nkundo	Babylonians	Marshallese	Hausa	Tikopia
	Tiv	Rwala	Micmac	Kanuri	Maori
	Ibo	Gheg	Eyak	Amhara	Yapese
	Fon	Armenians	Kutenai	Hebrews	Palauans
	Bambara	Kurd	Pawnee	Turks	
	Azande	Basseri	Huron	Irish	
	Otoro Nuba	U. Pradesh	Creek	Russians	
	Shilluk	Khalka	Natchez	Punjabi	
	Masai	Garo	Aztec	Gond	
	Wolof	Burmese	Yuca. Maya	Toda	
	Songhai	Vietnamese	Miskito	Santal	
	Fulani	Khmer	Saramacca	Burusho	
	Fur	Chinese	Inca	Lepcha	
	Konso	Koreans	Tupinamba	Sema Naga	
	Somali	Japanese		Siamese	
Bisharin	Ainu	58	Tanala	28	
0 Levels	Hadza	Jivaro	Mbuti	Huichol	
	Luguru	Amhuaca	Tallensi	Bribri	
	Kikuyu	Siriono	Nubians	Goajiro	
	Banen	Trumai	Lapps	Haitians	
	Lolo	Timbira	Palaung	Warrau	
	Rhade	Botocudo	Nicobarese	Yanomamo	
	Semai	Aweikoma	Andamanese	Carib	
	Yukaghir	Cayua	Vedda	Mundurucu	
	Iban	Abipon	Gilyak	Cayapa	
	Orokaiva	Mapuche	Chukchee	Shavante	
	Siuai	Tehuelche	Badjau	Yahgan	
	Trukese		Tiwi		
	Ifugao		Aranda		
	Aleut		Kwoma		
	Montagnais		Ingalik		
	Bellacoola		Copper Eskimo		
	Klamath		Saulteaux		
	Hidatsa		Slave		
	Comanche		Kaska		
	Chiricahua Apache		Twana		
	Zuni		Yurok		
	Papago		E. Pomo		
	Callinago		Yokuts		
	Cubeo		Havasupai	36	

$$Q = .361 \quad \phi = .184 \quad X^2 = 5.303 \quad p < .05 \quad C = .181 \quad \lambda_c = .016 \quad \lambda_r = .113$$

is only .05, but the lambdas indicate, albeit weakly, that the political attribute is predictable from the presence of defensive war 11.3 per cent of the time.

There is something else interesting happening here. Comparing the significance levels obtained by 1-4 jurisdictional levels to those by 2-4 jurisdictional levels, one finds the levels reached by the latter either non-significant or slightly higher. I think that the androcentric configuration of tribal societies, particularly African ones, is overlapping the state configuration dominated by the Circum-Mediterranean region. This explains the associations achieved with the prestige and reward variables. Certainly it is a thoroughly documented and commonly experienced characteristic of the state in Western history that warfare is highly valued, particularly as the means to solve political and international problems. Furthermore, within the state individual fighters do not receive the public prestige and accolades that they do in militaristic tribal societies. A military institution might, such as the Pentagon or the German General Staff. As I have discussed before, the state does not need incentives when it has legal obligation and force. But it is not the presence of statehood alone that eliminates individual prestige and rewards. Recall that in the Eurasian region, which includes tribal societies as well as modern and ancient states, prestige for warriors, rewards for warriors, and war as a positively valued phenomenon are

absent. I contend that this is further indication that it is the nature of the Circum-Mediterranean state that is dominating the world.

In Figure 1, I have attempted to summarize diagrammatically the highest values of lambda and their direction among the variables in the state configuration that generate any predictability at all. The variables are identified by their numbers; refer to Appendix B for clarification, if necessary. Many more correlations are statistically significant only, e.g., the one between food-producers and external war in Table 8 and others following in Table 17. The lambda values in Figure 1 generally are in the 20-40 percentile, which is only in the moderate range, yet these are the strongest that appear in the world sample. The patterning in the configuration I interpret as follows. There are 3 foci of "explanation"--attributes upon which most of the other variables are dependent for any predictive direction. They are the state (13), external war-attacking (22), and military success defined as territorial expansion of the cultural unit (38). There are also 3 points that seem to be the most dependent upon other variables. They are compact permanent or complex settlements (8), the belief in violence as a solution to problems (36), and war against non-members of the group as highly valued (37). There is no predictive direction between the state and external war, although the relationship is significant at the .01 level. The attributes

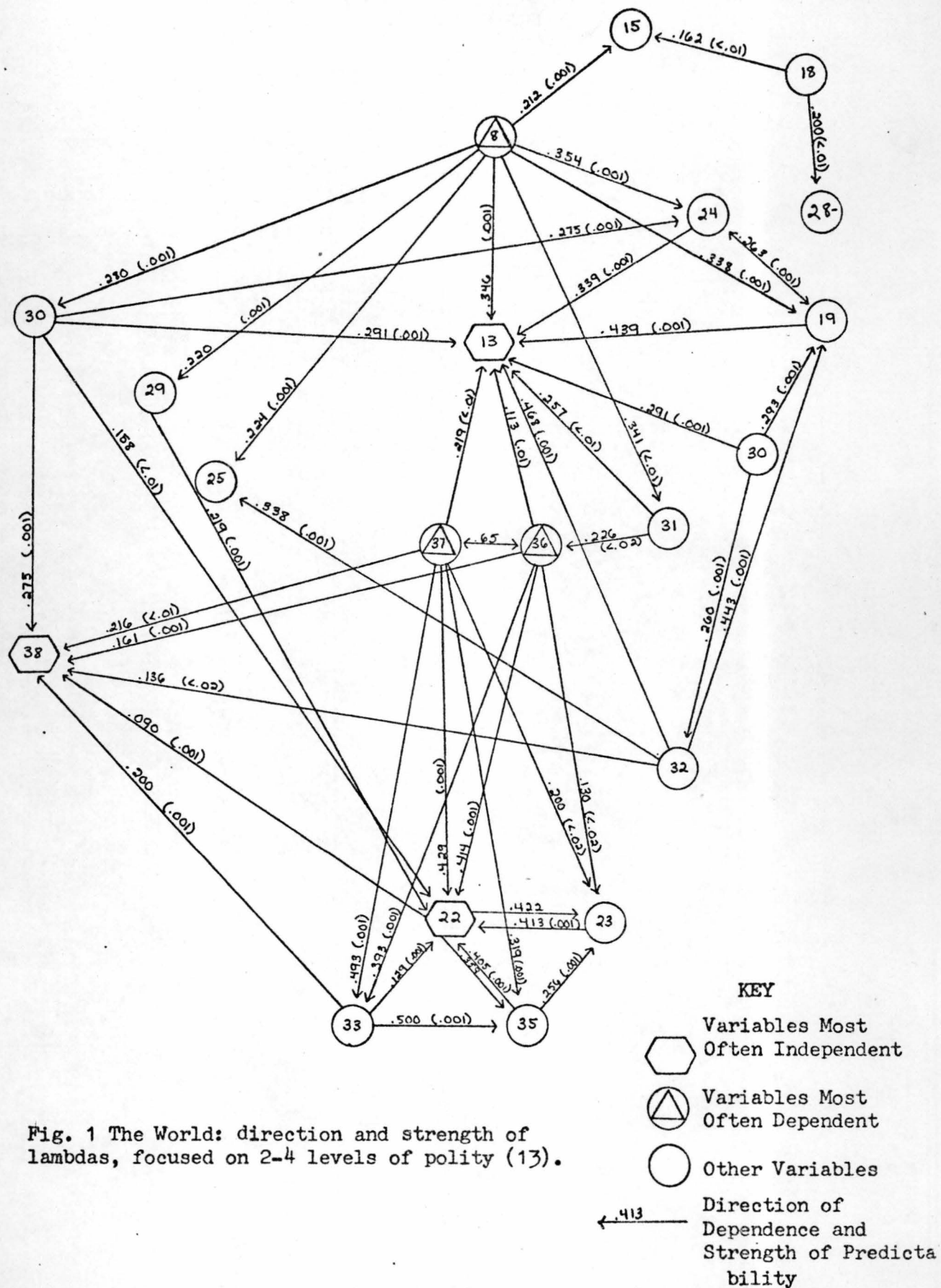


Fig. 1 The World: direction and strength of lambdas, focused on 2-4 levels of polity (13).

TABLE 17. SIGNIFICANT CORRELATIONS WITH O LAMBDA

Pairs of Variables	Phi	p
Compact, complex settlements (8) with:		
3-4 levels of local jurisdiction (12)	.150	<.05
2-4 levels beyond the local community (13) with:		
Food-producers (15)	.354	.001
External war-attacking (22)	.231	<.01
Official decision to go to war (25)	.232	<.01
Peace ceremony absent (28)	.273	<.02
Military expectations of subjugation, tribute, land, trophies and honors (29)	.258	.001
Food-producers (15) with:		
Class stratification (19)	.320	.001
Mobilization by age-sets...standing armies (24)	.298	.001
Official decision to go to war (25)	.260	<.01
Class stratification (19) with:		
External war-attacking (22)	.228	<.01
Military success (territorial expansion)(38)	.205	<.01
Frequent external war-attacking (22) with:		
Mobilization by age-sets...standing armies (24)	.177	<.05
Frequent external war-being attacked (23) with:		
Absent military success (expansion, replacing, unchanging)(39)	.246	<.01
Mobilization by age-sets...standing armies (24) with:		
Military expectations of subjugation, tribute, land, trophies and honors (29)	.240	<.01
Official decision to go to war (25) with:		
Military expectations of subjugation, tribute, land, trophies and honors (29)	.201	<.02
Military success (territorial expansion)(38)	.245	<.01

TABLE 17 (Continued)

Pairs of Variables	Phi	p
High prestige for warriors (33) with:		
Military success (expansion, replacing, unchanging)(39)	.196	<.02
Rewards for warriors (35) with:		
Trophies and honors (44)	.341	.001

generating the strongest predictions are continual or frequent external war-attacking (22), followed in strength by the state (13). Military success (38) is only a weak predictor.

In looking at Figure 1 as a whole, several attributes of the androcentric configuration that I commented upon earlier as impinging upon the state configuration are clustered in the lower half of the diagram, while the state and its dependent attributes are clustered in the upper half. Midway are the attributes of belief in violence as a solution to problems (36) and war as highly valued (37). They may be a point of transition between the 2 configurations, most likely to be predicted through knowledge of offensive external war but also linked to the state. On the world scene these attitude variables are dependent, not independent as I have generally hypothesized. Looking at the bottom of Figure 1, notice the relatively strong predictive

capability generated by the attributes of high prestige for warriors (33) and elaborate rewards for warriors (35), and the nearly balanced influence of external war-attacking (22) and external war-being attacked (23) upon each other. Reading the diagram as a map, I trace the following connections. If political communities of a culture experience attack from outsiders, they may then wage offensive war. Note that attacking offensively is not the primary condition. If warriors are bestowed with elaborate rewards, then being a warrior is predictably a position of high prestige in the community. The existence of such prestige gives war itself an activity of high value and makes very likely the belief that war solves problems. That in turn provides a moderate prediction that high casualties will be sustained. Military success is only a weak predictor of militarist values and attitudes. Once there exist 2-4 levels of political organization in a community, however, there is the relatively strong possibility that there will be class stratification in the community, authoritative military command, standing armies, and military expectations of subjugation, tribute, and land, and relatively moderate to weak predictions that attitudes toward violence and war are positive. I would like to suggest that the patterning in the diagram reflects, however faintly, the geographical distribution of configurations in the Circum-Mediterranean region and in the sub-Saharan African region.

Figure 2 shows the relationships among the variables when the attribute of jurisdictional levels is expanded from 2-4 levels to 1-4 levels. Now there is a direct association between polity and offensive war, where the type of warfare predicts polity almost 23 per cent of the time. There is also a direct association between polity and high military expectations, and the predictive directions of the relationships between jurisdictional levels and military organization, and jurisdictional levels and class stratification, are opposite to those in Figure 1. Internal war becomes part of this configuration through its relationship to unilineal descent, which in turn is related to polity. Internal war becomes tied into each configuration through a different attribute: for the world-wide intercorrelations it is descent groups; for Africa it is the absence of authoritarian military leaders; for the Circum-Mediterranean region it is personal profit and prestige; for North America it is complex settlements; for South America it is violence as an expected solution to problems and patrilocality. The only significant associations with internal war in the East Eurasian region are with endogamy and elaborate rewards for warriors. Neither of these attributes ties internal war into the larger regional configuration. There are no significant associations with internal war within the Insular Pacific region, although internal war is significantly associated with this region.

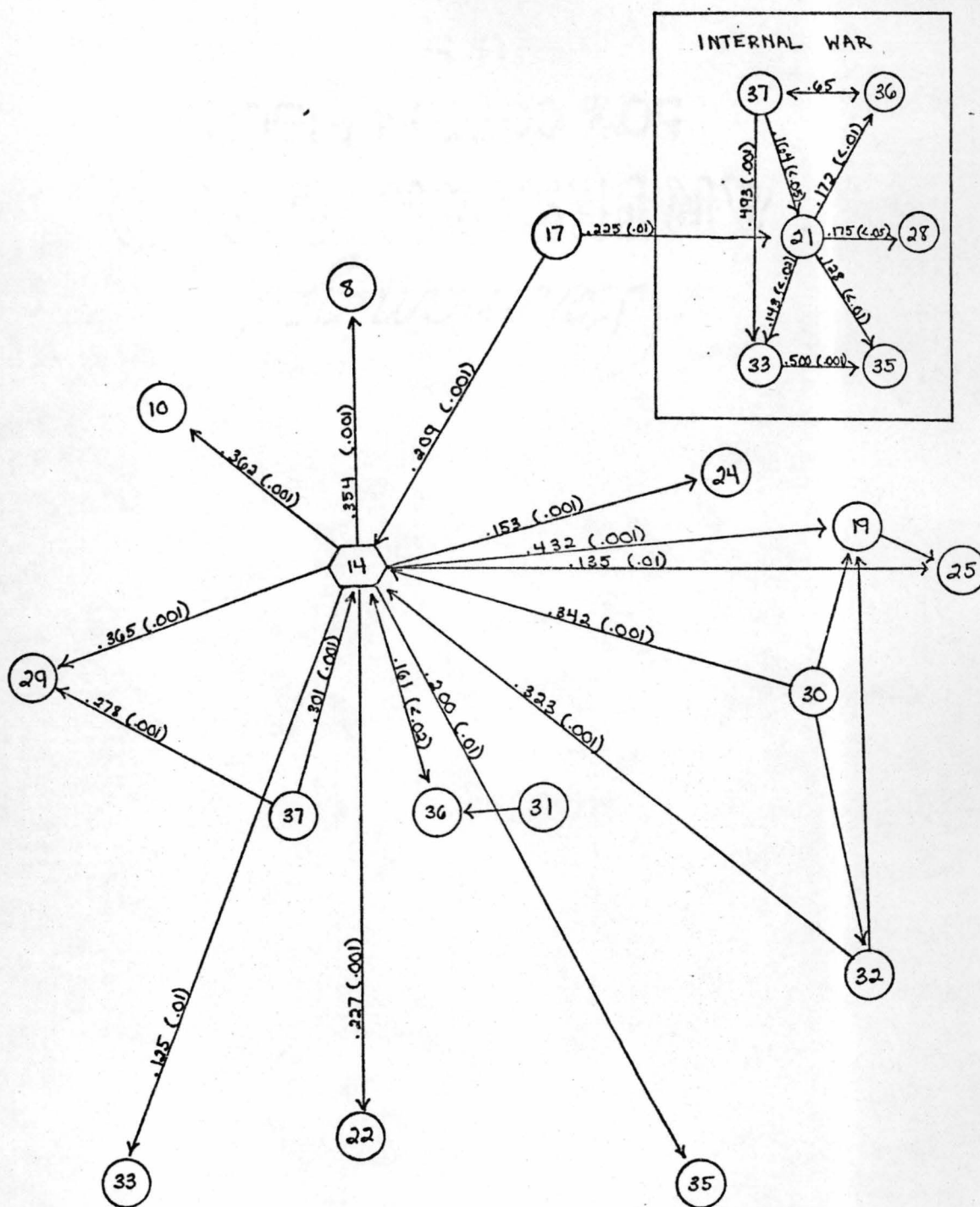


Fig. 2 The World: direction and strength of lambdas, focused on 1-4 levels of polity (14).

The "map" is multiplied in complexity, however, because several of the attributes are predictable in 2 directions. My inclusion of only the strongest lambda values and their directions obscures what I can only label as feedback relationships among the attributes, although one may have stronger influence than the other. Nevertheless, a feedback or circular relationship exists. For instance, one can predict from knowledge of high prestige for warriors that war itself will be highly valued 49 per cent of the time. But from knowledge that war is a highly valued activity, one can predict 39 per cent of the time that warriors will be a prestigious or mandatory role for every male. That while the presence of rewards explains prestige 50 per cent of the time, the reverse is the case 34 per cent of the time. Not all of the relationships are two-way, however. Many are predictable in only one direction. In Table 18, I include the values for both directions of lambda.

In further support of the configurational argument, I include more substantive data in Tables 19-21. illustrating the relationships between the state and military organization, military expectations, and military success, breaking the cell frequencies down into their geographical constituents. The effect of societies from the Circum-Mediterranean and part of the Eurasian region is apparent in all of them. Note, however, that the correlations in Tables 20 and 21 have no predictive direction.

TABLE 18. SIGNIFICANT CORRELATIONS WITH
LAMBDA IN TWO DIRECTIONS

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Complex Settlements (8) with:				
Class stratification (19)	.407	.001	.338	.121
Age-sets...standing armies (24)	.392	.001	.354	.177
Military expectations II (30)	.323	.001	.280	.272
High casualties (31)	.351	<.01	.341	.250
Authoritative command (32)	.431	.001	.408	.373
Expectations of violence (36)	.173	<.05	.067	.097
High value of war (37)	.175	<.05	.113	.149
Row attribute 2-4 Political Levels (13) with:				
Class stratification (19)	.567	.001	.289	.439
Age-sets...standing armies (24)	.467	.001	.188	.339
High casualties (31)	.246	<.01	.037	.257
Authoritative command (32)	.450	.001	.267	.468
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
Complex settlements (8)	.424	.001	.354	.346
Mean community size 0-100 (10)	.419	.001	.362	.073
Unilineal descent (17)	.245	.001	.171	.209
Class stratification (19)	.529	.001	.432	.193
Frequent external war-attacking (22)	.276	.001	.227	.134
Military expectations I (29)	.436	.001	.365	.319
Military expectations II (30)	.407	.001	.297	.342
Authoritative command (32)	.450	.001	.300	.323
Prestige for warriors (33)	.209	.01	.125	.031
High value for violence/war (37)	.323	.001	.271	.301
Row attribute Class Stratification (19) with:				
Military expectations II (30)	.355	.001	.086	.293
Authoritative command (32)	.502	.001	.370	.443

TABLE 18 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Frequent External War- Attacking (22) with:				
External war-being attacked (23)	.500	.001	.422	.413
Military expectations I (29)	.346	.001	.194	.219
Prestige for warriors (33)	.306	.001	.085	.129
Elaborate rewards for warrior (35)	.502	.001	.359	.405
Expectations of violence (36)	.455	.001	.346	.414
High value for violence/war (37)	.453	.001	.355	.429
Row attribute Frequent External War- Being Attacked (23) with:				
Rewards for warriors (35)	.397	.001	.194	.256
Expectations of violence (36)	.217	<.02	.041	.130
Row attribute Age-sets...Standing Armies (24) with:				
Military expectations II (30)	.292	.001	.065	.275
Authoritative command (32)	.380	.001	.241	.313
Row attribute High Military Expectations II (subjugation, tribute, land) (30) with:				
High casualties (31)	.269	<.02	.211	.167
Authoritative command (32)	.275	.001	.260	.194
Row attribute High Casualties (31) with:				
Authoritative command (32)	.258	<.05	.152	.152
Expectations of violence (36)	.297	<.02	.226	.040
Row attribute High Prestige for Warrior (33) with:				
Elaborate rewards for warrior (35)	.531	.001	.500	.341
Expectations of violence (36)	.486	.001	.333	.393
Highly valued war (37)	.503	.001	.390	.493
Row attribute Elaborate Rewards for Warriors (35) with:				
High value for war (37)	.379	.001	.220	.319

TABLE 19. INTERCORRELATION OF MILITARY ORGANIZATION
AND POLITICAL ORGANIZATION

		Mobilization	
		Age Sets...Armies	Informal or Absent
Political Organization	Petty Paramount Chiefdoms, Small and Large States	7 Africa 12 Circum-Mediterranean 10 East Eurasia 3 Insular Pacific 2 South America 34	2 Africa 4 Circum-Mediterranean 3 East Eurasia 3 Insular Pacific 1 North America 1 South America 14
	0-1 Level of Jurisdiction Beyond the Local Community	5 Africa 1 Circum-Mediterranean 2 East Eurasia 2 Insular Pacific 9 North America 6 South America 25	12 Africa 6 Circum-Mediterranean 14 East Eurasia 20 Insular Pacific 22 North America 16 South America 91

164

$$Q = .797$$

$$\phi = .467$$

$$\chi^2 = 35.799 \quad p < .001$$

$$C = .423$$

$$\lambda_c = .339 \quad \lambda_r = .188$$

TABLE 20. INTERCORRELATION OF MILITARY EXPECTATIONS
AND POLITICAL ORGANIZATION

		Military Expectations	
		Subjugation, Tribute, Land	Other
Political Organization	Petty Paramount Chiefdoms, Small and Large States	6 Africa 13 Circum-Mediterranean 10 East Eurasia 4 Insular Pacific 1 North America 2 South America 36	3 Africa 4 Circum-Mediterranean 3 East Eurasia 2 Insular Pacific 1 North America 13
	O-1 Level of Jurisdiction Beyond the Local Community	6 Africa 6 Circum-Mediterranean 8 East Eurasia 9 Insular Pacific 9 North America 5 South America 43	11 Africa 1 Circum-Mediterranean 11 East Eurasia 15 Insular Pacific 18 North America 23 South America 79

171

$$Q = .570$$

$$\phi = .258$$

$$\chi^2 = 11.349 \quad p < .001$$

$$C = .249$$

TABLE 21. INTERCORRELATION OF MILITARY SUCCESS
AND POLITICAL ORGANIZATION

		Military Success	
		Territorial Expansion	Net Expansion Absent
Political Organization	Petty Paramount Chiefdoms, Small and Large States	7 Africa 8 Circum-Mediterranean 3 East Eurasia 1 Insular Pacific 1 North America 2 South America 22	2 Africa 9 Circum-Mediterranean 10 East Eurasia 5 Insular Pacific 1 North America 27
	O-1 Level of Jurisdiction Beyond the Local Community	3 Africa 2 Circum-Mediterranean 1 East Eurasia 3 Insular Pacific 4 North America 8 South America 21	15 Africa 8 Circum-Mediterranean 17 East Eurasia 20 Insular Pacific 26 North America 20 South America 106

175

$$Q = .609$$

$$\phi = .296$$

$$\chi^2 = 15.406 \quad p < .001$$

$$C = .284$$

$$\lambda_r = .020$$

Going back to Naroll's and Otterbein's findings, although they are much more interested in military strategy and tactics than I, there are a few replications of their intercorrelations in my research. Naroll (1969) finds that states with greater political centralization are more likely than those with less centralization to go to war. My findings support an argument for an opposite trend (Tables 13 and 15), that in the absence of supra-community but nonstate organizations, offensive external war is less likely, i.e., greater centralization, less offensive war. Specifically, I have argued that there is some pattern of intervening variables between the "state" and "offensive war."

I have not included the mode of succession of state rulers as a variable, as Naroll has, but I do include the mode of succession of rulers of the local community, dichotomized as hereditary and nonhereditary. While Naroll (1969) finds that hereditary rulers are more likely to be involved in war than nonhereditary ones, I find nothing significant with regard to the frequency of war of any type and local rulers. There are, however, significant relationships between hereditary local rulers and communities of 100-400 persons, political organization of 1 to 4 levels beyond the local community, unilineality, class stratification, and--where war occurs--with official decision, commencement by announcement or agreement, authoritative command, and the specific military expectation of plunder. These

relationships are presented in Table 22a-h, once again broken down by geographical region.

These results are curious. In all 8 intercorrelations hereditary succession does not generate predictive capability, except in its relationship to unilineal descent (Table 22c), where the 2 attributes are moderate predictors equally of each other's presence. Otherwise, the mode of succession is moderately predictable only as a dependent variable, whether upon community size, class stratification, authoritative command, or plunder. Of all 8, the strongest predictor is official decision-making in going to war. I cannot discern any consistent geographical patterning among these variables, except that North America seems to load most often in the common absence cell, and the cultures of the Insular Pacific are appearing in the common presence cell in larger numbers than they have in most of the other correlations presented so far. The overall picture, as far as I can see, is that on a world-wide basis local hereditary rulers exist at the will of the community--the Insular Pacific or African or New World headman who claims the right to be headman through lineage membership but who is Harris' hard-working paragon leading by example and reward, including plunder in war, and not by force. I do not have the data to go beyond this to find any transition to an autocratic ruler, unless one wishes to interpret the predictive dependence of local succession upon the presence of authoritative command

TABLE 22. INTERCORRELATIONS OF EIGHT OTHER VARIABLES WITH MODE OF SUCCESSION OF LOCAL HEADMAN

(a)

Succession of Local Headman

Hereditary

Non-Hereditary

Mean Size of Community	100-400 Persons	9 Africa 4 Circum-Mediterranean 6 East Eurasia 9 Insular Pacific 8 North America 5 South America 41	4 Africa 2 Circum-Mediterranean 5 East Eurasia 7 Insular Pacific 6 North America 3 South America 27
	All Other Sizes	5 Africa 6 Circum-Mediterranean 7 East Eurasia 6 Insular Pacific 5 North America 7 South America 36	7 Africa 7 Circum-Mediterranean 11 East Eurasia 6 Insular Pacific 13 North America 10 South America 54

$$Q = .390 \quad \phi = .201 \quad X^2 = 6.385 \quad p < .02$$

$$C = .197 \quad \lambda_c = .182 \quad \lambda_r = .074$$

(b)

Succession of Local Headman

Hereditary

Non-Hereditary

Levels of Jurisdiction Beyond the Local Community	1-4 Levels	11 Africa 10 Circum-Mediterranean 9 East Eurasia 11 Insular Pacific 6 North America 2 South America 49	8 Africa 8 Circum-Mediterranean 10 East Eurasia 6 Insular Pacific 4 North America 1 South America 37
	0 Levels	5 Africa 4 East Eurasia 4 Insular Pacific 8 North America 10 South America 31	3 Africa 1 Circum-Mediterranean 5 East Eurasia 8 Insular Pacific 15 North America 12 South America 44

$$Q = .305 \quad \phi = .156 \quad X^2 = 3.922 \quad p = .05$$

$$C = .154 \quad \lambda_c = .150 \quad \lambda_r = .093$$

TABLE 22--Continued

(c)

Succession of Local Headman

		Hereditary	Non-Hereditary
Descent	Unilineal Descent	11 Africa 8 Circum-Mediterranean 11 East Eurasia 8 Insular Pacific 8 North America 5 South America 51	8 Africa 6 Circum-Mediterranean 7 East Eurasia 6 Insular Pacific 3 North America 3 South America 33
	Non-Unilineal Descent	5 Africa 2 Circum-Mediterranean 2 East Eurasia 7 Insular Pacific 4 North America 9 South America 29	3 Africa 4 Circum-Mediterranean 9 East Eurasia 8 Insular Pacific 16 North America 10 South America 50

$$Q = .454 \quad \phi = .240 \quad \chi^2 = 9.387 \quad p < .01$$

$$C = .233 \quad \lambda_c = .225 \quad \lambda_r = .215$$

(d)

Succession of Local Headman

		Hereditary	Non-Hereditary
Class Stratification	Present	8 Africa 7 Circum-Mediterranean 2 East Eurasia 9 Insular Pacific 4 North America 1 South America 31	3 Africa 3 Circum-Mediterranean 6 East Eurasia 2 Insular Pacific 2 North America 16
	Absent	8 Africa 3 Circum-Mediterranean 9 East Eurasia 5 Insular Pacific 10 North America 11 South America 46	8 Africa 6 Circum-Mediterranean 7 East Eurasia 12 Insular Pacific 17 North America 12 South America 62

$$Q = .446 \quad \phi = .215 \quad \chi^2 = 7.151 \quad p < .01$$

$$C = .210 \quad \lambda_c = .195$$

TABLE 22---Continued

(e)

Decision to Go to War

		Official	Unofficial
Succession of Local Headman	Hereditary	11 Africa 8 Circum-Mediterranean 9 East Eurasia 12 Insular Pacific 10 North America 7 South America 57	2 Africa 1 Circum-Mediterranean 2 East Eurasia 1 Insular Pacific 3 North America 9
	Non-Hereditary	5 Africa 5 Circum-Mediterranean 10 East Eurasia 8 Insular Pacific 5 North America 2 South America 35	3 Africa 2 Circum-Mediterranean 2 East Eurasia 3 Insular Pacific 10 North America 6 South America 26

$$Q = .649 \quad \phi = .324 \quad X^2 = 13.342 \quad p < .001$$

$$C = .308 \quad \lambda_r = .279$$

(f)

Commencement of War

		Announcement/Agreement	Surprise
Succession of Local Headman	Hereditary	4 Africa 2 Circum-Mediterranean 9 Insular Pacific 3 North America 18	7 Africa 6 Circum-Mediterranean 7 East Eurasia 2 Insular Pacific 10 North America 11 South America 43
	Non-Hereditary	1 Africa 2 Circum-Mediterranean 2 East Eurasia 2 Insular Pacific 2 Sou 2 South America 9	8 Africa 6 Circum-Mediterranean 8 East Eurasia 9 Insular Pacific 15 North America 8 South America 54

$$Q = .430 \quad \phi = .184 \quad X^2 = 4.216 \quad p < .05$$

$$C = .181 \quad \lambda_r = .148$$

TABLE 22--Continued

(g)

Authoritative Command

		Present	Absent
Succession of Local Headman	Hereditary	9 Africa 4 Circum-Mediterranean 6 East Eurasia 8 Insular Pacific 4 North America 3 South America 34	4 Africa 2 Circum-Mediterranean 4 East Eurasia 6 Insular Pacific 9 North America 8 South America 33
	Non-Hereditary	2 Africa 3 Circum-Mediterranean 9 East Eurasia 3 Insular Pacific 3 South America 20	7 Africa 4 Circum-Mediterranean 5 East Eurasia 8 Insular Pacific 16 North America 6 South America 46

$$Q = .406 \quad \phi = .208 \quad x^2 = 5.762 \quad p = .02$$

$$C = .204 \quad \lambda_c = .019 \quad \lambda_r = .197$$

(h)

Military Expectation of Plunder

		Present	Absent
Succession of Local Headman	Hereditary	13 Africa 9 Circum-Mediterranean 5 East Eurasia 10 Insular Pacific 10 North America 10 South America 57	3 Africa 1 Circum-Mediterranean 8 East Eurasia 5 Insular Pacific 3 North America 2 South America 22
	Non-Hereditary	8 Africa 4 Circum-Mediterranean 4 East Eurasia 7 Insular Pacific 12 North America 5 South America 40	2 Africa 5 Circum-Mediterranean 12 East Eurasia 5 Insular Pacific 4 North America 8 South America 36

$$Q = .400 \quad \phi = .202 \quad x^2 = 6.303 \quad p < .02$$

$$C = .198 \quad \lambda_r = .184$$

in war (Table 22g) as evidence for such a change. In that particular case, however, the geographical patterning seems to turn on North American cultures loading in the d cell. Otherwise, the cultures of all the Old World geographical regions have about a 50-50 chance of having either hereditary local headmen and authoritative command or non-hereditary succession and non-authoritative command.

As for Naroll's 1966 conclusions about the relationship between frequent war and numerous military expectations, my research also shows significant associations between the types of external war and various scalings of military expectations. External war-attacking correlates with a grouping of "high" military expectations that includes subjugation of people and territory, tribute, land, and trophies and honors at the .001 level, allowing one to predict the frequency of war from the presence of this class of expectations 21.9 per cent of the time and the class of expectations from frequency of war 19.4 per cent of the time. There also exists a significant relationship between frequent offensive war and the individual military expectations of tribute, land, trophies and honors, and plunder, although only plunder generates predictive direction, again nearly equal in both directions: frequent offensive war predicts plunder 22.6 per cent of the time; plunder predicts frequent offensive war 25 per cent of the time. The strongest relationship, however, exists between frequent external offensive war and the lumped

expectations of land and plunder, allowing one to predict that, where one or the other or both expectations exist, 29.7 per cent of the time one will find that the societies wage frequent offensive war. Prediction in the other direction is virtually absent. This moderate lambda value is one of the few relatively high ones obtained from the world-wide intercorrelations. Tables 23a and b show the significant results of intercorrelating external war-attacking and this last scaling of military expectations, and Table 24a-c the results of external war-being attacked. The lumping of "plunder" and "land" obscures important differences in the substantive data, a problem even with the class "plunder" by itself. The Fon capture slaves for market; the Rwala Bedouin raid for camels and horses; the Kazak are after cattle and domestic slaves; the Comanche take horses; the Yanomamo take wives. The point is that what is taken as plunder depends upon the interests of people living in certain ecological and traditional contexts--that one cannot predict what kind of plunder is taken, or what kind of land is fought over, without investigation of the geographical region, the subsistence base, and the culture area, at least. I should note here that, despite the persistent presence of pastoralists in ethnographic cases and illustrations, there is no relationship whatever between pastoralism alone and plunder, in the world-wide intercorrelations, or between pastoralism and lumped land and plunder. This is one instance where mathematical

summaries trip up any generalizations based upon impressions. In any case, while Naroll uses the wording, "... societies that expect more kinds of satisfactions from successful warfare tend to be societies which fight more frequently...." (1966:19), it appears that it is not the number of expectations but the class of expectations that one must look at. For offensive war the 2 classes are land and plunder, and trophies and honors. For defensive war they are these plus a third class lumping revenge, defense, and/or aggressive defense.

While I have not tested Naroll's deterrence hypothesis on the larger sample, where there is frequent offensive war, one can predict there will be frequent external war--being attacked 41.3 per cent of the time, and where there is the latter, one can predict knowledge of the former 42.2 per cent of the time. This relationship, one of the strongest lambdas in the world-wide correlations, is included in Figure 1. Offensive war goes with defensive war, and each is relatively strongly predictable from the other. War generates war. Table 25 identifies the societies in each cell. The 19 in cell c are the ones in difficulty. For example, the Luguru are in a refuge situation, picked at by surrounding slavers; the Songhai are encircled by the Tuareg; the Basques are an oppressed minority, beaten by Franco's forces in the Spanish Civil War; the Senai are in a forest enclave; the Siriono are in a marsh enclave; the Trumai are extinct. The Chinese

TABLE 23. INTERCORRELATION OF TWO CLASSES OF MILITARY EXPECTATIONS AND EXTERNAL WAR

(a)

Military Expectation of Land and/or Plunder

Present	Absent
18 Africa 16 Circum-Mediterranean 14 East Eurasia 4 Insular Pacific 14 North America 17 South America 83	4 Circum-Mediterranean 3 East Eurasia 3 Insular Pacific 2 North America 3 South America 15
2 Africa 1 Circum-Mediterranean 6 East Eurasia 8 Insular Pacific 10 North America 3 South America 30	4 Africa 4 Circum-Mediterranean 10 East Eurasia 5 Insular Pacific 3 North America 8 South America 34

$$Q = .725 \quad \phi = .403 \quad X^2 = 26.246 \quad p < .001$$

$$C = .373 \quad \lambda_c = .082 \quad \lambda_r = .297$$

(b)

Military Expectation of Trophies and Honors

Present	Absent
8 Africa 3 Circum-Mediterranean 3 East Eurasia 4 Insular Pacific 11 North America 8 South America 37	10 Africa 17 Circum-Mediterranean 14 East Eurasia 3 Insular Pacific 5 North America 12 South America 61
1 East Eurasia 2 Insular Pacific 6 North America 9	6 Africa 5 Circum-Mediterranean 15 East Eurasia 11 Insular Pacific 7 North America 11 South America 55

$$Q = .575 \quad \phi = .257 \quad X^2 = 10.689 \quad p < .01$$

$$C = .249$$

TABLE 24. INTERCORRELATIONS OF SPECIFIC MILITARY EXPECTATIONS
AND EXTERNAL WAR-ATTACKING

Infrequent External War Continual/Frequent External War

(a)
Land and/or Plunder

Present	Absent
16 Africa 15 Circum-Mediterranean 8 East Eurasia 6 Insular Pacific 15 North America 14 South America 74	4 Circum-Mediterranean 7 East Eurasia 4 Insular Pacific 2 North America 5 South America 22
3 Africa 3 Circum-Mediterranean 10 East Eurasia 7 Insular Pacific 6 North America 4 South America 33	4 Africa 5 Circum-Mediterranean 6 East Eurasia 4 Insular Pacific 2 North America 6 South America 27

$$Q = .467 \quad \phi = .231 \quad \chi^2 = 8.358 \quad p < .01$$

$$C = .226 \quad \lambda_r = .083$$

Infrequent External War Continual/Frequent External War

(b)
Trophies and Honors

Present	Absent
7 Africa 4 Circum-Mediterranean 1 East Eurasia 5 Insular Pacific 12 North America 6 South America 35	9 Africa 15 Circum-Mediterranean 14 East Eurasia 5 Insular Pacific 5 North America 13 South America 61
1 Africa 3 East Eurasia 1 Insular Pacific 2 North America 2 South America 9	6 Africa 8 Circum-Mediterranean 13 East Eurasia 10 Insular Pacific 6 North America 8 South America 51

$$Q = .530 \quad \phi = .232 \quad \chi^2 = 8.396 \quad p < .01$$

$$C = .226$$

TABLE 24--Continued

(c)

	Revenge, Defense and/or Aggressive Defense	
	Present	Absent
Infrequent External War	13 Africa 18 Circum-Mediterranean 12 East Eurasia 9 Insular Pacific 17 North America 18 South America 87	3 Africa 1 Circum-Mediterranean 3 East Eurasia 1 Insular Pacific 1 South America 9
Continual/Frequent External War	5 Africa 6 Circum-Mediterranean 10 East Eurasia 9 Insular Pacific 8 North America 7 South America 45	2 Africa 2 Circum-Mediterranean 6 East Eurasia 2 Insular Pacific 3 South America 15

$$Q = .526 \quad \phi = .211 \quad X^2 = 6.925 \quad p = .01$$

$$C = .206 \quad \lambda_r = .100$$

TABLE 25. INTERCORRELATION OF EXTERNAL WAR-ATTACKING
AND EXTERNAL WAR-BEING ATTACKED

Continual/Frequent War-Attacking		Infrequent War-Being Attacked	
Continual/Frequent War-Attacking	Nama	Abkhaz	Huron
	Ila	Kurd	Creek
	Kikuyu	Basseri	Natchez
	Ganda	U. Pradesh	Comanche
	Banen	Khalka	Chiricahua
	Tiv	Lolo	Zuni
	Fon	Garó	Papago
	Bambara	Vietnamese	Aztec
	Azande	Khmer	Yuca. Maya
	Otoro Nuba	Japanese	Miskito
	Shilluk	Ainu	Callinago
	Masai	Yukaghir	Saramacca
	Wolof	Javanese	Jivaro
	Fulani	Iban	Inca
	Fur	Orokaiva	Aymara
	Somali	Ajie	Timbira
	Bisharin	Marquesans	Tupinamba
	Teda	Trukese	Botocudo
	Tuareg	Aleut	Aweikoma
	Riffians	Micmac	Abipon
Infrequent War-Attacking	Egyptians	Eyak	Mapuche
	Babylonia	Klamath	Tehuelche
	Rwala	Hidatsa	
	Romans	Pawnee	
			70
			18
	Nyakyusa	Kung	Tiwi
	Luguru	Kongo	Aranda
	Songhai	Mbuti	Kapauku
	Gheg Albanians	Tallensi	Kwoma
	Basques	Nubians	Tikopia
	Burmese	Turks	Maori
	Rhade	Lapps	Yapese
	Semai	Gond	Palauans
	Chinese	Toda	Ingalik
	Koreans	Santal	Copper Eskimo
	Trobrianders	Lepcha	Saulteaux
	Marshallese	Palaung	Slave
	Montagnais	Nicobarese	Kaska
	Bellacoola	Andamanese	Twana
	Kutenai	Vedda	Yurok
	Cubeo	Tanala	E. Pomo
	Siriono	Negri Sem.	Yokuts
	Trumai	Gilyak	Havasupai
	Cayua	Badjau	Huichol
		Alorese	Goajiro
			45
			19

$$Q = .804 \quad \phi = .500 \quad p < .001 \quad X^2 = 37.953 \quad C = .447 \lambda_c = .413 \lambda_r = .422$$

appear in this cell even though one cannot really say they have had the problems of, say, the Songhai or the Trumai, but throughout its history, China has been attacked by would-be conquerors, and indeed conquered, but because of its remarkable historical and cultural capacity to absorb outsiders, China obviously has survived. The geographical breakdown by cells for Table 25 is:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	12	3	2	4
Circum-Mediterranean	14	6	3	3
East Eurasia	10	5	5	11
Insular Pacific	6	0	2	10
North America	14	0	3	11
South America	14	4	4	6

Otterbein has also intercorrelated offensive and defensive external war, finding a significant positive relationship, but one that is less strong than in my sample (Table 25). I have figured the lambda values for his table, and they are also lower. Moreover, according to the direction of his lambdas, the frequency of being attacked is more likely to be explained by the frequency of attacking than vice versa, while I found the 2 variables to be about equal in their predictive dependency, with a slight edge to being attacked explaining attacking.

		Frequency of Being Attacked		
		Continual or Frequent	Infrequent	
Frequency of Attacking	Continual or Frequent	18	9	27
	Infrequent	5	15	20
		<u>23</u>	<u>24</u>	<u>47</u>

$$\phi = .41 \quad X^2 = 7.98 \quad p < .01 \quad \lambda_r = .30 \quad \lambda_c = .3913$$

(adapted from Otterbein 1968:286)

From his coding information, I have been able to identify the cultural units occupying each cell (Table 26), and they break down geographically as follows.

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>	
Africa	2	3	1	3	9
Circum-Mediterranean	1	1	2	0	4
East Eurasia	2	2	0	3	7
Insular Pacific	1	1	0	6	8
North America	6	1	1	2	10
South America	<u>6</u>	<u>1</u>	<u>1</u>	<u>1</u>	<u>9</u>
	18	9	5	15	47

Within this comparison are also instances of disagreement in coding between Otterbein and me, i.e., the Tiv, the Orokaiva, and the Marshallese.

In relating my results further to those of Otterbein, recall my argument earlier in this chapter that polygyny as Otterbein has isolated it may be a misleading variable. Correlation of who decides to go to war ("an official" or "anyone") with the frequency of internal war is non-significant

TABLE 26. RECONSTRUCTION OF OTTERBEIN'S INTERCORRELATION OF
EXTERNAL WAR-ATTACKING AND EXTERNAL WAR-BEING ATTACKED

Continual/Frequent War-Attacking	Ila Nandi Ancient Egyptians Yukaghir Thai (1600 A.D.) Kurtachi Comox Wishram Plains Cree Fox Cherokee Papago Saramacca Jivaro Aymara Tehuelche Abipon Timbira	18	Mossi Tiv Ingassana Somali Tibetans Sema Naga Javanese (1300 A.D.) Aztec Mundurucu	9
	Gisu Albanians (Gheg) Mutair Santa Ana Trumai	5	Dorobo Ambo Amba Japan (1200 A. D.) Toda Andamanese Tiwi Orokaiva Marshallese Lau Tikopia Hawaiians Copper Eskimo Monachi Motilion	15

$$\phi = .41 \quad X^2 = 7.98 \quad p < .01 \quad \lambda_r = .30 \quad \lambda_c = .3913$$

in the ODYSSEY, contrary to Otterbein (1968). Comparing the results of Otterbein's "centralized political systems" with "initiating party" to my "2-4 jurisdictional levels beyond the local community" with "decision to go to war," the ODYSSEY produces something interesting. I have summarized the inter-correlations in tabular form and computed lambda for Otterbein's data.

	Initiating Party		
	Anyone	Official	
Centralized political systems	2	14	16
Uncentralized political systems	16	10	26
	18	24	42

$\phi = .48$ $X^2 = 9.73$ $p < .01$
 $[\lambda_c = .33; \lambda_r = .22]$

(Otterbein 1968:282)

	Decision by Official or Council	Decision by Anyone	
2-4 levels beyond the local community	42	6	48
0-1 level beyond the local community	60	31	91
	102	37	139

$\phi = .232$ $X^2 = 7.482$ $p < .01$ $\lambda = 0$

Notice that while the level of significance is the same in both intercorrelations, the moderate lambda values that one can compute out of Otterbein's table disappear in the larger run.

Next, recall that Otterbein (1968:285) attempts to test the amity-enmity complex: the more frequently political communities fight those who are culturally dissimilar, the less likely they are to fight political communities that are culturally similar. The results of his intercorrelations are not significant. Mine are, although not in the direction that Otterbein would wish. The ODYSSEY produces a positive association between frequent internal war and frequent offensive external war, while that between internal war and external defensive war is not significant. The significant correlation possesses no predictive direction, however.

		<u>External War-Attacking</u>		
		<u>Frequent</u>	<u>Infrequent</u>	
Internal War	Frequent	46	21	67
	Infrequent	45	42	87
		<u>91</u>	<u>63</u>	<u>154</u>

$$\varphi = .171 \quad X^2 = 4.489 \quad p < .05 \quad \lambda_c = 0 \quad \lambda_r = .-15$$

If I were to generalize, I could say that the cumulative record shows that violence within and without a cultural unit has occurred together. Sumner, Ardrey, and Otterbein would not find this generalization to their liking.

As another example of discrepancies between my results and those of Otterbein--and further evidence that one cannot tell beforehand how results may be different--I again quote Otterbein's table intercorrelating political system, internal

war, and initiating party, followed by results of analogous intercorrelations from the ODYSSEY. I have figured lambda values for Otterbein's table.

	Uncentralized Political Systems			Centralized Political Systems		
	Internal War			Internal War		
	Continual or Frequent	Infrequent		Continual or Frequent	Infrequent	
Initiating Party:						
Official	3	6	9	9	2	11
Anyone	12	2	14	1	1	2
	<u>15</u>	<u>8</u>	<u>23</u>	<u>10</u>	<u>3</u>	<u>13</u>
	$\varphi = .54$	$X^2 = 6.63$		$\varphi = -.27$	$X^2 = .97$	
	$p = .01$			n.s.		
	[$\lambda_c = .375$; $\lambda_r = .4444$]			(Otterbein 1968:283)		

	Uncentralized Political Systems			Centralized Political Systems		
	Internal War			Internal War		
	Continual or Frequent	Infrequent		Continual or Frequent	Infrequent	
Decision to Commence War:						
Official or Council	28	27	55	19	22	41
Anyone	17	12	29	3	3	6
	<u>45</u>	<u>39</u>	<u>84</u>	<u>22</u>	<u>25</u>	<u>47</u>
	$\varphi = -.0735$	$X^2 = .4536$		$\varphi = -.0245$	$X^2 = .0282$	
	n.s.			n.s.		

Thus, one of the major significant findings that Otterbein reports for his 1968 study does not hold up on a larger sample. He can no longer claim that "it is demonstrated that fraternal interest groups and unauthorized raiding parties influence the frequency of internal war in uncentralized political systems but not in centralized ones" (1968:277). This is also a rebuttal to the claim that if a correlation is significant on a small sample, it will be even stronger on a larger sample. Some are, some are not. One cannot predict.

I have the codings to replicate other intercorrelations of Otterbein's in his 1968 study. However, on the basis of my criticism in Chapter II of this work, its severe deficiencies in methodology and interpretation do not justify further effort or space here. It is a shame that Otterbein's tortured and convoluted interpretations are wasted upon erroneous inductions.

The only replication of Otterbein's 1970 research that is in the ODYSSEY is of Otterbein's hypothesis that political centralization leads to military success (1970:97), for which he finds no support at all. Table 21 presents the association between these attributes that comes out of my study, significant at higher than the .001 level but without predictive direction. Since I have not been concerned with "military sophistication," I have not tested my reinterpretation of Otterbein's results, in which I hypothesize that at some point militarism becomes the independent variable.

Indirectly related to Otterbein's 1970 results are mine on the relationship of high casualties to other attributes. The following associations are significant at the .05 level or less. Where militarism is highly valued, large numbers of people die. Where offensive war is frequent, people die. Where the state exists, people die. Where there are social classes, formal military organizations, authoritative command, or high military expectations, people die. Their deaths may not be contributing to the achievement of military success, however. So casualties is an important variable but it is tied to a larger matrix of other variables than simply military sophistication.

There is another area of investigation to look at among the world-wide intercorrelations: the relationships, if any, between subsistence variables and warfare variables. Otterbein dismisses the significance of any such relationship after preliminary investigation.

I wished to examine the relationship between terrain, mode of subsistence, and type of sociopolitical system, treated as independent variables, and military organization, tactics, and goals of war, treated as dependent variables.

... As the analysis of the data proceeded, it became clear that ecological and economic factors had little influence, in comparison with the type of sociopolitical system, upon the warfare variables (Otterbein 1970:vii-viii).

Subsequently, Otterbein eliminated discussion of results of these variables from his monograph, resting his generalizations upon the independent variable of sociopolitical

organization alone. When Russell (1972) performed factor analysis upon a large number of variables, he found that a social complexity or level of technological development factor which loaded with variables such as subsistence, community size, social stratification, and political autonomy "... is only minimally related to warfare" (1972:300). A second factor of "formal" variables consisting of descent and residence "... is not at all related to warfare" (1972:300). He then goes on to discuss the "psychocultural" factor that he finds most strongly related to warfare, concluding that formal factors and psychocultural factors pattern independently of each other.

I, however, have found subsistence to be significantly correlated with selected warfare variables, although the lambda values are largely absent, as I have already shown in isolating the food-collecting community configuration. Here I shall go into more detail with regard to the significant associations of agriculturalists. Pastoralists alone in the world-wide correlations do not associate significantly with any other variable; they are included in the largest class, "food-producers," in Table 11. Seeking associations of specific dominant subsistence types, I have correlated them only with the 3 types of warfare, specific military expectations, and expectations lumped into classes "subjugation and tribute," "land and plunder," "trophies and honors," and "revenge, defense, or aggressive defense." Thus, the use of

subsistence attributes is not exhaustive, although it is extensive. Despite these limitations, and the weakness of lambda in the world-wide run, the associations are interesting.

For instance, food-producers taken all together do engage in frequent offensive external war ($\varphi = .156, < .05$), and have high military expectations ($\varphi = .234, < .01$); age-sets, military societies, or standing armies ($\varphi = .298, < .001$); official decision-making when going to war ($\varphi = .260, < .01$); authoritative command ($\varphi = .395, < .001$); rewards for warriors ($\varphi = .271, < .01$); and a high value on war and violence ($\varphi = .166, < .05$). When pastoralists are excluded, however, food-producers are no longer significantly associated with any type of warfare. The pastoralists, famous for their external raiding, load the attribute of food-producers to the point of significance. But food-producers without pastoralists consisting of incipient, extensive, and intensive agriculturalists, if involved in war, are significantly associated with the military expectation of subjugation ($\varphi = .305, < .001$; Table 27) and the expectation class of subjugation and tribute ($\varphi = .266, < .001$), and are significantly not associated with revenge ($\varphi = -.204$). None of the relationships of the "agricultural summary" are predictable. Incipient agriculturalists, such as the Yanomamo, by themselves generate a significant negative relationship only with plunder ($\varphi = -.178$). Extensive

TABLE 27. INTERCORRELATION OF AGRICULTURE AND THE MILITARY EXPECTATION OF SUBJUGATION

		Incipient, Extensive, or Intensive Agriculture	
		Present	Absent
Military Expectation of Subjugation	Present	4 Africa 13 Circum-Mediterranean 8 East Eurasia 5 Insular Pacific 5 South America 35	1 Circum-Mediterranean 1 East Eurasia 2
	Absent	18 Africa 7 Circum-Mediterranean 15 East Eurasia 19 Insular Pacific 10 North America 14 South America 83	4 Africa 6 Circum-Mediterranean 10 East Eurasia 5 Insular Pacific 19 North America 13 South America 57

$$Q = .846$$

$$\phi = .305$$

$$\chi^2 = 16.419 \quad p < .001$$

agriculturalists, such as the Tiv, have more associations: with internal war ($\varphi = .208, < .01$) predictable 14.5 per cent of the time; with the specific expectation of trophies and honors ($\varphi = .223, < .01$), and negatively with the expectation class of revenge, defense, and aggressive defense ($\varphi = -.148, < .05$). Intensive agriculturalists, such as the Japanese, Hidatsa, and Aztec, by themselves are significantly associated with offensive external war ($\varphi = .153, < .05$). They do not go to war for trophies and honors ($\varphi = -.151, < .05$) or for revenge ($\varphi = -.306, < .001$). They do go to war for the class of expectations consisting of subjugation and tribute ($\varphi = .377, < .001$). This relationship possesses a moderate predictive direction: from knowledge that a cultural unit's military expectation includes subjugation or tribute, 16.4 per cent of the time one can predict the subsistence mode will be intensive agriculture. When one drops out tribute, the association becomes stronger ($\varphi = .392, < .001$), as does the predictive direction: 20.8 per cent of the time, one can predict intensive agriculture from the expectation of subjugation, but not the other way around. I find the direction of the lambdas especially interesting. Without them, we would not know that the expectation of subjugation is a sufficient but not necessary condition of intensive agriculture. Therefore, there must be unknown intervening variables connecting subsistence to such a military expectation. The technoenvironmental attribute by itself is not enough to

bring out a particular military circumstance. In Table 28, I have identified the societies. The geographical breakdown by cell is as follows:

	<u>a</u>	<u>b</u>	<u>c</u>	<u>d</u>
Africa	2	6	2	16
Circum-Mediterranean	12	7	2	6
East Eurasia	7	9	2	16
Insular Pacific	2	3	3	21
North America	0	3	0	26
South America	1	1	4	26
Cell frequencies	24	29	13	111

Subjugation as a specific military expectation is a phenomenon of the Circum-Mediterranean region, the cradle of civilization.

The last variable to be discussed is probably the most germane demographic one. The importance of population density as a variable in warfare research is obvious. Prior to the publication of Cross-Cultural Code 3 (Murdock and Wilson 1972), I did not have a source for such information, nor did I include the variable as yet 1 more to attempt to code myself. I have some preliminary results intercorrelating population density with internal war and external war-attacking on the whole Standard Sample. The variable was not included in the original computer runs, and I have not yet carried out supplemental runs on both the world-wide and regional samples. The following results are based upon computations by hand. The attributes of the variable are:

TABLE 28. INTERCORRELATION OF INTENSIVE AGRICULTURE AND THE MILITARY EXPECTATION OF SUBJUGATION

		Subjugation Present			Subjugation Absent		
Agriculture Present	Lozi	Turks	Burmese	Kikuyu	Riffians	Manchu	
	Nyakyusa	Romans	Khmer	Ganda	Gheg	Koreans	
	Hausa	Basques	Tanala	Bambara	Armenians	Kimam	
	Kanuri	Russians	Chinese	Tallensi	Santal	Ajie	
	Amhara	Kurd	Japanese	Otoro Nuba	Burusho	Ifugao	
	Egyptians	Punjabi	Javanese	Ingassana	Lolo	Hidatsa	
	Hebrews	U. Pradish	Balinese	Songhai	Lepcha	Zuni	
	Babylonia	Irish	Inca	Fur	Vietnamese	Aztec	
				Konso	Siamese	Mapuche	
			24	Teda	Negri. Sem.		29
Intensive Agriculture Absent		Fon		Nama	Chukchee	Kutenai	
		Azande		Kung	Iban	G. Ventre	
		Wolof		Thonga	Badjau	Pawnee	
		Abkhaz		Mbundu	Toradja	Omaha	
		Basseri		Kongo	Alorese	Huron	
		Palaung		Ila	Tiwi	Creek	
		Samoans		Luguru	Aranda	Natchez	
		Marshallese		Mbuti	Orokaiva	Comanche	
		Palauans		Nkundo	Kapauku	Apache	
		Bribri		Banen	Kwoma	Havasupai	
		Cuna		Tiv	Manus	Papago	
		Haitians		Ibo	New Ireland	Y. Maya	
		Saramacca		Ashanti	Trobriand	Miskito	
				Mende	Siuai	Goajiro	
				Shilluk	Tikopia	Callinago	
				Masai	Pentecost	Warrau	
				Fulani	Fijians	Yanomamo	
				Somali	Maori	Carib	
				Bisharin	Marquesan	Mundurucu	
				Tuareg	Gilbertese	Cubeo	
				Rwala	Trukese	Cayapa	
				Lapps	Yapese	Jivaro	
				Yurak	Ingalik	Amahuaca	
				Gond	Aleut	Aymara	
				Toda	Montagnais	Siriono	
				Kazak	Micmac	Nambicuara	
				Khalka	Slave	Trumai	
				Garro	Kaska	Timbira	
				Sema Naga	Eyak	Tupinamba	
				Rhade	Haida	Botocudo	
				Semai	Bellacoola	Shavante	
				Nicobarese	Twana	Aweikoma	
				Andamanese	Yurok	Cayua	
				Vedda	Pomo	Lengua	
				Ainu	Yokuts	Abipon	
			13	Gilyak	Paiute	Tehuelche	
				Yukaghir	Klamath	Yahgan	111

$$Q = .752 \quad \phi = .392 \quad X^2 = 27.194 \quad p < .001 \quad \lambda_r = .208$$

Less than one person per five square miles.
 From one person per square mile to one per five miles.
 From 1.1 to 5 persons per square mile.
 From 5.1 to 25 persons per square mile.
 From 26 to 100 persons per square mile.
 From 101 to 500 persons per square mile.
 Over 500 persons per square mile.

(Murdock and Wilson 1972:258)

Coding is on population density in the "area exploited or controlled by the focal or typical community" (Murdock and Wilson 1972:257-58).

In looking at correlations with internal warfare, one sees a vague continuum with clear extremes. Societies with fewer than 1 person per 5 square miles experience little or no internal warfare ($\phi = -.2128$, $p < .01$) and the absence of such war is predictable from population density 17.3 per cent of the time. Societies having over 500 persons per square mile do experience continual or frequent internal war ($\phi = .359$, $p < .001$) and that is predictable 14.7 per cent of the time. The individual attributes of population density between these 2 extremes do not associate significantly with internal warfare. Associations do develop, however, when attributes are grouped into larger classes. Density of 100 persons per square mile appears to be some sort of watershed or boundary. I have dichotomized the types of density 2 ways with the following results. First,

Population Density

Less than 1 person/5 26 to over 500
sq. mi. to 25/1 sq. mi. persons/sq. mi.

Internal War:

Frequent	38	43
Infrequent	51	24

156

$$Q = -.4126 \quad \varphi = -.2128 \quad X^2 = 7.0668$$

$$p < .01 \quad \lambda_c = .0746 \quad \lambda_r = .1733$$

That is, density of 25 persons or fewer per square mile is negatively associated with frequent internal war, making densities of 26 or more persons per square mile positively associated. The presence or absence of internal war is predictable from knowledge of density 17 per cent of the time, and density is predictable from frequency of internal war 7.5 per cent of the time. Upon dichotomizing another way, however, association, significance, and predictability increase.

Population Density

100 persons or fewer/
square mile 101 persons or
more/square mile

Internal War:

Frequent	51	30
Infrequent	67	8

156

$$Q = -.6625 \quad \varphi = -.3069 \quad X^2 = 14.6952$$

$$p < .001 \quad \lambda_c = 0 \quad \lambda_r = .2133$$

Now frequent internal war and density of 100 persons or fewer are negatively associated, and frequent internal war and density of 101 persons or more are positively associated at the .001 level of significance. The presence or absence of internal war is predictable from knowledge of density 21 per cent of the time and it is not reversible. Table 29a shows the results of this intercorrelation with geographical distribution. The only striking characteristic in such distribution that I can see is the loading of North American societies into the c cell, and the relatively large number of Insular Pacific societies in the a cell.

Results are more limited when intercorrelating population density with offensive external war. Out of all the individual attributes of population and their possible logical combinations, only 1 attribute--5.1 to 25 persons per square mile--correlates significantly with offensive external war, but then only at the .05 level and without predictability. Table 29b presents this intercorrelation, with each cell broken down into its regional constituents. I cannot discern any particular geographical patterning, except for the absence of Eurasian societies in the common presence cell. For obvious ecological reasons, population density must be tested further, especially within each geographical region, before reaching any conclusions.

TABLE 29a. INTERCORRELATION OF POPULATION DENSITY AND INTERNAL WAR

		100 Persons Or Fewer Per Square Mile	101 Persons Or More Per Square Mile
Internal War	Continual/Frequent	9 Africa 6 Circum-Mediterranean 8 East Eurasia 12 Insular Pacific 4 North America 12 South America 51	3 Africa 7 Circum-Mediterranean 8 East Eurasia 9 Insular Pacific 3 South America 30
	Infrequent/Never	6 Africa 10 Circum-Mediterranean 9 East Eurasia 5 Insular Pacific 27 North America 10 South America 67	3 Africa 3 East Eurasia 2 Insular Pacific 8

$$Q = -.6625 \quad \phi = -.3069 \quad X^2 = 14.695 \quad p < .001$$

$$r = .2133 \quad C = .171$$

TABLE 29b. INTERCORRELATION OF POPULATION DENSITY AND EXTERNAL WAR-ATTACKING

		5.1 to 25 Persons Per Square Mile	All Other Density Attributes
External War-Attacking	Continual/Frequent	5 Africa 3 Circum-Mediterranean 2 Insular Pacific 3 North America 3 South America 16	11 Africa 14 Circum-Mediterranean 16 East Eurasia 5 Insular Pacific 14 North America 14 South America 74
	Infrequent/Never	2 Circum-Mediterranean 1 East Eurasia 1 North America 4	5 Africa 3 Circum-Mediterranean 14 East Eurasia 13 Insular Pacific 13 North America 11 South America 59

$$Q = .5226 \quad \phi = .1669 \quad X^2 = 4.2687 \quad p < .05$$

$$C = .297$$

This concludes my analysis of correlations based upon the whole world and relating my results to those of Naroll and Otterbein where possible. Even in looking at the world, I have consistently avoided interpreting the statistical statements as universal generalizations but instead have tried to show where particular geographical regions and therefore particular ecological, historical, and traditional circumstances have produced the cultural cases that raise a correlation to statistical significance. In the next section I shall look closely at the particular regions themselves. Since to my knowledge no one has examined such variations in print, I will not be relating my material to that of anyone else.

Regional Configurations

Before discussing the results of correlations within each geographical region in detail, as an illustration of varying association between the same variables from one geographical region to another, Table 30 includes a few excerpts from the exhaustive list of phi comparisons in Appendix D.

TABLE 30. EXCERPTS OF REGIONAL COMPARISONS
OF PHI FROM APPENDIX D

	World	Africa	Cir-Med	Eurasia	Insular Pacific	North America	South America
External war-attacking with external war-being attacked:							
	.500	.447	.177	.354	.791	.804	.378
External war-attacking with military success (expansion):							
	.402	.293	.433	.410	.279	.180	.538
External war-attacking with high prestige for warrior:							
	.306	.630	.040	.280	-.191	.461	.422
Patrilineal descent with internal war:							
	.054	.094	.101	-.201	-.468	-.194	.514

By inspection of this table, and especially the Appendix, one can discern the wide variation in magnitude and positive or negative direction of the two-way association that phi measures. Because of the larger number of cases, the phi coefficients in the World can be low and still be significant at the .05 level. As a rough guide, World phis of .150-.155 are significant at this level, while within the regions, a phi must be about .400 to reach such statistical significance.

Following the procedure used by Driver and Schuessler (1967), to obtain a measure of overall similarity and dissimilarity in the occurrence of the phi values, I have compared the geographical regions 2 at a time and measured their joint

frequencies with Pearson's r. In Table 31, I have first presented Driver and Schuessler's results, and then my own. The latter overall are lower in similarity than Driver and Schuessler's, although the correlations between Africa and the Circum-Mediterranean and Africa and the Insular Pacific are virtually identical in their magnitude of joint frequency. The reader must be cautioned, however, that such measures of comparison do not measure specific variables but only the distribution of phi values without regard to the attributes they are measuring. Thus the utility of such intercorrelations of intercorrelations is severely limited. Its virtue here lies primarily, I think, because I can make a further comparison to Driver and Schuessler's results. It is possible that the warfare variables are pulling the degree of similarity between the regions down, although that is simply a guess.

1. Africa.

The analysis of Africa is based largely upon 19th century conditions, since this is the period of the first good ethnographic accounts. But sub-Saharan Africa had already experienced Europe, first through its technology. The flow of guns into the region, accelerating throughout the century until colonization, apparently radically changed older patterns of conflict, making existing ones more lethal and diffusing militancy throughout the continent. There is much

TABLE 31. COMPARISON OF DRIVER AND SCHUESSLER'S INTER-CORRELATIONS (PEARSON'S r) OF THE SIX GEOGRAPHICAL AREAS WITH THOSE OBTAINED FROM THE STANDARD SAMPLE

	2	1	3	4	6	5
2 Circum-Mediterranean		.24	.41	.21	.17	.16
1 Africa	.24		.57	.55	.40	.46
3 East Eurasia	.41	.57		.56	.48	.38
4 Insular Pacific	.21	.55	.56		.48	.46
6 South America	.17	.40	.48	.48		.48
5 North America	.16	.46	.38	.46	.48	
Totals	1.19	2.22	2.40	2.26	2.01	1.94

(Driver and Schuessler 1967:341)

	2	1	3	4	6	5
2 Circum-Mediterranean		.2297	.2975	.1143	.1738	.1713
1 Africa	.2297		.2548	.5556	.3024	.2856
3 East Eurasia	.2975	.2548		.2079	.2386	.2293
4 Insular Pacific	.1143	.5556	.2079		.1583	.2477
6 South America	.1738	.3024	.2386	.1583		.3330
5 North America	.1713	.2856	.1193	.2477	.3330	
Totals	.9866	1.6281	1.2281	1.2838	1.2061	1.2669

material in print within the past 5 years on the historiography of European armaments in Africa. This finely detailed work, which has also meant revision of earlier and, by comparison, simplistic descriptions of what life in 19th century Africa was about, makes my analysis here also simplistic.

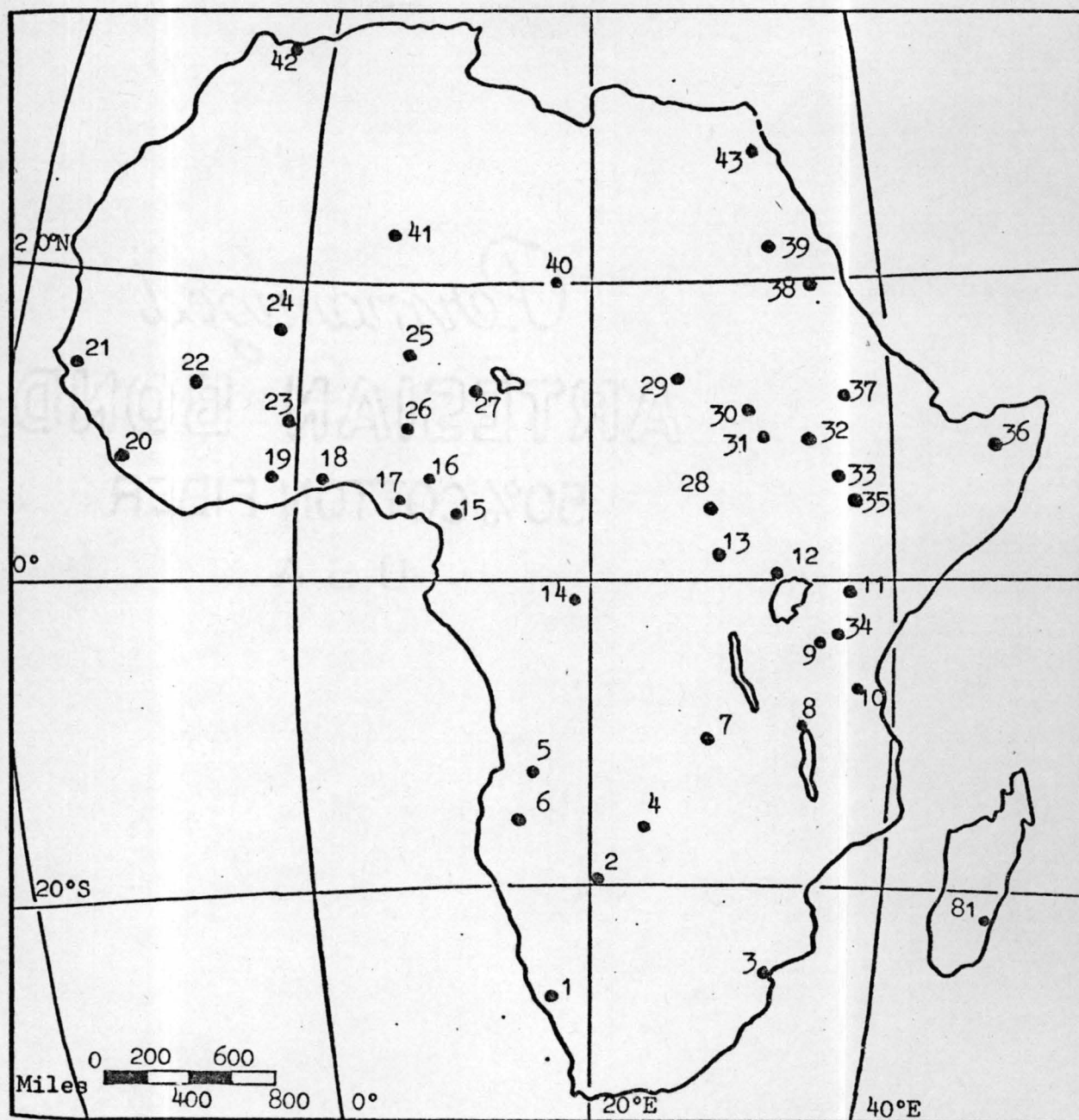
The statistical data summarize phenomena as they existed in a geographical region already exposed to the diffusion of particular elements of war. How these elements were integrated into aboriginal cultures was, of course, not necessarily diffused also. Guns in most cases were incorporated into an older tradition. The militant Sudanic states, whose warfare patterns are striking and famous, are classed here as belonging to the Circum-Mediterranean region. The 27 African societies with their Standard Sample identity numbers are:

1 Nama Hottentot	10 Luguru	19 Ashanti
2 Kung Bushmen	11 Kikuyu	20 Mende
3 Thonga	12 Ganda	22 Bambara
4 Lozi	13 Mbuti Pygmies	23 Tallensi
5 Mbundu	14 Nkundo Mongo	28 Azande
6 Kongo	15 Banen	30 Otoro Nuba
7 Ila	16 Tiv	31 Shilluk
8 Nyakyusa	17 Ibo	32 Ingassana
9 Hadza	18 Fon	34 Masai

Location of each society is indicated by identity number on Map 1.

Africa produces the largest number of significant internal correlations of the 6 regions. Continuing to use graphic presentation, I have diagrammed the relationships among the variables within Africa according to the strongest lambda values in Figures 3 and 4. Where the lambdas are

Map 1: Africa



(adapted from Murdock and White
1969: 342)

This map shows 27 societies classed as African, 16 as Circum-Mediterranean, and 1 (Madagascar) as Eurasian.

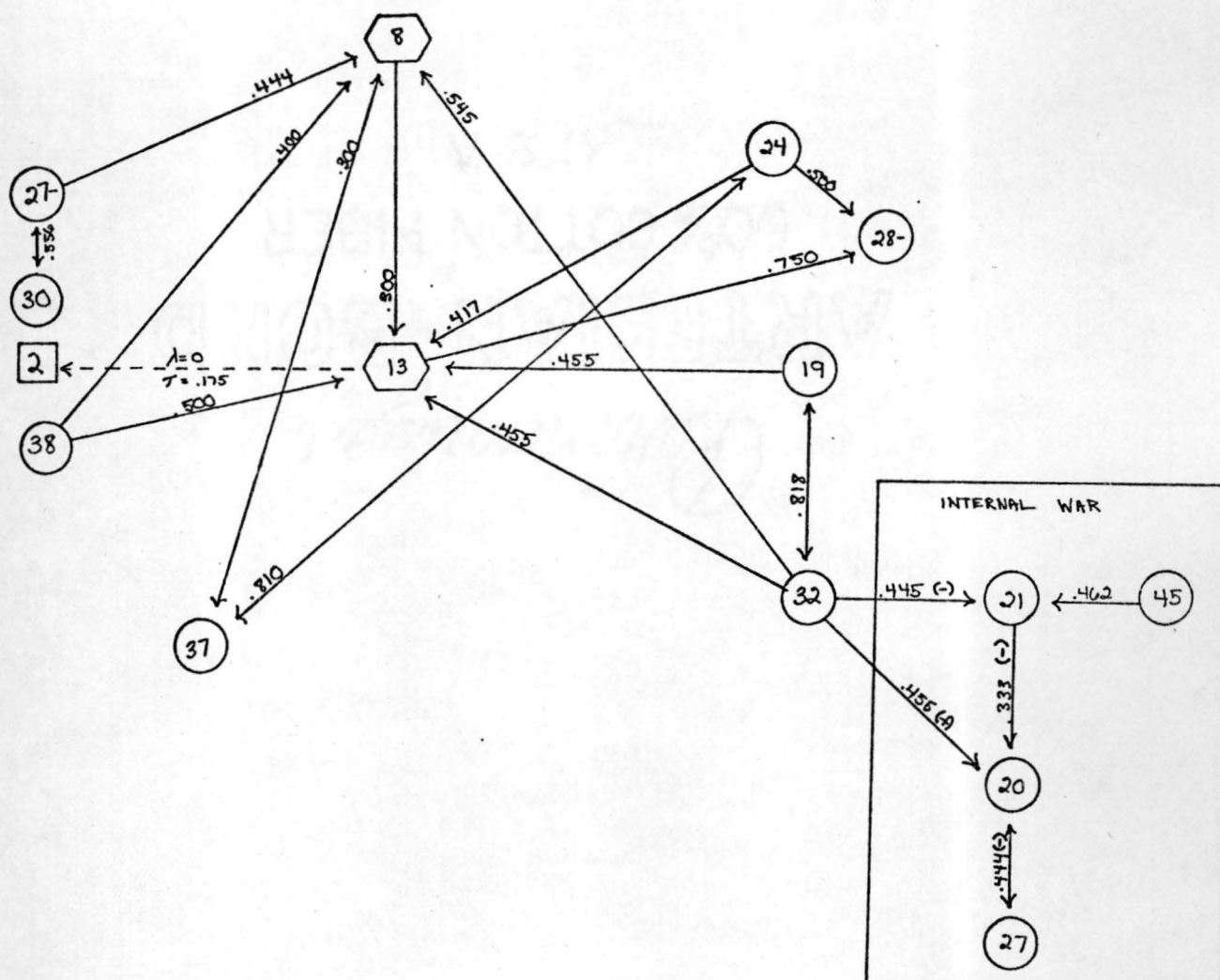


Fig. 3 Africa: direction and strength of lambdas, focused on 2-4 levels of polity (13).

two-way, I have included the strongest direction in the figure and both values in Table 33. In Figure 3, political centralization, indicated by the presence of from 2 to 4 levels of jurisdictional hierarchy beyond the local community (variable 13), is the attribute of primary focus, while in Figure 4 it is the presence of from 1 to 4 such levels (14). In Figure 3 there is no relationship between greater political centralization and warfare of any type, but 41.7 per cent of the time chiefdoms and states will have some sort of professional military organization (24) that is strongly predictive of a high value held for warfare (37), and 50 per cent of the time one can predict that these polities will be expanding territorially (38). The presence of authoritative military command (32) is better predicted by the presence of class stratification (19) or by complex settlement pattern (8) than by political organization. Conclusion of war through negotiation is absent (27), predictable from complex settlement, and peace ceremonies are absent (28), predictable from political organization. Notice that while the relationship between polygyny (2) and polity is significant, only tau gives a predictive value, and since it is in both directions I consider it to be tenuous. There is also a significant but not a predictive relationship between polity and the presence of high prestige for warriors (33) (Table 32). I have included the subsidiary configuration of internal war (21) in this Figure for readability. In the African case, where

local headmen are not hereditary (20), one-third of the time one can predict frequent internal war, and from that, that leaders will be informal (32) and the expectation will be revenge (45). Those nonhereditary headmen do not conclude their internal wars through negotiation (27), either.

Figure 4 is torturously elaborate, although I hope not snarled. Food-production (15), polygyny (2), 1-4 jurisdictional levels (14), frequent offensive war (22), elaborate rewards for warriors (35), and plunder (43) are all strong predictors of several other variables and in some cases of each other. Whereas external war-attacking (22) is not significant with "Africa," within Africa it is one of the strongest variables. High prestige for warriors (33) and high value for war (37) appear to be the dependent variables most often. There is a direct relationship between polity and frequent offensive war (Table 34) as in Figure 2, but in Africa the presence of polygyny is a better predictor of polity than is frequent war, and patrilineality (16) is the best predictor of polygyny, although food-production is a close second. Frequent offensive war is completely predictable from elaborate rewards for warriors (35), predictable half the time from patrilineality (16), half the time from plunder as a military expectation (43) as is defensive warfare (23), and predictable one-third of the time from patrilineality (3). In turn, both plunder (43) and a high value for war (37) are strong predictors of high prestige (33).

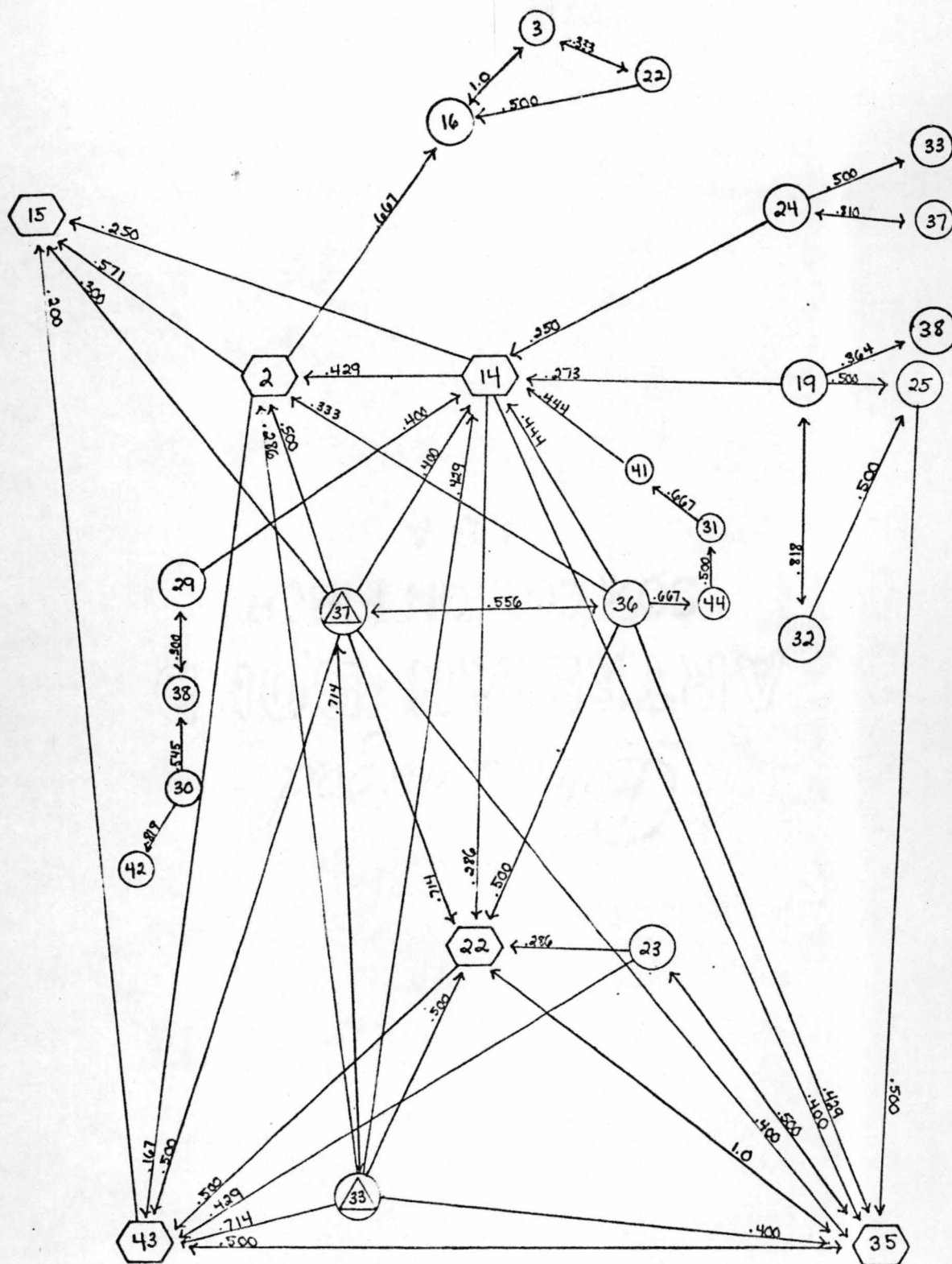


Fig. 4 Africa: direction and strength of lambdas, focused on 1-4 levels of polity (14).

Both rewards and plunder, and expectations of violence (36) and a high value for war are mutually predictable at least 50 per cent of the time, although trophies and honors (44) is the strongest predictor of the expectation that violence is a solution to problems (36). The strongest predictor of a high value for war remains the presence of professional military organization--age-sets, military societies, and standing armies (24). Not included in the diagrams are the findings that communities of less than 100 persons are predictably food-collectors 50 per cent of the time, and that where there is any type of warfare, communities of such size engage in it infrequently if at all. This is the only demographic attribute that has significant associations in Africa.

Warfare in Africa appears to be characteristically offensive, resulting in elaborate rewards for individual warriors who have fought usually for plunder. Subsequently, there are dependent variables of prestige, expectations of violence as a solution to problems, and war as highly valued. Most of the lambdas in Africa are two-way: there are consistent feedback relations among the variables (Table 33) and several are exactly mutually predictable. This complicates the configuration in Figure 4 and is evidence against any simple one-directional sequence of relationships. That is, there is circular causality at several points.

TABLE 32. SIGNIFICANT CORRELATIONS WITH O LAMBDA--AFRICA

Pairs of Variables	φ	p
Row attribute Polygyny (2) with:		
2-4 levels beyond the local community (13)	.418	.05
Military success (territorial expansion)(38)	.454	.02
Trophies and honors (44)	.399	.05
Row attribute 2-4 Levels Beyond the Local Community (13) with:		
High prestige for warriors (33)	.447	.05
Row attribute Frequent External War-Attacking (22) with:		
Trophies and honors (44)	.408	.05

TABLE 33. SIGNIFICANT CORRELATIONS WITH LAMBDAS
IN TWO DIRECTIONS--AFRICA

Pairs of Variables	ϕ	p	λ_r	λ_c
Row attribute Polygyny (2) with:				
Food-producers (15)	.705	.001	.571	.250
Patrilineality (16)	.792	.001	.667	.500
Row attribute Complex Settlements (8) with:				
2-4 levels beyond the community(13)	.434	.05	.300	.222
Class stratification (19)	.613	.01	.500	.545
Conclusion by negotiation (27)	-.472	.05	.375	.444
Authoritarian command (32)	.548	.02	.500	.545
Row attribute 2-4 Levels Beyond the Local Community (13) with:				
Class stratification (19)	.583	.01	.333	.455
Age-sets...standing armies (24)	.462	.01	.222	.417
Peace ceremony (28)	-.816	.01	.750	.667
Authoritarian command (32)	.462	.05	.333	.455
Military success (expansion)(38)	.597	.01	.444	.500
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
Frequent external war-attacking (22)	.476	.02	.286	.167
Prestige for warriors (33)	.553	.02	.200	.429
Rewards for warriors (35)	.564	.02	.400	.250
Expectations of violence (36)	.592	.01	.167	.400
Plunder (43)	.584	.01	.429	.200
Row attribute Class Stratification (19) with:				
Military success (expansion)(38)	.457	.02	.364	.300
Row attribute Hereditary Local Head- man (20) with:				
Internal war (21)	-.415	.05	.200	.333
Authoritarian command (32)	.462	.05	.333	.455
Row attribute Internal War (21) with:				
Authoritarian command (32)	-.462	.05	.333	.445
Revenge (45)	.463	.02	.417	.462

TABLE 33 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute External War-Attacking (22) with:				
External war--being attacked (23)	.447	.05	.167	.286
Prestige for warriors (33)	.630	.01	.400	.500
Expectations of violence (36)	.632	.01	.333	.500
High value for war (37)	.517	.02	.167	.444
Plunder (43)	.652	.01	.500	.400
Row attribute Frequent External War-Being Attacked (23) with:				
Plunder (43)	.588	.01	.429	.200
Row attribute Age-Sets...Armies (24) with:				
High prestige for warriors (33)	.539	.02	.500	.286
High value for war (37)	.840	.001	.818	.800
Row attribute Conclusion by Negotiation (27) with:				
High prestige for warriors (33)	-.620	.01	.556	.200
High value for war (37)	-.471	.05	.444	.167
Military success (expansion) (38)	-.472	.05	.444	.375
Land (42)	-.472	.05	.444	.375
Row attribute Military Expectations I (29) with:				
Expectations of violence (36)	.596	.01	.500	.556
Row attribute Military Expectations II (30) with:				
Military success (expansion) (38)	.592	.01	.545	.500
Land (42)	.848	.001	.819	.778
Row attribute High Casualties (31) with:				
Tribute (41)	.784	.01	.667	.500
Row attribute High Prestige for Warriors (33) with:				
Elaborate rewards for warriors (35)	.564	.02	.400	.250
Plunder (43)	.780	.001	.714	.600

TABLE 33 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Elaborate Rewards for Warriors (35) with:				
High value for war (37)	.555	.05	.250	.400
Row attribute Expectations of Violence (36) with:				
Trophies and honors (44)	.724	.01	.667	.500
Row attribute Military Success (Expansion) (38) with:				
Land (42)	.422	.05	.300	.222
Trophies and honors (44)	.422	.05	.300	.222

TABLE 34. AFRICA: INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR-ATTACKING

	Continual/Frequent External War	Infrequent External War
1-4 Levels	Nama Hottentot Thonga Lozi Mbundu Ila Ganda Tiv Fon Mende Ashanti Bambara Azande Otoro Nuba Shilluk Masai 15	Kongo Nyakyusa 2
0 Levels	Kikuyu Banen Ingassana 3	Kung Luguru Mbuti Pygmies Tallensi 4

$$Q = .818 \quad \phi = .476 \quad X^2 = 5.445 \quad p = .02 \quad C = .430 \quad \lambda_r = .167 \quad \lambda_c = .286$$

2. Circum-Mediterranean.

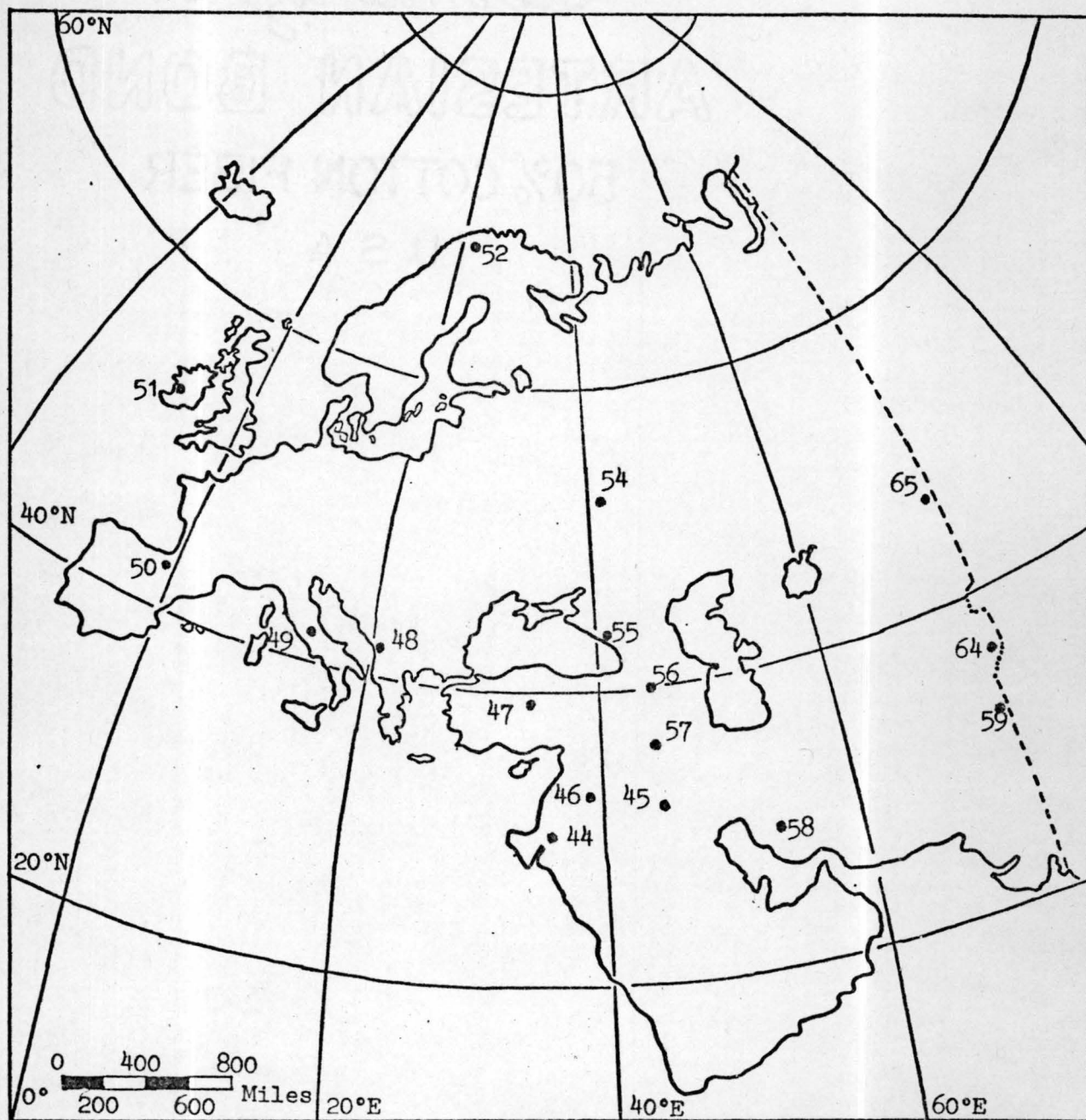
The configurations in this region appear to be less complex than those in Africa; there are fewer significant associations and generally the lambda values are lower. This is in contrast to the large number of variables in the world-wide intercorrelations that significantly associate with "Circum-Mediterranean." The representative societies in this region are spread more widely through time than those in Africa. Ancient Mediterranean states, Sudanic states, and modern states are included. The 29 societies in the region are:

21 Wolof	38 Bisharin	47 Turks
24 Songhai	39 Nubians	48 Gheg Albanians
25 Fulani	40 Teda	49 Romans
26 Hausa	41 Tuareg	50 Basques
27 Kanuri	42 Riffians	51 Irish
29 Fur	43 Egyptians	52 Lapps
33 Kafa	(1200 BC)	54 Russians
35 Konso	44 Hebrews	55 Abkhaz
36 Somali	45 Babylonians	56 Armenians
37 Amhara	46 Rwala Bedouins	57 Kurd

Location of each society is given by identity number on Maps 1 and 2. The Kafa do not turn up in any of the correlations with the warfare variables because of unavailability of data and the absence of an alternate society.

In Figure 5, 2-4 levels of jurisdictional hierarchy (13) is not the powerful predictor of other variables as in Africa. Instead, the attributes of violence as an expected solution to problems (36) and the presence of a high value for war (37) are predictors of the state, while the state is

Map 2. Circum-Mediterranean*



(adapted from Murdock and White 1969:343)

This map shows 13 societies classified as Circum-Mediterranean and 5 as East Eurasian.

*Murdock and White identify this map as "West Eurasia" and Map 3 as "East Eurasia," apparently because it is not practical to locate all and only those societies of a particular geographic region on 1 partial map.

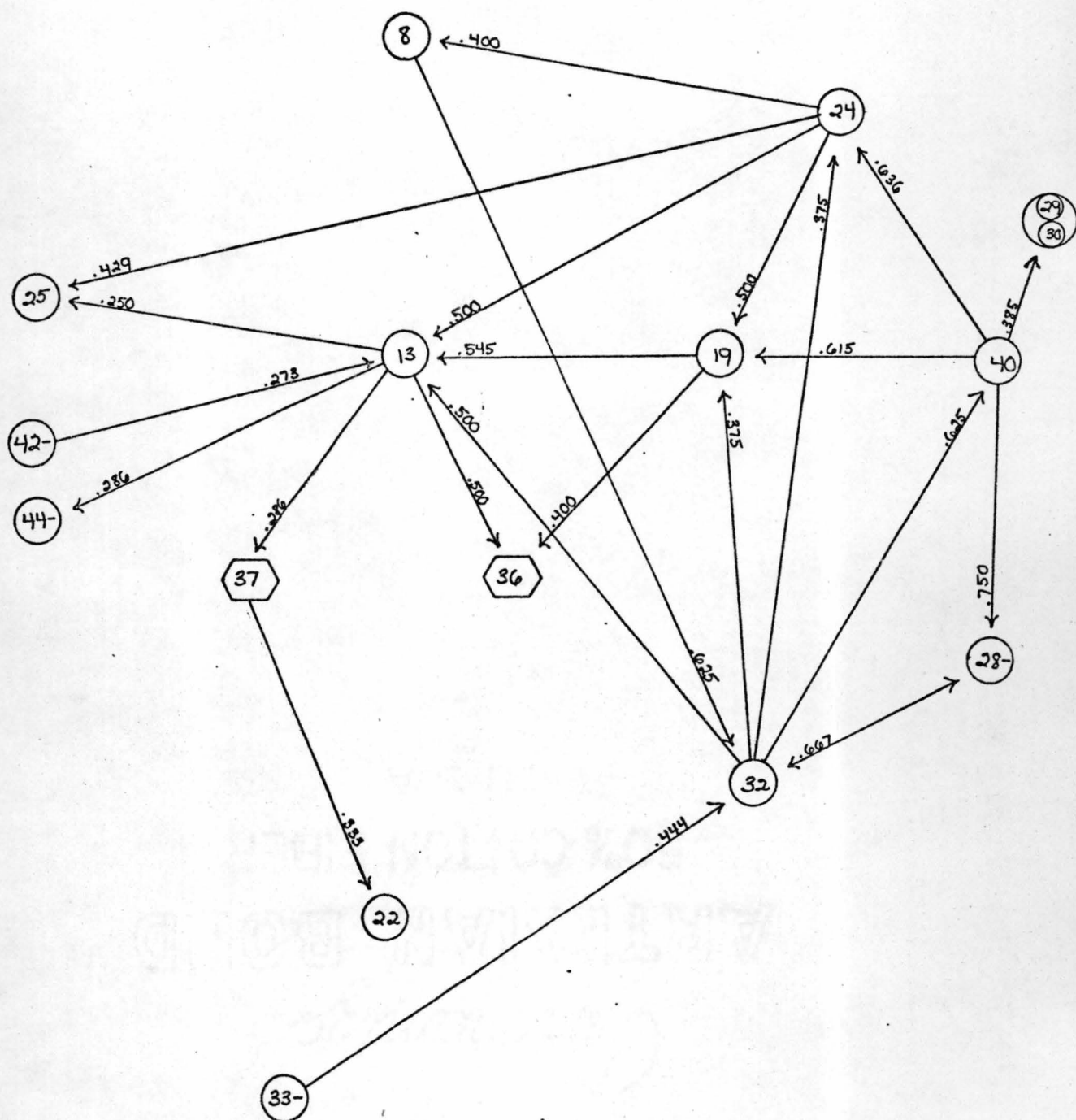


Fig. 5 Circum-Mediterranean: direction and strength of lambdas, focused on 2-4 levels of polity (13).

the predictor of the presence of class stratification (19) and professional military organization (24). Authoritative command (32) is best predicted by the presence of subjugation of people and territory as a military expectation (40), and that in turn is predicted by class stratification (19).

Where there is authoritative command, there is little or no prestige gained by individual warriors or soldiers. There is no relationship between the state and military success (38), nor is there a direct relationship between the state and frequent offensive war (22). There is, however, a significant but non-predictive relationship between frequent offensive war and military success ($\phi = .433, < .05$). The Circum-Mediterranean state does not go to war for tribute (41) or for trophies and honors (44).

In Figure 6, societies with 1 to 4 jurisdictional levels beyond the local community (14) have no significant relationships with settlement type, class stratification, type of military mobilization, command, and decision-making. This class of polities, however, is directly associated with and predictive of frequent offensive war (22), where the Sudanic states are having an effect (Table 35). Where in Figure 5 the presence of the state is predicted by expectations and values of violence (36 and 37), in Figure 6 the strongest predictive direction is reversed, so that expectations and values are predicted by type of polity. The lambdas here are in the same direction but stronger than

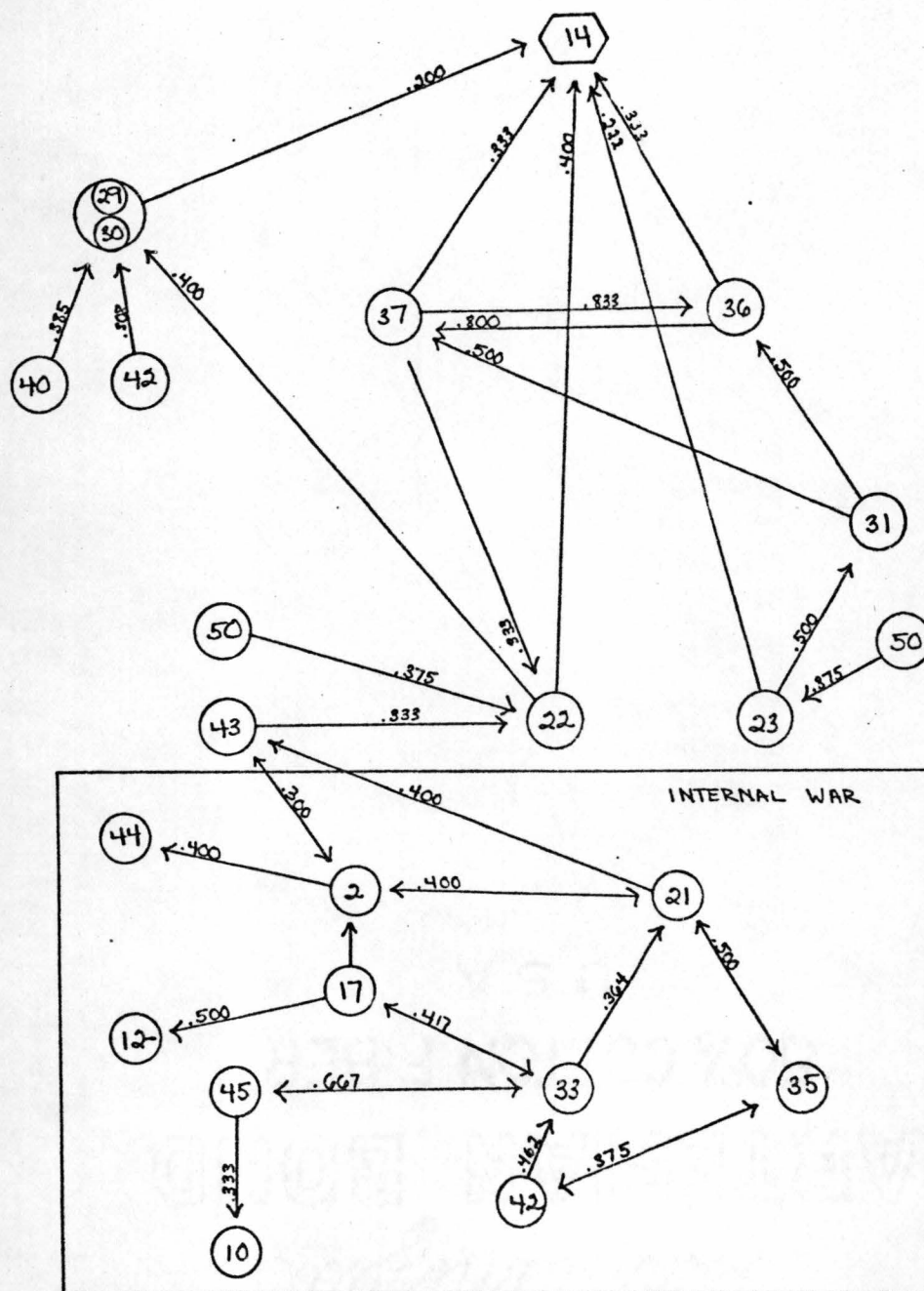


Fig. 6 Circum-Mediterranean: direction and strength of lambdas, focused on 1-4 levels of polity (14).

those in the world-wide configuration (Figure 2). In this region there is a very strong predictive relationship between expectations and values of violence, and both are predictors of high casualties (31). Frequent offensive war can be predicted equally well by either polity or the military expectations of subjugation, tribute, or land (29). In striking contrast to the African pattern, where internal war is associated with the absence of authoritative command and the presence of nonhereditary local headmen, in the Circum-Mediterranean region internal war (21) is best predicted by the presence of elaborate rewards for fighters (35), plunder (43), and polygyny (2). There is no relationship with residence, descent, or local political organization. Instead, personal gain and prestige seem to be the keys. Circular causality must be involved in these configurations also, although the variables with predictive values in both directions are not the same as those in Africa (Table 36). Actually, this is the case throughout the geographical regions: the attributes vary, as do the direction of the strongest prediction and the predictive values themselves.

TABLE 35. CIRCUM-MEDITERRANEAN: INTERCORRELATION OF LEVELS OF JURISDICTIONAL HIERARCHY BEYOND THE LOCAL COMMUNITY AND EXTERNAL WAR-ATTACKING*

	Continual/Frequent External War	Infrequent External War
1-4 Levels	Wolof Fulani Hausa Kanuri Fur Somali Amhara Bisharin Teda Tuareg Riffians Egyptians Hebrews Babylonians Rwala Bedouin Irish Russians Kurd 18	Songhai Turks Gheg Albanians 3
0 Levels	0	Nubians Lapps 2

$$Q = 1.00 \quad \phi = .586 \quad x^2 = 7.886 \quad p < .01 \quad C = \lambda_r = .400$$

* The Basques and Romans are not included in this table because the Ethnographic Atlas has not coded them for jurisdictional levels.

TABLE 36. SIGNIFICANT CORRELATIONS WITH LAMBDA S
IN TWO DIRECTIONS--CIRCUM-MEDITERRANEAN

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Complex Settlements (8) with:				
Authoritative command (32)	.675	.01	.625	.571
Row attribute 2-4 Levels Beyond the Local Community (13) with:				
Class stratification (19)	.610	.01	.375	.545
Age-sets...armies (24)	.564	.01	.286	.500
High expectations of violence (36)	.500	.01	.500	.333
High value for war (37)	.468	.05	.286	.167
Subjugation of people (40)	.590	.01	.143	.455
Row attribute Unilineality (17) with:				
Frequent internal war (21)	.458	.05	.417	.300
Subjugation of people (40)	-.564	.01	.500	.538
Row attribute Class Stratification (19) with:				
Age-sets...armies (24)	.578	.01	.444	.500
Subjugation of people (40)	.632	.01	.500	.615
Row attribute Frequent Internal War (21) with:				
High prestige for warriors (33)	.428	.05	.300	.364
Plunder (43)	.598	.01	.400	.250
Row attribute Frequent External War-Attacking (22) with:				
Military Expectations I (29)	.600	.01	.400	.250
Military Expectations II (30)	.600	.01	.400	.250
Row attribute Age-Sets...Armies (24) with:				
Authoritative command (32)	.471	.05	.286	.375
Subjugation of people (40)	.678	.001	.556	.636
Row attribute Peace Ceremony (28) with:				
Subjugation of people (40)	-.791	.02	.667	.750

TABLE 36 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Authoritative Command (32) with:				
High prestige for warriors (33)	-.492	.05	.375	.444
Subjugation of people (40)	.685	.01	.625	.571
Row attribute High Prestige (33) with:				
Land (42)	.484	.05	.375	.286
Row attribute Elaborate Rewards (35) with:				
Land (42)	.463	.02	.417	.462
Row attribute Expectations of Violence (36) with:				
High value for war (37)	.886	.001	.800	.833

3. East Eurasia

The 34 societies in this geographical region possess probably the greatest cultural and temporal diversity of the 6 areas. There are 19th and 20th century tribal societies and both ancient and 20th century states of various sizes.

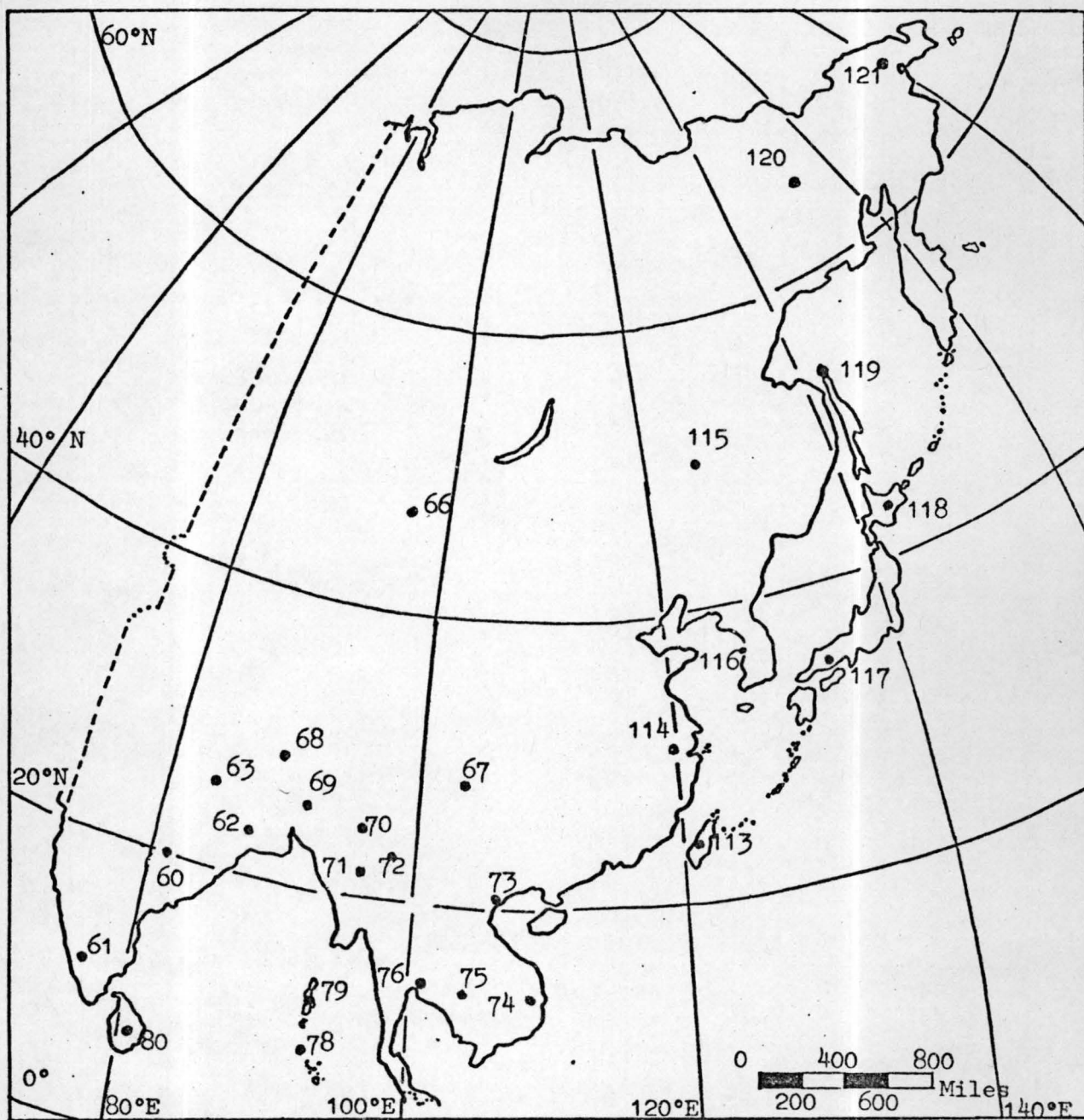
The representatives, with their identity numbers, are:

53 Yurak Samoyed	69 Garo	81 Tanala
58 Basseri	70 Sema Naga	82 Negri Sembilan
59 Punjabi	71 Burmese	114 Chinese
60 Gond	62 Palaung	115 Manchu
61 Toda	73 Vietnamese	116 Koreans
62 Santal	74 Rhade	117 Japanese
63 Uttar Pradesh	75 Khmer	117 Ainu
64 Burusho	76 Siamese	119 Gilyak
65 Kazak	77 Semai	120 Yukaghir
66 Khalka Mongols	78 Nicobarese	121 Chukchee
67 Lolo	79 Andamanese	
68 Lepcha	80 Vedda	

Several of these societies at the target dates were experiencing nationalist or imperialist warfare, in addition to the usual displacement warfare of the modern colonial period. Information was not considered from warfare directly involving European groups, except in the case of the Chukchee, who expanded successfully against the Russians, as well as the Yukaghir, and are one of the few tribal societies to succeed against a state. The societies are located by identity numbers on Maps 2, 3, and 4.

The configurations of Eurasian warfare are remarkably different from those of the rest of the world because there is no direct relationship between any form of political organization beyond the local community and any type of warfare. In Figure 7, from the presence of offensive war (22) or high prestige for warriors (33), one can predict expectations and values of violence (36 and 37). Where there is military success (38) or the expectations of land and plunder (50), one can make a moderately strong prediction that frequent offensive war will exist (22). Where violence is held as an expected solution to problems (36), not quite one-third of the time one can predict that political organization will be a chiefdom or state (13). Such political centralization in turn is a good predictor of the presence of class stratification (19) and a moderate predictor of complex settlements (8) and the initiation of war through agreement or announcement (26). The presence of some form of professional

Map 3: East Eurasia



(adapted from Murdock and White 1969: 344)

This map shows 25 societies classed as East Eurasian and 1 as Insular Pacific.

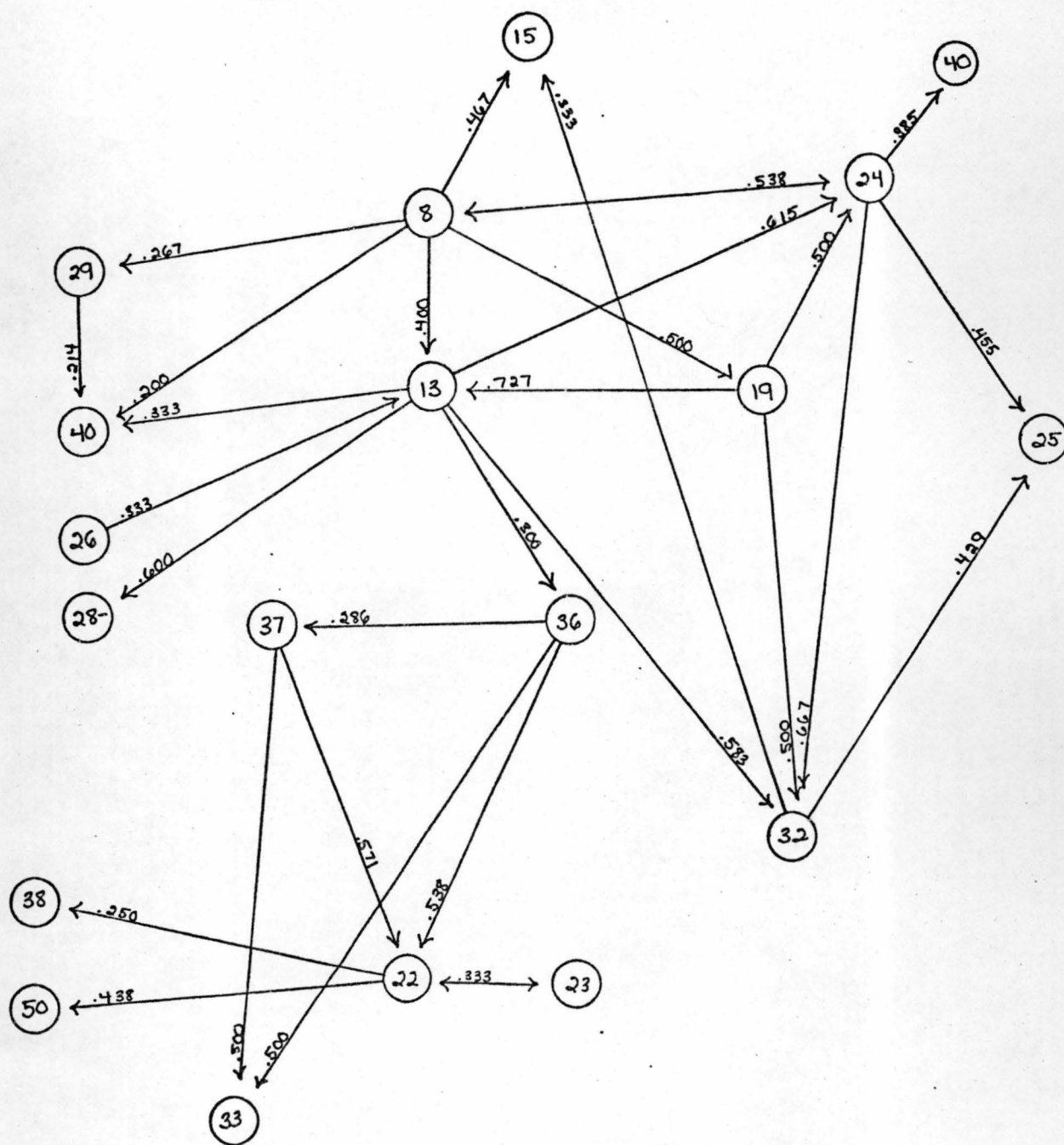


Fig. 7 East Eurasia: direction and strength of lambdas, focused on 2-4 levels of polity (13).

military organization (24), the absence of peace ceremonies (28), and the presence of authoritative command (32) are the best predictors of the presence of the state. Professional military organization (24) or food-production (15) is a better predictor of the presence of complex settlements (8) than the state (13). Authoritative command (32) is the best predictor of professional military organization, and follows it as the second-best predictor of the presence of the state.

There is little difference in pattern between Figures 7 and 8. When the political attribute is 1 to 4 jurisdictional levels beyond the local community (14), the only new direct relationship is with food-production (15): where food-production is the dominant subsistence mode, 45 per cent of the time there will be 1 to 4 levels of polity outside the local community. Other differences between the 2 Figures are in predictive direction and strength.

In general, the Eurasian warfare configurations appear not to include the androcentrism, individualism, or materialism of the regions considered so far. Moreover, the predictability between expectations of violence and high value for war is very low compared to the other regions. The patterning of a low value for violence and war isolated when geography was intercorrelated as a variable holds up within the region in spite of all those hordes out of Asia. Internal warfare has a tiny little configuration of its own and does not tie into the larger patterns at all. I would be

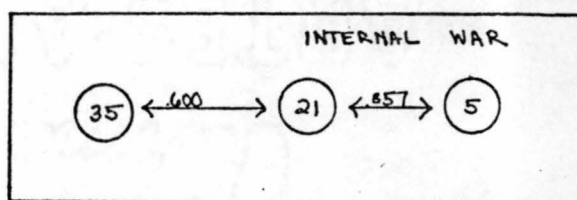
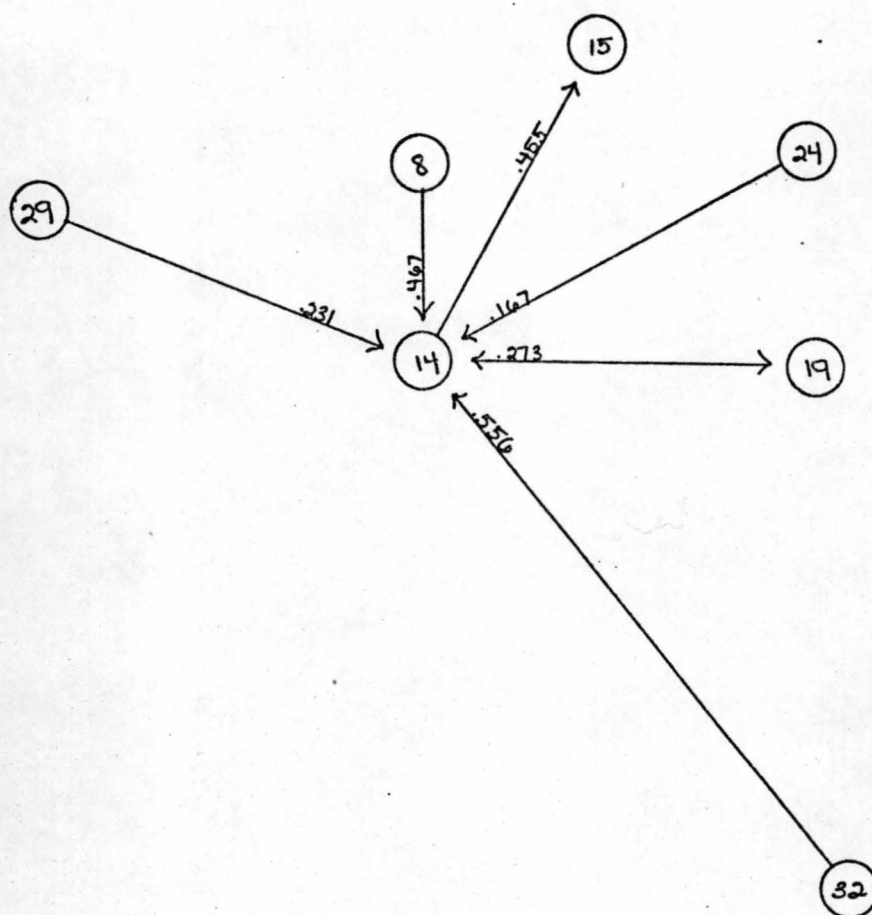


Fig. 8 East Eurasia: direction and strength of lambdas, focused on 1-4 levels of polity (14).

especially interested to see if the Eurasian pattern traced here holds up on a larger sample. Table 37 gives the lambda values that occur in both directions.

TABLE 37. SIGNIFICANT CORRELATIONS WITH LAMBDA
IN TWO DIRECTIONS--EAST EURASIA

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Complex Settlements (8) with:				
2-4 levels beyond local community (13)	.487	.01	.400	.308
1-4 levels beyond local community (14)	.516	.01	.467	.273
Class stratification (19)	.525	.01	.500	.364
Military expectations I (29)	.340	.05	.267	.214
Row attribute 2-4 Levels Beyond the Local Community (13) with:				
Class stratification (19)	.786	.001	.625	.727
Age-sets...armies (24)	.651	.001	.615	.583
Peace ceremony (28)	-.683	.02	.600	.333
Authoritative command (32)	.614	.01	.583	.444
Expectations of violence (36)	.422	.05	.300	.222
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
Food-producers (15)	.577	.001	.455	.143
Class stratification (19)	.497	.01	.273	.273
Military expectations I (29)	.351	.05	.091	.231
Authoritative command (32)	.646	.01	.429	.556
Row attribute Class Stratification (19) with:				
Age-sets...armies (24)	.578	.01	.500	.444
Authoritative command (32)	.552	.02	.500	.375
Row attribute Frequent External War-Attacking (22) with:				
Expectations of violence (36)	.566	.01	.538	.333
High value for war (37)	.632	.001	.571	.250
Land and plunder (50)	.459	.01	.438	.308

TABLE 37 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Age-sets...Armies (24) with:				
Authoritative command (32)	.721	.001	.667	.556
Subjugation of people (40)	.455	.02	.385	.111
Row attribute Official Decision (25) with:				
Authoritative command (32)	.574	.01	.200	.429
Row attribute High Prestige (33) with:				
High value for war (37)	.659	.001	.400	.500
Row attribute Expectations of Violence (36) with:				
High value for war (37)	.458	.05	.286	.167

4. Insular Pacific.

Most of the 31 societies in this region have as target dates the best descriptions made prior to severe culture change due to European contact in the 19th and 20th centuries. The temporal and cultural variety is not so dramatic as that within the Eurasian region. The societies are:

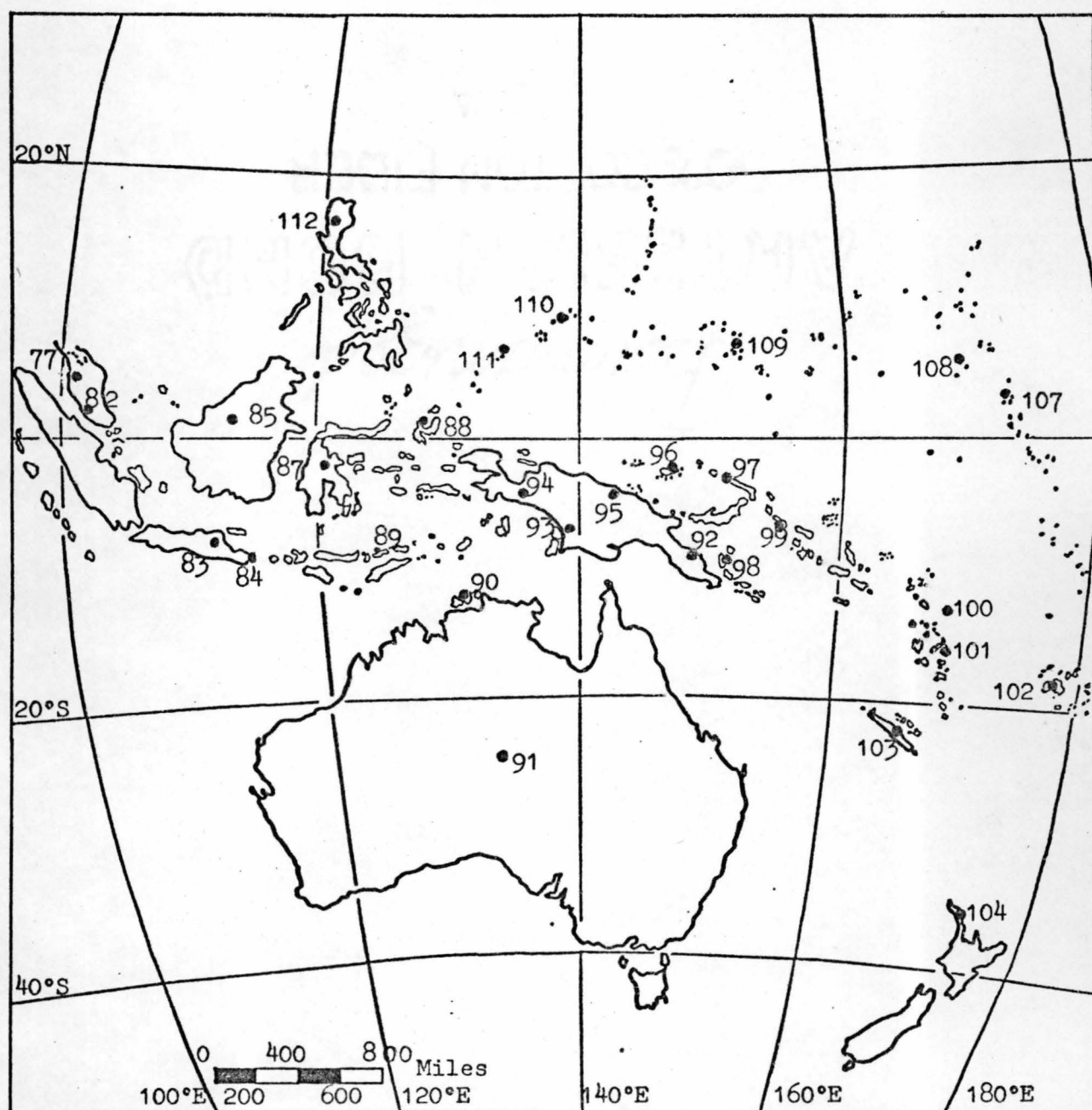
83 Javanese	94 Kapauku	104 Maori
84 Balinese	95 Kwoma	105 Marquesans
85 Iban	96 Manus	106 Samoans
86 Badjau	97 New Ireland	107 Gilbertese
87 Toradja	98 Trobrianders	108 Marshallese
88 Tobelorese	99 Siuai	109 Trukese
89 Alorese	100 Tikopia	110 Yapese
90 Tiwi	101 Pentecost	111 Palauans
91 Aranda	102 Mbau Fijians	112 Ifugao
92 Orokaiva	103 Ajie	113 Atayal
93 Kimam		

They are located by their identity numbers on Maps 3 and 4.

In this region political organization has relatively little to do with warfare. In Figure 9, one can predict the presence of the state in some form (13) from the presence of subjugation of peoples (40) as a military expectation 50 per cent of the time. Subjugation is the independent variable most often in this configuration. High casualties (31) can be predicted equally well by either the presence of the state or by military success (territorial expansion) (38), and military success in turn is predicted by the specific military expectation of land (42). The presence of the state is a secondary predictor of authoritarian command (32). Note that in this configuration violence as an expected solution to problems and a high value for war (36 and 37) are significantly absent, the former predicted by the presence of conclusion of war by negotiation (28) and the latter by some form of professional military organization (24).

In Figure 10, there is no longer any predictable association between attitudes toward war and attributes of military and political organization. What remains is an

Map 4: Insular Pacific



(adapted from Murdock and White 1969: 345)

This map shows 2 societies classed as East Eurasian and 28 as Insular Pacific.

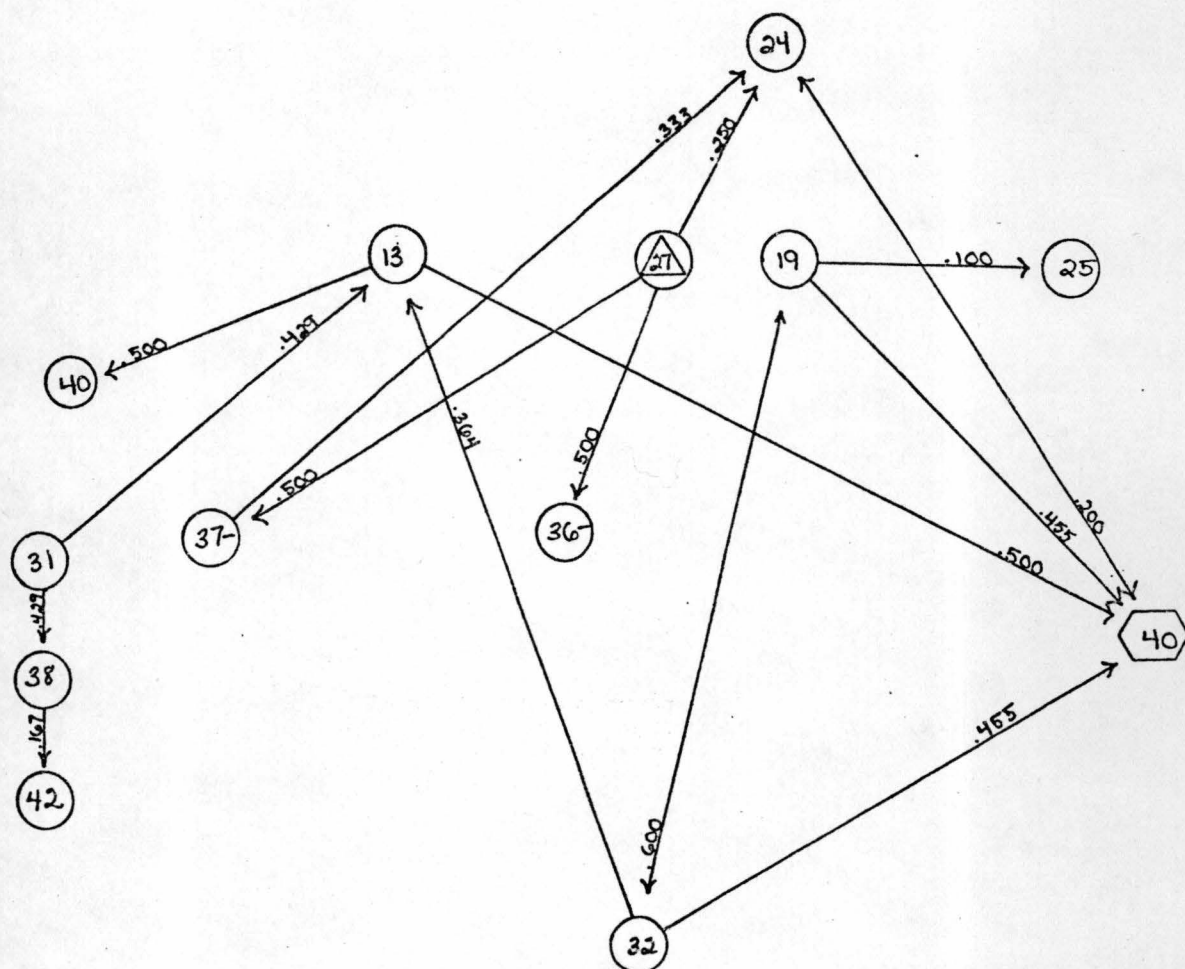


Fig. 9 Insular Pacific: direction and strength of lambdas, focused on 2-4 levels of polity (13).

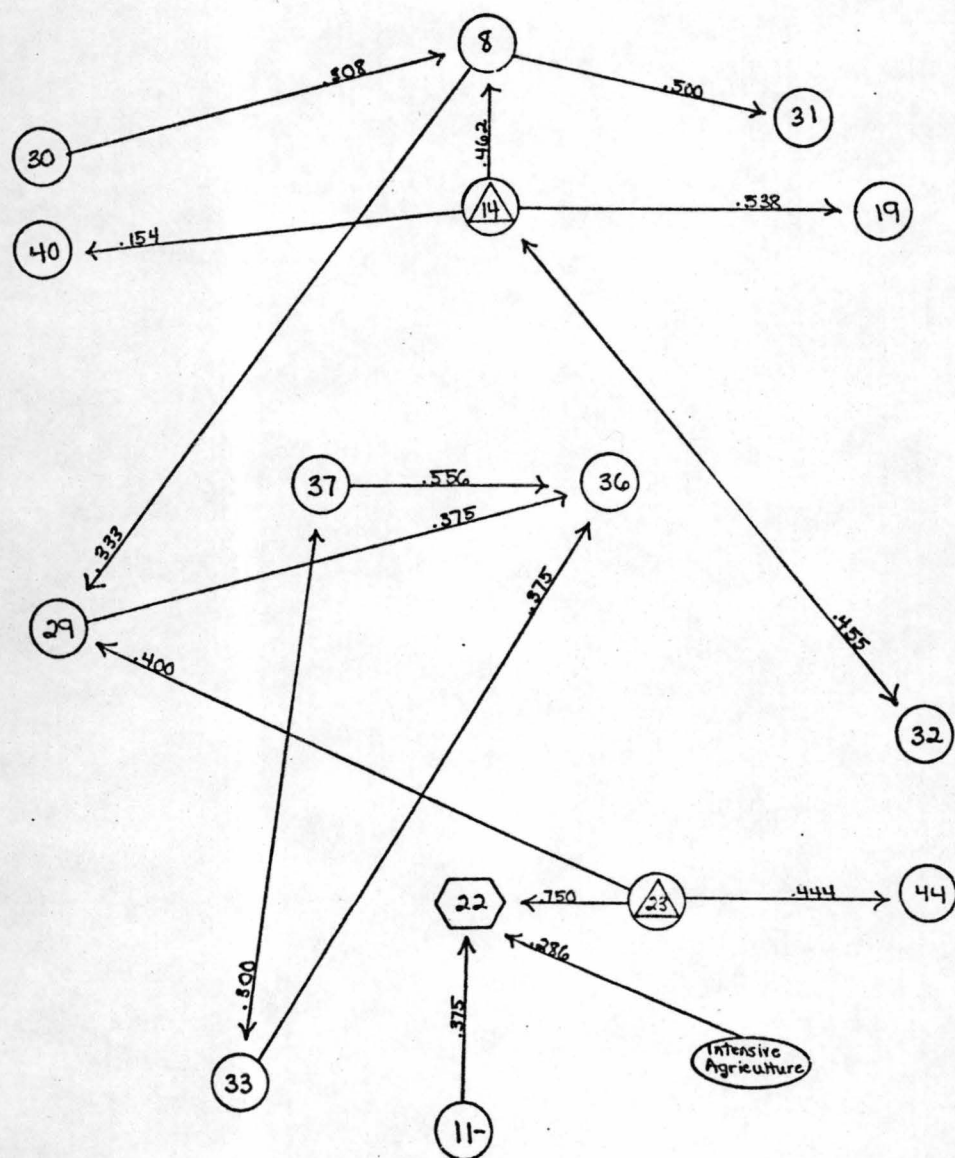


Fig. 10 Insular Pacific: direction and strength of lambdas, focused on 1-4 levels of polity (14).

association with high prestige for individual warriors (33). Offensive war (22) is the independent variable most often in predictions about its associations, yet there are only 3 attributes that it predicts. Where there is frequent offensive war, community size will be something other than 100-400 persons (11), the subsistence mode will be intensive agriculture, and there will also be frequent attacks by other cultural units (23). Table 38 is the intercorrelation of external war-attacking and intensive agriculture. There are only 2 cases in the common presence cell, the Javanese and the Ajie. They are, however, the only intensive agriculturists in the intercorrelation. Expanding the subsistence variable to include all agriculturalists, but not pastoralists, does not produce a significant correlation.

As a whole, the configurations of variables in the Insular Pacific are patchy. For instance, in Figure 10 the only warfare intercorrelations significant at the .01 level or higher are between complex settlements (8) and high casualties (31), 1 to 4 levels of jurisdiction (14) and authoritative military command (32), high prestige for warriors (33) and expectations of violence as a solution to problems (36), and offensive war and defensive war (22 and 23). Militarism is relatively weakly developed in this region, and practices and institutions of reconciliation are present. The opposite is the case in the Circum-Mediterranean region. Table 39 shows those relationships that generate lambdas in both directions.

TABLE 38. INSULAR PACIFIC: INTERCORRELATION OF INTENSIVE AGRICULTURE AND EXTERNAL WAR-ATTACKING ⁴⁰⁶

	Continual/Frequent External War	Infrequent External War
Intensive Agriculture Present	Javanese Ajie 2	0
Intensive Agriculture Absent	Iban Orokaiva Pentecost Marquesans Trukese 5	Badjau Alorese Tiwi Aranda Kapauku Kwoma Manus Trobrianders Tikopia Maori Marshallese Yapese Palauans 13

$$Q = 1.00 \quad \phi = .454 \quad X^2 = 4.127 \quad p < .05 \quad C = .414 \quad \lambda_c = .286$$

TABLE 39. SIGNIFICANT CORRELATIONS WITH LAMBDA S
IN TWO DIRECTIONS--INSULAR PACIFIC

Pairs of Variables	ϕ	p	λ_r	λ_c
Row attribute Complex Settlements (8) with:				
Military expectations III (30)	.439	.02	.250	.308
High casualties (31)	.651	.01	.500	.429
Row attribute Mean Community Size 100-400 Persons (11) with:				
Frequent External War-Attacking (22)	-.471	.05	.375	.286
Row attribute 2-4 Levels Beyond the Local Community (13) with:				
High casualties (31)	.535	.02	.200	.429
Subjugation of people (40)	.668	.001	.500	.400
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
Class stratification (19)	.665	.001	.538	.455
Row attribute Frequent External War-Attacking (22) with:				
Frequent external war--being attacked (23)	.791	.001	.667	.750
Row attribute Frequent External War -Being Attacked (23) with:				
Military expectations I (29)	.440	.05	.400	.333
Row attribute Conclusion by Negoti- ation (27) with:				
Expectations of violence (36)	-.567	.02	.500	.333
High value for war (37)	-.523	.02	.500	.333
Row attribute Military Expectations I (29) with:				
Expectations of violence (36)	.471	.05	.375	.286

TABLE 39 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute High Prestige (33) with:				
Expectations of violence (36)	.599	.01	.375	.286
Row attribute Expectations of Violence (36) with:				
High value for war (37)	.587	.02	.429	.556

5. North America.

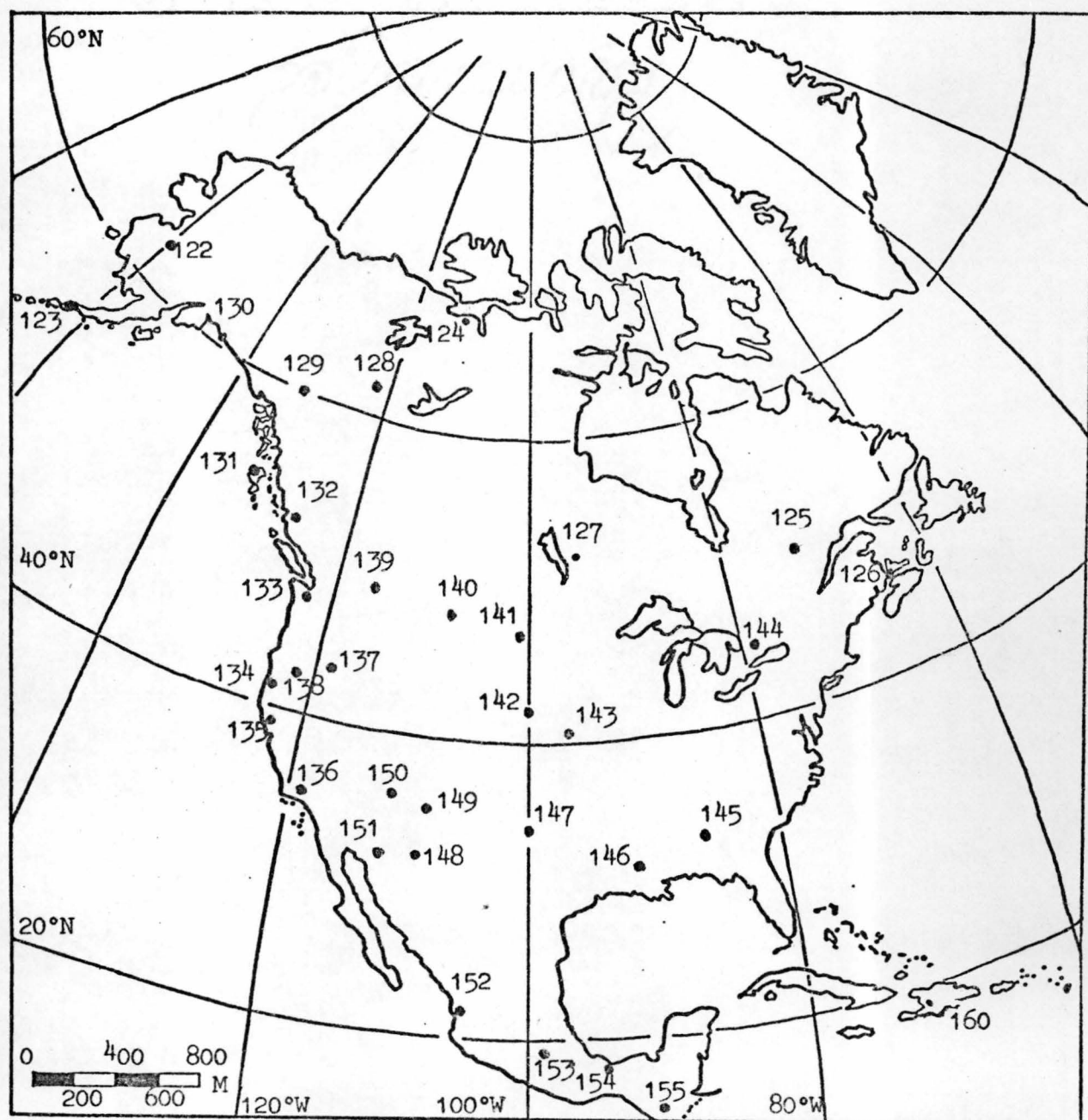
The 33 societies in North America are all American Indians prior to European contact or absorption. They are:

122 Ingalik	133 Twana	144 Huron
123 Aleut	134 Yurok	145 Creek
124 Copper Eskimo	135 E. Pomo	146 Natchez
125 Montagnais	136 Yokuts	147 Comanche
126 Micmac	137 Paiute	148 Chiricahua Apache
127 No. Saulteaux	138 Klamath	149 Zuni
128 Slave	139 Kutenai	150 Havasupai
129 Kaska	140 Gros Ventre	151 Papago
130 Eyak	141 Hidatsa	152 Huichol
131 Haida	142 Pawnee	153 Aztec
132 Bellacoola	143 Omaha	154 Popoluca

One could only wish that the superior quality of North American ethnographies were consistently matched by material from the rest of the world. Location of societies is given by identity number on Maps 5 and 6.

The configurations of attributes in North America are more complex than those in East Eurasia and the Insular Pacific, but not so complex as those in Africa and South

Map 5: North America



(adapted from Murdock and White 1969: 346)

This map shows all 33 societies classed as North American and 2 as South American.

America. There are 10 North American cases that have 1 to 4 levels of jurisdictional hierarchy beyond the local community, and of these only 2--the Creek and the Aztec--have 2 to 4 levels. One distinctive characteristic of this region is that warfare is predominantly defensive, and the attribute of frequent external war-attacking is usually predicted by other attributes rather than being the predictor, as is the case in the world-wide, African, South American, and even Insular Pacific configurations. In Figure 11, some form of centralized polity (13), i.e., the Creek and the Aztec, predicts the presence of complex settlements (8), authoritarian military command (32), and high prestige for warriors (33), while the presence of the state is predicted by the specific military expectation of tribute (41) (the Aztec at work). The state--or proto-state--predicts the presence of military command, military command predicts the presence of hereditary local headmen (20), and hereditary local headmen predict frequent offensive war (22), although the last is most strongly predicted by frequent defensive war (23), and secondarily predicted by elaborate rewards for warriors and high prestige for warriors (35 and 33). The state configuration and the androcentric configuration appear to be blending into each other. Internal war (21) is significantly associated with exogamous community (5) ($\varphi = .545, < .01$) and is predicted by such community organization 33 per cent of the time. Internal war is tied into the larger North American configuration

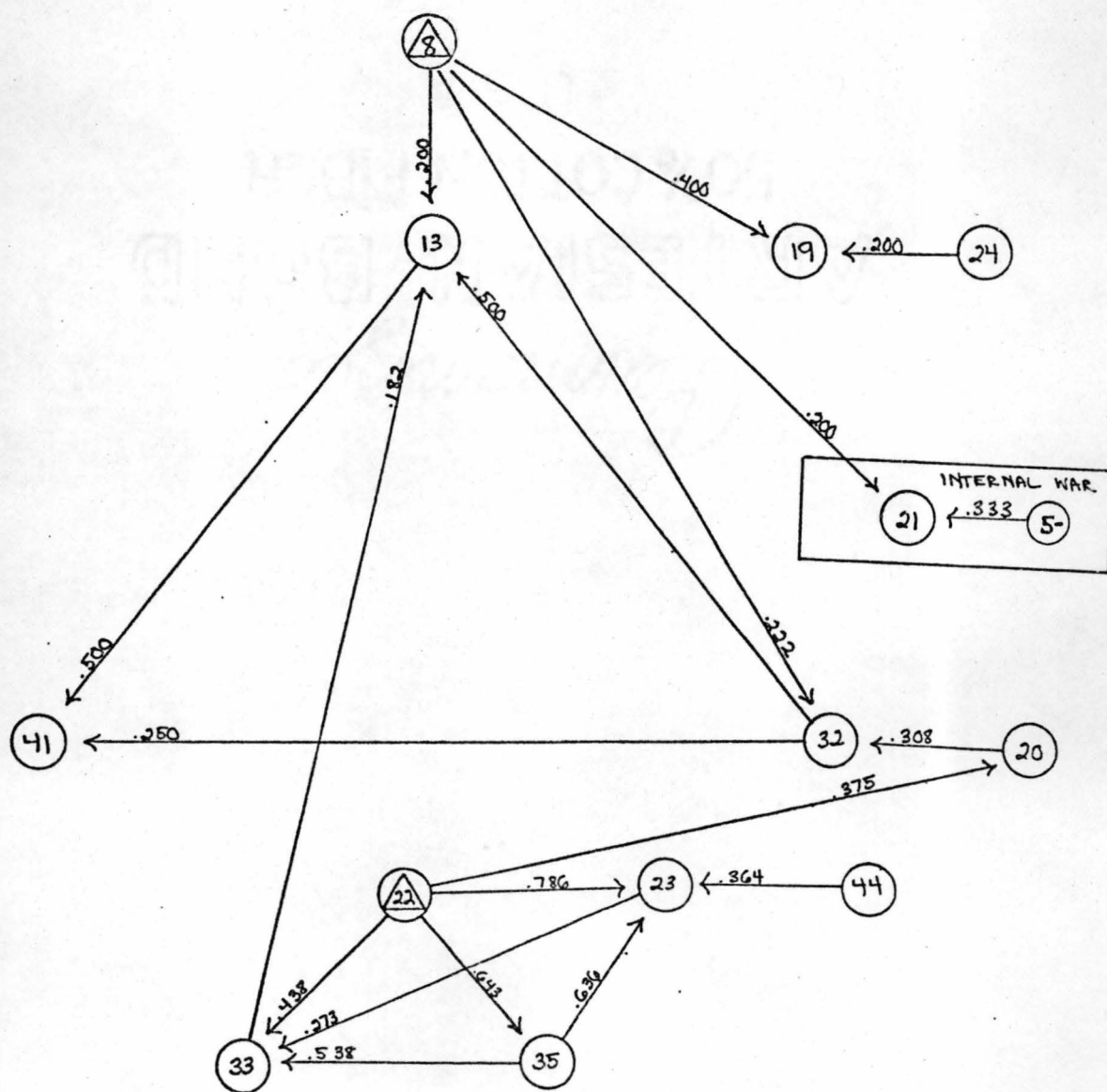


Fig. 11 North America: direction and strength of lambdas, focused on 2-4 levels of polity (13).

only through complex settlements (8): where there is frequent internal war, complex settlements will exist 20 per cent of the time. The last point seems nebulous, but recall that in the world-wide intercorrelations, internal war is significantly absent in North America in the first place.

In Figure 12, the presence of 1 to 4 levels of jurisdiction beyond the local community (14) is a predictor of frequent offensive war (22) (Table 40), high prestige for warriors (33), a high value for war (37), and frequent defensive war (23), while it is in turn predicted by authoritarian military command (32). However, the presence of the military expectations of subjugation, tribute, land, or trophies and honors (29) predicts frequent offensive war, high prestige for warriors, and a high value for war equally well. This class of military expectations predicts the presence of the specific military expectation of trophies and honors (44) 75 per cent of the time. That is, the class of expectations has significant associations at all only because of the inclusion of trophies and honors in the class; without this famous North American trait, the class does not relate significantly to anything. Polity (14) predicts the presence of high prestige (33) 36 per cent of the time, and high prestige predicts positive expectations of violence (36) 50 per cent of the time. Along another path, defensive war (23) predicts a high value for war nearly 54 per cent of the time, and high valuation (37) predicts expectations of violence (36) half the

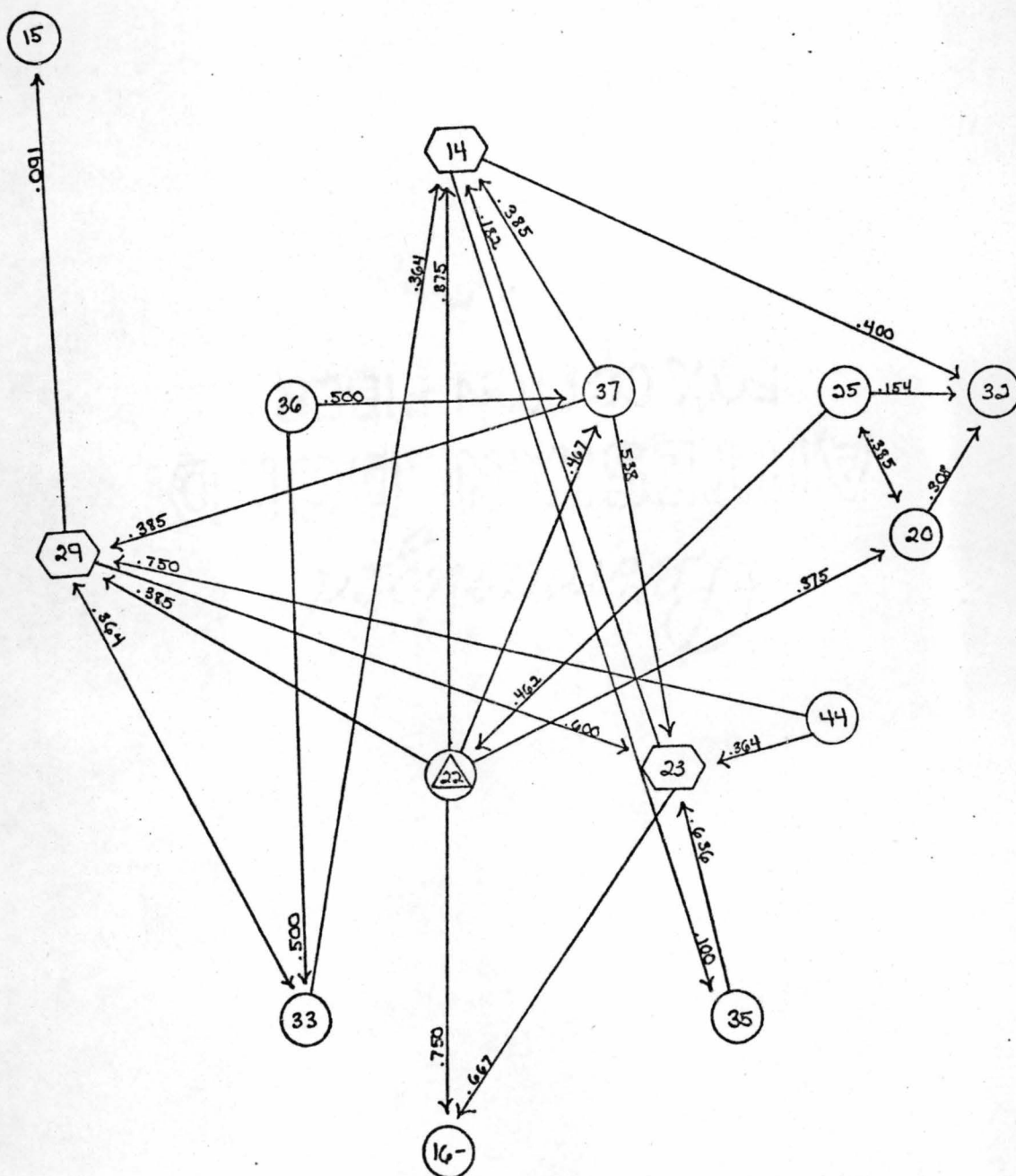


Fig. 12 North America: direction and strength of lambdas, focused on 1-4 levels of polity (14).

time and frequent offensive war 46 per cent of the time. Defensive war also predicts frequent offensive war (22), trophies and honors (44), and rewards for warriors (35), the core of the defensive pattern. There are 2 unusual intercorrelations that I have included here, even though they are based upon only 11 and 9 cases, respectively. Matrilineal descent (16) has a significant association with both frequent offensive war and frequent defensive war. I have included the tables in the text.

Descent	External War-attacking	
	Frequent	Infrequent
Patrilineal	0	3
Matrilineal	7	1 11
Q = -1.00 $\varphi = -.810$ $X^2 = 7.219$		
p < .01 $\lambda_c = .750$ $\lambda_r = .667$		

Descent	External War--Being Attacked	
	Frequent	Infrequent
Patrilineal	0	2
Matrilineal	6	1
Q = -1.00 $\varphi = -.756$ $X^2 = 5.143$		
p < .05 $\lambda_c = .667$ $\lambda_r = .500$		

On the one hand these results may be spurious, since the Ns are so small. On the other hand, matrilineal descent is a rare condition and one cannot help but wonder why nearly all matrilineal societies in North America are engaged in frequent external war, even though matrilineal descent correlates significantly with "North America." Furthermore, why is descent the independent variable and frequency of warfare the dependent variable in the directions of strongest prediction according to lambda?

Table 41 gives the lambda values where they occur in both directions between variables. By inspection, it appears that directional differences are less than, say, those between variables in the Insular Pacific. Several of them are within fractions of being perfectly mutually predictable, e.g., frequent external war-attacking with official decision-making to go to war. Such mutuality is, I think, evidence that patterns of circular causality can be very tightly interwoven and may be in the long run like a Gordian knot. From Table 40 one can discern finer geographical patterning within the continent. In cell a, the Pawnee, Huron, Creek, Natchez, and Aztec share a broad ecological and historical background of Eastern (or Southeastern) Woodlands, incipient or intensive agriculture, and paths of cultural diffusion. In cell c there are 4 societies from the southern Plains and the Southwest, although their subsistence bases are different. In cell d, 5 of the 14 cases are either Northern

Athapaskan or Algonkian food-collectors, 3 are from California, 1 the Great Basin, and 2 (the Havasupai and Huichol) are in refuge areas.

TABLE 41. SIGNIFICANT CORRELATIONS WITH LAMBDA S
IN TWO DIRECTIONS--NORTH AMERICA

Pairs of Variables	ϕ	p	λ_r	λ_c
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
High prestige for warriors (33)	.513	.01	.300	.364
Elaborate rewards (35)	.476	.01	.100	.083
High value for war (37)	.465	.01	.111	.385
Row attribute Patrilineal Descent (16) with:				
Frequent external war-attacking (22)	-.810	.01	.667	.750
Frequent external war--being attacked (23)	-.756	.05	.500	.667
Row attribute Hereditary Headman (20) with:				
Frequent external war-attacking (22)	.378	.05	.286	.375
Official decision (25)	.436	.05	.385	.385
Row attribute Frequent External War- Attacking (22) with:				
Frequent external war--being attacked (23)	.804	.001	.786	.727
Official decision (25)	.496	.01	.417	.462
Military expectations I (29)	.439	.02	.385	.273
High prestige (33)	.461	.01	.438	.182
Elaborate rewards (35)	.665	.001	.643	.615
High value for war (37)	.471	.01	.467	.385

TABLE 41 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Frequent External War--Being Attacked (23) with:				
Military expectations I (29)	.665	.001	.500	.600
High prestige (33)	.554	.01	.273	.111
Elaborate rewards (35)	.686	.001	.556	.636
High value for war (37)	.586	.01	.400	.538
Trophies and honors (44)	.428	.05	.125	.364
Row attribute Military Expectations I (29) with:				
High value for war (37)	.432	.05	.200	.385
Trophies and honors (44)	.786	.001	.727	.750
Row attribute High Prestige (33) with:				
Elaborate rewards (35)	.665	.001	.455	.538
Expectations of violence (36)	.573	.01	.400	.500
High value for war (37)	.532	.01	.300	.462
Row attribute Expectations of Violence (36) with:				
High value for war (37)	.529	.01	.500	.455

6. South America.

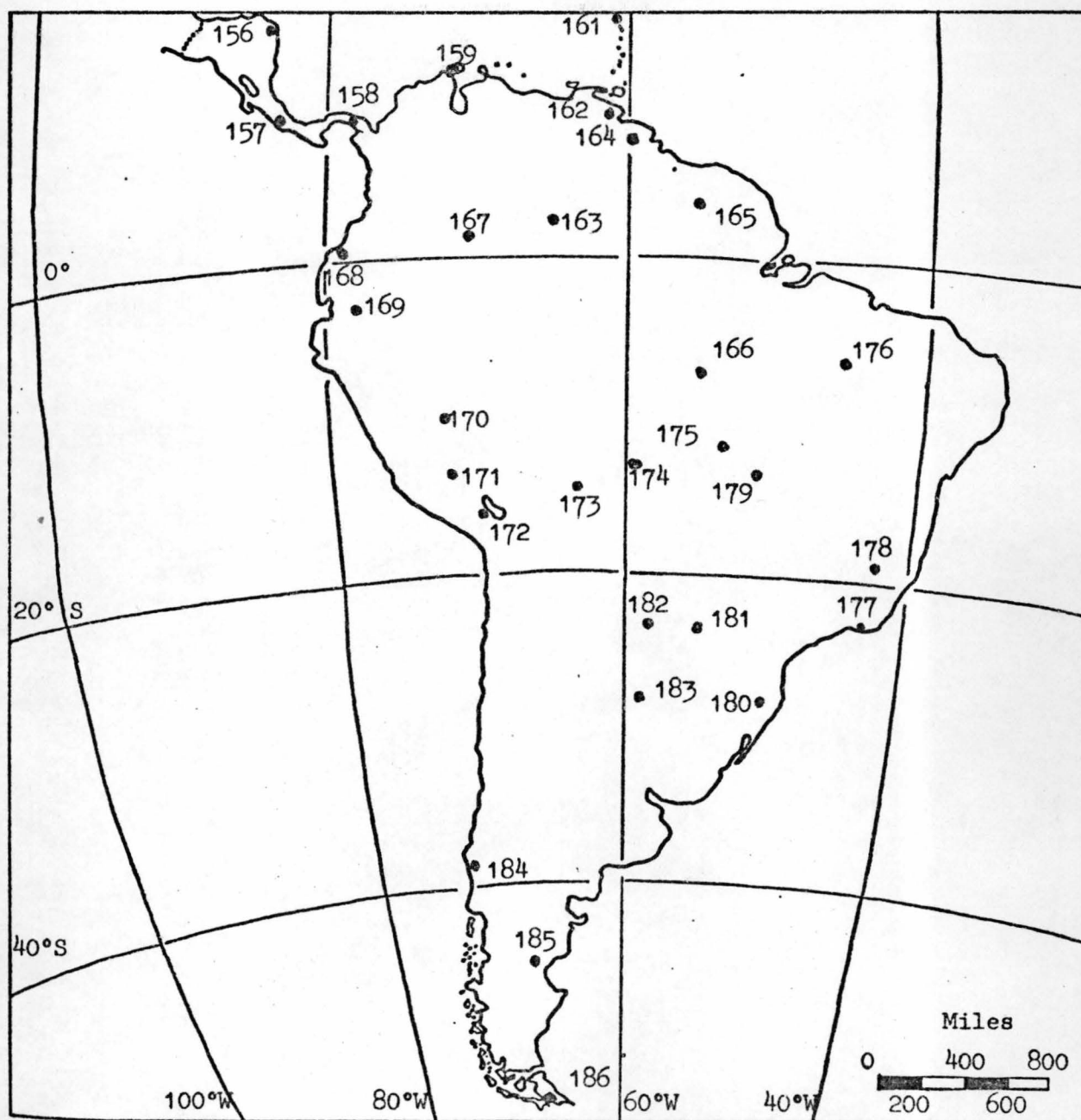
Not all of the 32 societies in this region are untouched by European contact. The Haitians and the Carib are the result of the slave trade between Africa and the New World. The 32 are:

155 Yucatec Maya	166 Mundurucu	177 Tupinamba
156 Miskito	167 Cubeo	178 Botocudo
157 Bribri	168 Cayapa	179 Shavante
158 Cuna	169 Jivaro	180 Aweikoma
159 Goajiro	170 Amhuaca	181 Cayua
160 Haitians	171 Inca	182 Lengua
161 Callinago	172 Aymara	183 Abipon
162 Warrau	173 Siriono	184 Mapuche
163 Yanomamo	174 Nambicuara	185 Tehuelche
164 Carib	175 Trumai	186 Yahgan
165 Saramacca	176 Timbira	

The societies are located by their identity numbers on map 6.

Despite the relatively high percentage of shared distribution of phis (Table 31) between North and South America, the significant patterns among the variables are notably different in associations, direction, and strength of predictability. Warfare in South America appears to be primarily offensive, and the attribute external war-attacking predicts attitudes, expectations, and values with regard to war and predicts that 18 per cent of the time the average community size involved will be 100-400 persons (11). The warfare variables pattern largely in isolation from political organization (13). In Figure 13, where there are significant associations with subjugation (40) and tribute (41), and both grouped together (30), the centralized polities involved are the Saramacca and the Inca. Furthermore, polity is not significantly related to anything other than military expectations and complex settlement pattern; settlement pattern by itself generates association with military organization (age-sets, military societies, standing armies (24); formal command (32). Even when political organization is expanded to

Map 6: South America



(adapted from Murdock and White 1969: 347)

This map shows 30 of the 32 societies classed as South American.

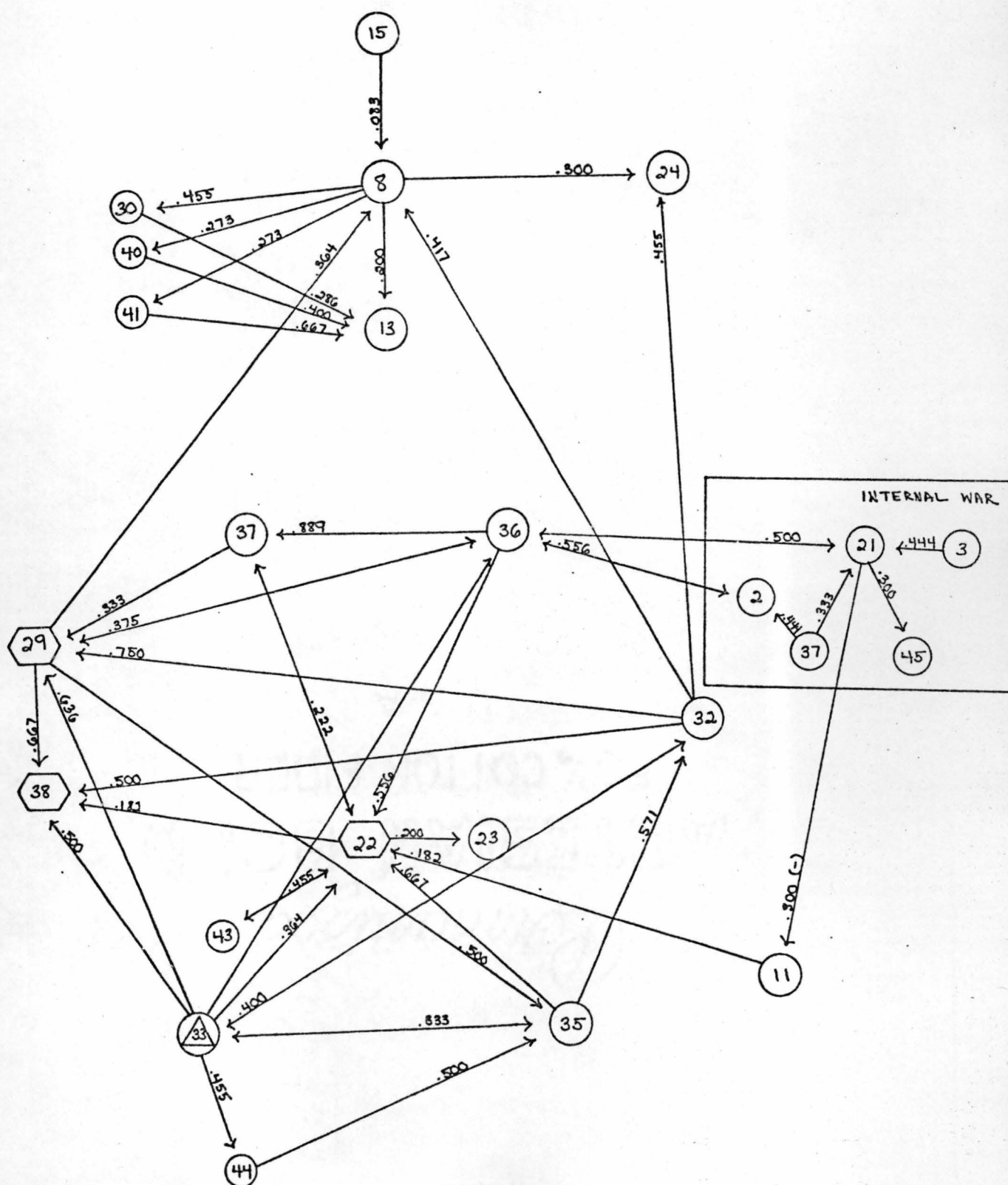


Fig. 13 South America: direction and strength of lambdas, focused on 2-4 levels of polity (13).

include 1 to 4 levels of jurisdiction (14), the only warfare variable other than military expectations that it is significantly associated with is high prestige for warriors (33) (Figure 14). In a large sense, the warfare complex in the lower half of Figure 13 exists in isolation, attributes of war predictable primarily through other attributes of war. For instance, military success (38) is a better predictor of the presence of authoritative military command (32) than is settlement pattern (8). It looks to me as though the warfare complex in South America exists independently, and it is offensive warfare. Even the relationship between offensive and defensive external war (22 and 23), so strong in association and predictability in North America, barely makes the .05 level of significance in South America. Table 42 is the intercorrelation of frequent offensive war with military success; it shows who is winning in South America. In the 6 regions, the association between offensive war and success is significant in 3, predictable in 2, and strongest in South America. The comparison looks like this:

Frequent External War-Attacking
with Military Success (Expansion)

	φ	p	λ_c	λ_r	τ
Circum-Mediterranean	.433	.05	0	0	.188
East Eurasia	.410	.02	0	.250	.168
South America	.538	.01	.100	.182	.289

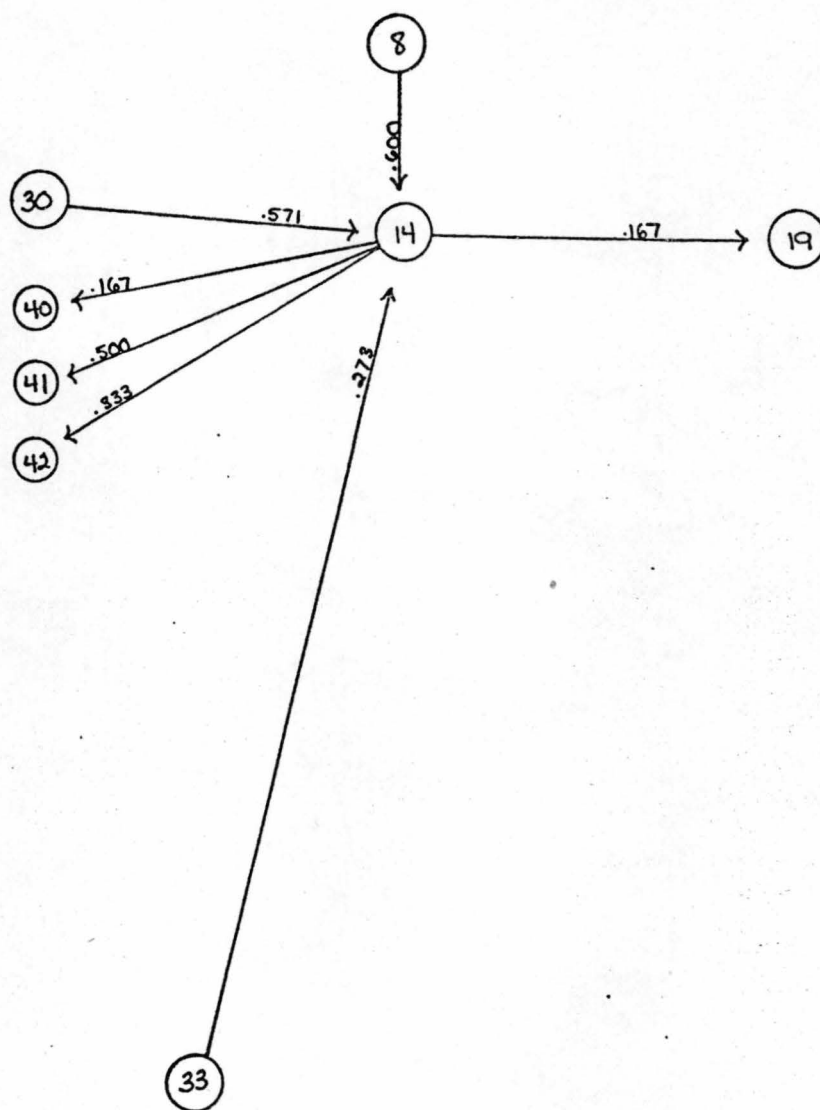


Fig. 14 South America: direction and strength of lambdas, focused on 1-4 levels of polity (14).

Earlier, I identified South America as being part of a world-wide androcentric configuration, secondary to Africa. That androcentrism is manifest in the intercorrelations within the region concerned with internal war (21). Where there is frequent internal war, 44 per cent of the time one can predict the presence of patrilocality (3), and 50 per cent of the time one can predict the presence of the expectation that violence is a solution to problems (36). David Maybury-Lewis' hypothesis that men's houses and conflict go together may be correct, but it appears to be limited to South America. That is, there is no functional link between male groupings and the perpetuation and intensification of attitudes and values held about violence. Instead, historically it has occurred in one geographic region; it is truly a cultural relationship, i.e., arbitrary.

Table 43 gives those attributes that are predictable in both directions. When compared to Table 41, which presents North American data, one can readily see that even in contiguous geographical regions that are supposed to have such strong sociocultural similarity, the relationships among the warfare variables within each region are sharply distinctive and have little in common.

TABLE 43. SIGNIFICANT CORRELATIONS WITH LAMBDA S
IN TWO DIRECTIONS--SOUTH AMERICA

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Polygyny (2) with:				
High value for war (37)	.537	.02	.375	.444
Row attribute Patrilocality (3) with:				
Frequent internal war (21)	.492	.05	.444	.375
Row attribute Complex Settlements (8) with:				
1-4 levels beyond the local community (14)	.710	.001	.600	.333
Military expectations I (29)	.518	.01	.364	.417
Military expectations II (30)	.567	.01	.455	.143
Authoritative command (32)	.462	.02	.222	.417
Row attribute Mean Community Size 100-400 Persons (11) with:				
Frequent internal war (21)	-.408	.05	.222	.300
Row attribute 2-4 Levels Beyond the Local Community (13) with:				
Tribute (41)	.802	.001	.500	.667
Row attribute 1-4 Levels Beyond the Local Community (14) with:				
Military expectations II (30)	.709	.001	.500	.571
Row attribute Frequent Internal War (21) with:				
High value for war (37)	.481	.05	.250	.333
Row attribute Frequent External War-Attacking (22) with:				
Rewards for warriors (35)	.756	.01	.500	.667
Expectations of violence (36)	.601	.01	.429	.556
Military success (expansion)(38)	.538	.01	.182	.100
Row attribute Age-Sets...Armies (24) with:				
Authoritative command (32)	.514	.02	.143	.455

TABLE 43 (Continued)

Pairs of Variables	φ	p	λ_r	λ_c
Row attribute Military Expectations I (29) with:				
Authoritative command (32)	.769	.001	.727	.750
High prestige (33)	.664	.01	.600	.636
Rewards for warrior (35)	.553	.02	.500	.333
High value for war (37)	.424	.05	.143	.333
Military success (38)	.722	.001	.667	.600
Row attribute Authoritative Command (32) with:				
Rewards for warriors (35)	.645	.01	.571	.500
Military success (expansion) (38)	.523	.01	.500	.400
Row attribute High Prestige (33) with:				
Expectations of violence (36)	.630	.02	.625	.571
Military success (expansion) (38)	.574	.01	.500	.444
Trophies and honors (44)	.516	.02	.455	.250

This concludes my presentation of the discoveries of the ODYSSEY. In the future, I hope to enlarge the regional samples and run the intercorrelations again, to see if the preliminary findings hold. The one weakness of the Standard Sample that I can see is that it is not large enough to make one confident when doing regional analyses because of the size of the Ns. I have commented earlier that since the regional subsamples are already similar, it is not the same thing as working with 30 societies spread out over the world. Nevertheless, I would like to enlarge the number from at least 1 or 2 regions, using Murdock's World Sampling

Provinces (1968), to see if any discrepancies show up. If they do not, I will be more confident about my regional analyses.

FINAL REMARKS

It has been a long voyage. I have logged and charted my exploration of the world of war and found its nature to be intricately variable, shifting in time, space, and perspective. I have argued against universal explanations of warfare and for the hypothesis of variability due to factors of geography and history. I have performed quantitative tests that I think show that a pattern of relationships among variables that is a summary of data from cultures throughout the world exists in the first place because of influences from particular geographical regions and yet does not exist within any 1 region in the same form or strength. Furthermore, it is not permissible to transform variability into uniformity through the magic of evolutionary seriation. The diachronic qualities built into the Standard Sample make such a practice questionable methodologically, without having to go into philosophical disputation. As Driver and Schuessler have done, I conclude that the geographical patterning and variability in configurations support geographical-historical explanations of warfare rather than psycho-functional ones. However warfare in a given region gets started, it becomes part of a lattice of variables--which ones we may not know a priori--that is probably in most cases self-perpetuating. Unilineal, one-directional, deterministic

causal chains are simply too crude and clumsy to account for the data.

Certainly polygyny is part of a warfare syndrome--in African external war and South American internal war. Certainly food-production is related to warfare--in Africa and the Insular Pacific. Of course political centralization is important--but it does not have any direct relationship with warfare anywhere in the world, and its relationship to the sociocultural institutions of war is highly variable. The presence of some supra-community organization--sodality, chiefdom, or state--does have a direct relationship with warfare--but not in East Eurasia, the Insular Pacific, or South America. Moreover, in the remaining regions, the predictive direction and strength differ. Yet indeed, "fraternal interest groups" are important, but on the world scene the principle of formation is unilineal descent and the warfare is internal. In Africa it is patrilineal descent and the warfare is offensive external. In the Circum-Mediterranean the principle may be unilineal descent but it may also be greed, individual prestige, and vengeance, and the form of war is internal. In the Insular Pacific region, the war is also internal, and the principle may be greed--or endogamous community organization. In South America, the war is internal and the principle is patrilocal residence.

And I may say to myself, certainly attitudes toward violence are primary--in the Circum-Mediterranean and South

America. In the world, Africa, and East Eurasia they are more often dependent than independent attributes. And yes, offensive warfare is an independent variable--in Africa, the Insular Pacific, and South America. Of course the presence of high individual prestige is related to the frequency of warfare, but in the world, Africa, and South America it is a dependent attribute, while in East Eurasia and North America it is an independent attribute, and in the Circum-Mediterranean it is significantly absent.

Thus, some of my own universal generalizations that I have offered throughout this dissertation are subject to geographic patterning. What more could I ask for? If my basic argument is acceptable, that the phenomenon of warfare can be understood only within the context in which it occurs, and that the most important component of the context is human actors themselves, warfare studies may proceed in a theoretical direction completely opposite from that of recent research. I have tried to show throughout this work that the current myths in anthropology--about the nature of explanation in social science, the nature of war, and the nature of the human beings who engage in war--are just that, myths. And surely one point that we can agree upon is that myths are not science, even though both try to make the world intelligible.

NOTES

¹One is reminded of a Herblock cartoon in which citizens are standing in line outside a jail with the sign "Police State" over its door, and one person remarks, "It looks so nice and safe in there."

²As support for this argument, note the following comment by Leach (1966:72):

The development of speech in Homo sapiens has completely altered our nature. A goose can communicate with another goose by means of "ritualized" gestures, but the kinds of message it can transmit are very narrowly delimited: "Hostility" and "friendship" are only the observer's labels for simple triggered responses. In contrast, human beings can say an infinite number of things in an infinite number of different ways; responses are intrinsically unpredictable; politicians and historians are what they are precisely because no man can ever know what his "opponent" is going to do next.

³One of the problems with disjunctions is that we think of them in either/or terms and that somehow one side must be completely free of the other, e.g., objectivity-subjectivity, fact-value, determinism-indeterminism, mind-body, description-explanation. What must be understood is that, while disjunctions are probably universal in human language, the two halves of the disjunction cannot be taken apart and still have any meaning. It is a logical impossibility.

⁴This business about life process could be called the Natural Law of Growth and Development, which implies political as well as physical attributes. Nevertheless, if taken as true, it is conceivable that the accommodation of the earth is coming to an end in terms of numbers, and qualitative changes do not appear to be happening.

⁵Popper mentions Empedocles' theory of evolution, Parmenides' theory of an unchanging universe, and astrological theories of planetary influences (cf. Newton's theory of gravity and the lunar theory of the tides).

⁶The use of "risky" may be misleading. Hanson (1958) notes the difficulty of applying falsification tests to laws of classical physics. It is almost impossible to imagine situations in which the laws would not apply, e.g., objects slowing down as they fall, or lead balls falling faster than feathers in a vacuum. Note how fascinated people are with weightless astronauts in space. I find floating liquids especially odd. While some philosophers are impressed by the inability to falsify and would deny statements of dynamics their law status, reducing them to conventions, rules, or definitions, Hanson demurs, arguing that our inability to imagine situations in which the dynamic laws do not apply is due not to the logical status of the law but to the vast inventory of confirmations and our patterns of thinking in dynamical terms (1958:93).

⁷"... every two material objects attract each other with a force proportional to the product of their masses, and inversely proportional to the square of the distance between them" (Gamow 1962:44).

⁸Because its increased efficiency produces a surplus freeing some food-producers from subsistence activity. "The ratio between the amount of energy diverted to food production and the amount of energy available for other activities is one of the most important cultural variables" (Harris 1971:203).

⁹Sic. Innovation seems more effectively limited to change by a conscious agent. Recombination and mutation in biology is, of course, not conscious. But in the realm of culture individuals do make conscious decisions and choices about change all the time. The distinction is important.

¹⁰Note Chagnon's descriptions of 2 Yanomamo brothers, one skeptical and cautious, the other subscribing to the ideal of a super-warrior (1968b).

¹¹Chagnon (1968b:74-75) describes a case of child neglect: a 2-year-old nearly starved because the mother's milk had dried up and she, well-fed and plump, refused to give it other foods because "it did not know how to eat other foods." The sex of the child is not clearly specified: Chagnon refers to the child as "it" and "he."

¹²Divale notes that, for the U.S.A., the sex ratio of live births is 104 males to 100 females; for Western Europe it is 105:100; for Japan it is 109:100 (1970:11).

¹³The prominent American figure in research on physiological environment and conception has been Dr. L. B. Shettles. He has found, for instance, that out of patients acting on his findings and suggestions for practical application, 23 of 26 couples who wanted to conceive boys were successful, and 19 of 22 couples who wanted girls were successful (Shettles 1970; Rorvik and Shettles).

¹⁴In 1970, Divale remarks that he is putting his warfare syndrome together in the master's thesis, which includes case studies of the Yanomamo, the Sioux, the Ibo, the Kapauku, the Central Eskimo, and the Yir Yoront.

¹⁵While it would not be polite to quote it here, as evidence that the Indian warrior's expectations of reward persisted at least through World War II, I recommend to the reader Patton's address to his troops, in "My Favorite General" in Dwight Macdonald's Memoirs of a Revolutionist, Meridian Books, 1957.

¹⁶I have based the position of population pressure in the chain upon Harris' assumption that in the long run "the rate of population growth has always tended to increase in response to new opportunities for such growth" (1971:223).

¹⁷[Culture] is a plan for behavior, not behavior itself; it is arbitrary, learned, and shared. In addition, culture is adaptive. Human beings cope with their natural and social environment by means of their traditional categories and plans. Unlike other animals, if we are cold we can invent a coat and pass along the technique to our descendants. Culture allows for rapid adaptation because it is flexible and permits the invention of new strategies--although change often appears to be painfully slow to those who are in a hurry for it. By the same token, the adaptive nature of culture accounts for the enormous variety of the world's distinct societies (Spradley and McCurdy 1971:4).

¹⁸Jamil Nammour (1973) has worked on the difficulties of those who want "to pry language off the world." I have borrowed and adapted this singularly effective phrase.

¹⁹Inflation is a fine and familiar example of circular causation, yet American economists in charge of moderating it appear to be using only the inadequate tools of equilibrium theory. One of the beauties of Myrdal's theory is that it does operate in the real world.

²⁰The following summary of quantification schools in anthropology is by Driver (1965:324-45; Chaney 1973:1359-60).

The California and Yale Schools

The California school, represented by A. L. Kroeber, his pupils and associates, used proximity coefficients first to determine geographical clusterings of ethnic units (tribes, tribelets, villages, bands, societies, and any other culture-bearing group of people) and second to reconstruct the history of these ethnic units, of their total cultures, or of a restricted part of their cultures. In every case the universe of investigation was limited to an area of less than continental scope. In most of these studies the ethnic units were treated as variables and they were compared in terms of the amount of culture trait inventory shared. The conclusions arrived at were regarded as applying only to a restricted area of each study, and no attempt was made to establish universal generalizations about the culture of the entire human species. The California school stemmed from Boas.

The Yale school, represented by G. P. Murdock, J. W. M. Whiting, and their pupils and associates, aimed at establishing world-wide generalizations about all ethnic units from samples thought to be representative of the whole. These studies treat subject units (culture traits, elements, complexes, components, themes, patterns, and any other parts of total culture) as variables and compare them in terms of the number of ethnic units sharing the various combinations of subject units. The Yale school stemmed from Tylor, Sumner, and Keller.

The California method was largely empirical in that it was not bolstered by formal postulates, theorems, or propositions. However, it implicitly relied on the general principle that continuity of geographical distribution resulted from geographical factors, diffusion, migration, and other geographico-historical processes. It sometimes employed the age-area hypothesis, which assumes a perfect correlation between size of geographical area and age of the subject unit distributed over the area. The California method has been frequently labeled historical, but it was inferential, undocumented history.

The Yale school, on the other hand, was largely postulational, in that considerable functional or psychological theory was formulated in advance, and only the data relevant to the theory collected. Murdock's Social Structure (1949) is the best known example of this method and

Whiting's psychological studies (e.g., Whiting and Child, 1953) follow similar procedures. However, when Driver bluntly asked Murdock a few years ago if every theorem and proposition in his Social Structure was formulated in advance of the collecting of data and printed without alteration after the data was collected, Murdock gave a negative reply; the postulates were modified in light of what the empirical data revealed. Therefore the dichotomy of postulational versus empirical (positive) method, so charming to some philosophers, does not fit the reality of actual research methods, in ethnology, and probably not in any other behavioral science. The Yale method has been labeled evolutionary because it interprets its correlations as causal sequences replicated over and over again among societies thought not to be connected historically. It has used tests of significance, especially chi-square, much more often than the California School.

²¹Raoul Naroll, "Warfare, peaceful intercourse, and territorial chance: A cross-cultural survey," mimeographed, 1964. Naroll 1966 is a highly condensed version.

²²It is also apparently being published by Naroll, Vern R. Bullough, and Frada Naroll as *Military Deterrence in History: A Pilot Cross-Historical Survey*, State University of New York, 1974. In *Books in Print* 1973 there is a second entry for these authors, *Military Deterrence in History: A Statistical Survey*, State University of New York, 1973. I have seen neither of these publications.

APPENDICES

APPENDIX A

STANDARD CROSS-CULTURAL SAMPLE

Each society in Murdock and White's Standard Cross-Cultural Sample (1969) is listed by identity number and name, followed by the target group, its location, and the date to which the ethnographic descriptions apply, exactly as given by Murdock and White.

1. Nama Hottentot. The Gei/Khauan tribe ($27^{\circ}30'S$, $17^{\circ}E$) reconstructed for 1860, just prior to their decimation and loss of independence in the Herero War.
2. Kung Bushmen. The Agau Kung of the Nyae Nyae region ($19^{\circ}50'S$, $20^{\circ}35'E$) in 1950.
3. Thonga. The Ronga subtribe around Lourenco Marques ($25^{\circ}50'S$, $32^{\circ}20'E$) in 1985.
4. Lozi or Barotse. The ruling Luyana (14° - $18^{\circ}20'S$, 22° - $25^{\circ}E$) in 1900, at the height of Barotse political expansion.
5. Mbundu or Ovimbundu. Bailundo subtribe ($12^{\circ}15'S$, $16^{\circ}30'E$) in 1890, just prior to Portuguese conquest and missionization.
6. Kongo. $17^{\circ}S$, $15^{\circ}E$. 1900. [Substitution for the Suku.]
7. Ila. $16^{\circ}S$, $27^{\circ}E$. 1957. [Substitution for the Bemba.]
8. Nyakyusa. The Nyakyusa around the towns of Mwaya ($9^{\circ}35'S$, $34^{\circ}10'E$) and Masoko ($9^{\circ}20'S$, $34^{\circ}E$) in 1934.
9. Hadza of Kindiga. The small Hadza tribe as a whole ($3^{\circ}20'$ - $4^{\circ}10'S$, $34^{\circ}40'$ - $35^{\circ}25'E$) in 1930, when still unacculturated.
10. Luguru or Waluguru. The Luguru of west central Morogoro District ($6^{\circ}25'$ - $7^{\circ}25'S$, $37^{\circ}20'$ - $38^{\circ}E$), in 1925, the last date of the traditional political organization.

11. The kikuyu of the Metume or Fort Hall district ($0^{\circ}40'S$, $37^{\circ}10'E$) in 1920, prior to intensive acculturation.
12. Ganda or Baganda. The ganda of Kyaddondo district ($0^{\circ}20'N$, $32^{\circ}30'E$) in 1875, just prior to the founding of Kampala and the initiation of significant administrative changes.
13. Mbuti Pygmies. The Epulu net-hunters of the Ituri Forest ($1^{\circ}30'-2^{\circ}N$, $28^{\circ}15'-28^{\circ}25'E$) in 1950.
14. Nkundo Mongo. The Mongo of the Ilanga subtribe ($0^{\circ}15'-1^{\circ}15'S$, $18^{\circ}35'-19^{\circ}45'E$) in 1930.
15. Banen or Banyin. The Ndiki subtribe ($4^{\circ}35'-4^{\circ}45'N$, $10^{\circ}35'-11^{\circ}E$) in 1935.
16. Tiv. The Tiv of Benue Province ($6^{\circ}30'-8^{\circ}N$, $8^{\circ}-10^{\circ}E$) in 1920, prior to extensive organizational changes wrought by the British.
17. Ibo or Igbo. The Eastern and Peripheral subgroups of the Isu-Ama division of the Southern or Owerri Ibo ($5^{\circ}20'-5^{\circ}40'N$, $7^{\circ}10'-7^{\circ}30'E$) in 1935.
18. Fon or Dahomeans. The Fon in the vicinity of Abomey ($7^{\circ}12'N$, $1^{\circ}56'E$) in 1890, prior to the conquest of the Dahomean kingdom by the French.
19. Ashanti. The Ashanti of the state of Kumasi ($6^{\circ}-8^{\circ}N$, $0^{\circ}-3^{\circ}W$) in 1895, just prior to British conquest.
20. Mende. The central Mende around the town of Bo ($7^{\circ}50'N$, $12^{\circ}W$) in 1945.
21. Wolof or Quolof. The wolof of Upper and Lower Salum in the Gambia (centering on $13^{\circ}45'N$, $15^{\circ}20'W$) in 1950.
22. Bambara. The Bambara along the Niger River from Segou of Bamako ($12^{\circ}30'-13^{\circ}N$, $6^{\circ}-8^{\circ}W$) in 1902.
23. Tallensi. The small Tallensi tribe as a whole ($10^{\circ}30'-10^{\circ}45'N$, $0^{\circ}30'-0^{\circ}50'W$) in 1934.
24. Songhai. The songhai of the Bamba or central division ($16^{\circ}-17^{\circ}15'N$, $0^{\circ}10'E-3^{\circ}10'W$) in 1940.
25. Fulani. The Alijam and Degeriji subgroups of Wodaabe Fulani around Adan and Damergou in Niger ($13^{\circ}-17^{\circ}N$, $5^{\circ}-10^{\circ}E$) in 1951.

26. Hausa. The Zazzagawa Hause ($9^{\circ}30' - 11^{\circ}30'N$, $6^{\circ} - 9^{\circ}E$) in 1900, just prior to the advent of British rule.
27. Kanuri of Burnu. $12^{\circ}N$, $13^{\circ}E$. 1870. [Substitution for Massa.]
28. Azande. The Azande of the Yambio chiefdom ($4^{\circ}20' - 5^{\circ}50'N$, $27^{\circ}40' - 28^{\circ}50'E$) in 1905, just prior to British conquest and the collapse of the Avongara political system.
29. Fur. The Fur of western Darfur around Jebel Marra ($13^{\circ}30'N$, $25^{\circ}30'E$) in 1880, prior to effective Egyptian subjugation.
30. Otoro Nuba. The Otoro of the Nuba Hills ($11^{\circ}20'N$, $30^{\circ}40'E$) in 1930, prior to substantial migration into the plains.
31. Shilluk. The politically unified Shilluk as a whole ($9^{\circ} - 10^{\circ}30'N$, $31^{\circ} - 32^{\circ}E$) in 1910.
32. Ingassana.
33. Kafa. The politically unified Kafa as a whole ($6^{\circ}50' - 7^{\circ}45'N$, $35^{\circ}30' - 37^{\circ}E$) in 1905. [The ethnographic materials for this society are in German. There are no substitutes offered in the Standard Sample. Therefore, there is no coding for warfare data.]
34. Masai. The Kisonko or Southern Masai of Tanzania ($1^{\circ}30' - 5^{\circ}30'S$, $35^{\circ} - 37^{\circ}30'E$) in 1900.
35. Konso. The Konso of the town of Buso ($5^{\circ}15'N$, $37^{\circ}30'E$)
36. Somali. The Dolbahanta subtribe ($7^{\circ} - 11^{\circ}N$, $45^{\circ}30' - 49^{\circ}E$) in 1900, subsequent to the earliest descriptions but prior to the later and fuller accounts.
37. Amhara. The Amhara of the Gondar district ($11^{\circ} - 14^{\circ}N$, $36^{\circ} - 38^{\circ}30'E$) in 1953.
38. Bisharin. $20^{\circ}N$, $35^{\circ}E$. 1930. (Substitution for the Bogo.)
39. Nubians. The Kenuzi or northernmost branch of the Barabra or Nile Nubians ($22^{\circ} - 24^{\circ}N$, $32^{\circ} - 33^{\circ}E$) in 1900, just prior to their displacement by the first Aswan dam.
40. Teda. The Teda of Tibesti ($19^{\circ} - 22^{\circ}N$, $16^{\circ} - 19^{\circ}E$) in 1959.

41. Tuareg. The Ahaggaren or Tuareg of Ahaggar (21° - 25° N, 4° - 9° E) in 1900, prior to the French military occupation of the Sahara.
42. Riffians. The Riffians as a whole ($34^{\circ}20'$ - $35^{\circ}30'$ N, $2^{\circ}30'$ - 4° W) in 1926.
43. Ancient Egyptians. 30° N, 31° E. 1200 B.C. [Substitution for 20th century Egyptians from the town of Silwa.]
44. Hebrews. The kingdom of Judah ($30^{\circ}30'$ - $31^{\circ}55'$ N, $34^{\circ}20'$ - $35^{\circ}30'$ E) in 621 B.C., the date of promulgation of the Deuteronomic laws.
45. Babylonians. The city and environs of Babylon ($32^{\circ}35'$ N, $44^{\circ}45'$ E) in 1750 B.C., at the end of the reign of Hammurabi.
46. Rwala. The Rwala Bedouin of south central Syria and northeastern Jordan (31° - $35^{\circ}30'$ N, 36° - 41° E) in 1913.
47. Turks. The Turks of the northern Anatolian plateau ($36^{\circ}40'$ - 40° N, $32^{\circ}40'$ - $35^{\circ}50'$ E) in 1950.
48. Ghég. The Mountain Ghég of northern Albania ($41^{\circ}20'$ - 42° N, $19^{\circ}30'$ - $20^{\circ}31'$ E) in 1910, just prior to the expulsion of the Turks in the two Balkan wars.
49. Romans. The Romans of the city and environs of Rome ($41^{\circ}50'$ N, $13^{\circ}30'$ E) in A.D. 110, the twelfth year of Trajan's reign at the approximate zenith of the imperial period.
50. Basques. The mountain village of Vera de Bidasoa ($43^{\circ}18'$ N, $1^{\circ}40'$ W) in 1934.
51. Irish. The Irish of County Clare ($52^{\circ}40'$ - $53^{\circ}10'$ N, $8^{\circ}20'$ - 10° W) in 1932.
52. Lapps. The Konkama Lapps of Karesuando parish in northern Sweden ($68^{\circ}20'$ - $69^{\circ}5'$ N, $20^{\circ}5'$ - 23° E) in 1950.
53. Yurak Samoyed. The Tundra Yurak (65° - 71° N, 41° - 62° E) in 1894.
54. Russians. The Great Russians of the peasant village of Viriatino ($52^{\circ}40'$ N, $41^{\circ}20'$ E) in 1955.
55. Abkhaz. The small Abkhaz tribe as a whole ($42^{\circ}50'$ - $43^{\circ}25'$ N, 40° - $41^{\circ}35'$ E) in 1880.

56. Armenians. The Armenians in the vicinity of Erevan (40°N, 44°30'E) in 1843.
57. Kurd. The Kurd of the town and environs of Rowandux (36°30'N, 44°30'E) in 1951.
58. Basseri. The nomadic Basseri (27°-31°N, 53°-54°E) in 1958.
59. Punjabi. The western Punjabi of the village of Mohla (32°30'N, 74°E) in 1950.
60. Gond. The Hill Maria Gond (19°15'-20°N, 80°30'-80°20'E) in 1938.
61. Toda. The small Toda tribe as a whole in 1900 (11°-12°N, 76°-77°E).
62. Santal. The Santal of the Bankura and Birbhum districts of Bengal (23°-24°N, 86°50'-87°30'E) in 1940.
63. Uttar Pradesh. The village of Senapur in the small kingdom of Dobhi Taluka (25°55'N, 83°E) in 1945, prior to a major shift in the traditional power base.
64. Burusho. The Burusho of Hunza state (36°20'-36°30'N, 74°30'-74°40'E) in 1934.
65. Kazak. The Kazak of the Great Horde (37°-48°N, 68°-81°E) in 1885.
66. Khalka Mongols. The Khalka of the Narobanchin temple territory (47°-47°20'N, 95°10'-97°E) in 1920.
67. Lolo. The independent and relatively unacculturated Lolo of the Taliang Shan mountains (26°-29°N, 103°-104°E) in 1910.
68. Lepcha. The Lepcha in the vicinity of Lingthem in Sikkim (27°-28°N, 89°E) in 1937.
69. Garo. The Garo of Rengsanggri and neighboring intermarrying villages (26°N, 91°E) in 1955.
70. Sema Naga. 26°N, 95°E. 1910. [Substitution for the Lakher.]
71. Burmese. The village of Nondwin in Upper Burma (22°N, 95°40'E) in 1965.

72. Palaung. 23°N, 97°E. 1920. [Substitution for the Lamet.]
73. Vietnamese. The Tonkinese or North Vietnamese of the delta of the Red River (20°-21°N, 105°30'-107°E) in 1930.
74. Rhade. The Rhade of the village of Ko-sier on the Darlac plateau (13°N, 108°E) in 1962.
75. Khmer or Cambodians. The city of Angkor (13°30'N, 103°50'E), the capital of the old Khmer kingdom at its height, in 1292.
76. Siamese or Central Thai. The Central Thai village of Bang Chan (14°N, 100°50'E) about 1955.
77. Semai or Senoi. 4°N, 102°E. [Substitution for the Semang; data extremely thin, while those for the Semai are better. Also, the Semai are clearly a non-warring, nonviolent group and therefore are necessary to augment the relatively small number of such peoples in the Standard Sample.]
78. Nicobarese. The Nicobarese of the northern islands of Car Nicobar, Chowra, Teressa, and Bompoka (8°15'-9°15'N, 92°40'-93°E) in 1870.
79. Andamanese. The Aka-Bea tribe of South Andaman (11°45'-12°N, 93°-93°10'E) in 1860, prior to significant acculturation and depopulation.
80. Vedda. The Danigala group of Forest Vedda (7°30'-7°N, 81°-81°30'E) in 1860.
81. Tanala. The Menabe subtribe (22°S, 48°E) in 1925.
82. Negri Sembilan. The district of Inas (2°30'-2°40'N, 102°10'-102°20'E) in 1958.
83. Javanese. The town and environs of Pare in central Java (7°43'S, 112°13'E) in 1954.
84. Balinese. The village of Tihingan in the district of Klunghung (8°30'S, 105°20'E) in 1958.
85. Iban or Sea Dayak. The Iban of the Ulu Ai group (2°N, 112°30'-113°30'E) in 1958.
86. Badjau. The Badjau of southwestern Tawi-Tawi and adjacent islands of the Sulu Archipelago (5°N, 120°E) in 1963.

87. Toradja. The Bare'e subgroup of eastern Toradja (2°S, 121°E) in 1910.
88. Tobelorese or Tobelo. The Tobelorese as a whole (2°N, 128°E) in 1900.
89. Alorese or Abui. The village complex of Atimelang in north central Alor (8°20'S, 124°40'E) in 1938.
90. Tiwi. The Tiwi of Bathurst and Melville Islands as a whole (11°45'S, 130°-132°E) in 1929.
91. Aranda or Arunta. The Arunta Mbainda of Alice Springs (23°30'-25°S, 132°30'-134°20'E) in 1896.
92. Orokaiva. The aiga subtribe (8°20'-8°40'S, 147°50'-148°10'E) in 1925.
93. Kimam. The village of Bamol in northeast central Frederick Hendrik Island or Kolekom (7°30'S, 138°30'E) in 1960.
94. Kapauku. The village of Botukebo in the Kamu Valley (4°S, 36°E) in 1955.
95. Kwoma. The Hongwam subtribe (4°10'S, 142°40'E) in 1937.
96. Manus. The village of Peri (2°10'S, 147°E) in 1929.
97. New Ireland. The village of Lesu (2°30'S, 151°W) in
98. Trobrianders. The island of Kiriwina (8°38'S, 151°4'E)
99. Siuai or Motuna. The northeastern Siuai of southern Bougainville (7°S, 155°20'E) in 1939.
100. Tikopia. The small island of Tikopia as a whole (12°30'S, 168°30'E) in 1930.
101. Pentecost. The village of Bunlap and neighboring intermarrying pagan villages in southeastern Pentecost Island (16°S, 168°E) in 1953.
102. Mbau Fijians. The island of Mbau off the east coast of Viti Levu (18°S, 178°35'E) in 1840, the approximate date
103. Ajie. The petty chiefdom of Nefe (21°20'S, 165°40'E) reconstructed for 1845, prior to strong European influence.

104. Maori. The Nga Puhi tribe of the northern isthmus ($35^{\circ}10'S$ - $35^{\circ}30'S$, 174° - $174^{\circ}20'E$) in 1820, prior to European settlement and missionization.
105. Marquesans. The Te-i'i chiefdom of southwestern Nuku Hiva Island ($8^{\circ}55'S$, $140^{\circ}10'W$) about 1800, at about the time of the earliest reliable descriptions.
106. Samoans. The kingdom of Aana in western Upolu Island ($13^{\circ}48'S$ - $14^{\circ}S$, $171^{\circ}54'W$ - $172^{\circ}3'W$) in 1829, prior to the military defeat of Aana and the beginning of intensive European contact.
107. Gilbertese. The northern Gilbertese of Makin and Butiritari islands ($3^{\circ}30'N$, $172^{\circ}20'E$), reconstructed for about 1890.
108. Marshallese. The atoll of Jaluit ($6^{\circ}N$, $165^{\circ}30'E$) in 1900, the mean date of the early German ethnographers.
109. Trukese. The island of Romonum or Ulalu ($7^{\circ}24'N$, $151^{\circ}40'E$) in 1947.
110. Yapese. The island of Yap as a whole ($9^{\circ}30'N$, $138^{\circ}10'E$) in 1910.
111. Palauans. The village of Ulimang in northern Babelthuap Island ($7^{\circ}30'N$, $134^{\circ}35'E$) in 1947.
112. Ifugao. The Central and Kiangnan Ifugao ($16^{\circ}50'N$, $121^{\circ}10'E$) in 1910.
113. Atayal. The Atayal proper (excluding the Sedeq) as a whole ($23^{\circ}50'N$ - $24^{\circ}50'N$, $120^{\circ}20'E$ - $120^{\circ}50'E$) about 1930, when the aboriginal culture was still relatively intact.
114. Chinese. The village of Kaihsienkung in northern Chekiang ($31^{\circ}N$, $120^{\circ}5'E$) in 1936.
115. Manchu. The Aigun district of northern Manchuria ($50^{\circ}N$, $125^{\circ}30'E$) in 1915.
116. Koreans. The village of Sondup'o and town of Samku Li on Kanghwa Island ($37^{\circ}37'N$, $126^{\circ}25'E$) in 1947.
117. Japanese. The village of Niiike in Okayama prefecture ($34^{\circ}40'N$, $133^{\circ}48'E$) in 1950.
118. Ainu. The Ainu of the basins of the Tokapchi and Saru rivers in southeastern Hokkaido ($42^{\circ}40'N$ - $43^{\circ}30'N$, 142° - $144^{\circ}E$) reconstructed for about 1880.

119. Gilyak. The Gilyak of Sakhalin Island ($53^{\circ}30'N$ - $54^{\circ}30'N$, $141^{\circ}50'E$ - $143^{\circ}10'E$) in 1890.
120. Yukaghir. The Yukaghir of the Upper Kolyma River ($63^{\circ}30'N$ - $66^{\circ}N$, 150° - $157^{\circ}E$) in 1850, prior to marked depopulation.
121. Chukchee. The Reindeer Chukchee (63° - $70^{\circ}N$, $171^{\circ}W$ - $171^{\circ}E$) in 1900.
122. Ingalik or Tinneh. The village of Shageluk ($62^{\circ}30'N$, $159^{\circ}30'W$), reconstructed for 1885, just prior to missionization.
123. Aleut. The Unalaska branch of the Aleut (53° - $57^{\circ}30'N$, 158° - $170^{\circ}W$) about 1800, prior to intensive acculturation.
124. Copper Eskimo. The Copper Eskimo of the Arctic mainland ($66^{\circ}40'N$ - $69^{\circ}20'N$, 108° - $117^{\circ}W$) in 1915.
125. Montagnais. The Montagnais of the Lake St. John and Mistassini bands (48° - $52^{\circ}N$, 73° - $75^{\circ}W$) in 1910.
126. Micmac. The Micmac of the mainland ($43^{\circ}30'N$ - $50^{\circ}N$, 60° - $66^{\circ}W$) in 1650.
127. Saulteaux. The Northern Saulteaux of the Berens River band ($52^{\circ}N$, $95^{\circ}30'W$) in 1930.
128. Slave. The Slave in the vicinity of Fort Simpson ($62^{\circ}N$, $122^{\circ}W$) in 1940, just prior to the heavy acculturation following World War II.
129. Kaska or Eastern Nahani. The Kaska of the Upper Liard River ($60^{\circ}N$, $131^{\circ}W$), reconstructed for 1900, just prior to intensive missionization.
130. Eyak. The small Eyak tribe as a whole (60° - $61^{\circ}N$, 144° - $146^{\circ}W$) in 1890, prior to full acculturation.
131. Haida. The village of Masset ($54^{\circ}N$, $132^{\circ}30'W$), reconstructed for 1875, immediately prior to missionization.
132. Bellacoola. The central Bellacoola along the lower Bella Coola River ($52^{\circ}20'N$, 126° - $127^{\circ}W$) in 1880.
133. Twana. The small Twana tribe as a whole ($47^{\circ}20'N$ - $47^{\circ}30'N$, $123^{\circ}10'W$ - $123^{\circ}20'W$), reconstructed for 1860, prior to missionization.
134. Yurok. The village of Tsurai ($42^{\circ}30'N$, $124^{\circ}W$) in 1850.

135. Pomo. The Eastern Pomo of Clear Lake (39°N , 123°W) in 1850, prior to the inrush of European settlers.
136. Yokuts. The Lake Yokuts ($35^{\circ}10'\text{N}$, $119^{\circ}20'\text{W}$), prior to the influx of settlers following the gold rush.
137. Paiute. The Wadadika or Harney Valley band of Northern Paiute ($43^{\circ}-44^{\circ}\text{N}$, $118^{\circ}-120^{\circ}\text{W}$), reconstructed for about 1870, just prior to the establishment of the reservation.
138. Klamath. The Klamath tribe as a whole ($42^{\circ}-43^{\circ}15'\text{N}$, $121^{\circ}20'\text{W}$) in 1860.
139. Kutenai. The Lower Kutenia ($48^{\circ}40'-49^{\circ}10'\text{N}$, $116^{\circ}40'\text{W}$) in 1890.
140. Gros Ventre or Atsina. The homogenous Gros Ventre as a whole ($47^{\circ}-49^{\circ}\text{N}$, $106^{\circ}-110^{\circ}\text{W}$) in 1880, shortly prior to missionization and the disappearance of the buffalo.
141. Hidatsa or Minitari. The village of Hidatsa (47°N , 101°W), reconstructed for 1836, prior to depopulation in a severe smallpox epidemic.
142. Pawnee. The Skidi or Skiri Pawnee (42°N , 100°W), reconstructed for 1867.
143. Omaha. The Omaha tribe as a whole ($41^{\circ}10'-41^{\circ}40'\text{N}$, $96^{\circ}-97^{\circ}\text{W}$) in 1860, prior to the disappearance of the buffalo.
144. Huron or Wendot. The Attignawantan (Bear People) and Attigneenongnahac (Cord People) tribes of the Huron Confederacy ($44^{\circ}-45^{\circ}\text{N}$, $78^{\circ}-80^{\circ}\text{W}$) in 1634, the date of the beginning of Jesuit missionary activity.
145. Creek or Muskogee. The Upper Creek of Alabama ($32^{\circ}30'-34^{\circ}20'\text{N}$, $85^{\circ}30'-86^{\circ}30'\text{W}$) in 1800, prior to Tecumseh's rebellion and removal to Oklahoma.
146. Natchez. The politically integrated Natchez as a whole ($31^{\circ}30'\text{N}$, $91^{\circ}25'\text{W}$) in 1718, the date of the arrival of the first missionaries and ethnographers.
147. Comanche. The Comanche as a whole ($30^{\circ}-38^{\circ}\text{N}$, $98^{\circ}-103^{\circ}\text{W}$) in 1870, just prior to pacification and removal to Oklahoma.

148. Chiricahua Apache. The central band or Chiricahua proper (32°N, 109°30'W) in 1870, immediately prior to the reservation period.
149. Zuni. The village of Zuni (35°-35°30'N, 108°30'-109°W) in 1880.
150. Havasupai. The small Savasupai tribe as a whole (35°20'-36°20'N, 111°20'-113°W) in 1918.
151. Papago. The Archic Papago near Seels, Arizona (32°N, 112°W), in 1910.
152. Huichol. The small Huichol tribe as a whole (22°N, 105°W) in 1890.
153. Aztec. The city and environs of Tenochtitlan (19°N, 99°10'W) in 1520.
154. Popoluca. The Sierra Popoluca of the town and vicinity of Soteapan (18°15'N, 94°50'W) in 1940.
155. Yucatec Maya. 18°N, 90°W. 1520. [Substitution for the Quiche, for whom the warfare material is severely deficient.]
156. Miskito. The Miskito in the vicinity of Cape Gracias a Dios (15°N, 83°W) in 1921.
157. Bribri. The Bribri tribe of the Talamanca nation (9°N, 83°15'W) in 1917.
158. Cuna or Tule. The Cuna of the San Blas Archipelago (9°-9°30'N, 78°-79°W) in 1927.
159. Goajiro. The homogeneous Goajiro tribe as a whole (11°30'-12°20'N, 71°-72°30'W) in 1947.
160. Haitians. The Haitians of Mirebalais (18°50'N, 72°10'W) in 1935.
161. Callinago or Island Carib. The Callinago of the island of Dominica (15°30'N, 60°30'W), reconstructed for 1650, shortly prior to missionization.
162. Warrau. The Warrau of the Orinoco delta (8°30'-9°50'N, 60°40'-62°30'W) in 1935.
163. Yanomamo. The Shamatari subtribe around the village of Bisaasi-teri (2°-2°45'N, 64°30'-65°30'W) in 1965.

164. Carib. The Carib along the Barama River in British Guiana ($7^{\circ}10'-7^{\circ}40'N$, $59^{\circ}20'-60^{\circ}20'W$) in 1932.
165. Saramacca. The Saramacca group of Bush Negroes in the upper basin of the Suriname River ($3^{\circ}-4^{\circ}N$, $55^{\circ}30'-56^{\circ}W$) in 1928.
166. Mundurucu. The savanna-dwelling Mundurucu of the Rio de Tropas drainage ($6^{\circ}-7^{\circ}S$, $56^{\circ}-57^{\circ}W$), reconstructed for about 1850, prior to the period of increasing assimilation.
167. Cubeo. The Cubeo of the Caduiari River ($1^{\circ}-1^{\circ}50'N$, $70^{\circ}-72^{\circ}W$) in 1939.
168. Cayapa. The Cayapa in the drainage of the Rio Cayapas ($0^{\circ}40'-1^{\circ}15'N$, $78^{\circ}45'-79^{\circ}10'W$) in 1908.
169. Jivaro. The Jivaro proper ($2^{\circ}-4^{\circ}S$, $77^{\circ}-79^{\circ}W$) in 1920.
170. Amahuaca. The Amahuaca on the upper Inuya River ($10^{\circ}10'-10^{\circ}30'S$, $72^{\circ}-72^{\circ}30'W$) in 1960.
171. Inca. The Quechua-speaking Indians in the vicinity of Cuzco ($13^{\circ}30'S$, $72^{\circ}W$) in 1530, immediately prior to the Spanish Conquest.
172. Aymara. The Aymara of the community of Chucuito in Peru ($16^{\circ}S$, $70^{\circ}W$) in 1940.
173. Siriono. The Siriono in the forests near the Rio Blanco ($14^{\circ}-15^{\circ}S$, $63^{\circ}-64^{\circ}W$) in 1942.
174. Nambicuara. The Cocozu or eastern Nambicuara ($12^{\circ}30'-13^{\circ}30'S$, $58^{\circ}30'-59^{\circ}W$) in 1940.
175. Trumai. The single surviving Trumai village ($11^{\circ}50'S$, $53^{\circ}40'W$) in 1938.
176. Timbira. The Ramcocamecra or Eastern Timbira ($6^{\circ}-7^{\circ}S$, $45^{\circ}-46^{\circ}W$) in 1915.
177. Tupinamba. The Tupinamba near Rio de Janeiro ($22^{\circ}30'-23^{\circ}S$, $42^{\circ}-44^{\circ}30'W$) in 1550.
178. Botocudo. The Naknenuk subtribe in the basin of the Rio Doce ($18^{\circ}-20^{\circ}S$, $41^{\circ}30'-43^{\circ}30'W$) in 1884.
179. Shavante. The Akwe-Shavante in the vicinity of Sao Domingos ($13^{\circ}30'S$, $51^{\circ}30'W$) in 1958.

180. Aweikoma. The Aweikoma of the Duque de Caxias Reservation (38°S , 50°W) in 1932.
181. Cayua. The Cayua of southern Mato Grosso, Brazil ($23^{\circ}\text{--}24^{\circ}\text{S}$, $54^{\circ}\text{--}56^{\circ}\text{W}$) in 1890, the approximate period of the earlier good descriptions.
182. Lengua. The Lengua in contact with the Anglican mission ($23^{\circ}\text{--}24^{\circ}\text{S}$, $58^{\circ}\text{--}59^{\circ}\text{W}$) in 1889, the date of the founding of the mission.
183. Abipon. The Abipon in contact with the Jesuit mission ($27^{\circ}\text{--}29^{\circ}\text{S}$, $59^{\circ}\text{--}60^{\circ}\text{W}$) in 1750.
184. Mapuche. The Mapuche in the vicinity of Temuco ($38^{\circ}30'\text{S}$, $72^{\circ}35'\text{W}$) in 1950.
185. Tehuelche or Patagon. The equestrian Tehuelche ($40^{\circ}\text{--}50^{\circ}\text{S}$, $64^{\circ}\text{--}72^{\circ}\text{W}$) in 1870.
186. Yahgan. The eastern and central Yahgan ($54^{\circ}30'\text{--}55^{\circ}30'\text{S}$, $67^{\circ}\text{--}70^{\circ}\text{W}$), reconstructed for 1865.

APPENDIX B

CODE SHEET FOR ODYSSEY

Variable numbers are in parentheses. Where lumping occurs in the formation of dichotomies, the 2 categories are indicated in italics. Numbers assigned to the attributes are those listed in Appendix C and are the codings for each society for each variable.

(1) Regional Identification

- A Africa
- C Circum-Mediterranean
- E East Eurasia
- I Insular Pacific
- N North America
- S South America

(2) Polygyny

Absent

- 1 Independent nuclear families with monogamy.
- 2 Independent nuclear families with an incidence of polygyny of less than 20 per cent where permitted.

Present

- 3 Polygyny with an incidence of 20 per cent or higher.
- 4 (Variations in the Ethnographic Atlas of codings for
- 5 the presence of polygyny in sororal, non-sororal, and
- 6 residence modes are not used.)

(3) Marital Residence I

- 1 Patrilocal, virilocal
- 3 Matrilocal, avunculocal, uxorilocal

(4) Marital Residence II

Unilocal

- 1 Patrilocal, virilocal
- 3 Matrilocal, avunculocal, uxorilocal

Other

- 2 Ambilocal, neolocal

(5) Community Organization

Endogamous or agamous communities

- 1 Demes (not segmented into clan-barrios)
- 2 Segmented communities (containing localized clans) without local exogamy
- 3 Agamous communities

Exogamous communities

- 4 Exogamous communities (not clans)
- 5 Segmented communities (containing localized clans)
- 6 Clan-communities (or clan-barrios)

(6) Settlement Pattern I

Nomadic or semisedentary communities

- 1 Fully migratory or nomadic bands
- 2 Seminomadic communities
- 3 Semisedentary communities

Other forms

- Variable 7
- Variable 8

(7) Settlement Pattern II

Settlements, homesteads, and hamlets

- 4 Compact but impermanent settlements
- 5 Neighborhoods of dispersed family homesteads
- 6 Separated hamlets, forming a single community

Other forms

- Variable 6
- Variable 8

(8) Settlement Pattern III

Compact and complex settlements

- 7 Compact and relatively permanent settlements
- 8 Complex settlements

Other forms

- Variable 6
- Variable 7

(9) Mean Size of Local Communities I

Fewer than 50 persons

- 1 Fewer than 50 persons

Other mean sizes

- 2 50-99 persons
- 3 100-199 persons
- 4 200-399 persons
- 5 400-1,000 persons
- 6 1,000 without any town of more than 5,000 persons
- 7 One or more towns of 5,000-50,000 persons
- 8 One or more cities of more than 50,000

(10) Mean Size of Local Communities II

50-99 persons

- 2 50-99 persons

Other mean sizes

- 1 Fewer than 50 persons
- 3 100-199 persons
- 4 200-399 persons
- 5 400-1,000 persons
- 6 1,000 without any town of more than 5,000 persons
- 7 One or more towns of 5,000-50,000 persons
- 8 One or more cities of more than 50,000

(11) Mean Size of Local Communities III

100-399 persons

- 3 100-199 persons
- 4 200-399 persons

Other mean sizes

- 1 Fewer than 50 persons
- 2 50-99 persons
- 5 400-5,000 persons
- 6 1,000 without any town of more than 5,000 persons
- 7 One or more towns of 5,000-50,000 persons
- 8 One or more cities of more than 50,000

(12) Local Jurisdictional Hierarchy

0-1 level

- 1 No levels
- 2 1 level

2-4 levels

- 3 2 levels
- 4 3 levels
- 5 4 levels

(13) Jurisdictional Hierarchy Beyond the Local Community I

2-4 levels

- 3 2 levels--petty paramount chiefdom
- 4 3 levels--small state
- 5 3 levels--large state

0-1 level

- 1 No levels
- 2 1 level

(14) Jurisdictional Hierarchy Beyond the Local Community II

1-4 levels

- 2 1 level
- 3 2 levels--petty paramount chiefdom
- 4 3 levels--small state
- 5 4 levels--large state

0 levels

- 1 No levels

(15) Dominant Subsistence Activity

Food-collectors

- 1 Gathering
- 2 Fishing
- 3 Hunting

Food-producers

- 4 Pastoralism
- 5 Incipient agriculture
- 6 Extensive agriculture
- 7 Intensive agriculture

(16) Descent I

- 1 Patrilineal
- 3 Matrilineal

(17) Descent II

Unilineal

- 1 Patrilineal
- 3 Matrilineal

Non-unilineal

- 2 Duolateral
- 4 Quasi-lineages
- 5 Ambilineal
- 6 Bilateral

(18) Division of Labor

Patrilineal

- 1 Males alone or almost alone
- 2 Males appreciably more

Non-patrilineal

- 3 Differentiation but equal participation
- 4 Equal participation without marked differentiation
- 5 Females appreciably more
- 6 Females alone or almost alone
- 7 Sex participation irrelevant

(19) Class Stratification

Absent

- 1 Absence among freemen
- 2 Wealth distinctions

Present

- 3 Elite (based on control of land or other resources)
- 4 Dual (hereditary aristocracy)
- 5 Complex (social classes)

(20) Succession to the Office of Local Headman

Hereditary

- 1 Hereditary patrilineally
- 2 Hereditary matrilineally

Nonhereditary

- 3 Nonhereditary (appointed)
- 4 Nonhereditary (seniority)
- 5 Nonhereditary (wealth or social status)
- 6 Nonhereditary (election)
- 7 Nonhereditary (informal consensus)

(21) Frequency of Internal War

Frequent

- 1 Continual (perpetual, constant)
- 2 Frequent (common, intensive)

Infrequent

- 3 Infrequent or never (occasional, sporadic, rare)

(22) Frequency of External War-Attacking

Frequent

- 1 Continual (perpetual, constant)
- 2 Frequent (common, intensive)

Infrequent

- 3 Infrequent or never (occasional, sporadic, rare)

(23) Frequency of External War-Being Attacked

Frequent

- 1 Continual (perpetual, constant)
- 2 Frequent (common, intensive)

Infrequent

- 3 Infrequent or never (occasional, sporadic, rare)

(24) Form of Military Mobilization

- 1 Age-grades, military societies, standing armies
- 2 Men are not organized into any of the above; includes friends, family, lineage, clan (kinship organization and informal non-kinship)

(25) Decision to Engage in War

- 1 An official or council of the political community
- 2 Anyone

(26) Commencement of War

- 1 By announcement or mutual agreement
- 2 By surprise attack

(27) Conclusion of War

- 1 By negotiation
- 2 By simply stopping, or war is not ended but is continual

(28) Peace Ceremony

- 1 Present
- 2 Absent

(29) Military Expectations I

High

- 1 Subjugation of territory and people
- 2 Collection of tribute
- 3 Land--fields, hunting territories, fishing territories, pastures
- 5 Trophies and honors (includes captives for sacrifice)

Low

- 4 Plunder (includes captives for slaves, hostages, and adoption)
- 6 Revenge
- 7 Defense
- 8 Aggressive defense

(30) Military Expectations II

High

- 1 Subjugation of territory and people
- 2 Collection of tribute
- 3 Land--fields, hunting territories, fishing territories, pastures

Low

- 4 Plunder (includes captives for slaves, hostages, and adoption)
- 5 Trophies and honors (includes captives for sacrifice)
- 6 Revenge
- 7 Defense
- 8 Aggressive defense

(31) Casualty Rate

- 1 High (1/3 or more of combatants)
- 2 Low

(32) Leadership During Battle

- 1 An official who could back up his decision by force
- 2 An informal leader whom people obeyed because of respect, but who had no means to force warriors to obey

(33) Prestige Associated with Being A Soldier or Warrior

High

- 1 A great deal; important for every male

Low

- 2 Some; not necessary to be a warrior to have influence in the community
- 3 No special consideration, respect, or distinctions for a man who fights

(34) Cowardice

- 1 Refusing to fight (defined as)
- 2 Leaving companions and the battlefield; running away

(35) Rewards (special gifts, praises, or ceremonies, not including ritual purification) for a man who has killed an enemy in battle or otherwise shown skill in war)

Present/Elaborate

- 1 Yes---usually or always

Absent/not elaborate

- 2 Sometimes
- 3 Rarely or never

(36) Did members of the society expect violence to solve their problems?

- 1 Yes
- 2 No
- 3 Not clear

(37) Value of War: Violence/war against non-members of the group is:

High

- 1 Enjoyed and considered to have high value

Low

- 2 Considered to be a necessary evil
- 3 Consistently avoided, denounced, not engaged in

(38) Military Success I: Is the political community/cultural unit winning or losing in the long run?

Present

- 1 Yes--its boundaries/population are expanding

Absent

- 2 No change--boundaries/population are stationary (the population is able to replace those lost in war)
- 3 Breaking even--what it loses in territory it takes from others
- 4 No--its boundaries/population are shrinking

(39) Military Success II

Present

- 1 Expanding
- 2 No change
- 3 Breaking even

Absent

- 4 Shrinking

(40) Subjugation of Territory and People

- 1 Present
- 2 Absent

(41) Collection of Tribute

- 1 Present
- 2 Absent

(42) Land--fields, hunting territories, fishing territories, pastures

- 1 Present
- 2 Absent

(43) Plunder (includes captives for slaves, hostages, adoption)

- 1 Present
- 2 Absent

(44) Trophies and Honors (includes captives for sacrifice)

- 1 Present
- 2 Absent

(45) Revenge

- 1 Present
- 2 Absent

(46) Defense

- 1 Present
- 2 Absent

(47) Aggressive Defense (defending oneself by attacking first, if it is thought that an enemy is planning to attack)

- 1 Present
- 2 Absent

Population Density:

- 1 Less than 1 person per 5 square miles
- 2 From 1 person per square mile to 1 per 5 square miles
- 3 From 1.1 to 5 persons per square mile
- 4 From 5.1 to 25 per square mile
- 5 From 26 to 100 persons per square mile
- 6 From 101 to 500 persons per square mile
- 7 Over 500 persons per square mile

APPENDIX C. CODING OF VARIABLES FOR THE STANDARD SAMPLE

Variable	1	2	3	4	5	6	7	8	9	10	11
1 Nama Hottentot	A	2	1	1	6	1	1	1	3	3	3
2 Kung Bushmen	A	2	2	2	4	1	1	1	1	1	1
3 Thonga	A	4	1	1	6	7	7	7	1	1	1
4 Lozi	A	5	1	1	3	3	3	3	5	5	5
5 Mbundu	A	5	1	1	6	7	7	7	4	4	4
6 Kongo	A	2	3	3	6	7	7	7	-	-	-
7 Ila	A	5	1	1	3	3	3	3	4	4	4
8 Nyakyusa	A	5	2	2	4	7	7	7	3	3	3
9 Hadza	A	2	1	1	-	1	1	1	1	1	1
10 Luguru	A	2	3	3	2	6	6	6	4	4	4
11 Kikuyu	A	5	1	1	6	7	7	7	3	3	3
12 Ganda	A	5	2	2	4	7	7	7	3	3	3
13 Mbuti Pygmies	A	2	2	2	3	1	1	1	2	2	2
14 Nkundo Mongo	A	5	1	1	6	6	6	6	3	3	3
15 Banen	A	5	1	1	6	5	5	5	4	4	4
16 Tiv	A	5	1	1	6	5	5	5	5	5	5
17 Ibo	A	5	1	1	2	6	6	6	6	6	6
18 Fon	A	5	1	1	2	7	7	7	7	7	7
19 Ashanti	A	5	3	3	3	7	7	7	4	4	4
20 Mende	A	5	1	1	2	8	8	8	3	3	3
21 Wolof	C	5	1	1	6	7	7	7	3	3	3
22 Bambara	A	5	1	1	2	7	7	7	5	5	5
23 Tallensi	A	5	1	1	6	5	5	5	5	5	5
24 Songhai	C	2	1	1	3	7	7	7	5	5	5
25 Fulani	C	5	1	1	1	1	1	1	3	3	3
26 Hausa	C	5	1	1	2	7	7	7	6	6	6
27 Kanuri	C	5	1	1	3	7	7	7	7	7	7
28 Azande	A	5	1	1	3	5	5	5	3	3	3
29 Fur	C	2	-	-	4	7	7	7	3	3	3
30 Otoro Nuba	A	5	1	1	2	6	6	6	5	5	5
31 Shilluk	A	5	1	1	2	6	6	6	5	5	5
32 Ingassana	A	1	2	2	1	5	5	5	-	-	-
33 Kafa	C	6	1	1	-	5	5	5	2	2	2
34 Masai	A	5	1	1	3	1	1	1	4	4	4
35 Konso	C	5	1	1	3	7	7	7	6	6	6
36 Somali	C	5	1	1	6	1	1	1	5	5	5
37 Amhara	C	1	1	1	4	6	6	6	4	4	4
38 Bisharin	C	2	1	1	6	2	2	2	-	-	-
39 Nubians	C	2	1	1	3	7	7	7	2	2	2
40 Teda	C	5	1	1	4	2	2	2	2	2	2
41 Tuareg	C	1	2	2	6	-	-	-	1	1	1
42 Riffians	C	2	1	1	6	7	7	7	5	5	5
43 Anc. Egyptians	C	1	2	2	-	7	7	7	8	8	8
44 Hebrews	C	6	1	1	-	7	7	7	7	7	7
45 Babylonians	C	1	2	2	-	7	7	7	8	8	8
46 Rwala Bedouins	C	5	1	1	3	1	1	1	2	2	2
47 Turks	C	2	1	1	1	7	7	7	5	5	5
48 Gheg Albanians	C	6	1	1	4	5	5	5	3	3	3

Variable	12	13	14	15	16	17	18	19	20	21	22
1 Nama Hottentot	3	2	2	3	1	1	1	2	1	2	2
2 Kung Bushmen	4	1	1	1	6	6	5	1	1	3	3
3 Thonga	3	3	3	6	1	1	5	4	1	3	1
4 Lozi	3	4	4	7	5	5	5	4	1	3	2
5 Mbundu	4	3	3	6	2	2	5	4	1	3	2
6 Kongo	4	2	2	6	3	3	5	4	2	2	3
7 Ila	4	2	2	6	2	2	5	2	2	1	1
8 Nyakyusa	3	3	3	7	1	1	2	1	6	2	3
9 Hadza	3	1	1	3	6	6	1	1	4	-	-
10 Luguru	4	1	1	6	3	3	2	1	1	3	3
11 Kikuyu	4	1	1	7	1	1	5	2	2	3	2
12 Ganda	3	4	4	7	1	1	5	4	3	3	2
13 Mbuti Pygmies	3	1	1	1	6	6	5	1	-	3	3
14 Nkundo Mongo	5	3	3	6	1	1	5	1	1	3	-
15 Banen	3	1	1	6	1	1	5	1	1	2	2
16 Tiv	5	2	2	6	1	1	3	1	-	2	2
17 Ibo	5	2	2	6	1	1	5	2	-	2	-
18 Fon	4	4	4	6	1	1	5	4	1	3	1
19 Ashanti	5	3	3	6	2	2	3	4	6	2	1
20 Mende	5	2	2	6	1	1	3	4	1	2	2
21 Wolof	4	3	3	6	2	2	2	5	1	2	2
22 Bambara	5	2	2	7	1	1	2	4	1	3	2
23 Tallensi	5	1	1	7	1	1	2	2	4	2	3
24 Songhai	4	4	4	7	1	1	2	5	1	-	3
25 Fulani	4	2	2	4	1	1	2	1	1	3	2
26 Hausa	4	4	4	7	1	1	2	5	1	1	1
27 Kanuri	4	4	4	7	6	6	4	4	-	2	2
28 Azande	4	3	3	6	1	1	5	4	1	1	1
29 Fur	4	4	4	7	3	3	4	4	1	3	2
30 Otoro Nuba	4	2	2	7	1	1	4	2	-	1	2
31 Shilluk	4	2	2	6	1	1	4	5	6	2	1
32 Ingassana	4	1	1	7	5	5	-	1	1	3	2
33 Kafa	4	5	5	7	1	1	-	4	-	-	-
34 Masai	4	2	2	4	1	1	3	1	-	2	2
35 Konso	4	2	2	7	1	1	4	1	1	2	-
36 Somali	4	3	3	4	1	1	3	2	6	1	2
37 Amhara	4	4	4	7	6	6	2	5	3	3	2
38 Bisharin	4	2	2	4	1	1	1	1	-	-	2
39 Nubians	4	1	1	7	1	1	1	2	-	3	3
40 Teda	4	2	2	7	1	1	5	4	4	1	1
41 Tuareg	4	3	3	4	3	3	3	3	2	2	1
42 Riffians	4	2	2	7	1	1	1	2	-	1	2
43 Anc. Egyptians	3	5	5	7	6	6	2	5	1	3	1
44 Hebrews	4	3	3	7	1	1	1	4	-	3	1
45 Babylonians	3	4	4	7	6	6	1	5	-	3	1
46 Rwala Bedouins	4	2	2	4	1	1	1	2	-	3	1
47 Turks	3	4	4	7	6	6	2	5	6	3	3
48 Gheg Albanians	4	3	3	7	1	1	4	1	1	1	3

Variable	23	24	25	26	27	28	29	30	31	32	33
1 Nama Hottentot	2	2	1	3	2	1	1	1	2	2	1
2 Kung Bushmen	3	3	-	-	-	-	2	2	2	-	3
3 Thonga	3	1	1	3	-	-	2	2	-	1	1
4 Lozi	3	1	1	-	-	-	1	1	-	1	-
5 Mbundu	-	2	1	-	-	-	1	1	-	1	-
6 Kongo	3	2	1	1	1	1	2	2	2	1	2
7 Ila	1	2	2	2	3	-	1	2	1	2	1
8 Nyakyusa	2	1	2	3	2	2	1	1	2	2	1
9 Hadza	2	-	-	-	-	-	-	-	-	-	-
10 Luguru	2	3	-	-	-	-	2	2	-	-	-
11 Kikuyu	2	1	1	3	2	1	2	2	2	2	1
12 Ganda	2	1	1	3	2	2	1	1	-	1	1
13 Mbuti Pygmies	3	3	-	-	-	-	-	-	-	-	3
14 Nkundo Mongo	2	2	2	2	1	1	2	2	2	2	-
15 Banen	2	2	1	-	1	1	1	1	2	-	2
16 Tiv	2	2	-	3	1	1	1	1	2	2	3
17 Ibo	2	1	1	3	1	1	1	2	2	2	2
18 Fon	1	1	1	3	2	-	1	1	1	1	1
19 Ashanti	3	1	1	3	1	-	1	2	2	1	1
20 Mende	-	1	1	3	2	-	1	1	-	1	1
21 Wolof	1	1	1	3	1	-	1	1	1	-	2
22 Bambara	2	2	1	1	2	-	1	1	-	1	1
23 Tallensi	3	2	2	2	1	-	2	2	2	3	3
24 Songhai	2	1	1	3	1	-	2	2	-	-	-
25 Fulani	1	2	-	-	-	-	1	1	-	2	2
26 Hausa	3	1	1	3	-	-	1	1	-	1	2
27 Kanuri	3	1	1	1	2	-	1	1	2	1	2
28 Azande	2	1	1	3	2	2	1	1	1	1	1
29 Fur	2	2	1	3	1	1	2	2	1	1	2
30 Otoro Nuba	2	1	2	3	1	1	1	2	-	2	1
31 Shilluk	2	2	1	3	1	-	2	2	-	2	1
32 Ingassana	-	2	-	3	-	1	2	2	-	1	-
33 Kafa	-	-	-	-	-	-	-	-	-	-	-
34 Masai	2	1	1	3	1	-	2	2	2	2	1
35 Konso	2	1	1	1	2	1	1	1	-	-	1
36 Somali	2	2	1	3	1	1	1	1	-	2	1
37 Amhara	3	1	1	1	2	-	1	1	-	2	1
38 Bisharin	2	2	-	3	-	-	1	1	-	-	1
39 Nubians	3	3	-	-	-	-	-	-	-	-	3
40 Teda	1	2	2	3	2	-	1	1	-	3	1
41 Tuareg	2	1	1	3	2	-	1	1	-	2	1
42 Riffians	2	2	2	3	3	-	1	1	1	3	1
43 Anc. Egyptians	2	1	1	-	2	-	1	1	1	1	2
44 Hebrews	3	1	1	1	2	2	1	1	1	1	1
45 Babylonians	1	1	1	-	2	-	1	1	-	1	2
46 Rwala Bedouins	1	2	1	1	2	1	1	1	-	2	1
47 Turks	3	1	1	1	1	2	1	1	-	1	1
48 Gheg Albanians	2	2	2	3	3	1	2	2	1	-	1

Variable	34	35	36	37	38	39	40	41	42	43	44
1 Nama Hottentot	-	1	2	2	4	4	2	2	1	1	2
2 Kung Bushmen	-	3	2	3	4	4	2	2	2	2	2
3 Thonga	1	1	2	1	4	4	2	2	2	1	2
4 Lozi	-	-	-	-	1	1	1	1	1	1	2
5 Mbundu	-	-	-	2	1	1	2	1	2	1	1
6 Kongo	-	-	2	2	2	2	2	2	2	2	2
7 Ila	1	1	1	1	4	4	2	2	2	1	1
8 Nyakyusa	2	2	2	1	1	1	1	2	1	1	2
9 Hadza	-	-	-	-	2	2	-	-	-	-	-
10 Luguru	-	-	2	3	4	4	2	2	2	1	2
11 Kikuyu	2	1	2	1	1	1	2	2	2	1	2
12 Ganda	2	1	-	1	1	1	2	2	1	1	1
13 Mbuti Pygmies	-	3	2	3	4	4	2	2	2	2	2
14 Nkundo Mongo	-	-	2	2	4	4	2	2	2	1	2
15 Banen	-	1	2	2	4	4	2	2	1	2	2
16 Tiv	-	-	1	2	1	1	2	2	1	1	1
17 Ibo	-	1	1	1	4	4	2	2	2	1	1
18 Fon	-	1	1	1	1	1	1	1	2	1	1
19 Ashanti	2	-	1	1	1	1	2	2	2	1	1
20 Mende	2	1	1	1	2	2	2	2	1	1	2
21 Wolof	2	1	1	1	4	4	1	2	2	1	2
22 Bambara	2	1	1	1	1	1	2	2	1	1	2
23 Tallensi	2	3	2	2	2	2	2	2	2	2	2
24 Songhai	-	-	1	1	4	4	2	2	2	2	2
25 Fulani	-	-	2	-	4	4	2	2	1	1	1
26 Hausa	-	1	1	1	4	4	1	1	1	1	2
27 Kanuri	-	3	1	1	1	1	1	1	2	1	2
28 Azande	2	1	1	1	1	1	1	1	1	1	1
29 Fur	1	3	1	1	2	2	2	2	2	1	2
30 Otoro Nuba	1	1	-	1	2	2	2	2	2	1	1
31 Shilluk	2	1	-	-	4	4	2	2	2	1	2
32 Ingassana	-	-	-	-	2	2	2	2	1	2	1
33 Kafa	-	-	-	-	-	-	-	-	-	-	-
34 Masai	1	1	1	1	4	4	2	2	2	1	2
35 Konso	3	1	2	2	2	2	2	2	1	1	1
36 Somali	-	-	1	1	1	1	2	2	1	1	1
37 Amhara	2	1	1	1	1	1	1	2	2	2	2
38 Bisharin	-	-	2	2	2	2	2	2	1	1	2
39 Nubians	-	-	2	3	2	2	-	-	-	-	-
40 Teda	1	1	1	1	4	4	2	1	1	1	2
41 Tuareg	1	-	1	1	1	1	2	1	1	1	2
42 Riffians	1	1	1	1	2	2	2	2	1	1	2
43 Anc. Egyptians	1	1	1	1	1	1	1	1	1	1	2
44 Hebrews	2	3	1	1	1	1	1	2	1	1	2
45 Babylonians	-	-	-	1	4	4	1	2	1	2	2
46 Rwala Bedouins	2	-	1	1	1	1	2	2	1	1	1
47 Turks	2	1	-	2	2	2	1	2	2	2	2
48 Gheg Albanians	1	1	1	1	2	2	2	2	2	1	2

Variable	45	46	47	Population Density
1 Nama Hottentot	1	1	2	1
2 Kung Bushmen	2	1	2	1
3 Thonga	2	1	2	6
4 Lozi	2	1	2	4
5 Mbundu	1	2	2	4
6 Kongo	1	1	2	-
7 Ila	2	1	2	-
8 Nyakyusa	1	2	2	5
9 Hadza	-	-	-	2
10 Luguru	1	1	2	7
11 Kikuyu	2	1	2	6
12 Ganda	2	2	1	6
13 Mbuti Pygmies	2	2	2	2
14 Nkundo Mongo	2	2	2	3
15 Banen	1	1	2	4
16 Tiv	1	2	2	5
17 Ibo	1	1	2	7
18 Fon	1	1	2	5
19 Ashanti	2	2	2	5
20 Mende	2	1	2	5
21 Wolof	2	2	2	6
22 Bambara	2	2	2	4
23 Tallensi	1	1	2	6
24 Songhai	2	1	2	4
25 Fulani	2	1	2	3
26 Hausa	2	1	2	5
27 Kanuri	2	2	2	-
28 Azande	1	1	2	3
29 Fur	2	1	2	4
30 Otoro Nuba	1	2	2	4
31 Shilluk	1	1	2	5
32 Ingassana	2	2	2	-
33 Kafa	-	-	-	5
34 Masai	2	2	2	3
35 Konso	1	1	2	6
36 Somali	1	2	2	4
37 Amhara	2	2	2	5
38 Bisharin	2	1	2	-
39 Nubians	-	-	-	-
40 Teda	1	1	2	1
41 Tuareg	1	1	2	1
42 Riffians	1	1	2	6
43 Anc. Egyptians	2	2	1	-
44 Hebrews	1	2	2	7
45 Babylonians	2	1	1	7
46 Rwala Bedouins	1	1	2	2
47 Turks	1	1	2	5
48 Gheg Albanians	1	2	2	5

Variable	1	2	3	4	5	6	7	8	9	10	11
49 Romans	C	1	2	2	3	7	7	7	8	8	8
50 Basques	C	1	2	2	3	5	5	5	6	6	6
51 Irish	C	1	1	1	3	5	5	5	5	5	5
52 Lapps	C	1	1	1	4	1	1	1	1	1	1
53 Yurak Samoyed	E	2	1	1	4	1	1	1	1	1	1
54 Russians	C	1	2	2	3	8	8	8	6	6	6
55 Abkhaz	C	2	1	1	4	7	7	7	4	4	4
56 Armenians	C	1	1	1	3	7	7	7	5	5	5
57 Kurd	C	2	1	1	1	3	3	3	6	6	6
58 Basseri	E	2	1	1	1	1	1	1	3	3	3
59 Punjabi	E	1	1	1	4	7	7	7	4	4	4
60 Gond	E	3	1	1	6	7	7	7	2	2	2
61 Toda	E	-	1	1	6	3	3	3	1	1	1
62 Santal	E	2	1	1	4	7	7	7	3	3	3
63 Uttar Pradesh	E	2	1	1	4	7	7	7	6	6	6
64 Burusho	E	2	1	1	3	7	7	7	3	3	3
65 Kazak	E	6	1	1	6	2	2	2	5	5	5
66 Khalka Mongols	E	1	1	1	3	2	2	2	1	1	1
67 Lolo	E	2	1	1	6	7	7	7	3	3	3
68 Lepcha	E	2	1	1	4	5	5	5	3	3	3
69 Garo	E	2	3	3	3	7	7	7	4	4	4
70 Sema Naga	E	2	1	1	2	7	7	7	4	4	4
71 Burmese	E	1	3	3	1	7	7	7	5	5	5
72 Palaung	E	2	1	1	1	7	7	7	2	2	2
73 Vietnamese	E	2	1	1	2	7	7	7	5	5	5
74 Rhade	E	1	3	3	2	6	6	6	5	5	5
75 Khmer	E	2	2	2	2	8	8	8	7	7	7
76 Siamese	E	2	3	3	3	7	7	7	6	6	6
77 Semai	E	2	2	2	3	4	4	4	2	2	2
78 Nicobarese	E	2	3	3	3	6	6	6	4	4	4
79 Andamanese	E	1	2	2	3	2	2	2	1	1	1
80 Vedda	E	1	3	3	6	2	2	2	1	1	1
81 Tanala	E	2	1	1	5	7	7	7	4	4	4
82 Nebri Sembilan	E	2	3	3	2	7	7	7	6	6	6
83 Javanese	I	2	3	3	3	7	7	7	5	5	5
84 Balinese	I	1	1	1	1	7	7	7	5	5	5
85 Iban	I	1	2	2	3	7	7	7	2	2	2
86 Badjau	I	2	2	2	3	1	1	1	4	4	4
87 Toradja	I	2	3	3	3	7	7	7	3	3	3
88 Tobelorese	I	1	1	1	4	7	7	7	-	-	-
89 Alorese	I	2	1	1	4	8	8	8	3	3	3
90 Tiwi	I	6	1	1	3	1	1	1	3	3	3
91 Aranda	I	5	1	1	6	1	1	1	1	1	1
92 Orokaiva	I	2	1	1	2	7	7	7	2	2	2
93 Kimam	I	2	1	1	2	5	5	5	5	5	5
94 Kapauku	I	6	1	1	6	7	7	7	3	3	3
95 Kwoma	I	4	1	1	6	6	6	6	4	4	4
96 Manus	I	2	1	1	2	7	7	7	4	4	4

Variable	12	13	14	15	16	17	18	19	20	21	22
49 Romans	-	-	-	7	6	6	-	5	-	3	1
50 Basques	-	-	-	7	6	6	1	5	-	3	3
51 Irish	3	3	3	7	6	6	2	5	-	3	2
52 Lapps	3	1	1	4	6	6	4	2	-	3	3
53 Yurok Samoyed	4	1	1	3	1	1	1	2	-	-	2
54 Russians	5	5	5	7	6	6	3	1	3	3	2
55 Abkhaz	-	-	-	4	1	1	2	-	4	-	2
56 Armenians	4	4	4	7	6	6	2	2	6	3	-
57 Kurd	4	3	3	7	1	1	2	4	1	2	2
58 Basseri	3	2	2	4	1	1	2	2	1	2	2
59 Punjabi	4	5	5	7	1	1	1	-	1	3	2
60 Gond	4	2	2	6	1	1	3	2	1	3	3
61 Toda	4	2	2	4	2	2	1	1	7	3	3
62 Santal	4	2	2	7	1	1	4	1	1	3	3
63 Uttar Pradesh	4	5	5	7	1	1	1	-	5	3	2
64 Burusho	5	2	2	7	1	1	4	5	-	-	2
65 Kazak	4	4	4	4	1	1	2	4	-	2	2
66 Khalka Mongols	4	4	4	4	1	1	3	4	1	2	2
67 Lolo	4	1	1	7	1	1	4	4	1	1	2
68 Lepcha	4	2	2	7	1	1	4	2	1	3	3
69 Garo	4	2	2	6	3	3	3	2	2	1	2
70 Sema Naga	4	3	3	6	1	1	4	4	-	2	2
71 Burmese	3	4	4	7	6	6	2	5	3	2	3
72 Palaung	3	1	1	6	6	6	4	2	1	3	3
73 Vietnamese	4	5	5	7	1	1	2	5	6	2	1
74 Rhade	4	1	1	6	3	3	2	2	2	-	3
75 Khmer	5	5	5	7	6	6	4	-	1	3	1
76 Siamese	4	5	5	7	6	6	4	5	6	2	2
77 Semai	4	1	1	6	6	6	3	1	-	3	3
78 Nicobarese	4	1	1	6	6	6	2	1	7	3	3
79 Andamanese	3	1	1	2	6	6	3	1	7	2	3
80 Vedda	4	1	1	1	3	3	3	1	-	3	3
81 Tanala	5	2	2	7	1	1	3	4	6	3	3
82 Negri Sembilan	5	5	5	7	3	3	-	-	6	2	3
83 Javanese	3	5	5	7	6	6	5	5	6	3	2
84 Balinese	4	3	3	7	5	5	1	4	1	2	-
85 Iban	4	1	1	6	6	6	5	1	7	2	2
86 Badjau	3	1	1	2	6	6	2	2	1	3	3
87 Toradja	4	2	2	6	6	6	4	2	6	-	-
88 Tobelorese	4	3	3	6	6	6	4	1	6	-	-
89 Alorese	4	3	3	6	1	1	5	2	-	3	3
90 Tiwi	4	1	1	1	3	3	6	1	1	2	3
91 Aranda	3	1	1	1	2	2	6	1	1	3	3
92 Orokaiva	4	1	1	6	1	1	5	1	7	2	2
93 Kimam	5	1	1	7	1	1	2	1	-	2	-
94 Kapauku	4	2	2	6	1	1	3	2	5	2	3
95 Kwoma	4	1	1	5	1	1	4	1	-	3	3
96 Manus	4	1	1	2	2	2	2	2	-	3	3

Variable	23	24	25	26	27	28	29	30	31	32	33
49 Romans	2	1	1	1	1	-	1	1	1	1	2
50 Basques	2	1	1	3	1	2	1	1	-	1	2
51 Irish	3	1	1	3	1	1	1	1	2	2	2
52 Lapps	3	-	-	-	-	-	2	2	-	-	3
53 Yurak Samoyed	-	2	-	-	-	-	1	1	-	-	3
54 Russians	3	1	1	3	2	-	1	1	1	1	2
55 Abkhaz	2	1	1	3	-	-	1	1	-	1	2
56 Armenians	2	-	-	-	-	-	2	2	1	-	3
57 Kurd	2	2	1	1	1	-	1	1	-	1	1
58 Basseri	2	2	1	-	1	-	1	1	-	1	2
59 Punjabi	3	1	1	3	1	2	1	1	-	1	2
60 Gond	3	-	-	-	-	-	1	2	2	-	-
61 Toda	3	3	-	-	-	-	2	2	-	-	3
62 Santal	3	3	1	-	2	-	1	1	1	2	2
63 Uttar Pradesh	2	2	2	3	1	-	1	1	-	2	2
64 Burusho	3	-	1	3	-	-	2	2	-	-	-
65 Kazak	-	2	2	3	2	-	1	1	2	1	2
66 Khalka Mongols	2	1	1	-	1	-	2	2	-	1	2
67 Lolo	2	2	1	3	1	1	2	2	2	2	1
68 Lepcha	3	3	-	-	-	-	2	2	-	-	3
69 Garo	1	2	2	3	2	-	1	1	-	2	-
70 Sema Naga	3	2	1	1	1	-	1	1	2	1	1
71 Burmese	2	1	1	3	-	-	1	1	1	1	1
72 Palaung	3	1	1	3	-	1	1	1	-	-	2
73 Vietnamese	1	1	1	3	3	2	2	2	1	1	2
74 Rhade	1	-	1	3	-	-	2	2	-	1	2
75 Khmer	1	1	1	-	1	1	1	1	1	1	-
76 Siamese	3	1	1	-	1	-	1	1	-	1	2
77 Semai	2	3	2	-	1	-	2	2	-	3	3
78 Nicobarese	3	3	-	-	-	-	1	1	-	-	3
79 Andamanese	3	2	-	3	1	1	2	2	2	3	-
80 Vedda	3	-	-	-	-	-	2	2	-	-	3
81 Tanala	3	1	1	3	2	1	1	1	-	1	2
82 Negri Sembilan	3	1	1	3	2	-	1	1	2	1	2
83 Javanese	1	1	1	3	1	1	1	1	1	1	2
84 Balinese	3	1	1	-	1	-	1	1	1	1	2
85 Iban	2	2	1	3	1	1	1	1	1	2	1
86 Badjau	3	3	-	-	-	-	2	2	-	-	3
87 Toradja	-	2	2	3	1	1	1	1	2	1	-
88 Tobelorese	-	-	-	-	-	-	-	-	-	-	-
89 Alorese	3	2	2	3	1	1	2	2	2	3	3
90 Tiwi	3	2	2	2	3	1	2	2	2	3	1
91 Aranda	3	2	1	2	3	-	2	2	2	2	1
92 Orokaiva	2	2	2	3	3	1	1	2	-	2	1
93 Kimam	-	2	-	-	1	1	1	2	-	-	1
94 Kapauku	3	2	1	1	1	1	2	2	1	2	2
95 Kwoma	3	1	1	3	1	-	1	1	2	3	1
96 Manus	-	2	1	3	2	1	1	1	2	2	2

Variable	34	35	36	37	38	39	40	41	42	43	44
49 Romans	1	2	1	1	1	1	1	1	2	1	2
50 Basques	-	3	1	2	4	4	1	2	2	2	2
51 Irish	-	2	2	2	1	1	1	2	2	2	2
52 Lapps	-	3	2	3	2	2	2	2	2	2	2
53 Yurak Samoyed	-	-	2	-	4	4	2	2	1	1	2
54 Russians	1	2	1	1	1	1	1	2	2	2	2
55 Abkhaz	1	-	-	-	4	4	1	2	2	2	2
56 Armenians	-	-	-	-	4	4	2	2	2	2	2
57 Kurd	-	1	-	1	4	4	1	1	1	1	2
58 Basseri	-	-	2	2	1	1	1	2	1	2	2
59 Punjabi	-	-	1	1	2	2	1	2	2	2	1
60 Gond	-	-	2	-	2	2	2	2	2	2	1
61 Toda	-	3	2	3	2	2	2	2	2	2	2
62 Santal	-	2	2	2	4	4	2	2	1	2	2
63 Uttar Pradesh	-	3	1	2	1	1	1	2	1	2	2
64 Burusho	-	-	2	1	4	4	2	2	2	1	2
65 Kazak	-	-	2	1	2	2	2	2	1	1	2
66 Khalka Mongols	-	-	2	2	4	4	2	2	2	2	2
67 Lolo	1	1	1	1	3	3	2	2	2	1	2
68 Lepcha	-	3	2	3	4	4	2	2	2	2	2
69 Garo	1	2	1	-	3	3	2	2	1	1	1
70 Sema Naga	-	1	-	1	1	1	2	2	1	2	1
71 Burmese	-	1	1	2	2	2	1	2	2	2	2
72 Palaung	-	-	2	3	2	2	1	2	2	2	2
73 Vietnamese	-	1	1	2	4	4	2	2	2	2	2
74 Rhade	-	-	2	3	4	4	2	2	2	2	2
75 Khmer	-	-	-	1	1	1	1	2	2	1	2
76 Siamese	-	-	1	-	2	2	2	2	1	2	2
77 Semai	-	-	2	3	4	4	2	2	2	2	2
78 Nicobarese	-	-	2	3	4	4	2	2	1	2	2
79 Andamanese	-	-	-	-	4	4	2	2	2	1	2
80 Vedda	-	3	2	2	4	4	2	2	2	2	2
81 Tanala	1	-	2	2	2	2	1	2	2	1	2
82 Negri Sembilan	-	-	-	3	2	2	2	1	1	2	2
83 Javanese	-	-	1	2	1	1	1	2	2	2	2
84 Balinese	-	-	2	3	2	2	1	2	2	1	2
85 Iban	-	1	1	1	1	1	2	2	1	1	1
86 Badjau	-	-	2	3	2	2	2	2	2	2	2
87 Toradja	1	1	-	-	-	-	2	2	1	1	1
88 Tobelorese	-	-	-	-	-	-	-	-	-	-	-
89 Alorese	-	1	2	1	2	2	2	2	2	2	2
90 Tiwi	-	1	1	2	2	2	2	2	2	2	2
91 Aranda	2	-	1	1	2	2	2	2	2	1	2
92 Orokaiva	-	1	1	1	3	3	2	2	2	1	1
93 Kimam	-	-	-	-	4	4	2	2	2	2	1
94 Kapauku	1	1	2	2	2	2	2	2	2	1	2
95 Kwoma	1	1	1	2	2	2	2	2	1	2	1
96 Manus	-	3	2	3	2	2	2	2	1	1	2

Variable	45	46	47	Population Density
49 Romans	2	1	1	7
50 Basques	2	1	2	4
51 Irish	1	1	2	5
52 Lapps	1	1	2	3
53 Yurak Samoyed	1	1	2	1
54 Russians	2	1	2	4
55 Abkhaz	2	1	2	5
56 Armenians	2	1	2	5
57 Kurd	1	2	2	6
58 Basseri	2	2	2	3
59 Punjabi	1	1	2	6
60 Gond	2	2	2	4
61 Toda	2	2	2	5
62 Santal	2	1	2	6
63 Uttar Pradesh	1	2	2	7
64 Burusho	2	2	2	3
65 Kazak	1	2	2	3
66 Khalka Mongols	2	1	2	3
67 Lolo	1	2	2	3
68 Lepcha	2	1	2	5
69 Garo	1	1	2	5
70 Sema Naga	1	2	2	-
71 Burmese	2	1	2	6
72 Palaung	2	2	2	-
73 Vietnamese	2	1	2	7
74 Rhade	1	2	2	7
75 Khmer	2	1	2	5
76 Siamese	2	1	2	6
77 Semai	2	2	2	1
78 Nicobarese	2	2	2	5
79 Andamanese	1	2	2	3
80 Vedda	1	1	2	1
81 Tanala	2	1	2	3
82 Negri Sembilan	1	1	2	7
83 Javanese	2	1	1	7
84 Balinese	2	1	2	7
85 Iban	2	1	2	4
86 Badjau	2	2	2	1
87 Toradja	1	1	2	4
88 Tobelorese	-	-	-	4
89 Alorese	1	2	2	7
90 Tiwi	1	1	2	2
91 Aranda	1	1	1	2
92 Orokaiva	1	1	1	3
93 Kimam	1	1	2	2
94 Kapauku	1	1	2	6
95 Kwoma	1	2	2	5
96 Manus	1	2	2	5

Variable	12	13	14	15	16	17	18	19	20	21	22
97 New Ireland	4	1	1	5	3	3	5	1	5	2	-
98 Trobrianders	3	2	2	6	3	3	3	4	2	2	3
99 Siuai	4	1	1	6	3	3	4	2	5	2	-
100 Tikopia	4	2	2	5	1	1	6	4	1	3	3
101 Pentecost	4	1	1	6	2	2	-	2	-	2	2
102 Moau Fijians	5	5	5	2	1	1	-	-	1	2	-
103 Ajie	4	2	2	7	1	1	5	1	1	2	2
104 Maori	5	2	2	5	5	5	2	4	1	1	3
105 Marquesans	4	2	2	6	6	6	1	4	1	3	2
106 Samoans	5	3	3	6	5	5	3	4	1	2	-
107 Gilbertese	4	2	2	5	5	5	5	4	4	3	-
108 Marshallese	4	2	2	6	3	3	2	4	2	2	3
109 Trukese	4	1	1	5	3	3	2	1	2	2	2
110 Yapese	4	2	2	6	2	2	5	5	2	2	3
111 Palauans	5	3	3	6	3	3	5	4	2	2	3
112 Ifugao	4	1	1	7	6	6	5	2	-	2	-
113 Atayal	4	2	2	6	1	1	5	1	6	-	-
114 Chinese	5	5	5	7	1	1	4	-	7	2	3
115 Manchu	5	-	-	7	1	1	2	2	6	3	-
116 Koreans	4	3	3	7	1	1	4	5	4	3	3
117 Japanese	4	5	5	7	6	6	4	5	6	3	1
118 Ainu	4	2	2	2	3	3	3	1	1	2	2
119 Gilyak	3	1	1	2	1	1	1	1	1	2	3
120 Yukaghir	4	1	1	3	6	6	1	1	4	3	2
121 Chukchee	4	1	1	2	6	6	1	2	5	3	2
122 Ingalik	3	1	1	2	6	6	2	2	4	3	3
123 Aleut	4	1	1	2	4	4	1	4	1	2	1
124 Copper Eskimo	3	1	1	2	6	6	2	1	-	3	3
125 Montagnais	4	1	1	3	6	6	1	1	1	3	3
126 Micmac	3	2	2	3	6	6	1	1	1	3	2
127 North Saulteaux	4	1	1	3	1	1	1	1	1	3	3
128 Slave	3	1	1	3	6	6	1	1	7	3	3
129 Kaska	3	1	1	2	3	3	4	1	7	3	3
130 Eyak	4	2	2	2	3	3	1	2	2	3	2
131 Haida	4	1	1	2	3	3	1	4	2	2	1
132 Bellacoola	4	1	1	2	5	5	1	4	7	3	3
133 Twana	4	1	1	2	6	6	2	2	5	3	3
134 Yurok	4	1	1	2	6	6	2	2	5	2	3
135 East Pomo	4	1	1	1	6	6	5	2	2	3	3
136 Yokuts	4	1	1	1	1	1	4	2	1	3	3
137 Paiute	3	1	1	1	6	6	6	1	7	3	3
138 Klamath	4	1	1	2	6	6	2	2	7	3	1
139 Kutenai	3	2	2	2	6	6	1	1	5	3	3
140 Gros Ventre	3	2	2	3	6	6	1	1	5	1	1
141 Hidatsa	4	1	1	7	3	3	6	1	7	3	2
142 Pawnee	4	2	2	6	3	3	6	4	1	3	2
143 Omaha	4	2	2	5	1	1	5	1	5	3	3
144 Huron	4	2	2	5	3	3	6	2	2	3	1

Variable	23	24	25	26	27	28	29	30	31	32	33
97 New Ireland	-	-	-	3	1	1	2	2	2	1	2
98 Trobrianders	2	2	1	2	2	1	1	2	-	1	1
99 Siuai	2	2	1	1	2	1	1	1	-	3	2
100 Tikopia	3	3	-	-	-	-	2	2	-	2	3
101 Pentecost	-	1	2	3	1	-	2	2	-	3	2
102 Moau Fijians	-	2	1	1	2	-	1	2	1	1	2
103 Ajie	2	2	1	3	1	1	1	1	-	2	-
104 Maori	3	2	1	3	2	1	1	1	1	2	1
105 Marquesans	2	2	1	1	2	1	1	2	2	1	2
106 Samoans	-	2	1	1	2	-	1	1	1	1	2
107 Gilbertese	-	2	1	-	1	-	1	1	-	-	2
108 Marshallese	2	2	1	1	2	1	1	1	2	1	1
109 Trukese	2	2	1	1	1	1	2	2	2	2	2
110 Yapese	3	2	1	1	1	1	2	2	2	1	2
111 Palauans	3	1	1	-	-	-	1	1	-	1	1
112 Ifugao	2	2	1	3	3	1	2	2	2	2	1
113 Atayal	-	-	-	-	-	-	2	2	-	-	-
114 Chinese	2	1	1	1	3	-	1	1	-	1	2
115 Manchu	-	1	1	-	-	-	2	2	-	1	-
116 Koreans	2	1	1	-	3	2	2	2	-	1	2
117 Japanese	1	1	1	1	2	1	1	1	1	1	1
118 Ainu	2	3	1	3	1	1	1	1	1	1	2
119 Gilyak	3	2	2	3	1	1	2	2	1	2	3
120 Yukaghir	2	2	1	3	-	1	1	1	-	2	2
121 Chukchee	3	2	-	3	2	-	2	2	-	2	1
122 Ingalik	3	2	1	3	2	-	2	2	-	3	3
123 Aleut	2	2	1	3	1	1	1	2	1	2	1
124 Copper Eskimo	3	3	-	-	-	-	-	-	-	-	3
125 Montagnais	2	2	2	2	2	-	1	2	-	2	2
126 Micmac	2	2	1	3	2	1	1	2	-	2	1
127 North Sauteaux	3	3	-	-	-	-	-	-	-	-	3
128 Slave	3	2	2	3	1	-	2	2	-	2	2
129 Kaska	3	1	2	3	2	1	2	2	-	2	2
130 Eyak	2	2	1	3	2	2	2	2	2	1	2
131 Haida	-	2	1	3	1	1	1	1	1	2	2
132 Bellacoola	2	1	2	3	2	1	1	1	2	2	2
133 Twana	3	1	2	-	2	-	2	2	-	2	2
134 Yurok	3	2	2	3	1	1	2	2	1	2	2
135 East Pomo	3	2	1	3	1	1	2	1	2	2	2
136 Yokuts	3	2	2	2	1	-	2	2	2	2	3
137 Paiute	-	2	-	3	-	-	2	2	-	2	2
138 Klamath	2	2	2	3	2	2	2	2	-	2	2
139 Kutenai	2	2	2	3	2	-	1	1	-	2	1
140 Gros Ventre	-	1	2	3	-	-	1	2	2	2	1
141 Hidatsa	2	1	1	3	1	1	1	1	1	2	2
142 Pawnee	1	1	2	3	2	2	1	2	1	2	2
143 Omaha	-	2	1	3	1	1	1	1	-	2	1
144 Huron	1	2	1	3	1	-	1	2	2	1	1

Variable	34	35	36	37	38	39	40	41	42	43	44
97 New Ireland	-	-	2	2	2	2	2	2	2	1	2
98 Trobrianders	-	-	-	1	2	2	2	2	2	1	1
98 Siuai	2	1	1	1	2	2	2	1	2	2	2
100 Tikopia	-	-	-	-	2	2	2	2	2	2	2
101 Pentecost	-	-	-	2	2	2	2	2	2	2	2
102 Moau Fijians	1	1	1	-	4	4	2	2	2	1	1
103 Ajie	-	-	-	-	4	4	2	2	1	1	1
104 Maori	1	-	1	1	1	1	2	2	1	1	2
105 Marquesans	-	1	1	1	3	3	2	2	2	1	1
106 Samoans	3	1	1	1	3	3	1	2	2	1	1
107 Gilbertese	-	-	-	-	2	2	2	2	1	2	2
108 Marshallese	2	1	1	1	2	2	1	2	2	2	2
109 Trukese	-	-	2	2	2	2	2	2	2	2	2
110 Yapese	-	2	-	1	4	4	2	2	2	1	2
111 Palauans	-	-	-	1	2	2	1	1	2	1	2
112 Ifugao	-	1	1	1	2	2	2	2	2	1	2
113 Atayal	-	-	-	-	1	1	-	-	-	-	-
114 Chinese	-	-	2	2	4	4	1	2	2	2	2
115 Manchu	1	-	-	1	4	4	2	2	2	2	2
116 Koreans	-	2	2	2	4	4	2	2	2	2	2
117 Japanese	1	2	1	1	4	4	1	2	1	2	2
118 Ainu	-	-	-	2	4	4	2	2	1	1	2
119 Gilyak	-	-	-	2	2	2	2	2	2	1	2
120 Yukaghir	2	1	-	2	4	4	2	1	2	1	2
121 Chukchee	-	2	1	1	1	1	2	2	2	1	2
122 Ingalik	-	3	1	1	2	2	2	2	2	1	2
123 Aleut	2	1	1	1	4	4	2	2	2	1	1
124 Copper Eskimo	-	-	1	3	2	2	-	-	-	-	-
125 Montagnais	-	2	2	1	2	2	2	2	2	1	1
126 Micmac	-	1	1	1	2	2	2	2	2	1	1
127 North Sauteaux	-	3	1	3	1	1	-	-	-	-	-
128 Slave	-	3	2	2	2	2	2	2	2	1	2
129 Kaska	-	1	2	2	4	4	2	2	2	1	2
130 Eyak	-	3	1	2	2	2	2	2	2	1	2
131 Haida	-	2	2	2	1	1	2	2	1	1	1
132 Bellacoola	2	2	-	2	-	-	2	2	1	1	1
133 Twana	-	3	2	3	2	2	2	2	2	1	2
134 Yurok	-	2	1	2	2	2	2	2	2	2	2
135 East Pomo	-	2	2	2	2	2	2	2	1	2	1
136 Yokuts	-	-	-	-	4	4	2	2	2	1	2
137 Paiute	-	3	2	2	4	4	2	2	2	2	2
138 Klamath	-	1	1	1	2	2	2	2	2	1	2
139 Kutenai	2	1	-	1	2	2	2	2	1	1	1
140 Gros Ventre	-	1	1	-	4	4	2	2	2	1	1
141 Hidatsa	2	1	2	2	4	4	2	2	1	1	1
142 Pawnee	1	1	2	1	4	4	2	2	2	1	1
143 Omaha	-	1	2	2	3	3	2	2	1	1	1
144 Huron	-	1	1	1	2	2	2	2	2	2	2

Variable	45	46	47	Population Density
97 New Ireland	1	2	2	4
98 Trobrianders	2	2	2	5
99 Siuai	1	1	2	5
100 Tikopia	2	1	2	6
101 Pentecost	1	1	2	4
102 Moau Fijians	1	1	2	7
103 Ajie	2	1	2	3
104 Maori	1	1	2	2
105 Marquesans	1	2	2	5
106 Samoans	1	1	2	6
107 Gilbertese	1	1	2	6
108 Marshallese	1	2	2	6
109 Trukese	1	2	2	7
110 Yapese	1	2	2	5
111 Palauans	2	2	2	5
112 Ifugao	1	2	2	6
113 Atayal	1	-	-	4
114 Chinese	2	1	2	7
115 Manchu	1	1	2	6
116 Koreans	2	1	2	7
117 Japanese	2	1	2	7
118 Ainu	2	2	2	2
119 Gilyak	1	1	2	1
120 Yukaghir	1	2	2	1
121 Chukchee	2	2	2	1
122 Ingalik	1	2	2	1
123 Aleut	1	2	2	4
124 Copper Eskimo	-	-	-	1
125 Montagnais	1	2	2	1
126 Micmac	1	2	2	1
127 North Sauteaux	-	-	-	1
128 Slave	1	2	2	2
129 Kaska	1	1	1	1
130 Eyak	1	1	2	1
131 Haida	1	1	2	2
132 Bellacoola	1	1	2	3
133 Twana	1	1	2	3
134 Yurok	1	2	2	3
135 East Pomo	1	2	2	3
136 Yokuts	1	2	2	4
137 Paiute	1	1	2	1
138 Klamath	1	1	2	2
139 Kutenai	1	1	2	1
140 Gros Ventre	1	1	-	1
141 Hidatsa	1	1	2	1
142 Pawnee	1	1	2	2
143 Omaha	1	1	2	3
144 Huron	1	1	2	5

Variable	1	2	3	4	5	6	7	8	9	10	11
145 Creek	N	2	3	3	2	7	7	7	4	4	4
146 Natchez	N	3	1	1	3	7	7	7	5	5	5
147 Comanche	N	3	2	2	1	1	1	1	4	4	4
148 Apache	N	3	3	3	3	1	1	1	2	2	2
149 Zuni	N	1	3	3	2	7	7	7	6	6	6
150 Havasupai	N	2	1	1	1	3	3	3	3	3	3
151 Papago	N	3	1	1	4	3	3	3	4	4	4
152 Huichol	N	2	2	2	1	6	6	6	4	4	4
153 Aztec	N	-	1	1	2	7	7	7	8	8	8
154 Popoluca	N	-	1	1	-	7	7	7	5	5	5
155 Yucatec Maya	S	1	1	1	3	8	8	8	7	7	7
156 Miskito	S	2	3	3	3	7	7	7	4	4	4
157 Bribri	S	2	2	2	3	5	5	5	2	2	2
158 Cuna	S	2	3	3	1	7	7	7	5	5	5
159 Goajiro	S	5	3	3	3	1	1	1	2	2	2
160 Haitians	S	2	1	1	2	7	7	7	7	7	7
161 Callinago	S	2	3	3	2	6	6	6	4	4	4
162 Warrau	S	2	3	3	1	3	3	3	2	2	2
163 Yanomamo	S	6	1	1	2	4	4	4	3	3	3
164 Carib	S	2	2	2	3	5	5	5	1	1	1
165 Saramacca	S	5	3	3	2	7	7	7	3	3	3
166 Mundurucu	S	2	3	3	3	7	7	7	3	3	3
167 Cubeo	S	2	1	1	6	4	4	4	1	1	1
168 Cayapa	S	1	1	1	1	5	5	5	5	5	5
169 Jivaro	S	6	2	2	3	5	5	5	2	2	2
170 Amahuaca	S	2	1	1	4	5	5	5	1	1	1
171 Inca	S	1	1	1	1	7	7	7	3	3	3
172 Aymara	S	1	1	1	1	7	7	7	5	5	5
173 Siriono	S	3	3	3	2	2	2	2	2	2	2
174 Nambicuara	S	2	1	1	3	2	2	2	2	2	2
175 Trumai	S	2	1	1	3	7	7	7	1	1	1
176 Timbira	S	1	3	3	2	2	2	2	4	4	4
177 Tupinamba	S	5	3	3	3	7	7	7	5	5	5
178 Botocudo	S	3	-	-	4	1	1	1	2	2	2
179 Shavante	S	6	3	3	3	2	2	2	4	4	4
180 Aweikoma	S	6	2	2	1	1	1	1	1	1	1
181 Cayua	S	1	1	1	3	7	7	7	1	1	1
182 Lengua	S	2	3	3	3	1	1	1	3	3	3
183 Abipon	S	2	2	2	3	1	1	1	4	4	4
184 Mapuche	S	5	1	1	5	6	6	6	2	2	2
185 Tehuelche	S	2	1	1	3	1	1	1	3	3	3
186 Yahgan	S	2	1	1	4	1	1	1	1	1	1

Variable	12	13	14	15	16	17	18	19	20	21	22
145 Creek	5	3	3	5	3	3	5	1	2	3	1
146 Natchez	4	2	2	5	6	6	5	3	3	3	2
147 Comanche	4	1	1	3	6	6	1	1	7	3	1
148 Apache	4	1	1	1	6	6	6	1	7	3	1
149 Zuni	5	1	1	7	3	3	2	1	2	3	2
150 Havasupai	4	1	1	5	4	4	2	1	7	3	3
151 Papago	4	1	1	5	6	6	2	1	1	3	2
152 Huichol	4	1	1	6	6	6	2	2	6	3	3
153 Aztec	4	3	3	7	5	5	2	5	1	3	1
154 Popoluca	3	1	1	6	6	6	1	1	6	3	-
155 Yucatec Maya	4	2	2	6	1	1	1	5	-	1	1
156 Miskito	4	2	2	1	6	6	4	1	-	3	1
157 Bribri	3	1	1	6	3	3	4	1	-	-	2
158 Cuna	4	2	2	6	6	6	1	2	6	-	3
159 Goajiro	4	1	1	4	3	3	3	2	2	2	3
160 Haitians	4	1	1	5	6	6	-	-	3	1	3
161 Callinago	4	1	1	2	3	3	2	1	7	-	2
162 Warrau	4	1	1	1	6	6	-	1	7	3	3
163 Yanomamo	5	1	1	5	1	1	2	1	5	1	2
164 Carib	3	1	1	5	4	4	3	1	7	3	3
165 Saramacca	4	3	3	5	3	3	5	1	2	3	1
166 Mundurucu	4	1	1	5	1	1	3	1	1	3	2
167 Cubeo	3	1	1	5	1	1	5	1	1	2	3
168 Cayapa	4	1	1	6	6	6	5	1	1	-	3
169 Jivaro	3	1	1	6	6	6	5	1	-	1	1
170 Amahuaca	3	1	1	5	6	6	3	1	-	-	-
171 Inca	3	5	5	7	6	6	2	5	-	3	2
172 Aymara	4	-	-	6	1	1	4	2	-	2	2
173 Siriono	4	1	1	3	6	6	1	1	1	3	3
174 Nambicuara	4	1	1	1	6	6	5	1	7	2	2
175 Trumai	4	1	1	5	4	4	3	1	1	3	3
176 Timbira	5	1	1	5	3	3	5	1	-	3	2
177 Tupinamba	4	2	2	5	6	6	5	2	1	1	1
178 Botocudo	4	1	1	1	6	6	6	1	7	1	2
179 Shavante	5	1	1	1	1	1	5	1	5	2	2
180 Aweikoma	3	1	1	3	6	6	1	1	-	1	2
181 Cayua	3	1	1	5	6	6	2	1	-	-	3
182 Lengua	4	1	1	3	6	6	1	1	1	-	1
183 Abipon	3	1	1	3	6	6	1	4	1	3	2
184 Mapuche	5	1	1	7	1	1	2	2	1	2	1
185 Tehuelche	4	1	1	3	6	6	1	1	1	2	2
186 Yahgan	3	1	1	2	6	6	3	1	-	2	3

Variable	23	24	25	26	27	28	29	30	31	32	33
145 Creek	1	2	1	3	1	1	1	2	-	1	1
146 Natchez	2	1	1	3	2	1	1	1	-	2	2
147 Comanche	1	2	2	3	1	1	1	1	2	2	1
148 Apache	1	2	1	3	3	-	1	1	-	2	1
149 Zuni	2	1	1	3	1	1	2	2	-	2	2
150 Havasupai	3	2	2	3	1	2	1	2	2	2	2
151 Papago	1	2	1	3	2	2	1	2	1	2	1
152 Huichol	3	3	-	-	-	-	-	-	-	-	3
153 Aztec	1	1	1	1	1	2	1	1	-	1	1
154 Popoluca	-	1	-	-	-	-	-	-	-	-	2
155 Yucatec Maya	1	1	1	3	3	-	1	1	-	1	1
156 Miskito	1	1	1	3	3	-	1	1	-	1	1
157 Bribrí	3	-	1	3	1	-	1	1	1	1	1
158 Cuna	-	-	-	-	-	1	1	-	-	-	-
159 Goajiro	3	2	-	3	3	1	2	2	-	2	2
160 Haitians	3	1	2	3	-	-	1	1	-	1	1
161 Callinago	2	2	1	3	2	-	1	2	-	1	2
162 Warrau	3	-	-	-	2	-	2	2	-	-	3
163 Yanomamo	3	2	2	3	2	2	2	2	2	2	1
164 Carib	3	-	-	-	-	-	-	-	-	-	-
165 Saramacca	1	2	1	3	1	-	1	1	1	1	-
166 Mundurucu	3	1	1	3	3	2	1	2	-	1	1
167 Cubeo	2	2	-	3	-	-	2	2	2	2	3
168 Cayapa	3	-	-	-	-	-	2	2	-	-	-
169 Jivaro	1	2	2	1	3	1	1	2	2	1	1
170 Amahuaca	1	2	-	3	3	-	2	2	-	-	-
171 Inca	2	1	1	1	2	1	1	1	-	1	1
172 Aymara	2	2	1	3	2	1	2	2	-	1	-
173 Siriono	2	2	-	3	2	2	2	2	-	2	3
174 Nambicuara	-	2	2	3	-	-	2	2	-	2	2
175 Trumai	2	2	-	3	2	2	2	2	2	3	3
176 Timbira	2	1	1	3	1	1	2	2	1	1	2
177 Tupinamba	1	2	1	3	3	-	1	2	-	2	1
178 Botocudo	2	-	-	3	3	-	2	2	-	2	-
179 Shavante	3	1	1	3	3	2	2	2	-	2	2
180 Aweikoma	2	2	2	3	3	2	2	2	1	3	3
181 Cayua	1	2	-	3	-	-	2	2	-	-	-
182 Lengua	-	2	1	3	1	1	2	2	2	2	1
183 Abipon	2	2	1	3	-	-	2	2	2	2	1
184 Mapuche	1	2	1	3	3	1	1	2	-	1	1
185 Tehuelche	2	2	1	3	2	-	2	2	-	2	2
186 Yahgan	3	2	2	2	1	-	2	2	2	3	-

Variable	34	35	36	37	38	39	40	41	42	43	44
145 Creek	-	1	1	1	4	4	2	2	2	1	1
146 Natchez	-	1	-	1	4	4	2	2	1	2	1
147 Comanche	2	1	1	1	4	4	2	2	1	1	1
148 Apache	-	1	1	1	1	1	2	2	1	1	2
149 Zuni	-	1	2	2	4	4	2	2	2	1	2
150 Havasupai	-	3	-	3	2	2	2	2	2	2	1
151 Papago	1	1	1	2	2	2	2	2	2	2	1
152 Huichol	-	3	2	3	2	2	-	-	-	-	-
153 Aztec	2	1	1	1	1	1	2	1	1	1	1
154 Popoluca	-	-	-	2	1	1	-	-	-	-	-
155 Yucatec Maya	2	1	1	2	4	4	2	2	1	1	1
156 Miskito	-	1	-	-	1	1	2	1	2	1	2
157 Bribri	-	1	-	-	1	1	1	2	2	1	1
158 Cuna	-	-	-	-	2	2	1	2	2	2	2
159 Goajiro	2	-	-	2	2	2	2	2	2	1	2
160 Haitians	-	3	1	1	2	2	1	2	2	2	2
161 Callinago	2	1	-	-	1	1	2	2	2	1	1
162 Warrau	-	-	2	2	2	2	2	2	2	2	2
163 Yanomamo	1	1	1	1	3	3	2	2	2	1	2
164 Carib	-	-	2	2	2	2	2	2	2	2	2
165 Saramacca	-	-	1	-	1	1	1	1	1	1	2
166 Mundurucu	2	1	1	1	1	1	2	2	2	1	1
167 Cubeo	-	2	2	3	3	3	2	2	2	1	2
168 Cayapa	-	-	-	3	2	2	2	2	2	2	2
169 Jivaro	-	1	1	1	1	1	2	2	2	1	1
170 Amahuaca	-	-	-	-	4	4	2	2	2	2	2
171 Inca	2	1	2	2	1	1	1	1	2	1	1
172 Aymara	-	1	-	2	4	4	2	2	2	1	2
173 Siriono	-	3	2	3	4	4	2	2	2	1	2
174 Nambicuara	-	-	2	2	4	4	2	2	2	1	2
175 Trumai	-	3	2	3	4	4	2	2	2	2	2
176 Timbira	-	1	2	2	2	2	2	2	2	2	2
177 Tupinamba	-	1	1	1	1	1	2	2	2	1	1
178 Botocudo	-	-	1	-	4	4	2	2	2	2	2
179 Shavante	1	-	1	1	2	2	2	2	2	2	2
180 Aweikoma	-	3	1	1	4	4	2	2	2	1	2
181 Cayua	-	-	2	3	4	4	2	2	2	2	2
182 Lengua	-	-	-	-	2	2	2	2	2	1	2
183 Abipon	-	3	-	2	1	1	2	2	2	1	2
184 Mapuche	1	1	1	1	1	1	2	2	2	1	1
185 Tehuelche	2	-	-	1	-	-	2	2	2	1	2
186 Yahgan	-	-	-	-	2	2	2	2	2	2	2

Variable	45	46	47	Population Density
145 Creek	1	2	2	3
146 Natchez	1	1	2	4
147 Comanche	1	1	2	1
148 Apache	1	1	2	1
149 Zuni	1	1	1	3
150 Havasupai	1	1	2	1
151 Papago	1	1	2	5
152 Huichol	-	-	-	3
153 Aztec	1	1	2	7
154 Popoluca	-	-	-	5
155 Yucatec Maya	1	1	2	-
156 Miskito	2	2	2	4
157 Bribri	1	2	2	-
158 Cuna	2	2	2	5
159 Goajiro	1	2	2	2
160 Haitians	2	2	2	6
161 Callinago	1	2	2	4
162 Warrau	1	2	2	2
163 Yanomamo	1	2	2	2
164 Carib	2	2	2	1
165 Saramacca	1	1	2	4
166 Mundurucu	2	2	2	1
167 Cubeo	1	1	2	2
168 Cayapa	2	2	1	3
169 Jivaro	1	2	1	2
170 Amahuaca	1	2	2	1
171 Inca	2	1	2	4
172 Aymara	1	1	1	6
173 Siriono	1	1	2	1
174 Nambicuara	1	2	2	2
175 Trumai	1	1	2	1
176 Timbira	1	1	2	3
177 Tupinamba	1	2	2	2
178 Botocudo	1	2	2	1
179 Shavante	1	1	2	1
180 Aweikoma	1	2	1	1
181 Cayua	2	1	2	2
182 Lengua	1	1	2	1
183 Abipon	1	2	2	2
184 Mapuche	1	1	2	5
185 Tehuelche	1	2	2	1
186 Yahgan	1	2	2	2

APPENDIX D
Comparison of Phi Coefficients Among Geographical Regions

	<u>World</u>	<u>Africa</u>	<u>C-Med</u>	<u>Eurasia</u>	<u>Pacific</u>	<u>N Amer</u>	<u>S Amer</u>
Variable 2 with variables:							
21	.159	.225	.500	-.078	-.242	.159	.333
22	.225	.556	.207	.096	-.171	.270	.223
23	.081	.256	.034	-.254	-.355	.294	.068
24	.026	.507	-.187	-.234	-.088	.139	-.259
25	-.111	-.181	-.219	.410	.089	-.053	-.224
26	.039	-.081	.140	-.140	.123	-.017	-.017
27	-.144	.018	-.283	-.346	-.134	-.028	-.225
28	-.084	-.333	.316	0	0	-.121	-.238
29	.112	.408	.168	-.050	-.123	.269	.018
30	.014	.242	.168	-.141	-.071	.237	-.208
31	-.030	.250	-.134	-.461	-.049	.458	.214
32	-.228	-.132	.257	-.093	-.402	-.290	-.256
33	.268	.429	.240	.233	.197	.093	-.032
34	-.094	0	-.444	0	.350	.333	.791
35	.251	.478	.255	-.207	.237	.279	0
36	.276	.522	.122	-.010	.252	.214	.616
37	.321	.601	.282	.406	-.078	.177	.537
38	.176	.454	.067	.164	-.023	-.078	.114
39	-.011	.197	-.067	.311	.023	-.425	-.015
40	-.110	.234	-.182	-.187	-.282	0	-.104
41	.056	.234	.071	-.078	-.168	-.137	.014
42	.175	.207	.355	-.013	.193	-.237	.104
43	.304	.428	.588	.204	.049	-.099	.244
44	.237	.399	.544	.208	.039	.168	.078
45	.097	0	.240	-.050	.023	0	.389
46	-.043	-.099	-.342	-.330	.205	.016	.035
47	-.023	.110	-.271	0	.044	-.207	.153

Variable 3 with variables:

21	.069	-.040	0	-.124	-.226	.107	.492
22	.019	.604	0	.191	-.171	-.309	-.131
23	-.042	.404	0	-.045	-.509	-.183	.096
24	-.010	.117	0	-.060	-.167	-.031	-.182
25	-.183	-.177	0	0	-.106	-.415	-.523
26	.179	-.165	0	.259	-.036	.309	.271
27	-.009	-.306	0	.122	-.094	-.033	-.201
28	-.007	-.125	0	-.218	0	-.118	.100
29	.018	.240	0	-.166	-.145	.145	-.220

30	.070	.354	0	-.202	-.218	.043	-.042
31	-.026	.234	0	.043	.107	-.169	-.730
32	-.123	-.362	0	.037	-.424	-.291	-.045
33	.129	.165	0	.064	.039	.119	.156
34	.103	.239	0	.632	.300	-.300	.158
35	-.035	0	0	-.189	-.222	-.074	-.228
36	.062	.182	0	-.164	.123	.267	-.126
37	.115	.215	0	.347	0	-.017	-.095
38	-.009	.025	0	.227	-.073	-.007	-.292
39	-.064	-.061	0	.019	-.258	.316	-.421
40	.095	.167	0	.176	-.312	0	-.033
41	-.078	.198	0	-.445	-.470	.163	-.149
42	.111	.289	0	-.137	.159	.043	-.022
43	.010	.222	0	.092	.015	-.081	-.098
44	.045	.040	0	.007	.065	.320	-.042
45	-.108	-.079	0	-.246	.275	0	-.040
46	.024	0	0	-.091	.260	-.208	.083
47	-.075	0	0	0	-.035	-.337	.267

Variable 4 with variables:

21	.115	.331	.327	.078	-.121	.138	.076
22	.029	.415	-.127	.115	.015	.189	-.155
23	-.027	.205	-.221	-.120	-.266	.299	.042
24	.036	.060	-.302	.067	.217	.245	.219
25	-.018	.200	-.256	.225	-.218	.020	.230
26	.024	.243	.184	.096	-.020	.096	-.184
27	-.011	.362	0	-.363	.247	-.273	-.109
28	.037	.556	.189	-.258	0	-.233	.033
29	0	.089	-.064	.141	0	.068	-.080
30	-.108	-.053	-.064	.033	-.150	-.375	-.015
31	-.070	.196	-.356	.243	-.141	.354	-.214
32	-.097	-.132	-.385	.090	-.066	.109	-.017
33	.008	.171	.359	0	-.187	.066	-.210
34	-.030	.284	-.577	0	.598	.218	0
35	.171	.679	.444	.146	-.237	.157	.189
36	-.088	.380	-.353	-.112	-.182	.033	-.122
37	-.116	.060	-.190	-.164	-.250	.132	-.102
38	-.138	-.029	-.258	.104	-.082	.163	-.253
39	-.058	-.007	-.086	-.048	-.183	-.075	-.067
40	-.167	-.062	-.232	.078	-.275	0	-.046
41	-.004	.208	-.229	.230	.124	.051	.144
42	.004	-.055	.063	-.204	.044	-.375	.115
43	-.009	.257	.071	.114	-.013	.164	-.142
44	.034	.150	.247	.050	-.053	.229	-.142
45	.171	.293	.359	.122	.135	0	-.058
46	.143	.372	-.199	0	.173	.112	.349
47	-.241	-.410	-.598	-.019	.155	.077	-.354

Variable 5 with variables:

21	-.036	.071	-.354	.433	.200	-.545	-.356
22	.046	.241	-.172	.149	.182	0	.117
23	.049	.123	-.263	.107	.309	.299	-.139
24	.034	.238	.292	.086	.016	-.234	.259
25	.095	.139	.464	.015	-.047	.212	.154
26	.022	-.174	.472	.015	-.026	.162	-.140
27	.027	-.056	.224	.259	-.141	.113	-.046
28	-.049	0	-.447	.293	0	-.073	-.267
29	.061	.196	.146	-.021	.102	.127	.168
30	.003	-.116	.146	.046	.101	.191	.237
31	.092	.612	-.478	.386	-.039	-.411	.356
32	.147	.183	.408	.089	.284	.204	.181
33	.040	.337	-.329	.247	.007	.011	.013
34	-.303	.184	-.350	.316	-.350	-.655	-.500
35	.083	.329	-.270	.258	-.237	.067	.125
36	.160	.704	-.181	.243	.061	.042	-.099
37	.089	.489	-.232	.005	.060	.391	-.069
38	.053	-.061	.090	.202	.204	-.055	.115
39	.076	-.012	-.161	.059	.289	-.022	.106
40	.086	.181	.343	-.004	.233	0	.185
41	.069	.181	.145	-.030	.139	.097	.138
42	-.033	-.137	-.071	.160	-.110	.191	.111
43	.072	.331	-.194	.036	-.083	.110	.200
44	.093	.187	.187	-.043	-.167	.089	.050
45	-.038	-.154	-.161	-.219	-.113	0	-.248
46	-.060	-.012	.241	-.126	-.083	-.159	-.022
47	.022	-.216	.192	0	-.106	-.233	.163

Variable 6 with variables:

21	-.128	-.042	.149	.206	-.256	-.328	.066
22	-.047	-.111	.142	.171	-.308	-.270	.036
23	-.081	-.193	.239	-.050	-.389	-.240	-.084
24	-.288	-.141	-.688	-.476	-.162	-.233	-.123
25	-.274	-.014	-.156	-.196	-.221	-.334	-.155
26	-.102	.081	-.022	-.275	.322	-.009	-.042
27	-.044	-.167	.050	.232	-.312	-.210	.191
28	-.061	.174	.378	.408	0	-.191	-.071
29	-.209	0	.076	-.188	-.408	-.064	-.632
30	-.191	-.040	.076	-.145	-.291	-.141	-.429
31	-.129	.075	0	-.051	-.262	-.258	.069
32	-.420	-.236	-.535	-.216	-.247	-.380	-.637
33	-.056	-.079	.358	-.153	.187	.091	-.565

34	-.033	.640	-.059	-1.00	-.395	.447	0
35	-.127	-.265	.100	-.141	.105	.095	-.472
36	-.133	.058	-.313	-.259	.015	-.040	-.179
37	-.115	-.176	-.053	-.128	-.181	.084	-.095
38	-.181	-.279	-.060	.068	-.136	-.267	-.368
39	-.099	-.369	-.071	-.063	.136	.080	-.120
40	-.278	.019	-.482	-.272	-.155	0	-.333
41	-.081	.019	.079	.094	-.092	-.282	-.249
42	.015	-.015	.482	.015	-.192	-.141	-.200
43	.094	-.196	.302	.410	-.174	-.030	-.067
44	-.064	-.207	.462	-.253	-.246	-.110	-.447
45	.208	-.365	.358	.188	-.050	0	.447
46	-.013	.099	.029	-.230	.055	.033	-.067
47	-.061	-.110	-.219	0	.256	-.106	-.098

Variable 7 with variables:

21	.111	.256	-.115	-.297	-.018	-.066	.204
22	-.084	0	-.266	-.383	.061	-.180	-.025
23	.019	.308	-.147	.012	.135	-.239	-.141
24	-.151	-.256	.134	-.291	-.088	-.177	-.332
25	-.034	-.316	-.156	-.225	.281	0	-.141
26	.070	0	-.131	-.096	.123	0	.067
27	.127	.596	.050	.199	.116	0	-.059
28	.129	.293	0	0	0	0	.141
29	-.111	-.167	-.062	-.251	-.208	0	.137
30	-.178	-.230	-.062	-.227	-.279	0	-.142
31	-.284	-.068	-.389	0	-.519	0	-.179
32	-.043	-.378	-.268	-.093	-.065	0	.225
33	-.018	-.357	.066	-.190	-.107	-.125	.172
34	-.003	-.083	-.059	0	-.060	0	.500
35	.070	.094	-.060	-.207	-.139	-.212	.250
36	-.056	-.032	.024	-.310	.023	-.216	.058
37	-.083	-.280	-.205	-.268	.008	-.155	.132
38	-.027	-.271	.147	-.152	-.268	-.077	.167
39	-.007	-.145	.104	-.365	-.164	.130	.253
40	-.089	-.118	.181	-.219	-.306	0	-.078
41	-.078	-.118	-.234	-.091	.112	0	-.201
42	-.174	-.077	-.395	-.079	-.204	0	-.162
43	-.089	-.015	-.320	-.270	-.193	0	.054
44	.011	.089	-.182	-.133	-.016	0	.281
45	.088	.474	.066	-.120	.230	0	.040
46	-.184	.037	-.178	-.204	-.344	0	-.197
47	-.036	-.158	-.154	0	-.228	0	.184

Variable 8 with variables:

21	.034	-.220	-.044	-.017	.177	.361	-.238
22	.111	.098	.068	.089	.171	.348	-.014
23	.062	-.130	-.107	.038	.145	.347	.221
24	.392	.378	.500	.629	.184	.283	.411
25	.277	.309	.250	.309	-.129	.334	.279
26	.041	-.058	.085	.308	-.302	.009	-.017
27	-.060	-.472	-.082	-.311	.061	.210	-.145
28	-.055	-.408	-.316	-.408	0	.191	-.051
29	.286	.167	-.023	.340	.444	.064	.518
30	.323	.263	-.023	.284	.439	.141	.567
31	.351	0	.389	.051	.651	.258	.134
32	.431	.548	.675	.256	.197	.380	.462
33	.069	.408	-.370	.280	-.017	-.047	.444
34	.030	-.386	.091	1.00	.316	-.447	-.500
35	.062	.152	-.025	.244	.080	-.015	.158
36	.173	.082	.261	.459	-.032	.141	.123
37	.175	.415	.201	.305	.115	-.027	-.024
38	.194	.524	-.053	.034	.336	-.305	.209
39	.100	.480	-.012	.296	.070	-.133	-.120
40	.335	.101	.300	.399	.383	0	.413
41	.140	.101	.099	-.030	-.048	.282	.444
42	.127	.089	-.144	.036	.310	.141	.357
43	-.016	.185	-.037	-.211	.289	.030	.017
44	.052	.089	-.282	.324	.168	.110	.190
45	-.268	-.158	-.370	-.099	-.185	0	-.494
46	.161	-.123	.104	.349	.289	-.033	.255
47	.087	.253	.309	0	.055	.106	-.075

Variable 9 with variables:

21	-.085	-.118	0	-.354	.013	.200	-.356
22	-.139	-.138	-.068	-.050	-.105	-.311	.149
23	-.116	-.018	.107	-.314	-.023	-.366	.053
24	-.348	0	-.408	-.385	-.211	-.459	-.222
25	-.188	.061	-.259	.055	-.218	-.478	-.038
26	-.075	-.122	-.212	.015	.112	-.189	.141
27	-.127	-.342	-.204	.160	-.060	-.365	.194
28	-.008	-.429	.500	.258	0	-.149	-.267
29	-.239	-.150	-.216	-.231	-.068	-.208	-.281
30	-.255	-.032	-.216	-.265	.101	-.337	-.321
31	-.208	-.055	.289	-.098	-.408	-.033	0
32	-.268	.123	-.471	-.359	-.175	-.284	-.247
33	-.004	.206	.055	.088	.081	-.094	-.371

34	.077	.083	.055	0	-.316	.333	.250
35	-.132	-.094	.158	-.337	-.154	-.437	-.094
36	-.212	-.391	-.158	-.287	-.105	.136	-.380
37	-.037	-.094	.050	.081	.181	-.010	-.009
38	-.117	-.140	-.168	-.127	-.043	-.183	.163
39	.002	-.263	.012	-.200	.289	.122	.046
40	-.312	-.158	-.378	-.244	-.218	0	-.234
41	-.138	-.158	-.123	-.133	.139	-.369	.155
42	-.129	0	-.098	-.115	.288	-.337	-.207
43	.085	-.079	.137	.164	.089	.101	.124
44	.037	-.183	.101	.219	.012	-.064	-.092
45	.101	-.354	.061	-.040	.264	0	.277
46	-.149	-.060	.013	-.165	-.256	-.330	.041
47	-.151	.147	-.286	0	-.106	-.189	-.303

Variable 10 with variables:

21	-.034	-.411	0	-.131	.221	-.075	.131
22	-.195	-.417	-.187	-.149	.435	-.226	-.411
23	-.138	-.511	-.067	-.178	.241	-.301	-.025
24	-.238	-.126	-.355	-.333	.162	-.062	-.520
25	-.254	.132	-.233	-.196	-.134	-.240	-.533
26	-.140	-.130	-.040	-.275	-.172	-.200	-.141
27	-.143	0	-.354	.354	-.042	-.474	-.060
28	.040	0	.250	.488	0	-.026	-.071
29	-.280	-.423	-.062	-.257	.129	-.533	-.322
30	-.234	-.295	-.062	-.335	.154	-.244	-.335
31	-.069	-.145	0	-.051	.094	-.141	-.069
32	-.292	.254	-.514	-.470	-.344	-.091	-.324
33	-.128	-.336	.175	-.153	.282	-.247	-.389
34	-.060	.433	.068	-1.00	-.598	-.218	.189
35	-.219	-.478	-.040	-.141	.196	-.238	-.403
36	-.130	-.411	-.224	-.310	.367	.187	-.414
37	-.150	-.208	-.178	-.289	.321	-.084	-.233
38	-.148	-.356	.029	-.133	.008	-.115	-.254
39	-.045	-.273	.168	-.123	-.008	.329	-.376
40	-.244	-.169	-.461	-.304	-.044	0	-.232
41	-.024	-.169	.222	.077	.482	-.143	-.302
42	-.175	-.293	.247	-.288	.058	-.244	-.243
43	.020	-.507	.118	.227	.147	.258	-.178
44	-.147	-.293	.114	-.079	.099	-.621	-.108
45	.084	-.378	.461	.007	-.202	0	.253
46	-.056	.064	.284	-.290	.310	-.189	-.081
47	.055	-.079	-.154	0	.338	.076	.024

Variable 11 with variables:

21	-.047	.161	0	-.189	-.181	.238	-.408
22	.059	.158	.096	.103	-.471	-.029	.550
23	.025	.371	.189	-.107	-.236	.012	.068
24	-.104	.084	-.164	-.027	-.314	-.323	.358
25	.040	0	-.109	.220	-.040	-.169	.452
26	.058	-.060	-.208	.279	.233	.032	-.042
27	.009	-.342	.070	-.169	-.012	.158	.191
28	-.042	-.429	.378	-.293	0	-.109	-.071
29	.035	.096	-.196	.043	-.165	.344	.103
30	-.021	.137	-.196	.090	-.050	.043	.083
31	-.133	.026	.289	-.051	-.420	.098	.069
32	.011	.010	-.102	.077	.168	-.147	.144
33	.115	.436	-.099	.236	-.169	.157	.137
34	.113	-.158	0	1.00	.316	.447	0
35	.072	.267	.209	-.149	-.277	-.126	.322
36	-.070	-.045	.035	.031	-.385	-.071	.154
37	.105	.060	.258	.344	-.164	.073	.244
38	.029	.131	-.225	.015	-.042	-.045	.410
39	.044	-.045	-.143	-.061	.243	-.206	.441
40	-.063	-.037	-.042	.077	-.139	0	.051
41	-.105	-.037	-.332	-.197	-.302	-.167	.444
42	.043	.194	-.324	.179	.184	-.043	.085
43	.060	.262	.058	-.074	-.053	-.167	.289
44	.171	.022	.019	.276	-.075	.549	.038
45	.016	-.084	-.324	-.043	.385	0	-.038
46	-.086	-.099	-.228	.136	-.476	-.091	.119
47	-.192	.192	-.198	0	-.377	-.235	-.274

Variable 12 with variables:

21	-.114	-.134	-.462	.307	-.375	-.019	-.036
22	-.159	-.053	-.233	-.266	-.105	-.289	-.120
23	-.079	-.130	-.250	-.074	.023	-.293	.068
24	.049	.134	.402	-.030	.076	.040	-.217
25	-.055	.106	.210	-.062	.185	-.212	-.300
26	-.031	-.289	.094	-.210	.146	-.200	.503
27	-.050	-.286	.141	.363	-.149	-.304	.178
28	-.010	-.408	-.333	.333	0	.233	.141
29	-.035	.268	.011	.010	-.080	-.223	-.071
30	-.013	.445	.011	.034	-.145	-.240	-.005
31	-.076	-.250	-.375	.141	.094	-.240	-.069
32	-.005	.108	.245	-.138	.178	-.226	-.040
33	.026	0	-.308	.076	.079	0	.172

34	-.159	.030	-.033	0	-.395	-.218	-.250
35	-.062	-.189	-.149	.372	0	.005	-.250
36	-.165	-.522	-.344	-.010	.015	.040	-.285
37	-.144	-.078	-.407	-.268	-.060	-.053	-.143
38	.065	.006	.042	.062	.130	-.078	.114
39	.051	-.122	.129	.249	.160	.014	-.015
40	.160	.222	.352	.316	.082	0	.081
41	-.043	-.018	-.059	-.104	-.109	-.107	.014
42	-.078	.470	-.146	-.133	-.226	-.240	-.174
43	-.045	-.144	-.508	.041	-.070	.130	-.035
44	-.102	-.259	-.229	-.152	-.080	-.181	.078
45	-.051	-.087	.103	-.010	-.429	0	-.078
46	-.067	-.007	.145	-.108	-.070	-.144	-.104
47	.301	.330	.588	0	.521	.193	.153

Variable 13 with variables:

21	.001	-.299	.008	.261	-.013	-.094	-.357
22	.231	.204	.110	.286	-.015	.258	.203
23	.039	-.130	-.157	.291	-.220	.223	.207
24	.467	.462	.564	.651	.438	.109	.144
25	.232	.032	.490	.120	.047	.258	.218
26	.159	-.182	-.039	.464	.020	.352	.350
27	.059	-.263	.394	-.100	.060	.273	.211
28	-.273	-.816	-.333	-.683	0	-.136	.302
29	.258	.272	-.103	.269	.238	.213	.327
30	.347	.342	-.103	.316	.235	.089	.484
31	.340	.354	-.167	.071	.535	0	.418
32	.533	.462	.588	.614	.455	.680	.333
33	.061	.447	-.177	.162	-.262	.359	.204
34	-.025	-.184	.051	.250	-.060	-.218	-.250
35	.066	.033	-.149	.258	.196	.234	.185
36	.224	.066	.589	.422	-.061	.267	-.034
37	.252	.272	.468	.251	.037	.309	-.182
38	.296	.597	.336	.178	.043	.244	.378
39	.031	.267	-.103	.162	-.043	-.085	.161
40	.560	.586	.590	.342	.668	0	.599
41	.373	.586	.159	.055	.197	.694	.802
42	.109	.150	-.406	.164	-.288	.089	.466
43	-.039	.355	-.259	-.352	.083	.154	.209
44	-.051	.320	-.451	.081	-.012	.229	.145
45	-.261	-.081	-.275	-.015	-.264	0	-.145
46	.069	-.195	-.454	.410	.083	-.112	.354
47	.045	.275	.193	0	.106	-.077	-.086

Variable 14 with variables:

21	.108	.308	.271	.175	-.027	-.043	-.191
22	.276	.476	.586	.214	-.179	.405	.208
23	.184	.193	.393	.116	-.138	.509	.348
24	.305	.388	.243	.412	.027	.139	.356
25	.274	.091	0	.187	.204	.246	.378
26	.182	-.081	0	.308	.220	-.017	.135
27	-.014	-.122	0	-.277	.071	-.105	-.022
28	-.043	-.333	0	-.488	0	-.185	.302
29	.436	.497	.406	.351	.247	.418	.612
30	.407	.309	.406	.303	.222	.084	.709
31	.246	.302	0	.225	.430	-.228	.418
32	.450	.132	0	.646	.576	.551	.363
33	.209	.553	.302	-.042	-.257	.513	.439
34	.175	.284	0	.612	.316	.149	-.378
35	.259	.564	.333	-.189	.026	.476	.410
36	.214	.592	.513	.135	.032	.214	.201
37	.323	.478	.519	.097	.313	.465	-.061
38	.174	.330	.221	.120	.159	-.104	.354
39	-.009	.122	-.202	.214	-.159	-.080	.113
40	.353	.259	.192	.289	.411	0	.451
41	.228	.259	.120	-.090	-.028	.260	.668
42	.290	.259	.227	.267	.022	.084	.536
43	.103	.584	.295	-.267	.228	.070	.222
44	.127	.442	.093	.263	.070	.315	.271
45	-.288	.087	-.209	-.351	-.223	0	-.271
46	.151	-.169	-.147	.343	.087	.173	.149
47	-.101	.121	.063	0	-.149	-.191	-.160

Variable 15 with variables:

21	-.118	-.149	0	.048	-.278	.281	0
22	-.156	-.364	0	.058	-.367	-.329	.174
23	-.137	-.205	0	-.148	-.389	-.294	.068
24	-.298	-.334	0	-.437	-.217	-.324	-.123
25	-.260	.125	0	-.129	-.055	-.395	-.091
26	-.117	-.132	0	-.243	.250	.017	-.042
27	-.151	-.248	0	.194	-.461	-.279	-.017
28	.016	.174	0	.408	0	.263	-.386
29	-.261	-.060	0	-.165	-.183	-.418	-.360
30	-.234	.036	0	-.134	-.211	-.084	-.271
31	-.140	-.196	0	.141	-.127	-.258	-.179
32	-.395	-.218	0	-.465	-.149	-.341	-.480
33	-.153	-.289	0	-.016	-.007	-.134	-.389

34	-.239	0	0	-1.00	-.060	-.577	0
35	-.271	-.478	0	-.026	-.294	-.386	-.351
36	-.047	-.380	0	-.010	-.061	.137	-.066
37	-.166	-.442	0	-.109	-.292	.004	.051
38	-.130	-.320	0	-.006	-.183	-.022	-.079
39	-.077	-.291	0	-.218	-.082	.119	-.120
40	-.305	-.154	0	-.306	-.208	0	-.333
41	-.122	-.154	0	.182	-.124	-.260	-.028
42	-.106	-.010	0	-.072	-.044	-.084	-.200
43	.150	-.435	0	.537	.013	.269	.067
44	-.071	-.263	0	-.186	-.139	-.315	-.298
45	.282	-.120	0	.313	.067	0	.298
46	-.111	.066	0	-.103	.013	-.330	-.200
47	-.016	-.072	0	0	.145	-.083	-.098

Variable 16 with variables:

21	.054	.094	.101	-.201	-.468	-.194	.524
22	.077	.658	-.196	.158	.149	-.810	.076
23	-.068	.316	-.182	-.115	-.333	-.756	-.029
24	.074	.362	-.098	.077	-.059	-.463	.179
25	-.100	-.139	-.234	.050	-.192	-.218	-.272
26	-.005	-.451	.258	.258	-.507	.667	0
27	-.045	-.236	-.134	.160	.381	.327	-.671
28	-.226	-.194	-.200	-.354	0	.218	-.548
29	.065	.402	.294	-.037	-.016	-.102	-.169
30	.111	.325	.294	-.083	-.221	.218	-.293
31	-.140	.135	0	-.060	.577	-.447	-1.00
32	-.130	-.299	-.043	-.033	-.447	-.327	-.239
33	.193	.387	.139	.177	-.143	.083	.408
34	.091	0	-.378	0	-1.00	0	.548
35	.085	0	.655	.378	0	-.218	-.250
36	.081	.333	-.258	-.055	0	.189	.333
37	.185	.443	-.213	.327	-.069	-.327	.478
38	.085	.262	-.218	.204	.228	.241	-.314
39	-.096	.055	-.293	.083	-.424	.261	-.378
40	.057	.149	.237	.278	-.419	0	-.529
41	-.143	.118	-.207	-.405	-.419	0	-.357
42	.181	.293	.207	-.225	.367	.218	-.076
43	.021	.322	-.154	-.058	.071	.167	.076
44	.062	.233	.237	-.036	.491	0	.029
45	-.241	-.262	.080	-.294	.073	0	-.255
46	.024	-.262	-.237	.009	.464	-.375	.314
47	-.047	-.081	0	0	.250	-.250	.255

Variable 17 with variables:

21	.268	.308	.458	.267	.279	-.066	.167
22	.124	.053	.085	.086	-.179	.197	.313
23	.117	.476	.331	.044	.055	.084	-.127
24	.011	.214	-.625	-.033	-.093	.093	.182
25	.090	.014	-.340	-.076	-.120	.246	.392
26	-.017	-.081	-.214	-.031	.220	-.017	-.300
27	.061	.018	-.138	-.149	0	.279	.146
28	.028	-.174	.500	-.488	0	.121	.071
29	.008	-.076	-.043	-.068	-.082	-.031	.184
30	-.006	.121	-.043	-.108	-.261	-.068	.046
31	-.053	-.167	.356	-.283	-.209	.125	.449
32	.112	-.325	-.328	.216	-.144	.341	.381
33	.099	.171	.500	-.278	.196	-.091	-.038
34	.157	-.178	.167	.632	.100	.333	.378
35	.291	.478	.350	-.067	.419	.190	.471
36	-.106	-.050	-.071	-.216	-.105	-.299	.328
37	.028	.176	.165	-.033	-.038	-.167	.087
38	-.041	-.006	-.258	-.068	-.243	.051	.160
39	-.088	-.043	-.043	-.063	-.159	-.307	.216
40	-.102	.018	-.564	-.155	-.107	0	.022
41	-.003	-.222	.019	-.094	.263	-.137	-.028
42	.169	.259	.564	.116	-.261	-.068	.333
43	.119	.144	.549	-.015	-.251	.240	.333
44	.059	-.105	.373	.253	.120	-.127	.298
45	.121	.434	.350	.323	.040	0	.298
46	.146	.182	.073	.230	-.111	.173	.333
47	-.085	.121	-.395	0	-.125	.372	-.098

Variable 18 with variables:

21	-.134	.040	-.393	-.027	-.040	.263	.024
22	.002	-.407	.016	.139	.026	-.066	.110
23	.084	.159	0	.069	.030	-.084	.330
24	-.064	-.280	.140	-.194	-.053	-.093	-.121
25	-.088	-.298	.250	-.204	.273	-.246	.058
26	-.019	.278	.139	-.411	.139	.042	-.053
27	-.088	-.286	.250	0	-.146	-.125	-.207
28	-.289	-.241	-.632	-.408	0	-.284	-.071
29	.092	-.026	.260	-.008	.185	-.025	.065
30	.081	.146	.260	.045	.045	-.180	.089
31	.072	-.250	.102	.239	0	.043	-.179
32	-.054	-.220	.109	-.007	.098	-.099	-.066

33	-.078	.086	-.137	-.078	-.090	-.045	.145
34	-.285	-.365	-.458	-.632	0	-.333	0
35	-.169	-.357	.271	.258	-.284	-.333	-.167
36	.029	-.290	-.094	.195	-.321	.299	.025
37	-.089	-.176	-.118	-.027	-.226	-.113	.113
38	-.007	-.058	-.167	.150	-.042	.130	.031
39	.033	.099	-.380	.149	-.029	.446	-.090
40	.140	.055	.404	.025	.108	0	.080
41	-.055	-.218	-.177	.206	-.184	.148	-.045
42	.061	.250	.081	.043	-.014	-.180	.055
43	.021	0	-.243	-.012	-.122	.223	.289
44	-.057	-.375	-.138	-.131	-.115	.065	.123
45	.084	.280	-.299	.312	.050	0	-.032
46	-.005	0	.040	-.149	-.236	-.064	.167
47	-.055	-.102	.210	0	-.229	-.061	-.120

Variable 19 with variables:

21	.078	-.144	0	.378	-.053	.306	-.192
22	.228	.338	.164	.366	-.171	.320	.236
23	.045	-.260	-.107	.220	-.055	.375	.238
24	.430	.300	.578	.578	.187	.373	.369
25	.364	.586	.351	.266	.434	.137	.243
26	.227	-.058	.139	.378	.356	.101	.250
27	-.015	-.258	.335	-.181	-.175	-.020	-.155
28	-.162	-.556	-.632	-.500	0	-.026	.267
29	.292	.230	.216	.044	.368	.399	.208
30	.355	.351	.216	.098	.296	.346	.389
31	.246	.452	-.356	-.100	.403	.411	-.239
32	.502	.818	.459	.552	.667	.043	.169
33	.119	.408	-.144	.399	-.103	0	.397
34	-.108	-.386	-.043	.447	-.258	0	-.378
35	.086	.426	-.055	.537	-.194	.101	-.040
36	.254	.390	.478	.373	.284	-.033	-.018
37	.288	.344	.275	.387	.344	.246	-.300
38	.205	.457	.168	-.032	.090	.289	.236
39	.011	.381	-.267	.153	.042	-.232	-.024
40	.430	.282	.632	.208	.580	0	.199
41	.316	.498	.480	-.148	.061	.370	.262
42	.149	.195	-.158	.031	-.127	.346	.358
43	.059	.220	-.077	-.025	.106	.089	.243
44	-.004	.195	-.539	-.032	-.084	.429	.306
45	-.247	-.078	-.220	-.172	-.313	0	-.084
46	.110	.103	-.184	.240	-.042	.159	.188
47	.024	.234	.286	0	-.042	-.145	-.126

Variable 20 with variables:

21	.014	-.415	.292	.125	.104	.057	-.314
22	.100	.149	0	.138	-.321	.378	0
23	.095	-.038	.346	.005	-.314	.309	.277
24	-.140	-.220	.156	-.196	-.175	-.166	-.183
25	.324	.252	.222	-.020	.262	.436	.764
26	.184	.290	0	-.306	.636	.372	-.340
27	0	-.478	.167	.414	-.333	.187	.149
28	.070	.293	.612	.192	0	-.191	.250
29	-.004	-.102	-.231	.053	.016	.133	0
30	-.075	.161	-.231	-.024	-.138	-.216	-.126
31	.101	.408	0	.069	.025	.167	-.060
32	.208	.462	.238	-.044	.299	.444	-.066
33	.067	.135	-.156	-.151	.066	.173	.125
34	-.019	.051	0	.316	-.350	.577	-.417
35	.076	.329	.076	-.125	-.059	.086	-.169
36	-.046	-.179	.073	-.203	.036	.083	-.098
37	.103	-.164	.311	.068	.267	.205	-.224
38	.049	.012	-.115	.042	-.256	.141	.330
39	-.018	-.074	-.204	.177	-.316	-.025	.145
40	.040	.118	-.045	-.005	.235	0	-.108
41	.146	.337	.328	-.245	-.032	.210	.212
42	-.041	.077	.156	-.005	-.321	-.216	.212
43	.202	.015	.489	.145	.086	.022	.458
44	.072	-.256	.122	.377	0	.194	.120
45	-.071	-.158	-.045	.075	-.272	0	.080
46	.043	.443	-.191	-.024	-.135	-.295	.471
47	-.060	-.253	.224	0	-.158	.020	-.108

Variable 21 with variables:

22	.171	.048	.207	.267	.171	.189	.140
23	.132	.182	.272	.239	.270	-.061	-.060
24	-.083	.083	-.195	-.038	-.067	-.043	-.295
25	-.091	-.208	-.428	-.060	-.041	-.029	-.500
26	.053	-.061	-.258	.051	.204	-.141	.012
27	.057	.391	-.123	.169	-.064	.341	-.229
28	.237	0	.500	.098	0	.294	.169
29	.044	.263	.151	.056	.073	.107	-.111
30	-.022	-.026	.151	.114	-.022	-.080	-.238
31	-.035	-.075	-.134	-.283	.322	.411	-.316
32	-.022	-.462	-.233	.175	.145	-.160	-.280
33	.200	-.071	.428	.136	.267	.131	0
34	.093	.083	.167	.316	0	-.218	.447

35	.301	.122	.575	.689	.194	-.053	.098
36	.271	.328	.258	.270	.121	.181	.568
37	.175	.115	.321	-.043	.182	-.057	.481
38	-.099	-.220	-.167	-.045	-.009	.098	-.299
39	-.030	-.012	-.140	.172	-.281	-.124	-.170
40	-.056	-.033	-.240	-.152	.121	0	-.201
41	.005	-.247	.387	.026	.191	-.076	-.452
42	.045	.187	.338	.275	-.200	-.080	-.060
43	.164	-.060	.598	.206	.331	-.008	.140
44	.031	.187	.076	.037	.146	.133	.076
45	.170	.463	.338	.210	.073	0	.408
46	-.070	.144	-.299	.042	.177	-.164	-.167
47	-.074	-.216	-.319	0	-.256	-.096	.302

Variable 22 with variables:

23	.500	.447	.177	.354	.791	.804	.378
24	.177	.338	0	-.087	.157	.209	.190
25	.255	.484	.156	0	-.122	.496	.513
26	-.142	-.478	.086	.057	-.310	-.167	-.060
27	0	-.182	-.302	.259	.310	.033	.022
28	-.028	.218	.467	-.098	0	-.218	.225
29	.346	.441	.600	.211	.242	.439	.289
30	.217	.294	.600	.271	.043	.070	.056
31	.287	.365	-.167	.071	.278	.344	.463
32	.191	.183	-.272	.068	-.049	.361	.324
33	.306	.630	.040	.280	-.191	.461	.422
34	.088	.284	.122	-.250	0	.333	.250
35	.502	1.00	.033	.344	.327	.665	.756
36	.455	.632	.224	.566	.244	.381	.601
37	.453	.517	.556	.632	-.079	.471	.460
38	.402	.293	.433	.410	.279	.180	.538
39	-.039	.149	-.015	.151	-.105	-.313	.010
40	.087	0	.161	.050	-.015	0	-.041
41	.205	.258	.312	-.008	-.168	.170	.243
42	.239	.248	.480	.355	.061	.070	.195
43	.377	.652	.458	.355	.032	.140	.577
44	.257	.408	.185	.175	.435	.228	.437
45	-.003	-.192	-.161	.162	-.121	0	.333
46	.065	-.098	-.129	-.092	.314	.295	.036
47	.101	.120	.185	0	.279	-.020	.084

Variable 23 with variables:

24	-.034	.066	-.356	.116	-.220	.185	-.265
25	-.079	-.086	-.256	-.115	.167	.371	.365
26	-.115	-.265	-.295	.122	-.070	-.035	-.052
27	-.129	-.400	.122	.028	-.224	-.199	-.195
28	.060	-.194	.500	-.098	0	-.119	.354
29	.225	.321	-.101	.038	.440	.665	.084
30	.101	.302	-.101	.100	.138	.287	-.013
31	.219	.302	.624	.507	-.043	.239	.089
32	.007	-.459	-.171	.111	.105	.299	.092
33	.235	.435	.158	.098	.101	.554	-.067
34	-.058	0	.431	-.250	-.707	0	-.316
35	.397	.673	.325	.344	.346	.686	-.094
36	.217	.350	.190	.258	.405	.248	-.121
37	.209	.304	.275	-.106	.288	.586	-.140
38	.092	.118	-.285	.102	.156	.053	.244
39	-.246	-.038	-.353	-.225	-.015	-.267	-.471
40	-.049	.054	-.463	.234	.023	0	-.131
41	.079	-.024	.014	.008	.015	.140	.246
42	.183	.285	.301	.022	.023	.287	.197
43	.273	.568	.342	.022	.055	.217	.330
44	.232	.285	.271	-.180	.452	.428	.123
45	.096	.313	-.073	-.044	-.037	0	.269
46	.145	-.143	.112	.033	-.045	.379	.418
47	.095	.141	.229	0	.156	-.114	.080

Variable 24 with variables:

25	.236	.194	.586	.558	0	-.126	.252
26	-.084	-.638	.179	.200	-.397	.037	.052
27	-.057	-.267	0	-.267	.419	-.186	-.038
28	-.312	-.577	-.632	-.488	0	.049	-.051
29	.240	.294	.236	.107	.153	.064	.411
30	.292	.116	.236	.107	.314	.141	.584
31	.236	.200	-.327	.371	.255	.091	.509
32	.380	.183	.471	.721	.204	-.052	.514
33	.032	.539	-.360	-.023	-.023	-.187	.270
34	-.031	.083	-.289	.447	.316	0	-.158
35	.127	.329	-.278	.158	.105	.279	.288
36	.085	.394	.211	.169	-.016	-.308	.118
37	.101	.840	-.050	.051	-.431	-.111	.066
38	.181	.378	.300	-.211	.135	.114	.089
39	-.019	.324	-.077	.009	.199	-.425	.237
40	.484	.461	.678	.455	.513	0	.324

41	.229	.247	.097	.036	.233	.282	.324
42	-.001	.137	-.387	-.302	-.054	.141	.150
43	-.107	.452	-.514	-.386	-.162	.204	-.218
44	-.025	.299	-.355	-.067	-.153	.110	.218
45	-.344	-.154	-.342	-.302	-.324	0	-.584
46	.230	.012	.021	.629	-.007	.450	.134
47	.122	.216	.277	0	.140	.439	-.219

Variable 25 with variables:

26	.044	-.446	.320	.239	.325	-.158	-.344
27	.033	-.124	.354	-.058	-.103	.182	.033
28	.088	.048	-.250	-.194	0	.185	.356
29	.201	.079	.233	.055	.300	.203	.241
30	.271	.309	.233	.055	.320	.246	.167
31	.192	-.030	-.250	.134	.357	.289	.350
32	.441	.577	.449	.574	.225	.380	.304
33	.057	.036	-.397	.222	-.060	.309	.088
34	-.188	-.278	-.408	-.200	-.316	0	-.488
35	.174	.655	-.387	.500	-.237	.240	.452
36	.079	.289	-.149	-.083	.055	.169	-.058
37	.024	.108	-.187	.079	.119	.078	-.289
38	.245	.258	.340	-.062	.169	.316	.436
39	-.006	.022	-.012	-.256	-.169	.086	.218
40	.229	-.014	.483	.150	.250	0	.038
41	.182	.271	-.024	.141	.147	.176	.258
42	.079	.208	-.146	-.216	.089	.246	.205
43	-.070	.091	-.283	-.260	.250	-.137	.230
44	-.022	-.022	.150	-.129	-.042	.131	.279
45	-.274	-.085	-.405	-.372	-.281	0	-.038
46	.092	-.252	-.012	.175	-.204	-.027	.496
47	-.057	.125	.150	0	-.123	-.032	-.344

Variable 26 with variables:

27	-.060	.124	-.169	-.134	-.394	.093	.109
28	.104	.289	-.316	.167	0	-.347	.395
29	.091	-.236	.300	.308	-.124	.017	.194
30	.052	-.236	.300	.308	-.313	-.017	.101
31	-.101	.030	-.189	0	0	-.240	-.356
32	.203	-.058	.214	.357	.302	.189	.149
33	.059	-.357	.283	.378	-.138	-.042	.289
34	-.230	.030	-.708	.250	-.632	-.218	-.250
35	.022	-.294	.016	-.060	.026	-.076	.250
36	.011	-.221	-.016	-.059	.124	-.029	-.081

37	.112	-.446	-.022	.255	.204	.289	.034
38	.054	-.236	.332	.216	-.397	.250	.217
39	.100	-.061	.314	-.229	-.132	.053	.250
40	.175	-.243	.311	.279	.146	0	.189
41	.135	-.192	.174	-.140	.223	.556	.253
42	-.067	-.182	.069	.279	-.569	-.017	-.096
43	-.079	-.577	.111	-.420	-.032	.200	-.037
44	.027	-.236	.250	.216	-.054	.042	.292
45	-.033	-.115	.189	-.183	.210	0	-.140
46	-.050	.058	-.295	.183	-.038	-.256	-.067
47	.042	-.132	.289	0	-.112	-.100	.253

Variable 27 with variables:

28	.303	.671	-.158	.356	0	.338	.395
29	-.090	-.418	-.196	-.121	-.187	.028	-.017
30	-.009	-.578	-.196	-.121	.083	.125	.242
31	-.095	-.603	-.102	-.100	.069	-.051	.350
32	-.013	-.342	.214	-.121	0	.193	.108
33	-.177	-.620	-.287	-.027	-.338	.125	.122
34	.032	.293	-.091	0	.632	-.577	0
35	-.020	-.080	-.238	.500	-.080	0	.196
36	-.164	-.126	-.081	.126	-.567	-.055	-.139
37	-.258	-.471	-.315	.069	-.523	-.231	-.258
38	-.086	-.472	-.248	.277	.108	.093	.009
39	-.081	-.288	-.304	.169	-.201	-.187	.349
40	.023	-.456	.167	.036	-.033	0	.427
41	-.117	-.362	-.122	-.239	-.216	.189	.116
42	-.020	-.472	-.440	-.083	.214	.125	.217
43	-.179	-.411	-.204	.017	-.256	-.158	-.122
44	-.003	-.045	-.079	.097	-.061	.106	-.145
45	-.001	-.258	-.138	.203	-.256	0	.194
46	-.038	-.169	.085	-.388	.061	-.210	.191
47	-.130	-.248	-.079	0	-.149	-.010	-.194

Variable 28 with variables:

29	.042	-.408	-.378	.293	0	.026	.283
30	.069	-.577	-.378	.293	0	.350	.267
31	-.142	-.661	-.333	-.258	0	.069	0
32	-.162	-.386	-.750	-.463	0	-.327	.548
33	.032	-.535	0	.327	0	.049	.333
34	.087	.612	.500	0	0	-.730	-.333
35	.263	.488	.167	.167	0	.026	.655
36	-.165	-.189	-.333	-.167	0	-.262	-.167

37	-.087	-.599	.316	.039	0	-.109	-.333
38	-.075	-.683	.158	.174	0	-.125	.283
39	.021	-.408	.500	.192	0	-.395	.386
40	-.184	-.775	-.791	.098	0	0	.267
41	-.087	-.522	0	.174	0	-.347	.267
42	.075	-.577	.158	.258	0	.350	0
43	.165	-.258	.500	.683	0	.108	.225
44	.028	-.293	.500	-.522	0	.108	.283
45	.016	0	.189	.098	0	0	.033
46	-.127	.192	0	-.488	0	-.406	.220
47	0	-.522	0	0	0	.233	.141

Variable 29 with variables:

31	.264	.408	-.289	.051	.430	.228	.386
32	.300	.189	-.187	.256	.357	.107	.769
33	.307	.206	.181	-.007	.227	.611	.664
34	-.154	-.158	-.320	-.316	.060	0	-.316
35	.325	.304	.200	.149	.139	.469	.553
36	.353	.596	.026	.132	.471	.302	.511
37	.294	.332	0	.005	.292	.432	.424
38	.357	.500	.366	.179	.133	.279	.722
39	.156	.333	.029	.359	-.133	-.048	.386
40	.423	.356	.495	.502	.357	0	.552
41	.285	.356	.282	.209	.213	.148	.412
42	.513	.612	.459	.618	.441	.418	.330
43	.159	.312	.227	-.007	.209	.057	.313
44	.505	.612	.199	.306	.567	.786	.742
45	-.097	.196	.043	-.029	-.185	0	-.363
46	.080	-.167	-.101	.049	.209	.244	-.088
47	-.034	.167	-.169	0	.032	-.345	-.108

Variable 30 with variables:

31	.269	.272	-.289	.220	.368	-.043	.624
32	.275	.365	-.187	.256	.144	-.080	.592
33	.121	.154	.181	-.007	.080	.180	.505
34	-.226	-.527	-.320	-.316	-.158	-.745	-.378
35	.093	.127	.200	.149	-.026	.142	.175
36	.155	.258	.026	.187	.242	-.123	.179
37	.076	.017	0	.005	.062	.028	-.061
38	.340	.592	.366	.202	.273	.503	.279
39	.151	.395	.029	.296	.133	.084	.189
40	.551	.492	.495	.533	.506	0	.812
41	.372	.492	.282	.222	.302	.260	.606

42	.702	.846	.459	.656	.626	1.00	.486
43	.045	.167	.227	.036	.053	.070	.078
44	.116	.175	.199	.141	.075	.462	.210
45	-.170	.206	.043	.021	-.233	0	-.446
46	.132	-.099	-.101	.112	.194	.330	.046
47	-.049	.230	.169	0	-.079	-.207	-.208

Variable 31 with variables:

32	.258	.426	.333	.120	.150	-.354	.607
33	-.071	.452	.289	-.167	-.169	-.043	-.316
34	.297	.300	0	0	.258	.500	0
35	.106	.375	.408	-.500	.284	.238	.258
36	.297	.535	.667	.417	.118	-.250	.167
37	.118	.468	.667	-.449	-.044	0	.091
38	.180	.272	-.430	-.033	.561	.312	.214
39	-.073	.134	-.222	-.415	-.081	-.071	-.134
40	.391	.583	-.356	.507	.408	0	.624
41	.172	.784	-.241	-.312	0	0	.418
42	.061	0	.289	-.071	.039	-.043	.418
43	.110	.302	.241	-.071	.284	.043	.039
44	.038	.612	0	-.461	.185	.228	.134
45	-.327	.068	-.134	-.690	-.567	0	0
46	.304	.354	.043	.690	.725	-.091	.069
47	.129	0	.222	0	.094	0	.134

Variable 32 with variables:

33	.010	.209	-.492	-.095	-.200	.306	.467
34	-.022	-.283	.120	.447	-.100	-.218	-.316
35	.133	.320	-.316	.267	-.080	.119	.645
36	.151	.270	.378	-.236	.049	.371	.203
37	.107	.191	-.051	.086	.128	.224	.045
38	.198	.365	-.204	-.055	-.050	.189	.523
39	.066	.488	-.171	-.025	-.201	.119	.243
40	.473	.236	.685	.310	.570	0	.461
41	.261	.471	.171	-.093	.045	.472	.390
42	-.004	.189	-.328	.077	-.284	-.080	.312
43	-.083	0	-.171	-.422	.296	-.008	.141
44	-.030	.092	-.514	.010	.195	-.070	.553
45	-.403	-.273	-.492	-.299	-.182	0	-.461
46	.024	0	-.089	.359	-.212	.052	.144
47	.078	.218	.343	0	-.066	-.113	.149

Variable 33 with variables:

34	-.091	.192	-.389	.577	0	-.149	.158
35	.531	-.564	.575	.350	.354	.665	.485
36	.486	.342	.149	.628	.599	.573	.630
37	.503	.780	.092	.659	.415	.532	.367
38	.295	.347	.086	.343	.187	.051	.574
39	.196	.204	.316	.280	.053	-.030	.376
40	.002	.289	-.381	.118	-.007	0	.371
41	.097	.229	-.040	-.129	.053	.242	.295
42	.163	.139	.463	.007	.187	.180	.204
43	.339	.791	.349	.118	.135	.109	.339
44	.319	.139	.247	.233	.287	.368	.516
45	.054	-.270	.690	.007	-.025	0	-.439
46	-.080	-.139	-.219	-.127	-.017	.064	-.124
47	-.031	.158	-.334	0	.187	-.207	-.013

Variable 34 with variables:

35	-.102	.316	-.239	-.577	0	.218	0
36	.115	.089	.365	0	-.378	-.250	.447
37	.155	.213	.471	.612	-.730	-.149	.600
38	-.224	-.617	-.055	0	.378	-.258	-.258
39	-.111	-.639	-.068	.447	-.378	.091	.293
40	-.077	-.284	-.218	.316	-.598	0	-.250
41	-.019	-.192	.492	-1.00	-.395	-.218	-.250
42	-.119	-.527	-.055	.316	.632	-.745	-.250
43	-.117	.192	.123	-.316	.316	-.655	-.500
44	-.181	.158	-.480	.200	.350	0	-.316
45	-.081	-.184	-.218	-.316	0	0	.378
46	.196	-.051	.289	.632	.060	.218	.316
47	.018	-.192	.320	0	-.395	0	0

Variable 35 with variables:

36	.351	.564	.160	.488	.527	.360	.350
37	.379	.555	.194	.158	.178	.534	.218
38	.003	.094	-.433	-.026	.113	-.070	.471
39	-.041	0	-.305	.149	.417	-.462	.351
40	-.079	-.120	-.158	-.026	.154	0	0
41	.157	.189	.158	.372	.105	.155	.250
42	.092	.152	.484	-.244	-.207	.142	.171
43	.298	.679	.316	.189	-.277	.211	.316
44	.341	.378	.217	.122	.367	.251	.632
45	.118	0	.224	.258	-.105	0	-.094

46	.044	.189	-.255	-.258	.419	.301	-.079
47	.088	.130	-.040	0	.105	.235	0

Variable 36 with variables:

37	.669	.616	.886	.458	.587	.529	.894
38	.317	.390	.321	.243	.308	.181	.373
39	.216	.302	-.127	.404	-.168	.137	.179
40	.180	.183	.277	.216	.105	0	.099
41	.239	.369	.332	0	.168	.204	-.034
42	.077	.179	-.108	.150	.105	-.123	.302
43	.209	.522	.155	.105	.043	-.083	.287
44	.250	.724	-.307	.243	.480	.038	.373
45	.081	-.101	-.158	.216	.157	0	.174
46	-.040	-.179	-.370	.187	.390	-.151	-.302
47	.131	0	.171	0	.308	-.316	.302

Variable 37 with variables:

38	.258	.344	.327	.286	.079	-.030	.386
39	.109	.292	-.190	.247	-.187	-.161	.314
40	.127	.340	.048	.033	.037	0	.069
41	.171	.079	.370	-.183	.271	.204	-.171
42	.084	.088	.192	.033	-.060	.028	-.171
43	.378	.601	.408	.356	.375	.232	.211
44	.234	.344	-.073	.406	.321	.196	.335
45	-.009	-.313	-.192	-.244	.078	0	.071
46	-.143	-.062	-.408	-.098	-.038	-.186	-.344
47	.069	.189	.218	0	.079	-.273	.102

Variable 38 with variables:

40	.307	.539	.279	.316	.140	.311	.260
41	.327	.539	.246	-.104	-.096	0	.474
42	.237	.422	.028	.215	.382	.556	.100
43	.250	.386	.112	.041	.067	.503	.548
44	.216	.422	.112	.106	.009	.200	.697
45	-.073	0	.086	-.010	-.201	.067	-.066
46	-.118	-.283	-.342	-.274	.300	0	-.123
47	.099	.253	.217	0	.253	.238	-.060

Variable 39 with variables:

40	.152	.365	-.125	.333	.190	-.100	.280
41	.107	.365	-.071	0	.113	.155	.209
42	.031	.296	-.028	.123	-.036	.084	-.121
43	.068	.175	.206	.123	-.147	-.127	.075
44	.101	.296	.104	.365	-.375	-.106	.215
45	.107	.078	.377	.120	-.023	0	-.215
46	-.163	-.261	-.162	-.118	-.147	-.084	-.221
47	-.019	.171	.027	0	-.141	-.369	-.178

APPENDIX E

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