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ECONOMIC AUTONOMY AND SOCIAL DISTANCE: ARCHAEOLOGICAL EVIDENCE

by

Mark Paul Leone

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I hereby recommend that this dissertation prepared under my direction by Mark Paul Leone entitled Economic Autonomy and Social Distance: Archaeological Evidence be accepted as fulfilling the dissertation requirement of the degree of

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ABSTRACT

The following hypothesis is explained and tested: increasing dependence on agriculture leads to increasing social distance between the minimal economic units needed to make agriculture a successful economic base. The tests will be two: an intensive one in the prehistoric Mogollon area of the American Southwest, and a less extensive one in pre- and proto-historic New York State. The covariation of these two variables is traced through 800 years in the Southwest and for about 300 years in New York State.

The two variables, changing dependence on agriculture, and changing social distance, are measured. Changing variability in tool kits will be used to examine degree of specialization in the economic base, hence measuring agricultural dependence.

Social distance in both the prehistoric Southwest and New York State is inferred from the degree of endogamy in villages. The degree of internal homogeneity in the decorative styles of female products found in prehistoric sites is used to measure the degree to which any one village is isolated. From this the degree of endogamy and the intensity of regional interaction are inferred.
CHAPTER 1

THE HYPOTHESIS

The point of this study is the creation of a generalization. The generalization involves two variables, one economic and one social. The aim of the study is to demonstrate a consistent relationship between agricultural economics in pre-industrial societies and the scope of social isolation those societies develop. I suggest that the economic and social dimensions of any culture have the same measurements, or perhaps better, economic autonomy leads to social autonomy. This statement can lead logically to a more specific version which will be tested in two neolithic cultures. That version follows in the next paragraph. The two cultures are the prehistoric Mogollon of the North American Southwest and the prehistoric and historic Iroquois of New York State. The results of the testing can be constructed to suggest that the rise of neolithic economics produces a consistently observable decline in social interaction between units that become economically self-sufficient.

The specific hypothesis reads, with increasing dependence on agriculture the social distance between minimal economic units will become increasingly great. This describes a trend to fission that can be counterbalanced by factors like: (1) regionally interdependent ecological niches, sometimes combined with the need for water.
control; (2) the imposition of an overarching, amalgamating network of some kind: economics (trade), or politics (state or empire) may produce a similar result; (3) competition within one ecological niche can produce cooperation between farmers against exploiting hunter-gatherers, or between some farmers against others. It is also possible that the tendency to fission not be counterbalanced. This may lead to divisiveness and social conflict. Factors one through three are stated as corollaries to the hypothesis.

The hypothesis requires the existence of the minimal economic unit. This unit is the minimal group needed to cope with the harshest environmental circumstances a culture must face. The unit may be brought into existence through the action of one of the corollaries to the hypothesis as is the case in Southern Mesopotamia and the Maya region. I refer to regionally interdependent ecological niches sometimes coupled with water control. But more frequently, viable neolithic communities are the result of other processes (Binford 1968), not these. And they cannot be discussed here.

The corollaries should be seen as vehicles for increasing the scope and size of the minimal economic unit. And, of course, they cancel the isolation and fission that the process produces. This is the case until a new equilibrium of interaction is established. Then autonomy, the result of the process the hypothesis describes, would again develop. Economic equilibrium leads to fission and the increase of social distance between those units in equilibrium. When the economic balance is disturbed, social bonds will be loosened and
retied until a new economic adjustment is established. The corollaries to the hypothesis are some of the major vehicles for interrupting equilibrium.

A line must be drawn beyond which the hypothesis as stated cannot be held responsible for change. For example, other economic factors beside agriculture are responsible for the expansion of the size of the political unit in Mesopotamia after the early historic period.

The most general version of what I have suggested above was proposed to me by Professor Lewis R. Binford of U.C.L.A. His precis is "increasing economic autonomy leads to increasing social autonomy." Stated this way there are two inevitable reactions to the hypothesis. The first is that the statement is so obvious that it is a fact and not a hypothesis. "I was born knowing it" epitomizes a common reaction. The second is that the hypothesis as an explanatory mechanism to describe the consistent interaction between two variables, is so accurate a reflection of cultural reality that everything from prehistoric pueblos to the American Revolution to the automation problem is more clearly seen in its light. These two views are not beyond reconciliation, for the more encompassing rules in science have often been greeted with claims of millenia of precedence. Where there is precedence in this case it is a matter of decades and belongs to Gordon Childe, if to anyone.

The general hypothesis has its alternative form which is equally testable. It says that increasing economic autonomy leads
to decreasing social autonomy. A third alternative is that the state of economic independence is not positively correlated one way or the other with social distance among groups.

Alternative working hypotheses to the agriculture-specific version of the hypothesis would take similar forms. But the critical use of a method of alternative working hypotheses comes when any specific case is subjected to testing. The alternative hypotheses have to grow out of familiarity with the particular culture-historical data any one case presents. One has to be certain that increasing dependence on agriculture and not a private source of mana from heaven is the causal variable in the individual situation.

The general hypothesis about economic and social autonomy can, of course, be used to describe cultural conditions in any culture, extinct or extant, if the data are available, and the analyst's imagination fertile enough to discover them. In the rest of this paper I am going to examine data from Robert Whallon's work on the Iroquois and proto-Iroquois. Included will be historic Iroquois data as well. Then, in several chapters I would like to present in some depth an analysis of an 800 year period in one region of the Mogollon area of the prehistoric Southwest. It should also be possible to consider data from a wider range and a period closer to us.

**Explanation of the Hypothesis**

After I had read a paper by Robert Whallon (1966) which suggested that a measure of the degree of regional intercommunication or interaction could be found in the degree of completeness with which a
group married into itself (how endogamous they were), it seemed that his successful experiment could be applied to data in the Southwest. It was also clear that it had further potential beyond New York and the Southwest. I have borrowed the method and part of the hypothesis.

Whallon has suggested in his dissertation (1966: 38-58) that as a village becomes more endogamous, the measurable variation in goods produced by its women will be diminished. This happens because the makers will have less and less contact with the many microtraditions for doing things which are inevitably represented in any given larger region. Whallon also reasoned, that a measure of internal variation in a relevant range of goods would be both a measure of endogamy and by reflection a measure of regional interaction. If this logic turned out to be valid, then Whallon had a precise measure of change. He claimed to be measuring changing degree of "regional interaction." Although his experiment was successful, I would like to suggest here that more of the vast amount of potential represented in his idea might be realized by founding the hypothesis, or at least one of its variables, in economics.

Whallon demonstrated that in New York State, among the proto-Iroquois and Iroquois, villages became more internally homogeneous through time. There is less variation in the goods produced by women as one comes up in time. And on the reverse, earlier sites, when endogamy was less emphasized, variation in female manufactured articles was measurably greater. At the earlier horizons women were drawn into a village from a larger region, a region presumably reflecting all the
environmental zones making up the economic base. The women would consequently reflect as many variations for making any given class of goods, as would exist in the region. At any one site, therefore, one would find the stylistic equivalent of the number of regions from which the females at that site happen to have come. For example, there may be as many ways of making chevrons represented in a village as there are regional traditions of chevron-making drawn from by that village.

James Deetz initiated studies based on precisely measured stylistic change. Robert Whallon paraphrases Deetz in his own dissertation stating the arguments of relevance for these data clearly. Deetz's hypothesis predicted that there would be a greater tendency for patterning of attribute association in an assemblage pertaining to a matrilocal group than in any non-matrilocal situation (Deetz 1965: 2). In a highly matrilocal group the interaction between women, producers of pottery, could be channeled along the lines of the matrilocal extended families or lineages. The learning situation was thus structured in terms of lifetime mother-daughter relationships, and the train of transmission of stylistic ideas would be unbroken in this matrilineal line. There would be a strong tendency, therefore, for the individual patterns of attribute combination characteristic of the women of any particular group to be perpetuated in their daughters' products. Variations present in other descent groups would not be presented to the girls as models during the production of pottery. Interactional boundaries, and thus boundaries to learning, were thought to be created by the localization of females in these residence groups (Whallon 1966: 39-40).

Whallon's data allowed him to infer that as the number of traditions represented at an Iroquois site decreased interaction between villages decreased. He inferred that an inverse relationship existed between village homogeneity and regional homogeneity. As villages became less internally varied, the region became more
differentiated. It also happened that as this process went on the proto-Iroquois were adopting and relying more heavily on agriculture. These are the variables that lead me to the hypothesis. That is, the trend toward decreasing regional interaction was brought about by an increase in the degree of dependence on agriculture.

Inherent in agriculture is economic independence of a sort known only rarely before the neolithic. Economic independence or autonomy is the degree to which the minimal economic unit in any society is dependent upon itself for those things that insure survival. A minimal economic unit is that social unit any society has that permits adaptation to the harshest circumstances imposed on that culture. A minimal economic unit may be autonomous for long or short periods. Usually these periods occur at regular intervals. Complete self-sufficiency may not be realized in any given agricultural community, but what I suggest is that the group that depends on agriculture can feed itself and may rely on none other out of basic necessity. This does not include farmers in an industrial, capitalist society, but rather farmers in an agricultural one. If the minimal economic unit is the nuclear or extended family in both agricultural and hunting-gathering societies, then I suggest that unit is more self-sufficient, dependent less on the help of neighbors among agriculturalists than it is among hunter-gatherers.

The differences which so clearly separate neolithic cultures are not surprising in view of the distinctive character of the economy, the self-sufficiency of each community. Because each group was economically independent of any neighbors, it could remain isolated from them. And in such
isolation each group could work out its own arts and crafts, its own styles and institutions independent of the rest. Only the most bigoted evolutionist will contend that these independent developments would converge everywhere to like results. The reverse may actually be observed. If one studies in detail several closely allied neolithic groups - on the Central European löss, for example - one notices a continual divergence, the multiplication of individualized groups each differing from one another ever more pronouncedly in the fashionable shape for vases, the style of their decoration, and so on (Childe 1951: 74).

When either farmers or hunter-gatherers face starvation or a difficult season of the year, the agriculturalist relies on stored surpluses and does not change the range of people he depends upon. The hunter-gatherers often break up into minimal social units, families, and get along by spreading out thinly over a vaster area. Hunter-gatherers are capable of this kind of extreme autonomy, but so are pioneering farmers. The normal behavior of hunter-gatherers, however, seems to include a wider range of dependence on their fellows than does the behavior of the farmer who plants, tends, and harvests usually with the other members of his family. Both agriculturalists and hunter-gatherers can be economically autonomous at the level of the basic producing unit, the family. For farmers this can be the normal situation. For hunter-gatherers this is more often the situation only in times of hardship (Eggan 1964: 45-77).

If agriculture really can lead to self-sufficiency to the point where cooperation between villages is not required to insure survival, then it is possible that increasing dependence on agriculture leads to an increased social, and consequently stylistic, differentiation between villages. With a decrease in the degree of cooperation necessary to manipulate the economy, the social ties have no
reason in economics to be maintained. There is no economic value in
drawing one's wives or husbands from a larger area than one depends
on for economic well-being. It is sensible then that villages become
increasingly endogamous.

An increase in the degree of dependence on agriculture is
probably directly correlated with a decrease in regional cooperation,
communication and general interaction. Villages become more economi­
cally self-sufficient and self-reliant and simultaneously need less
and less from their neighbors. There is a consequent sociological
effect. There is no longer need for all those social mechanisms en­
forcing and facilitating economic ties. Wife exchange between villages
is no longer the way to insure assistance in hard times that it once
was. The hard times of hunter-gatherers have been alleviated in good
part by surpluses storable for several years.

I suspect that it is also possible to see this process operate
within a village. If the basic economic unit among farmers is the
nuclear or extended family, this unit then is what becomes increasingly
self-sufficient. A family can be removed from its context, ideally,
and survive by farming, just about as well as it can survive in its
village context. That condition ought to cause a certain tendency
toward heterogeneity, or fission within a village. There is some
logic to this, for if the family is really economically autonomous,
then we would expect it to be socially self-sufficient as well. There­
fore, I hypothesize that as villages become more disparate from one
another, so should families within a village. Fission outside and within the walls is taking place.

Fission and disparateness can be measured by stylistic diversity, Whallon argued, and I have used his method for measuring that diversity. Southwestern pottery is amenable since in its early stages of development the basic exterior color is variable, and in its later stages variation in painted design is enormous. Both types of variation are argued to be stylistic and to reflect social and demographic facts. Some standard Southwestern pottery types from the Mogollon area were used and broken down into elements of analysis relevant to this problem. The types span about 800 years and possess characteristics which I argue can effectively measure degree of social isolation.

The tendency that this hypothesis describes is an essentially destructive one. If there were no means of counterbalancing the trend to increasing social distance created by agricultural self-sufficiency, then it would seem human beings would coexist in very small social groups, and certainly never in urban agglomerations. The trend is, of course, counterbalanced. It is counterbalanced in a number of ways and to varying degrees. But I would suggest that there are some societies where the tendency is never effectively contradicted. In these, the movement toward village and even family disparateness leads to total breakdown of communication and cooperation (Beals and Siegel 1966). This sometimes even leads to warfare and warfare's less-overt form, witchcraft.
It is my duty to give primary consideration to two societies in which fission was a predominant characteristic. Since I have inferred that fission is destructive, this consideration may give unnecessary emphasis to cultures without evolutionary potential (Sahlins and Service 1962: 93). But since they illustrate what I suggest is a basic fact of the neolithic, their use is justified. However, throughout this paper consideration is given to vehicles for counterbalancing fission and societies in which they were effective. This study, therefore, is centered on neolithic fission and the cultural devices that either support or counterbalance it.
CHAPTER 2

OWASCO AND IROQUOIS

This chapter on the Iroquois contains the first test for the hypothesis outlined earlier. In fact, it is three tests using one culture at different times in its history. Before the burden of the chapter begins, it would be useful if I say what I want it to accomplish. First a brief outline of the four major sections of this chapter will be presented. Following this, Robert Whallon's work on the Iroquois and proto-Iroquois will be discussed at length, for it is a tested situation for the hypothesis. Then three situations from later Iroquois history stand as testable conditions for the hypothesis. In other words, Iroquois dependence on agriculture is measured against some of the changing patterns in Iroquois social organization. This is done four times in one culture's history.

Of the statements that are to follow the last three are hypotheses. The first statement is a fact, not a hypothesis. But it is the fact from which the hypotheses are derived. Each of the hypotheses has alternative expressions. These will remain implicit. The prime aim of the chapter will be tests for the three specific hypotheses.

The initial situation and the one from which the hypotheses are derived concerns the Owasco and proto-Iroquois. The era involved
is A.D. 1000 to 1650. In this era increasing reliance on agriculture led to an increase in village isolation.

In 1965 and 1966 Robert Whallon studied the archaeological remains of the proto-Iroquois, called Owasco, and of the early Iroquois proper. The village was his unit of cultural analysis and it will be mine all through this chapter. He demonstrated that changes in regional interaction and degree of matrilocality were positively correlated with one another. As matrilocality became dominant, rate of regional interaction declined. Whallon demonstrated a decreasing rate of village wife exchange. He inferred and partly demonstrated a changing rate of social autonomy, in the direction of greater village independence or isolation.

In his discussion of the economic base for the period A.D. 1000-1650 Whallon describes the introduction of agriculture into a hunting-gathering society. Agriculture never fully replaces dependence on wild food in the entire prehistoric and ethnohistoric sequence, but it does become more and more firmly established. Whallon argues it reaches a peak of acceptance and more or less remains static, but the evidence he cites seems to show no such agricultural, hunting-gathering equilibrium. But an argument for increasing dependence on agriculture, and hence, for increasing economic autonomy exists. This is the natural complement for the argument of increasing social autonomy which Whallon has successfully designed.

The era of the League of the Iroquois is placed between A.D. 1650 and 1776. The hypothesis involving this period suggests that
a trade network enlarged the economic base, necessitating broader social ties between villages.

The hypothesis is firmly backed when the historic Iroquois are examined. The League of the Iroquois is an example of a mechanism counterbalancing the trend to heterogeneity between villages. It is an example of a force prevailing over an internal tendency toward fission. What is its economic base? Does this base create economic interdependence?

The Post-League era dates from 1776 to 1805. The hypothetical relationship between economics and social organization for this period is that emphasis on economic independence at the village level leads to the decline of regional social bonds.

The economic plight of the Iroquois in defeat and the subsequent rise of Handsome Lake and his religion can be seen in terms of the hypothesis. The Iroquois in defeat no longer could depend on a vast over-arching trade network. Part of the Iroquois nation's economic base was shattered. Agriculture became the major part of the base once more, as the forces linking the League fell apart. The cohesion binding the League might have been preserved by close economic integration with the conquerors, but this did not occur. The dominant society would not, or could not absorb them into its economic life. So the Iroquois were once more independent farmers. They had few concrete ties between villages among themselves, and fewer with the English and Americans.
The Handsome Lake era begins at 1805. The hypothesis for the period suggests that ideological unification followed an externally imposed economic threat forcing greater internal economic cooperation.

Handsome Lake's religion established a bond of social coherence that had not been seen since the advent of the League. The bond was ideological, not political. And it can be seen as a response to being on the losing end of an economic battle with the Americans. Economic competition with the foreigners bred greater social cohesion. The vehicle for cohesion in this case was ideological, whereas with the League it had been political.

If the hypothesis, increasing economic autonomy leads to increasing social autonomy, describes reality, these hypothetical relationships should be demonstrable.

**Owasco and Proto-Iroquois**

Of the recent studies in archaeology dealing with inferences about social organization, three are noteworthy here, although only one deals with the Owasco. Deetz's will be considered in the next chapter. Whallon's will be the bulk of this one. And for the moment a study by Constance Cronin will be discussed (Martin, Rinaldo, Longacre and others 1962: 105-114). Cronin did her work under the direction of Paul S. Martin of the Field Museum of Natural History, Chicago. In the Southwest she demonstrated that if a standard list of design elements is used to describe a series of pottery types and the percentage of occurrence per element per site is calculated, then the frequency of designs used becomes centered in proportionately
fewer elements as time advances through the period A.D. 750-1225. Fewer elements come to represent proportionately more and more of everything that is used. Although the absolute number of design elements increases through time, proportionately fewer elements take up a greater bulk of all the elements used. If popularity were charted on a graph, the curve created would be flatter earlier in the sequence. This reflects the more even distribution of use of any given element earlier in time. The curve becomes more peaked later as proportionately fewer elements account for a greater percentage of total elements used.

Cronin stated her case in terms of "drift" and stylistic trends in ceramics (Martin, Rinaldo, Longacre and others 1962: 108-9). Whallon has, however, extended the social implications. After doing a very similar, but much more comprehensive study in New York State archaeology, he arrived at substantially the same conclusions. His statistics showed a general trend to increasing in-site homogeneity as one came toward the present. That is, although the specific number of elements measured, changes both through time and from site to site, a curve measuring popularity or use of elements is steeper later in time. Whallon calls this an increasingly high level of homogeneity in his sites and suggests that it is indicative of a high level of village matrilocality. Women are not being recruited from a wider region, but increasingly from the village itself. Iroquois ethnography substantiates this inference. Also, of course, the measured change in stylistic homogeneity can be "interpreted as a reflection
of a gradually decreasing rate of social interaction between villages. Decreasing variability is thus thought to reflect decreasing communication" (Whallon 1966: 56). Whallon cites the rise of fortifications in late Owasco times as further proof of the high state of regional fragmentation. The Owasco people, one gathers, were increasingly socially autonomous at the village level.

The Iroquois and their immediate predecessors were the most recent representatives of a long indigenous occupation of New York State. They were agriculturalists and hunter-gatherers who represented the latest adaptation to an environment largely unchanged for several thousand years. The original Archaic adaptation involved hunting and gathering as the economic base, and not until quite late in the sequence did agriculture become a fraction of the subsistence base. Whallon says that, "By middle Owasco times the shift in emphasis from hunting to agriculture was complete, and the major changes from that period on seem only to have been slight additions to the to the diversity of crops raised" (1966: 88). Middle Owasco times are about A.D. 1000. In his succeeding discussion of Owasco assemblages there is a parade of artifacts and functionally different types of sites. Whallon concludes after this information is presented that indeed Owasco economy and historic Iroquois economy were basically the same. There was a "basic stability of economic pattern in this area since ca. A.D. 1000" (1966: 104).

I have no desire to argue with that conclusion even though the evidence does indicate that the economic adjustment was not in
static equilibrium. It is naturally in my interest to demonstrate an increasing reliance on agriculture, since this is the source of economic autonomy. It is not necessary to press the point, however, because, "the changes we have mentioned are all minor intensifications and diversifications of the dependence on an agricultural base at the expense of hunting activities" (Whallon 1966: 105).

The Owasco is the earliest culture in New York State for which the cultivation of corn and beans can positively be asserted. While corn has been found on the village sites of every phase, beans are not known from the early or Carpenter Brook horizon of this culture, which marks the beginning, so far as we are presently aware, of a diversified economy wherein an accelerating emphasis was accorded hoe tillage of garden crops, including probably, but not demonstrably, pumpkins and squashes. This was essentially the Iroquois pattern, and it is abundantly clear that it preceded recognized Iroquois culture by several centuries in our area.

Hunting, fishing and the collecting of wild vegetable foods retained importance in the food quest down to historic times. Earlier Owasco stations yield more hunting equipment than do later ones,..." (Ritchie 1965: 275).

A trend in the direction of increasing reliance on agriculture is clearly present. I would construct this trend to greater reliance on agriculture to mean increasing economic self-sufficiency at the village level. And I suggest that it is congruent with the demonstrated trend to greater social independence at the village level as well.

The League of the Iroquois

The study I have just borrowed from so heavily ends just prior to the founding of the League of the Iroquois. The factors leading to the establishment of the League are probably complex and lay in a part
of Iroquois history over which we have little control. The earliest function of the League we know about was ceremonial. The organization seems to have been the same for a long period and for most of this time it also seems to have been of secondary importance in Iroquois culture. The major cause for its transformation into a trading and waring empire was pressure exerted by the French, Dutch, and subsequently English colonial powers impinging on the Iroquois resources and territory. The League in its mature form was a union within the Iroquois nation and of the Iroquois with other groups. Its chief activities were the expansion and defense of certain economic activities and long occupied territory. All of this centered around trading (Spencer, Jennings and others 1965: 386-393; Underhill 1953: 83-92).

The League directly contradicted the trend away from regional cohesiveness. The inter-site heterogeneity Whallon found was the product of a breakdown in regional "interaction." The League effectively stopped the warfare of Iroquois with other Iroquois (Underhill 1953: 92) and consequently must be looked on as contradicting the trend toward social autonomy between villages. The economic base behind the League was a trading network which had developed with the Europeans and their New World representatives. The complex of alliances and systems of representation making up the League created links between tribes, between regions, and between villages hitherto having minimal or even predatory relations with each other. The
change in level of social intermixture and communication is obviously great, compared to the isolation of the Owasco era.

The scope of regional economic and social interaction was broadened during the life of the League. That the League did not last, an interesting and useful fact in itself, is not the immediate point. But rather my main concern regarding the League is that it demonstrates that a positive increase in degree of economic cooperation was accompanied by a similar one in social cohesion.

The League and its economic base inaccurately reflected the major source of subsistence for these people which was still agriculture. When it was relied on as the sole base, agriculture provided inadequate support for supra-village union in this area. The defeat of the League and destruction of the economic activities behind it lead to a swing among the Iroquois to the fission common prior to its advent (Wallace 1961: 144-146).

The lessening of social autonomy between villages in the time of the League was not accompanied by a firm economic base to underpin inter-village cooperation. Decreasing social autonomy was not accompanied by a permanent economic resource demanding decreased economic autonomy. The beaver market was everlasting, but the beavers were not. Consequently there is some justification for the argument that the League would not have existed in its advanced form had it not been for the outside pressure of colonial Europeans. The beaver trade and its economic and political ramifications provided the impetus to supra-village cooperation among the Iroquois. The League, a vehicle to
facilitate cooperation, existed aboriginally and was elaborated upon
and turned into an institution with real power as well as ideological
potency only when competition and coordination between the Iroquois
and the outside forced its development.

The era of the League represents an increase in inter-village
economic cooperation and also a broadening of the social ties between
them. We can also say then that unless the stimulus for a change in
the status of social autonomy is founded in the internal economics of
the culture, rather than outside it, the resulting change in social
autonomy will dissolve when the outside force is removed.

The Post-League Era

After the dissolution of the League, the Iroquois were thrown
back to dependence on farming and on subsidiary positions in the cul­
ture now encroaching on them. In defeat the Iroquois might have been
absorbed into the surrounding culture with some speed, but this sort
of utopian guess contradicts what we know of the action of British
colonials from 1607 to the action of their descendents up to the
present. Since there was no effective means to weld the Iroquois to
the surrounding society, and since there was no economic force
welding them strongly to each other on a village level, the trend
toward economic autonomy began once more (Wallace 1961: 145). There
was, of course, no effective institution to take advantage of what­
ever was left of the Iroquoian consciousness of being a single people.
Nor was there now economic justification for such an institution.
Consequently, in the post-League period there was a trend to economic
autonomy which was created as much by remoteness and rejection from American society as by continued dependence on farming. The destruction of the beaver and of the amalgamating trade that furry little animal created bolstered the trend to fission.

This economic autonomy on the level of the village or large family was matched sociologically by the absence of any effective supra-village organization. The status of economic autonomy after the destruction of the League seems to have been only slightly different from what it was before it. It is interesting to note that in the immediate post-League period no organization like it in overarching nature appeared. The hypothesis suggests that none appeared because there was neither economic backing nor base for one. The increasing economic autonomy in the post-League period, as compared with the economic cooperation during the League's effective period, was matched by the obvious increase in social distance between groups in this post-defeat era.

**Handsome Lake Era**

The rise of Handsome Lake, the prophet, and his religion in the eighteenth century, form the end of the Iroquois sequence examined here. The Handsome Lake religion is often presented as a revival or nativistic movement. It revitalized the Iroquois, or really provided the most obvious form of a new set of principles for Iroquois to live their daily lives by. The religion was famously successful and revolutionized Iroquois behavior and what was left of the course of Iroquois history (Wallace 1961: 146).
The religion created an ideologically and sociologically more homogeneous society. It counteracted social fission. Whether or not there was a concomitant lessening of economic autonomy to accompany it, is the open question. Internally in Iroquois society the breakdown of farming family autonomy and village autonomy began with the advent of the League. By the time Handsome Lake became a part of the scene, the aboriginal social structure was dissolving. Village residence patterns were destroyed during the League defeat, and never effectively re-established. However, in the course of American expansion the Iroquois were exposed to the whole series of acculturation threats a more efficient culture presents to a less efficient culture. The threat forced a certain degree of cohesion, which was manifested most noticeably in the new religion. This culture, with a still viable economic base, agriculture, was faced with the threat of complete submergence by another. It had a degree of cohesion forced upon it when outside pressure once again forced the Iroquois to cooperate among themselves. This time real political cohesion was impossible, and consequently the form emphasized to support increased social homogeneity was the Handsome Lake religion. There was still no economic base in Iroquois society inherently necessitating cooperation. But American society with its power and pervasiveness became ever more permanent and effectively contradicted Iroquois economic autonomy to the point where a mechanism facilitating social cohesion emerged (Wallace 1966: 31-33).
The degree to which the Iroquois depended on agriculture is directly correlated with the degree to which they tended to disintegrate. This was the hypothesis about the Iroquois and has been shown to be a fact of Iroquois history. If one graphed the changing degree of social distance in the period of proto-Iroquois and Iroquois history I have discussed here, the League of the Iroquois and the Handsome Lake religion would represent the major counterbalancing vehicles in the general tendency to increasing social distance and heterogeneity between groups of Iroquois. Both were reactions to external pressure and do not represent trends inherently founded in Iroquois economics. Nevertheless, among the Iroquois the external forces were potent enough so that when economic cooperation was demanded, social cohesion followed. Competition with the outside world can reverse a trend to social fission in a society. This can occur even though there is no internal economic reason for such a move. We all recognize, of course, that war with your neighbors can be functional.
The point of a generalization is to order data. And the point of science is to order the data so as to account for all of them empirically. This task in science is accomplished by the tested hypothesis. This chapter and those immediately following are an attempt to test a hypothesis in some depth. These chapters inevitably contain material about the history of the particular culture, the prehistoric Mogollon, used for the testing of the hypothesis, but culture history is not their chief aim. The history of these Indians is interesting and even intriguing, but its sole use in anthropology may be as a laboratory for testing.

In the course of utilizing the Mogollon laboratory, it was necessary to formulate methods for testing the hypothesis involving agriculture and social distance. Many of these arguments of relevance were developed by two students who worked on the problem as a project in the National Science Foundation's Undergraduate Research Participation Program. This work was done at the Field Museum's Southwest Expedition Headquarters in Vernon, Arizona in the summer of 1967. Nancy E. Cohn of Miami University, Ohio, and Timothy K. Earle of Harvard University are responsible along with me for this reasoning (Cohn and Earle 1967). I am indebted to Keith H. Basso
and Clifford Sifton for the burden of the arguments involving male mobility. The statement of them is mine, so are any inadequacies in it.

The Mogollon culture area occupies a central section in the Southwest. The geographical area is broken and mountainous, but environmentally homogeneous. The Southwestern Expedition of the Field Museum of Natural History, Chicago, under the direction of Paul S. Martin, has been investigating a valley on the geographical and environmental periphery of the Mogollon area since 1961. The Hay Hollow Valley (Fig. 1), east of Snowflake, Arizona, has been the scene of archaeological and paleoclimatological investigations by members of the Expedition. These include important studies involving the reconstruction of social organization by William A. Longacre (1966) and James N. Hill (1966).

The valley members of the Expedition have been working in is today cattle land and has been overgrazed. This and the genuine decline in rainfall since the turn of the century have desiccated the area and made it inhospitable to humans. Prehistorically the case was different, and the archaeological record shows a long occupation of varying density. The earliest occupation in the region is Desert Culture. However, the earliest radiocarbon date the Expedition has obtained from the valley is 1000 B.C. Up to A.D. 1300 the population and settlement patterns vary in density and ecological adjustment. At about A.D. 1300 the valley was abandoned. We have just finished a very thorough survey of about one-third of the valley, and found
Figure 1. The Hay*
Hollow Valley)
that different environmental settings were utilized throughout the historical sequence. We defined three major zones and several minor ones. The major zones are the valley bottom, the alluvial fans forming a kind of piedmont area, and the uplands which are mesa and mountain tops.

We found that initially occupation was principally on the valley floor. With the introduction of agriculture, occupation began to ascend the slopes and was sometimes placed on the mesa and mountain tops. When the maximum utilization of agriculture was reached in the valley, occupation tended once more to center on the borders of the valley floor and on the floor itself. This outline does some violence to specifics, but so do all outlines. In general it is accurate.

The key sites I am interested in for this chapter all date within the horizon A.D. 400-800. The sites we utilized are called N.S. 5, N.S. 11A, N.S. 92, N.S. 94, N.S. 137, and L.S. 199. N.S. means New Survey. L.S. means Longacre Survey. The numerals simply order the sites and reflect no more than the order in which they were discovered and recorded. The survey by Longacre was carried on in 1960-1962. The New Survey, the third one to involve some of this area, and the completest one of this valley, was done in the summer of 1967 and continued in the summer of 1968.

The horizon, 400-800, was chosen and takes that temporal form for several reasons. Pottery appears as early as 650 B.C. in the Pine Lawn area of the Mogollon region. For that area Martin has obtained a University of Michigan radiocarbon date (M-718) of
641 B.C. ± 200. But a plentiful supply of sherds was needed and that is unobtainable until a much later time. Dating also had to be controlled fairly accurately. The scattering of early black-on-white painted types available by A.D. 500 aided here. The boundaries of the horizon were created by the time range of the tree-ring dated pottery types we used, not by a predetermined plan to use this period. The period does not identify any particular cultural stage.

I have utilized six sites from this horizon, but the valley from which they are all drawn has dozens, some of which could just as easily serve the purpose. Therefore, this experiment could be rerun many times on the basis of site frequency. These sites are all pithouse villages, with the exception of one that may be a small pueblo. Since one of the interesting features of the valley is simultaneous occupation of pithouse villages and small pueblos around A.D. 600-700, the inclusion of one of these in our sample is not unwarranted. We think these two kinds of sites may be functionally different, since they occupy different ecological niches and consequently have different settlement patterns.

The sites extracted from our survey for use in this analysis had to be selected, not on a random basis, but on the basis of whether or not they had enough potsherds on the surface to guarantee us a large enough sample for statistical validity. Consequently, while there are dozens of sites of the appropriate period, we wanted those which were both spatially close and littered with potsherds. Littered with potsherds means simply that we felt we had to have at
least 1000 of them. Even then we had a hard time meeting this criterion. The sites were chosen only from the area surveyed. Our criteria were such that the sample taken is total. Since none of the sites was chosen to influence positively either variable in the hypothesis, it cannot be claimed our experiment is circular. The sites were chosen because we felt the data were available on them that were relevant in testing the hypothesis. We could not predict the results of testing before we began, however.

To get the sherds we needed, only surface collections were made. No excavation was done. Most of the sites in the valley are single components, so mixture was no problem when surface collections were taken. An occupation may, however, be 200 years long and as a result may contain a fair amount of style variation. We are able to hold length of occupation fairly constant in all the sites we used as a result of the dated pottery types. Therefore, variation coming as a result of differing lengths of occupation can be ruled out. The same types were found in roughly the same proportions at all the sites. These assigned the same range of dates to the sites initially. The range was raised or lowered by using other less prominent types and by giving weight to proportion of occurrence. But this does not change the fact that each of the six sites was occupied around 200 years. This figure was determined by using tree-ring dated pottery types.

The pottery from any site was homogeneous enough temporally in terms of the classically defined Southwestern pottery types so
that we had little doubt about creating the horizon. There was an amount of pottery at every site we analyzed that obviously was temporally aberrant from the bulk of the collection. These types were always a minor percentage of the collection and were never included in the later calculations. Most of the temporally different pottery was indigenous to the immediate region. It either represented an earlier or later occupation, or the overlap from a nearby site from a different era. In all cases it was small enough to be ignored, especially since we were sure of the dates of the types we did choose for analysis.

Many an archaeologist can be heard to murmur and sometimes bellow that sequence must come before social organization. I will never argue the point straight on. In fact, if sequence had not been so well controlled here, the experiments of the Southwestern Expedition would never have been possible. Tree-ring dated pottery and the use of that system enabled us not only to date our sites relatively but to assign dates in the common calendar to the ends and some of the points in the sequence. There is overlap in some of the sites. This was intended. There is a gap in the middle of our total sequence: A.D. 400-800 and 1000-1300. The gap between 800 and 1000 was not intended, but does not alter the results. We have not used radiocarbon dating for this material.

Figure 2 summarizes the pertinent dates for sites used in this chapter. Table 1 presents the pottery types used and their dates. And Table 2 lists the sherd counts used for the analysis in this chapter.
Figure 2. Dates for sites in the era A.D. 400-800 based on tree-ring dated pottery types.
Table 1. Tree-ring dated pottery types used to date the sites in the era A.D. 400-800.

<table>
<thead>
<tr>
<th>Types</th>
<th>Approximate Tree-ring Dates</th>
</tr>
</thead>
<tbody>
<tr>
<td>Alma Plain</td>
<td>A.D. 300 - 950</td>
</tr>
<tr>
<td>Alma Scored</td>
<td>A.D. 775 - 925</td>
</tr>
<tr>
<td>Forestdale Red</td>
<td>A.D. 600's - 700's</td>
</tr>
<tr>
<td>Forestdale Smudged</td>
<td>A.D. 640 - 715</td>
</tr>
<tr>
<td>Lino Black-on-gray</td>
<td>A.D. 570 - 870</td>
</tr>
<tr>
<td>Lino Gray</td>
<td>A.D. 570 - 870</td>
</tr>
<tr>
<td>White Mound Black-on-white</td>
<td>A.D. 750 - 910</td>
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Table 2. Sherd count by site, type, and color category

<table>
<thead>
<tr>
<th>Site</th>
<th>Smudged</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>red</td>
<td>brown</td>
<td>gray</td>
<td>red</td>
<td>brown</td>
<td>red</td>
<td>brown</td>
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<tr>
<td>L.S. 199</td>
<td>317</td>
<td>286</td>
<td>220</td>
<td>91</td>
<td>26</td>
<td>14</td>
<td>35</td>
</tr>
<tr>
<td>N.S. 92</td>
<td>218</td>
<td>127</td>
<td>111</td>
<td>45</td>
<td>9</td>
<td></td>
<td>13</td>
</tr>
<tr>
<td>N.S. 137</td>
<td>126</td>
<td>105</td>
<td>65</td>
<td>45</td>
<td>18</td>
<td>1</td>
<td>17</td>
</tr>
<tr>
<td>N.S. 94</td>
<td>40</td>
<td>29</td>
<td>29</td>
<td>15</td>
<td>1</td>
<td></td>
<td>14</td>
</tr>
<tr>
<td>N.S. 5</td>
<td>55</td>
<td>38</td>
<td>18</td>
<td>19</td>
<td>3</td>
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<td>5</td>
</tr>
<tr>
<td>N.S. 11A</td>
<td>22</td>
<td>11</td>
<td>13</td>
<td>24</td>
<td>6</td>
<td>1</td>
<td>9</td>
</tr>
<tr>
<td>Total</td>
<td>778</td>
<td>596</td>
<td>456</td>
<td>239</td>
<td>63</td>
<td>16</td>
<td>93</td>
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Table 2. Sherd count by site, type, and color category—Continued

<table>
<thead>
<tr>
<th>Site</th>
<th>Alma</th>
<th>Plain</th>
<th>Scored</th>
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<tr>
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<td></td>
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<td>L.S. 199</td>
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<td>237</td>
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<td>N.S. 137</td>
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<td>1168</td>
<td>137</td>
</tr>
<tr>
<td>N.S. 94</td>
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<td>506</td>
<td>140</td>
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<tr>
<td>N.S. 5</td>
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<td>178</td>
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<td>N.S. 11A</td>
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<td>475</td>
<td>140</td>
</tr>
<tr>
<td>Total</td>
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<td>5977</td>
<td>1243</td>
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Table 2. Sherd count by site, type, and color category—Continued

<table>
<thead>
<tr>
<th>Site</th>
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<th>Miscellaneous</th>
<th>Total</th>
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</thead>
<tbody>
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<td>Black-on-Gray</td>
<td>Corrugated black-on-red</td>
<td>Corrugated black-on-white</td>
</tr>
<tr>
<td>L.S. 199</td>
<td>464</td>
<td>43</td>
<td>1</td>
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<tr>
<td>N.S. 92</td>
<td>658</td>
<td>30</td>
<td>1</td>
</tr>
<tr>
<td>N.S. 137</td>
<td>106</td>
<td>17</td>
<td>4</td>
</tr>
<tr>
<td>N.S. 94</td>
<td>185</td>
<td>20</td>
<td>1</td>
</tr>
<tr>
<td>N.S. 5</td>
<td>205</td>
<td>5</td>
<td>10</td>
</tr>
<tr>
<td>N.S. 11A</td>
<td>87</td>
<td>13</td>
<td>14</td>
</tr>
<tr>
<td>Total</td>
<td>1705</td>
<td>115</td>
<td>1</td>
</tr>
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</table>
Dependence on Agriculture

There are two variables in the hypothesis being tested here, changing dependence on agriculture and changing social distance between villages. Measuring variations in social distance was relatively complex and involved the bulk of time spent on the experiment. The technique for doing this was borrowed immediately from Robert Whallon's dissertation, *The Owasco Period: A Reanalysis*, and ultimately from Lewis R. Binford. The efforts at measuring social distance are discussed later. First I want to outline the method of measuring increasing dependence on agriculture.

Most investigators who have thought about it assume that all the classic Southwestern prehistoric cultures were dependent on agriculture to a large, if not complete extent. We know some of these cultures used more agriculture than others. Some farmed earlier, and some more completely. But it is assumed that they were dependent on farming, and that that condition existed roughly from the time of Christ.

It seemed reasonable to us that there should be a continuum in degree of dependence on domesticates for the prehistoric Mogollon. The continuum would, after all, begin at least a couple thousand years before Christ, and would run in largely unbroken fashion until the Mogollon ceased to exist. Actual measures of this increasing dependence using archaeological materials can take several forms. The method I did use to illustrate and measure increasing dependence on agriculture was the work of Fred Plog, a member of the Expedition.
Borrowing an initial piece of evolutionary logic from Sahlin's and Service's *Evolution and Culture*, he reasoned that a generalized economic base such as that of hunter-gatherers would have a generalized or varied tool kit. The wider the range of niches or ecological opportunities utilized, the wider would be the range of technology to capture the food potential latent in those niches. By the same token a specialized economic base such as agriculture would require a specialized and narrower range of variation in its tool kit. Farmers do not necessarily have fewer tools, but of the entire range possible, greater development occurs in those ranges touching on agriculture. Therefore, of the total range of tools possible for hunter-gatherers and agriculturalists in a region, the hunter-gatherers would tend more to using all categories equally, and the farmers to emphasize a specific range over others. Although fewer categories of tools would tend to dominate the farming assemblage, these general categories would be proliferated for precise tasks. If one could categorize and measure this differential variation in tool assemblages, one would have a measure of the change of dependence on agriculture.

To test the validity of this hypothesis, Plog utilized data from Mogollon site reports, principally from Martin's work in the Reserve area of New Mexico (*Martin, Rinaldo, Bluhm* and others 1952; *Martin, Rinaldo, Bluhm* 1954). By measuring the variability in the tool kit he was able to show that in the Mogollon area covered by the reports the coefficients measuring variability fluctuated slightly from the time of Christ to about A.D. 600. Plog reasoned that
measured variability in types of tools could be used as an indicator of dependence on agriculture. He created curves (Fig. 3) describing the variability in projectile points, manos, and scrapers. These cover the period from about 300 B.C. to about A.D. 1200 or 1300. I have combined the curves for these three tool categories and created one curve which will generally represent changing degrees of reliance on agriculture (Fig. 4).

From before the time of Christ to about A.D. 550 the graph shows the Mogollon economic base to have been a varying combination of agriculture and hunting-collecting. We know from archaeological data that this dual exploitation existed. We can now say that the combination existed in some equilibrium through this time. In the period after A.D. 550, to about 750 the curve describing variability slumps, indicating that dependence on agriculture decreases. The reasons for this are not clear. This is a period of population expansion. This is inferred from the increase in number of sites for the era. An expanding population utilizing hitherto unoccupied niches may have had to increase the scope of its technology to adapt to a range of new resources. But the reason for the increase in variability is still not really clear. It is accompanied, as I will discuss shortly, by a marked decrease in social distance over the same period.

Figure 4 shows that from A.D. 750 to about 900 reliance on agriculture rises, only to decline once more until around 1050. After this last date reliance on agriculture increases steadily. This last era, however, belongs to the next chapter. It should be understood,
Figure 3. Summary in variation in projectile points, manos, and scrapers for the Reserve area.
Figure 4. Variability and covariation of social distance and dependence on agriculture.
naturally, that at no point in this entire sequence was hunting or gathering reduced to negligible proportions.

The result of Plog's work on measurement of variability is included in graphs in Figures 3 and 4. Each curve in Figure 3 represents the measured variability in the artifact class indicated. The categories making up variation in manos, projectile points, or scrapers at any single period are derived from the reports providing the data in the first place. The graph in Figure 4 representing the combined curves measuring changing reliance on agriculture also shows the curve describing changing social distance. The whole sequence is presented here to increase perspective, although the period after A.D. 800 will not be discussed until the next chapter. The formula measuring variability is that used for measuring variability elsewhere in this study. It appears in Appendix 2.

A measure of dependence on agriculture, I have argued, is also a measure of economic autonomy. So long as there is no mechanism, like exchange of goods or services of a sort without which agriculture cannot work, there is nothing in farming itself to require members of different villages to assist one another in the tasks of agriculture. Agriculture probably does not lead to complete village self-sufficiency, but it does provide an economic base which can be efficiently run on a village basis alone.

It is the variations in this economic base which have been measured. And it is these which show that agriculture undergoes fluctuations in intensity of use. If we could be confident that there
were no exchange of economic essentials between producing units in this period, the case for growing economic independence would be stronger. But I have to assume there is no system of economic links dealing in exchange of primary economic goods between villages. That is not a bad assumption since there is no archaeological evidence for such exchange, and ethnographically only salt can be constructed as a major primary economic resource that could not be obtained locally. Trade *per se* does not accurately reflect the state of economic independence. A better key to that state is the relevance to economic success of the items or services exchanged. Therefore, shell and turquoise do not count, regardless of their assumed ritual potency. But salt, or for example, stone for grinding or cutting do. As a result of all this, I suggest that in this area as farming increases in a group so does economic independence. This comes as close as I can now to measuring changing use of agriculture and by inference the first variable in the hypothesis, economic autonomy.

**Social Distance**

It is necessary to go from the central Mogollon area where Plog drew the evidence used to measure increasing dependence on agriculture to the particular area on the Mogollon periphery where variations in social distance were measured. By inference we have to let what was measured in one place stand for what was not measured in another. This cannot cause major concern, however, since the Hay Hollow Valley is adjacent to the core of the Mogollon region proper and is more like it environmentally than not. Obviously a good
scientist would measure both of his variables on identical data in
the same laboratory. We could not do this because we did not have
equal data, or enough time to augment them.

Social distance in an archaeological situation seems an anom­
lous phrase. Archaeologists have not usually addressed themselves to
this sociological phenomenon. However, with one bit of insight, Lewis
Binford and Robert Whallon have been able to do this successfully. I
have borrowed their technique, and I acknowledge every debt to both
of them for breaking such hard and hitherto impenetrable earth.

I have discussed in Chapters 1 and 2 the method Whallon used
in his work in New York State to measure change in social distance.
He assumed on the basis of ethnographic analogy that women made the
pottery. He assumed that mothers taught daughters how to make the
pottery, and consequently that if women were permanently resident in
a single place, there would not be as much variation in the pottery
styles as there would be if women shifted their place of residence
after marriage. If women were recruited from outside the village, a
possibility even in matrilineal societies, there may be more varia­
tion in female-produced artifacts in a site than there would be if
the women were recruited from within the village's walls. It is in­
ferred that in the latter case, there are fewer style traditions and
the results are, by nature of the contact situation, less variable.

A discussion of the method for measuring the variation in
ceramic style has preceded this. It is necessary, however, to de­
scribe what was measured for these villages, and why those data are
relevant for sociological inference. This transition from Whallon’s New York data to mine from the Southwest involves different cultures, but analogous analytical categories.

Six sites were used in the period A.D. 400-800 and these are littered with pottery. Most of it is plain. There is some early black-on-white painted ware which was used for dating. The two black-on-white types were Lino Black-on-gray and White Mound Black-on-white. The bulk of the experiment used the Forestdale types, as they have been classically defined (Haury 1940: 68-77).

There is a wide range of color variation in the Forestdale types. The color variation involves both the interior and exterior of the jars and bowls, and we used both pairs of data in this study. The variation in color on the exterior and interior is caused by differing manufacturing and firing techniques, we argue. We presume a degree of homogeneity in the region's clay resources. This, when fired, produces pottery of a standard brown or brown-red color. We reason that variation beyond the constant basic color is due to firing technique. We argue, furthermore, that the technique is learned, passed down by mother and grandmother to daughter and may produce variations which can be used to distinguish one group of manufacturers from another. We assume firing techniques are taught in the same lines that carry ceramic techniques. Consequently we suggest that color variation beyond a certain point is the result of learned techniques for controlling oxygen to produce a desired shade. Because the region from which our samples come is no more than two
or three square miles, we can hold constant regional differences in fuel, raw material, tempering inclusion, or any other natural, non-cultural factor that could account for the non-random variation in color range we found. It is possible that varying groups used different fuel resources or different tempering materials in the firing and manufacturing processes, and that this resulted in color differences. However, there is no reason to assume this is accidental, or available only to some, not to all, since the area is so small. This is the reason we chose sites that were close together. In short, whatever is used to produce color variation is chosen, not because nothing else is available but because of the range of choices available, one or a combination has a desired result.

All of the above arguments can be conceived of as hypotheses that are testable. Opposed would be the equally testable hypothesis that ceramic techniques are carried on more or less haphazardly. Should the latter hypothesis be the case, the variation which we know exists would be close to random. Should the former hypothesis more accurately reflect the circumstances under which pottery is made, then the results will be ordered, and ordered in a predictable way.

To measure color variation we began by using the Forestdale types: Forestdale Smudged, Forestdale Red, and Forestdale Black. Two problems arose. These classical types do not provide enough categories to measure the color variation that exists, and one type, Forestdale Smudged, contains only bowls. The solution to the first problem was reached by subdividing the types on the basis of exterior
color. This gave us three kinds of Forestdale Smudged: black interior-red exterior, black interior-brown exterior, black interior-gray exterior. Forestdale Red was divided into red interior-red exterior, and red interior-brown exterior. Forestdale Plain was divided into brown interior-red exterior, and brown interior-brown exterior. This division does no more than subdivide the initial types more finely; it does not cross-cut them. But instead of three classes to measure color variation, we now had seven.

This initial segregation was done subjectively. But because the range of color is not precisely broken, the categories were given Munsell Soil Color Charts designations to avoid total subjectivity. This procedure did not make divisions less arbitrary, it merely made them consistent. The Munsell designations appear in Table 3.

The second problem was more difficult. Of the three types, one, Forestdale Smudged, contained only bowls. The other types cross-cut functional classes of containers. The problem is that variation between sites may be due to the variation in the proportions of functionally different types of containers from site to site.

To address this problem ratios were created between the number of sherds of bowls, the Forestdale Smudged category, and sherds of all other kinds of vessels that fall into the Forestdale Red and Plain types. A z-test for comparison of proportions was used on the proportions created by the raw data, the sherd counts (Simpson, Roe, Lewontin 1960: 194). A .95 confidence limit was established. On the basis of this test N.S. 5, 94, 137, and 199 are shown to be of the
Table 3. Color definitions from Munsell Soil Color Charts

<table>
<thead>
<tr>
<th>Name</th>
<th>Hue</th>
<th>Value</th>
<th>Chroma</th>
</tr>
</thead>
<tbody>
<tr>
<td>red</td>
<td>2.5 yr</td>
<td>4-6</td>
<td>6-8</td>
</tr>
<tr>
<td></td>
<td>5 yr</td>
<td>4-6</td>
<td>4-8</td>
</tr>
<tr>
<td>brown</td>
<td>7.5 yr</td>
<td>4-6</td>
<td>2-6</td>
</tr>
<tr>
<td>gray</td>
<td>10 yr</td>
<td>3-5</td>
<td>1-3</td>
</tr>
</tbody>
</table>
same population. That is, they can be validly compared. N.S. II A and 92 fall outside the confidence limits and consequently are excluded from final consideration. N.S. II A was eliminated by inspection and was not included in the statistical tests, and did not influence the test's results.

As a result of the test for comparing proportions, functional variation between N.S. 5, 94, 137, and 199 can be held constant. N.S. II A cannot be used to measure color variation and then compared with the others since it is not possible to say what is causing the variation in it. Nor is N.S. 92 to be used in the comparison. Variation in function is held constant, and does not account for the variation within the color categories from village to village. As long as the sites measured have the same proportions of container types, the analysis is safe to this point. Excepting N.S. II A and 92, they do and it is. The variation within the categories, then, does not represent primary function. The cause of color variation, different manufacturing or firing techniques, seems logically established. The variations within the categories, then, can be used to distinguish between groups of manufacturers and manufacturing traditions.

The frequency of color categories at any one of these sites may be described by a curve. The curve for every site is different and the coefficient of variability describes that curve. The more equally represented all the categories are, the smaller the coefficient will be. N.S. II A, for example, has comparatively little variation between its categories and its coefficient is .15.
The four most functionally similar sites, N.S. 5, 94, 137, and L.S. 199, have marked variation in the proportions between their color categories. For example, the temporally and functionally equal N.S. 94 and 137 have quite different internal pictures (Table 4).

A chi-square test was run on this table. The hypothesis tested was that the two sites represent one population, or that the way sherds were distributed in the categories at one site was similar to the way they were distributed in the same categories at the other. To obtain expected frequencies for the test, the two Forestdale Plain categories and two Forestdale Black categories were each collapsed to one category apiece. A .01 significance level was set on the test. Given these conditions the hypothesis was not positively tested, and one is forced to adopt the conclusion that the two sites represent two populations, not one. Despite differences in absolute numbers, it can be seen that the ratios between the categories differ. This difference is the basis of our argument that sites, otherwise equal, contain significant variation caused by differing methods of manufacture and firing. We conclude that there is a variety of firing traditions represented at these sites, and that these are the result of series of micro-traditions coming from a wider region to any specific site. Therefore, the coefficient of variability measures a part of the external contact any site has with the world outside itself.

At this point it is necessary to discuss some technical problems arising from measuring variability. One is the objection that the coefficients are only a seriation. The objection arises from the
Table 4. Sherd counts for color categories at sites 94 and 137.

<table>
<thead>
<tr>
<th>Site</th>
<th>All Other Container Types</th>
<th>Bowls</th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Forestdale Smudged</td>
<td>Forestdale Plain, Forestdale Black</td>
<td>red</td>
<td>brown</td>
<td>gray</td>
<td>red</td>
<td>brown</td>
<td>red</td>
<td>brown</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td>red</td>
<td>brown</td>
<td>gray</td>
<td>15</td>
<td>1</td>
<td>0</td>
<td>14</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>N.S.</td>
<td></td>
<td>126</td>
<td>105</td>
<td>65</td>
<td>45</td>
<td>18</td>
<td>1</td>
<td>17</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>94</td>
<td></td>
<td>40</td>
<td>29</td>
<td>29</td>
<td>15</td>
<td>1</td>
<td>0</td>
<td>14</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>137</td>
<td></td>
<td>126</td>
<td>105</td>
<td>65</td>
<td>45</td>
<td>18</td>
<td>1</td>
<td>17</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>


fact that the categories used to measure variation represent only a
fraction of the whole range of recorded variation across all types.
It can be said that all that is measured is style change in three
types through 400 years. First, if this experiment works, it will
inevitably seriate these sites. That does not mean that the results
of calculating the coefficient of variability are predetermined. The
fact that style changes through time is just that, a fact. This study
depends on that change and describes it more precisely. It is an
effort to explain why it occurred.

If the popularity of the Forestdale types varied from time
to time between A.D. 400-800, this may have affected the absolute num-
bers of sherds in any category. It may not be held to account for
changes in relative proportions between categories, however. This is
especially true in a functional class like bowls, for example, where
proportions do not vary greatly.

Style change, an increase or decrease in the use of any of
the characteristics of the Forestdale types, may be due to a large
collection of factors. I have tried to hold constant functional vari-
ation and any variation due to spatial distribution. It might be
argued that when the Forestdale types come to predominate in the sites
as they do through time, this increase would affect the variability
measured. There is little evidence that this happens, however, for
as the proportion of Forestdale types increases, the internal vari-
ability measured on them goes down, not exactly a predictable re-
sult. Table 5 illustrates this. One might expect greater variability
with greater popularity. The table does not reflect this.
Table 5. Coefficients of style variability and popularity of Forestdale sherds.

<table>
<thead>
<tr>
<th></th>
<th>Sites from Early to Late</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>11A</td>
</tr>
<tr>
<td>Coefficient of Variability</td>
<td>.15</td>
</tr>
<tr>
<td>Ratio of Forestdale types to all others at a site.</td>
<td>1</td>
</tr>
<tr>
<td></td>
<td>10.5</td>
</tr>
</tbody>
</table>
The table in effect shows that as numbers of Forestdale sherds increase, the range of color variation found on them does not increase. In fact it decreases. Recall that the scale measuring variability reads from zero, complete variability, to one, no variability. The fractions in Table 5, which are ratios, show increasing popularity of Forestdale types. This happens as internal variability decreases, which indicates that increasing popularity of Forestdale types is not a factor forcing the coefficients of variability to show more internal variation.

If a given color category loses or gains in popularity through time in these six sites, it obviously affects the coefficient. A given category, or in another analysis a given element, may fluctuate in popularity, thus changing its relationship to other categories or elements. This changed relationship affects the coefficient of variability. Therefore, ordinary style change through time affects the variability of any single synchronic collection. Variability at a synchronic instant is caused by the number of potting traditions present. The changes in variability from site to site through time are due to fluctuating popularity of style categories. The relative constancy of the Forestdale types in relation to one another has been shown. However, fluctuating popularity of style categories through time is merely a reflection of the number of microstyle traditions present. The popularity or even presence or absence of traits depends on the number of traditions present. Therefore, when relative frequencies between categories from site to site change, it can safely
be inferred that number of microstyle traditions does also. In this chapter I have tied microstyle traditions to social groups. From changes in microstyle traditions I have inferred changes within and between social groups.

Social groups are tied together by kinship, and it is within this framework that endogamy must be considered. This issue can be explored briefly. It is possible to have weak matrilocality even when a group is matrilineal; the Western Apache are an example (Goodwin 1942: 127). I do not find it necessary to suppose that the Mogollon recognized descent in one way as opposed to another. If they were matrilineal and weakly matrilocal, the pattern we have found in these villages between A.D. 400 and 800 would be complementary. It would also be complementary under an assumption of patrilineality and weakening patrilocality. Either of these two alternatives is acceptable since the coefficients of variability for the period A.D. 400-800 show far more variation, and hence regional recruitment, than they do female stability. It is not impossible that women were still being recruited to villages in appreciable numbers well into the Mogollon era. This society relied so heavily on hunting-gathering that it may have retained a patrilineal adaptation for a much longer time than we commonly suppose. Among the pertinent and safer conclusions is that the potters have to move if we are to account logically for what the patterns in style distribution show. And as long as the potters move, the groups are neither endogamous, nor independent.
Although I have presented a measure that reflects ever stronger matrilocal residence, it is possible that while this measures the decreased mobility of women, it says nothing about male mobility. Consequently increased social isolation or independence at the village level is not necessarily a safe inference. Males may be drawn from other regions or other villages. Should male mobility exist, then village isolation or independence has not been established at all. All that has been established, it might be argued, is that women have stopped moving on being married.

In order to face this question, I would ask what is the logic in recruiting males from outside the system, that is, outside the village? Given that matrilocal residence is established and a system of increasingly heavy dependence on agriculture has been demonstrated, then what is the economic function of drawing men from outside the farming village? Men hunt. They may farm and do the heavy chores, and they may collect. But they are the chief means for keying into the energy represented by game. Group hunting is usually more productive than single men stalking. Hence ties to other villages insure several things: groups large enough to carry on successful drives of animals, automatic entree to hunting preserves controlled by regions, villages, or families, and a guarantee that some of the men, if not all, are going to be well versed in the idiosyncrasies of the microenvironment entered, as well as the ways of the game in it. As long as hunting is an important part of the subsistence base
it would then seem to be economically advantageous to preserve a network of male cooperation between groups.

With agriculture becoming an ever greater proportion of the economic base, the importance of hunting naturally declined, and declining, too, would be the importance of maintaining all the sociological equipment necessary to key into game resources more effectively. The cause for a declining need to obtain men from other villages is tied to a declining need to exploit game. But more forceful reasons for localizing males come from the social system growing up within village walls as agriculture develops.

Borrowing both from pueblo ethnography and cross-cultural circumstances among agriculturalists, it is possible to say that in the prehistoric Southwest agriculture probably involved: corporate systems of land tenure and joint estate ownership; men doing some of the farm labor; the mother's brother playing an important role in raising the sister's children; men managing family affairs; and being chief principals in religious rituals. All or any one of these is more effectively done if men are natives of the village and are not brought in as strangers after having been enculturated in some other village. Conversely, it is equally disadvantageous to lose men at the age of chief productivity, obliging them to return periodically to fulfill economic, kin, or ritual obligations.

I cannot demonstrate archaeologically that land was conceived of as jointly owned estate. Nor can I demonstrate that men leaving a village to join another would be completely devoid of real and
ritual property, and probably difficult to place in the new system as a result.

On the other hand, it is observable that generally once wealth and possessions begin to accumulate and are controlled in family lines, the desire to maintain the property in a coherent mass grows. Sending off male members of a family who own, helped accumulate, and manage property would not aid in any facet of property control.

If property were communally owned and none was concentrated in family hands or in the hands of individuals, then segmenting males from the community would not be so economically disruptive. But we have evidence that family lines were increasingly emphasized. Style traditions, which are supposed to be connected with family traditions, segregate out from one another more clearly through time. By A.D. 1000 every village is clearly different from every other and so are traditions of style within the village (see Chapter 5). With the increasing prominence of family lines, it is only too likely to find that one of the corollaries to this is the concentration and lineal definition of property.

Among the pueblos, children are brought up by their mothers and by their maternal uncles as well as by their biological fathers. The maternal household is the fundamental organization (Eggan 1950: 298-300). The mother's brother plays a key role in raising his sisters' children. This is very difficult to do when living in a different village. It is also either difficult or impossible to help govern the affairs of one's natal household and its property when
living apart from the village. Matriarchies do not exist, and the role of the adult son in any matrilineal household cannot be dismissed as secondary or subsidiary in decision making. Marrying him out of the village is not usually done lightly.

Religious rituals are usually male specific and require by their very nature long training in esoterica. All the divisions of the Mogollon sequence have kivas which are taken to be ceremonial chambers where rituals justifying and sustaining the economic and sociological activities of the group were carried out. The villages I have used in this chapter have not been dug, but if they are typical Mogollon sites many would have a kiva or what we suppose to be a ceremonial chamber. Kiva ritual, and in fact any sacred manipulations, involve men who handle the ritual paraphernalia and who have easier entree to deity and the supernatural generally. All of this is private information involving long training and initiation, and usually includes material sacred and idiosyncratic to single locations, groups and villages. It is presumed that effectiveness in dealing with the supernatural is tied up with ritual competence. Since aspects of this are based partly on village specific phenomena, marrying out of ritual functionaries is not advantageous to a community's ceremonial and spiritual health.

I would argue that in the archaeological situation presented here there is no good economic reason for men to be brought into a village, as opposed to having husbands recruited from within the resident population. However, in the face of all the above argument, it
must be remembered that for most, if not all, of the period A.D. 400-800, agriculture is not the sole subsistence base. There is a fluctuating equilibrium for most of this period between hunting-collecting and farming. Consequently, even though the tendencies I have shown or suggested for these villages are in the direction of greater isolation, nothing like complete breakdown is even vaguely suggested for the 400-800 era. The neolithic was becoming better established; it seems that matrilocality was too. I argue that a whole series of economic and social corollaries follow from this. The most obvious is that men also become localized. In fact, the men always were localized, and a process of multiple village recruitment never had to be contradicted for males. Among hunter-gatherers, males remain, out of sheer economic necessity, the core to which females from plural sources are added. At no stage of economic change in the Southwest would there be a period when it would have been advantageous to draw males from outside the group on a consistent basis. Economics do not seem to demand it in the process of ecological adjustment in the Southwest.

To contradict the last conclusion the existence of cooperative efforts at various times in the Southwest can be constructed as a basis for inter-village cooperation. Joint expeditions for salt, cooperative defense against marauding outsiders, and reciprocal ritual relationships can be cited as examples. It must be kept in mind that I am not trying to establish a case for utter regional constriction and isolation in the Southwest. The indices of variability indicate nothing approaching maximum social exclusiveness or complete
endogamy. Nor is it my aim to pronounce in favor of an interpretation like that. If increasing agriculture is causing the rate of interaction to go down, it is equally obvious that full dependence on agriculture was never realized for this or any other period. Consequently all that the hypothesis expects is a rising index of regional isolation. The fact that some exogamy existed both prehistorically and ethnographically is not upsetting, nor is cooperation in some economic tasks, nor defense alliances, nor ritual reciprocity. Agriculture does lead to isolation. But agriculture was never the sole economic base, and the other economic activities, various forms of hunting and gathering, demanded a broader base of cooperation. I do suggest, however, that the forms of interdependence, cooperation, and reciprocity were not of the sort to interfere with the isolation increasing reliance on agriculture produced.

I would argue that in the process of adaptive change in the Southwest the pan-regional communication and cooperation that probably describes the interaction of the big game hunters of the Palaeo-Indian era gave way to groups with successively more limited economic and social horizons. Desert Culture adaptation required less mobility than big game exploitation. Early agriculturalists, in their turn, required even less external contact. In this process of regional atomization, which was a consequence of economics, the need for external ties became even smaller. The need for drawing men from distances never existed; the need for drawing them from short ones is difficult to justify at best, unless a village is so small it cannot
provide its own women with husbands. In this case, men would have to be recruited externally and logic and tradition dictate that recruitment be on a very local level.

It is my suggestion and argument, then, that increased reliance on agriculture and increased matrilocality include, at least in the Southwest, a positively correlated trend to endogamy. Ultimately males are no more mobile than females.

To actually carry out the experiment the Forestdale types were singled out as most likely to fit the requirements of our analysis. After defining, collecting, and quantifying the data relevant for our purposes, we used a formula to compute the variation for each site (Appendix 2). The formula is designed to hold constant the number of categories used to describe the variations at any one site. It allows the frequency of use of the categories, not the number of categories, to determine the coefficient of variability or homogeneity. Consequently a set of variations at any site can be adequately described, and expressed as a coefficient that is comparable with all other coefficients when derived by the same formula. This is so even when the categories measuring variation vary with every site. The categories, however, have to be analogous between sites.

The same set of color variations was used to describe all six of our sites dating from A.D. 400-800. But these variables were not used to describe sites dating from A.D. 1000 to 1300. Nevertheless, both sets will be validly compared later.
We have previously ordered the sites chronologically. From earlier to later they are 11A, 5, 94, 137, 92, 199. The coefficients measuring degree of internal variation are .15, .44, .23, .38, .38, .40. All these appear in Table 6. With an initial exception, the trend is toward greater internal homogeneity in style, as measured by color variation. The coefficients of N.S. 92 and 11A are to be ignored for reasons discussed earlier. The prediction, if the hypothesis were to be positively tested, was that with increasing reliance on agriculture, endogamy, our measure of social distance, would increase.

Summary

This chapter covers the span A.D. 400-800. The fluctuations of agricultural dependence have been described. In summary, from 500 to 700 there was a general decline in dependence on agriculture; from 700 to 900 there was a general increase in use of domesticates. The results of measured social distance for the same period coincide to a flattering degree with the swings of the agricultural dependence graph. Beginning with an emphasized degree of distance between villages at 500 to 550 there is a general down swing to greater social interaction until 700. This coincides nicely with what happened to the economic base, namely decreased reliance on agriculture. I suspect the reasons for this lie in the conflict of different economic bases. By 500 or 600 successful agriculture had clearly been differentiated from hunting-gathering. Since at this horizon both types of economies existed and utilized the same environment, it may have
Table 6. Coefficients of internal variation based on color categories in Forestdale types.

<table>
<thead>
<tr>
<th>Site</th>
<th>Sample Size (Sherds)</th>
<th>Date</th>
<th>Coefficient of Variability</th>
</tr>
</thead>
<tbody>
<tr>
<td>L.S. 199</td>
<td>989</td>
<td>A.D. 600-800</td>
<td>.40</td>
</tr>
<tr>
<td>N.S. 92</td>
<td>523</td>
<td>A.D. 600-800</td>
<td>.38</td>
</tr>
<tr>
<td>N.S. 137</td>
<td>377</td>
<td>A.D. 500-700</td>
<td>.38</td>
</tr>
<tr>
<td>N.S. 94</td>
<td>128</td>
<td>A.D. 500-700</td>
<td>.23</td>
</tr>
<tr>
<td>N.S. 5</td>
<td>139</td>
<td>A.D. 500-600</td>
<td>.44</td>
</tr>
<tr>
<td>N.S. 11A</td>
<td>85</td>
<td>A.D. (400) 500-600</td>
<td>.15</td>
</tr>
</tbody>
</table>
been natural for hunter-gatherers to regard the agriculturalists' surpluses as a part of their harvestable resources. If this exploitative relationship was the case, then the agriculturalists would have been forced to cooperate among themselves to a greater degree for protection. This would have counteracted the trend to fission, and would account for the decline in stylistic differentiation which identifies a lessening of social distance between villages. A decreasing reliance on agriculture here may have been precipitated by pressure from exploiters who forced the agriculturalists to diversify their economic base while the competition lasted. From A.D. 700 to 900 or 1000 there is a steady, even sharp rise in social distance between villages; this is, of course, an era of increased reliance on agriculture.

I construct these fluctuations as a positive test for the hypothesis. Style variability, which measures endogamy, and by reflection social distance, first goes down significantly and then rises. I have reasoned that this is due to a changing number of microstyle traditions present at a site. Fewer microtraditions, for example, mean fewer women drawn from outside the village to be wives. When this happens I infer an increase in endogamy, realizing that male mobility has been inferred, not established. With a rise in endogamy, it is presumed village interaction is decreased. Social distance increases with the absence of connecting ties, like wife exchange, and the absence of trade in goods of primary economic importance.
Trade of a sort like that in the prehistoric Southwest does not create connecting ties which effect social distance, especially endogamy. Items were brought to villages; villages did not come to central points. There seem to have been no redistribution centers, and hence no effective way for trade to counterbalance the isolation.

Both variables in the hypothesis have been examined, changing dependence on agriculture and positively corollated changes in social distance, endogamy. Given all the appropriate qualifications about incompleteness and imprecision, the variables seem to accompany each other in the same directions. Is this an accident?
CHAPTER 4

THE MOGOLLON: LATER VILLAGES

The economic and social dimensions of any culture have the same measurements. Is this a fact or a hypothesis? Regardless of its state of certainty, the statement may take a more specific form which no one will deny is a hypothesis. That is, that degree of dependence on agriculture and social distance are positively correlated.

If the hypothesis is really only a more specific version of my initial statement, then it probably is a fact, and testing would be superfluous. However, the trick is in establishing the logical equality of the two propositions. Since that is so difficult to do without explaining the variety of forms the neolithic took, around the world, the choice remaining to me is to regard the agriculture-specific statement as a hypothesis, and to test it. That is what this chapter, like the last, and the next, is all about.

The following section is a measured demonstration that in-site variation of style declines over the 250-year period between A.D. 1000 and 1250. The hypothesis suggests that decreasing dependence on agriculture produces decreasing social distance between groups. It seems that either degree of dependence on agriculture, or agriculture's basic-nature was changed between A.D. 1000 and 1250. The evidence indicates that both occurred. The evidence also indicates a basic change in the social system for the same era.
To measure social distance between the villages we were concerned with, we used the design elements on black-and-white painted pottery. The rationale for using these elements specifically had previously been worked out by William A. Longacre (1963) and James N. Hill (1965). The rationale for using elements or style for analytical units in prehistoric ethnography was constructed by James F. Deetz and has already been discussed.

The real labor in measuring social distance described in this chapter was done by Eliza F. Howe (1967) of Vassar College, Susan Menkes of the University of Indiana, and Charles L. Redman of the University of Chicago. As participants in the Expedition they manipulated the ideas and data forming this chapter's central concern. The analysis in this chapter is their work essentially. I will assume responsibility for the shortcomings.

Between A.D. 1000 and 1300 occupation in the Hay Hollow Valley reached a maximum, declined, and ended. This corner of Mogollon territory saw its highest development by 1000 and declined afterwards until by 1300 the valley had been abandoned forever by pueblo building peoples. Some Athabascans may have frequented it later, but I know of no archaeological evidence to support that idea. The Spanish and the Mexicans went through the area. The Americans and the Mormons came. Then the sheep. Then the cattle.

At A.D. 1000 several dozen small pueblos were dispersed throughout the Hay Hollow Valley. These averaged 20 to 30 rooms and were invariably perched on low mounds at varying distances from the
main stream in the valley. They were all set on rises on the valley floor. The closer to 1300 one comes, the larger the villages become. They also became fewer, until by 1250 only two or three 100-room units were left widely scattered in the valley. General geographic preference did not change in this period.

The 300-year era which forms the horizon for this chapter has been the most thoroughly investigated of all the eras in the Hay Hollow Valley. Two of the villages, Carter Ranch and Broken K, have been dug. These represent the beginning and end of the 300-year sequence forming the end of this valley's prehistory. They were dug and analysed by William A. Longacre (1963) and James N. Hill (1965) respectively. The analyses of aspects of the sociology of these pueblos were the first ever done in the Southwest; some of it was among the first ever attempted in North American archaeology. These will come under consideration in the following chapter.

It is very difficult to say accurately how many sites occur between A.D. 1000 and 1300 and with what declining frequency in time and space they were placed in the Hay Hollow Valley. Surface collections from six sites were taken. After these six no other large sites were left to us in the area surveyed, except Broken K which had already been dug. The area surveyed, however, is only a fraction of the valley and outside this area there are dozens of other sites from the same period. Not all are large; some have ten, five, or two rooms. Only a few sites in the survey area had sufficient pottery to allow us at least 1000 sherds in a surface collection. All the sites
in the survey area with that many sherds were collected. These sites represent a total sample. The area these sites come from is the same as that for the sites used in the last chapter.

The conditions we worked with have just been outlined. One other facet of Expedition research, however, was a resource of extremely useful knowledge. This is the research done in palaeoenvironmental reconstruction. These investigations were done, as were all the others, in the early 1960's. They involve the work of specialists in fossil pollen analysis. The data cover four to five thousand years, and possibly more. The palynologists worked with the Expedition and analyzed the data at the Geochronological Laboratories at the University of Arizona in Tucson. James Schoenwetter and Richard Hevly are responsible for this work (Martin, Rinaldo, Longacre and others 1962: 168-209; Martin, Rinaldo, Longacre and others 1964: 171-187).

**Environmental Reconstruction and the Economic Base**

Inferences drawn from reconstructed ancient environments show that beginning about A.D. 1000 there were changes in both fossil pollen frequencies and the pattern of arroyo channeling. These are taken to indicate an environmental shift of some consequence in a large area of the Southwest. These data are well controlled for the east-central Arizona and west-central New Mexico area. To explain the pattern found in the palynological and geological records a hypothesis concerning a shift in rainfall pattern has been set forth (Martin, Schoenwetter and Arms 1961). This has been subscribed to by a number
of palaeoclimatologists (Martin, Rinaldo, Longacre and others 1962: 202). It is sometimes proposed in opposition to a hypothesis of Ernst Antevs (1955), and is generally antithetical to the "Cyclical Drought" hypothesis.

Schoenwetter suggests that the shift causing the patterns he found involves a change from rainfall evenly distributed throughout the year, to rains falling in two seasons, with most rain falling principally as torrential downpours in the summertime after the shift. Total amount of annual rainfall would not have to have been affected, suggest the geochronologists.

This new pattern initiated the arroyo cutting cycle with the sudden and swift pressure and volume of the rains. Light winter rains soak in and do not run off, but the summer rains produce so much water so quickly that there is no opportunity for it to soak in. The torrents carry away soil and soon begin cutting deeper stream channels. If this is accompanied by a thinning of the ground cover, the process is aggravated. The thinning of the cover is adequately demonstrated in the pollen record. Accompanying this is a lowering of the water table from previous equilibrium. This is caused by decreased penetration and increased run-off.

With the change in rainfall pattern the population of Ponderosa pines retreated to higher altitudes and the population of low weeds grew much larger. The environmental shift worsened the conditions for growing corn. Not only was the water table lowered by arroyo cutting, but the total length of the growing season was made
more precarious. Both heavy gully-washes and unpredictable periods of rainless weather, hampered farming (Martin, Rinaldo, Longacre and others 1962: 201).

There is a wide range of evidence to show how much more difficult it was becoming to farm. "There is evidence at Broken K, for example, that agriculture was becoming difficult and that it was replaced to a large extent by the collection of wild vegetal foods" (Hill 1965: 244). This is illustrated by the declining frequency of Zea pollen from early to late Broken K (Hill 1965: 244). Deteriorating agricultural conditions necessitated an increase in storage space. This space houses both the larger amounts of wild plant harvests and the larger amounts of harvested domesticates. The larger amounts of corn would come from the need to grow more in any one year due to the undependability of any given harvest. Hill's figures on changing storage capacity (Hill 1965: 246) are shown in Table 7.

It was evidently becoming more difficult to hunt, also. Hill (1965: 252) takes averages of hunting tools per habitation room and demonstrates a shift in frequency of occurrence of 5.20 at Carter Ranch to 5.53 at early Broken K to 2.87 at late Broken K. The hunting tools he used are arrowshaft tools, projectile points and antler flakers. Game unquestionably became scarcer, and hence reliance on it declined. All ranges of fauna were becoming scarcer (Hill 1965: 248). This does not mean that hunting was abandoned because other forms of subsistence were more productive; there were simply fewer things available to hunt. The rainfall fluctuation eliminated the
Table 7. Changing requirements for storage space in Carter Ranch and Broken K.

<table>
<thead>
<tr>
<th>Site</th>
<th>Date</th>
<th>Percentage of storage space*</th>
</tr>
</thead>
<tbody>
<tr>
<td>Carter Ranch</td>
<td>ca. 950-1125</td>
<td>21-25</td>
</tr>
<tr>
<td>&quot;Early&quot; Broken K</td>
<td>ca. 1150-1220</td>
<td>25</td>
</tr>
<tr>
<td>&quot;Late&quot; Broken K</td>
<td>ca. 1220-1283</td>
<td>1.1</td>
</tr>
</tbody>
</table>

* Percentage of total floor space devoted to storage rooms.

(Dates for Carter Ranch should be increased by about a century, and those for "Late" Broken K by about 50 years.)
habitat of mountain goats and deer. It either created or expanded that for cottontail rabbits (Hill 1965: 248). Consequently this species' presence in archaeological context predominates. It is safe to infer that the environment was getting harsher for survival both in terms of farming and hunting.

Despite what has just been said, it is not clear whether or not there was a marked return to heavy reliance on hunting and collecting. Hunting as a part of the economic base increased in importance between Carter Ranch and Broken K. This is supported by changes in frequency of hunting equipment (Hill 1965: 252) and changing frequency of faunal material (Hill 1965: 253). But in later Broken K hunting, too, had fallen off in importance. There is an increase of faunal material per unit between Carter Ranch and early Broken K. But by late Broken K times this is not so evident. After A.D. 1000 hunting as a part of the economic base was initially emphasized, but as the range of wildlife decreased with the onset of habitat change, hunting declined.

During this change when small game was hunted extensively, communal drives would have been vastly more effective than a net or trap which handles only one animal at a time. The archaeological record shows an increase in use of rabbits as a food source. Any emphasis on this kind of game is simultaneously an emphasis on the kind of cooperative effort needed to harvest it successfully. This is just an instance of what must have taken place in economic adjustment, therefore necessitating some kind of social rearrangement. The
imperatives to greater economic cooperation seem to be demanding that if survival in this particular niche is to continue, the degree of economic cooperation between villages or between groups must increase.

Lack of clarity also surrounds the status of agriculture in the Hay Hollow Valley from A.D. 1000 to 1300. It is not wholly clear whether dependence on agriculture decreased or stayed more or less the same. We can say out of hand that under adverse climatic conditions dependence on it did not increase in this valley after A.D. 1000. We know that circumstances for agriculture worsened until final abandonment by farmers around 1300. Even if the ratio between hunted and gathered food to domesticated food remained the same, with a worsened environment farming required a greater effort than ever before. Should the ratio have changed in favor of using naturally available resources, even then agricultural effort may not have gone down, just the resulting product from it.

Regardless of whether the ratio of wild to domesticated products changed or remained static, the environment was responsible for a change in the economic base of the Mogollon beginning about A.D. 1000. At first hunting was intensified and agriculture was not. Subsequently much more effort was put into gathering wild plants as it became harder, and riskier to farm and as hunting produced less. Economic independence was compromised in any case. I argue that both a reversion to hunting and intensified collecting compromised the social base used for intensive dependence on agriculture.
The changes in social organization were most sharply demanded by the conditions to which farming had to respond. Some farming adjustments we can hypothesize; some factors we know. Greater amounts of land probably had to be tilled to increase yields to insure against environmental disaster. Swift and violent downpours and run-offs had to be guarded against. Dikes and ditches, play a role here. Rain flattened plants and eroded fields had to be tended to quickly. We know of no water-control devices in the Hay Hollow Valley, but alluviation or erosion can have eliminated very primitive preventive measures. In fact, as a social unit this valley was not an adaptive success, so any water-control measures probably failed, and failure probably involved destruction of the preventive efforts. Positive measures like irrigation never were used here.

A reversion to hunting, collecting, and an intensification of agriculture and its technology all required reliance on more than nuclear or extended families. All these factors argue for a shift in social relations toward a broader extension of ties between economic units.

The earlier indicators of changing reliance on agriculture borrowed from Schoenwetter, Hevly, and Hill have taken the form of varying frequencies of economic plant pollens and the remains of the plants themselves. Hunting tools have been used to measure changing dependence on wild fauna. The changing storage capacity in two pueblos suggest the changing relationships in wild and domesticated plants. All concur in indicating that an ecological adjustment
occurred between A.D. 1000 and 1250. This involved an ultimate decrease in the amount of domesticates available for consumption. It involved a short term emphasis on game which slackened subsequently. And it involved a massive concentration on wild plant foods. All this is most profitably viewed as an effort to make agriculture work under changed circumstances, not as a mindless impulse to hunting-collecting. All of this had social consequences leading to greater cooperation and integration. This will be measured shortly.

Demonstrating the condition of agriculture in the Mogollon region for the era A.D. 1000 to 1300 has been a two-part task. The generalized chart (Fig. 4) which describes the fluctuations of dependence on agriculture shows a fairly clear picture for the west-central New Mexico region. The data from which the graph is derived come from the Reserve area of the Mogollon region in New Mexico. They consequently measure dependence on agriculture there, as was described in the last chapter. The data do not necessarily accurately reflect conditions in the Hay Hollow Valley.

The graph shows an unmistakable rise in dependence on agriculture from A.D. 700 on to about 900 or 950. With the inception of the environmental changes beginning around A.D. 900, a decrease in agricultural dependence occurred. This period of decline and readjustment lasted about a century before another rise in agricultural dependence began at about 1050. This final rise is the economic adjustment that led to the climax known as the swamping of the Mogollon (Wheat 1955: 205-233). This manifestation known either as
Prehistoric Western Pueblo (Johnson 1965) or Late Mogollon (Wheat 1955: 52-3) is an apical adaptation to agriculture that leads to some of the largest proto-urban developments in the American Southwest. The rise in the curve beginning at 1050 or 1100 is the economic base to this highly successful neolithic adaptation.

Neolithic survival in the Mogollon after 1000 was a differential phenomenon. In some regions like the Reserve area the culture changed and survived. In well-defined pockets by permanent streams and at lower altitudes adaptation was possible (Martin, Rinaldo, Longacre and others 1962: 224-5; Martin, Rinaldo, and Longacre 1964: 221-225). In some areas like the Hay Hollow Valley, an adapted neolithic did not work. What the graph of agricultural dependence shows is a regionally successful neolithic culture, the Reserve Mogollon. It does not show the doomed neolithic of the Hay Hollow Valley that this chapter is about. If the graph described the course of dependence on agriculture in the Hay Hollow Valley, it would slope in the reverse direction — and to extinction. It does not because it does not describe that valley.

The final adaptation of farming to changed circumstances involved selecting those areas where farming had to undergo the least change. The Hay Hollow Valley was abandoned not because survival became impossible there under any circumstances, but because it was easier to move and continue farming some other place, rather than to stay and revert to hunting-gathering. In a way, the adaptation to agriculture had become too complete. However, in the process of
adaptation, various permutations on the economic base were experimented with before abandonment. Most, if not all, probably involved greater cooperation. The evidence for the social consequences of that experimentation and cooperation should exist. And they should corroborate the hypothesis, that decreased reliance on agriculture produced a lessening of social distance between villages. The form social distance takes in this experiment is a decrease in endogamy.

**Dating Controls and Measuring Style Variation**

We do not have a precise numerical curve for Hay Hollow agriculture. We simply know that by 1300 there was no agriculture. Some of the data used to demonstrate the condition of agriculture come from the Hay Hollow Valley. But they do not come from the sites used to measure the condition of social cohesion. To measure that variable, six sites with extensive numbers of potsherds on the surface were collected. These all had large numbers of painted sherds on them and since there was more black-on-white than red-on-black ware, we decided to use the black-on-white ware for the sociological experiment and the other for dating. However, in reality both kinds of painted wares figured in the dating.

Most of the painted pottery types we dealt with are well dated by tree-ring association. We were able to break the sites we collected down into credible intervals of 100 years using the presence or absence of dated types. Securely dated types like McDonald Painted Corrugated and St. Johns Polychrome that have relatively narrow temporal ranges made this arrangement possible. McDonald dates
approximately between A.D. 1200 and 1300; and St. Johns dates between A.D. 1175 and 1300 (Breternitz 1966: 84, 93).

This left us with groups of sites. To order all of them chronologically we then allowed relative frequency of earlier versus later types to calibrate the sequence into 50-year intervals. This was not always trustworthy, consequently not all the temporal gaps are the same length.

The elements on the pottery are the visible manifestation of learned habits of decorating ceramics. The painter has been taught by her mother and grandmother and replicates what she has assimilated and reproduces the tradition she was taught. This is the way the already familiar reasoning goes. If a presumably single-lined tradition is localized in one place, it ought to be distinguishable from all other such encapsulated small potting and painting traditions. It has been demonstrated convincingly by Deetz, Longacre, and Hill, and by this time, undoubtedly by others too, that such localized traditions do exist, and that they exist within and between villages. Both Cronin and Whallon have adequately demonstrated intervillage differentiation. Whallon showed that the more traditions drawn on by a village, the more variable the pottery was at any given site. This is at least what might occur given certain circumstances of economic adjustment. For example, this model would be a logically valid hypothesis for testing if an increasingly successful economy were producing a budding population that failed to maintain contacts between parent and daughter communities.
An early demonstration of the behavior of style through time in this area of the Southwest was carried out by Constance Cronin of the Field Museum's Southwest Expedition. This was explained in an earlier chapter, but needs repetition here because her experiment provides a temporal link between the material in this chapter and Chapter 3. In 1960 Cronin examined collections of sherds from seven sites dating from A.D. 750 to 900. After sorting the two basic types, Kiatuthlanna Black-on-white and Red Mesa Black-on-white, into design element categories, she calculated the relative frequency of use of each category. Basically she compared the elements to establish the degree of similarity of pottery types at any one site, and the degree of similarity between representatives of any one type at plural sites. She found that "there is a greater degree of similarity (shared design elements) between types at one site than between different time levels of one type" (Martin, Rinaldo, Longacre and others 1962: 109). Among other things this means that at any site the types on it have more in common with each other than any one type does with a member of its like type at another site. Red Mesa at one site has more in common with Kiatuthlanna at that site than it does with Red Mesa at any other site. The degree of similarity can be expressed as a coefficient and this changes through time in Cronin's study. The coefficient grows larger. This means that types at sites become increasingly homogeneous through time, A.D. 750-900, and members of any one type become increasingly differentiated from one another between sites through time.
From A.D. 750 to 900 I have no data on measured frequency changes for style categories. On the graph (Fig. 4) describing that changing curve there is an assumption that between A.D. 800 and 1000 the coefficients measuring internal variability continue to rise. Cronin's work makes this assumption as close to being a fact as it is possible to have it now (Whallon 1966: 47).

In some ways Cronin's study is an independent test for the changing degree of variability I have examined in this paper. Her work is independent of mine, of course. It is also in a closely adjacent but not identical area. Cronin, Whallon, and I have all tried to measure the same phenomenon. Our inferences have, however, not been the same. Cronin's work has shown implicitly, for example, how the number of pottery types in the Southwest has increased through time. She has taken a microscope to this problem and has shown the process of differentiation. My own work is an attempt to explain why it happens, and, of course, I have borrowed from Whallon.

It would be difficult to over-emphasize just what the range of variability in design elements is. Those elements I am most familiar with are derived from Snowflake Black-on-white and its varieties (Martin, Rinaldo, Longacre and others 1964: 110-125, 160-1). Cronin used similar variation to measure pottery type homogeneity in and between sites. The variation in elements can also measure in-site variability and by reflection can also measure inter-site similarities and differences. It can also measure the variation between residence units, given data on residence patterns in a village. The
last point is the subject for the next chapter. For measuring variation between villages no in-site divisions were controlled, and provenience of sherds on a site before collection was not noted.

In the process of measuring the stylistic variability of the material in this chapter, surface collections from six sites were taken. Five were analyzed. One collection was misplaced. The analysis began with the definition of design elements common to all the sites. This gave a consistent core of measurable units for every site. Every site had a number of elements that were not shared either with the common list or with any other site. The number of these unique elements decreased through time. All elements were noted and frequency of occurrence counted, so that when internal variability was calculated no range of variation failed to be considered within the class measured. This class, of course, is design elements on all the black-on-white pottery from all the sites collected. Table 8 orders the sites chronologically and shows the coefficients of stylistic variability.

**Interpretation of the Coefficients of Variability**

The scale measuring internal variability runs from 0 to 1. Zero and those numbers near it mean a very high degree of internal variation. There is nothing close to that in this chapter. In the last chapter there was a coefficient of .15 which, if the sample it was derived from can be trusted, a matter of some doubt, is an extraordinarily varied assemblage. One, on the other hand, would be
Table 8. Dates and coefficients of variability for sites in the era A.D. 1000-1300.

<table>
<thead>
<tr>
<th>Sites</th>
<th>Approximate Date A.D. (± 50 years)</th>
<th>Number of Black-on-white sherds</th>
<th>Coefficient of Variability</th>
</tr>
</thead>
<tbody>
<tr>
<td>83</td>
<td>1250</td>
<td>717</td>
<td>.52</td>
</tr>
<tr>
<td>302</td>
<td>1200</td>
<td>214</td>
<td>.50</td>
</tr>
<tr>
<td>201</td>
<td>1150</td>
<td>264</td>
<td>.52</td>
</tr>
<tr>
<td>301</td>
<td>1050</td>
<td>353</td>
<td>.59</td>
</tr>
<tr>
<td>81</td>
<td>1000</td>
<td>217</td>
<td>.62</td>
</tr>
</tbody>
</table>
obtained by having a single category containing and representing all variation. One is, in effect, no variation.

It should be noted that these coefficients are markedly different from those obtained in the first chapter. They are far higher on the scale of internal homogeneity, but also change in the opposite direction from the earlier sites. The earlier sites became increasingly homogeneous. The later sites, although any one of them is more internally homogeneous than any early site, become less internally homogeneous through time.

It might be argued that the high jump in coefficients between A.D. 800 and 1000 is more a factor of statistical irresponsibility than of culture change. The argument has its strengths, but cannot reduce the increase to a decrease, and above all, cannot reduce the downslope between A.D. 1000 and 1300 to an upslope. That slope is incontrovertible and does not function as a result of what happened at any earlier era.

Figure 4, the graph showing the covariation between dependence on agriculture and social distance, holds a seemingly paradoxical situation. Agricultural dependence rises and social distance falls between 1100 and 1200. Is this a negative test for the hypothesis? Or better, is it evidence for the introduction of some mechanism affecting agricultural success and demanding greater social cohesion to achieve that success? No, the negative covariation represents agricultural success in another region, as discussed earlier. It represents social distance in the Hay Hollow Valley where the curve
of agricultural dependence plummets in the opposite direction. So there is no paradox, but rather a favorable test of the hypothesis.

The rise in coefficients, and hence the decline in internal variation between A.D. 800 and 1000 is provocative and requires discussion. It is, as the graph illustrates, paralleled by an increase in dependence on agriculture for the same period. The jump in increased internal homogeneity between A.D. 800 and 1000 (40's to 60's and 50's) is a marked overall trend to increasing social isolation between villages. This is an era of successful neolithic exploitation.

Then at A.D. 1000 the coefficients decline, indicating greater internal variability, or social interaction between villages. The decline is steady until abandonment of the valley around 1300. The course of agriculture for the era has been described; it declines. From A.D. 1000 to 1300 there are, in effect, two descending curves.

The lowest coefficient of internal variability in the later sites, .50, is higher than the highest similar coefficient, .41, for the earlier sites. This would seem to suggest that the process of isolation between villages continued. We know that agricultural dependence also did. When the final end of the sequence is examined, the lowest coefficient, .50, is still higher than anything previously reached. This suggests that social and economic adjustment to environmental shift never demanded or never gave the population a chance to revert to a social state equaling former regional economic cooperation and social gregariousness. In fairness, it must be admitted
that adaptation aimed at maintaining farming as the chief economic base was probably not an awesome task. Conditions favoring agriculture existed within a ten-mile span of the Hay Hollow Valley. To the west of the valley begins the region where the great fourteenth-century pueblos lay. Both Shumway ruin on Silver Creek and Four Mile ruin on Four Mile Creek are within ten direct miles of the valley. Both of these large ruins were populous right after the demise of sedentary village life in Hay Hollow. Both are on permanent streams, and both probably received population remnants from Hay Hollow.

Fusion of social units as seen in the Hay Hollow Valley between 1000 and 1250, and as a factor expanding the great pueblos around 1300 accounts for the increased internal heterogeneity within villages, and the lessening social distance between villages. Fusion probably exists to varying degrees everywhere in the greater Mogollon region after 1000. But once a successful adaptation was reached, fission between villages began again. This would be the case even though population levels in villages were larger and were made up in part of formerly disparate groups. The trend toward nucleation in Hay Hollow was a deceptive one. There was still no reason for people to cooperate between villages after they were settled in hospitable areas.

Ultimately there is little doubt that agriculture became unreliable enough in the Hay Hollow Valley to force abandonment. Pueblo-building peoples did not abandon agriculture, nor revert back to a hunting-collecting economic base. They moved, as they did over
this entire area, to places where agriculture was still feasible
(Martin, Rinaldo, Longacre and others 1964: 221-4). It would be in-
teresting to see if after re-establishment at lower elevations by
permanent streams, the trend toward fission began once more. This
reduces to a testable hypothesis: fission between minimal economic
units, villages occurred after economic equilibrium was re-established.

Models for Population Dynamics

A trend to agglomeration was begun as an adaptive measure to
cope with an environmental shift. The point of adaptation was to
maintain productive agriculture. What we see in the Hay Hollow Valley
is only the initial effort at a social corrective aimed at adapting
to farming in straitened circumstances. The large seemingly well-
adapted pueblos of the fourteenth century probably represent the maxi-
mum amount of social cohesion needed to cope with the kind of agri-
culture that involved check dams, catch basins, small canals, exten-
sive and extended field systems, and an authority structure to arrange
the parceling out of property, work, and responsibilities. The large
settlements of the fourteenth century are the result of the coopera-
tive efforts needed before the environmental shift plus those neces-
sitated by that shift.

From a larger point of view than the Hay Hollow Valley pro-
vides, it is possible to see that the change in weather pattern
affected the pattern of population expansion. What is seen in Hay
Hollow Valley between 1000 and 1300 is only an ancillary reflection
of a widely extended process. Population budding, hitherto the chief
means of relieving strain on the resources of a region, was completely reversed with the full brunt of the weather change. Population expansion became population implosion. After 1000 aggregation replaced and then reversed the trend to proliferation.

The shift in demographic dynamics we see was from population budding, to more stable populations characterized more by internal population fixatives. This latter model which I will label centripetal as opposed to budding, which is centrifugal, is also the model under which urbanism arose. But the differences between the circumstances in the Southwest and, say, either Mesopotamia or Mesoamerica, is that in the Southwest no cultural force short of economic catastrophe prevailed against the more natural tendency to fission. In Mesopotamia and Mesoamerica economics demanded cooperation in more complex ways from the very start of agricultural activities in those areas. Agglomeration in Mesopotamia and Mesoamerica meant easier access to more resources. In the Mogollon area agglomeration meant a reorganization of social resources so that an essentially unchanged economic base would continue to serve.

A reorganization of social ties can effect a more efficient adaptation. The biological success of the adapted Mogollon is well attested by the large number of huge pueblos of the fourteenth and fifteenth centuries. But regardless of how they reorganized themselves to better exploit an agricultural base, that resource did not change in kind.
Also there seems to be no evidence that a basic change in social organization took place. Families may have been ranked now instead of egalitarian, but the lines of kinship were not weakened. Economic task differentiation did not change. None of these characteristics we see in real proto-urban circumstances, like craft specialization and weakening of kin lines is evidenced.

In some circumstances, like Mesopotamia or Mesoamerica, the process of social isolation that goes on when population expansion by budding occurs can be contradicted very soon after it is initiated. When a region contains multiple, economically interdependent ecological zones, population concentration in a place that can capitalize on all the essential zones is naturally advantageous. The same is true when other aspects of the environment have to be controlled or effectively husbanded. Water control is an example. From these circumstances proto-urbanism emerges. Population would not tend to expand in proto-urban situations by hiving off new communities, except perhaps at a much more complex level. Where there is ecological overlap instead of ecological diversity there is a trend established toward fission because of the resulting economic redundence. In the Southwest ecological overlap occurred everywhere. The region and especially subregions are ecologically homogeneous (Kirchhoff 1954: 533). In Mesopotamia and Mesoamerica ecological redundence occurs on a far more limited scale because the regions are so varied in their natural resources. For the moment I am not concerned with the analogy in fission between small neolithic villages among the Mogollon and early
villages in Mesopotamia. I think there is an analogy and its dimensions are drawn in the Conclusions.

The social isolation produced by agriculture in a homogeneous environment such as the Mogollon area was contradicted by a perverse weather change. The change did not make the environment more productive. In forcing greater cohesion, the ecological adjustment produced no jump in economic benefits. Food resources did not increase. It neither broadened the number of ecological niches nor made any one more secure or productive. It narrowed the number in fact, and added no security whatsoever. It did not contradict the centrifugal pattern of population expansion by massing energy resources, it contradicted the pattern by making the economic base less productive rather than more. This is what happened starting by 1000, and the process served as a base for the pattern after 1300 as well.

At A.D. 1000 the basic and fundamental shift in population movement was the change from a centrifugal to a centripetal system. The population condensed and agglomerated as had never happened before. In the course of this, much of the economic and social isolation that had existed before the environmental shift was eliminated. We do see in the Hay Hollow Valley a lessening of the number of pueblos through time after A.D. 1000. And those that do remain became continually larger. What is demonstrable for Hay Hollow is also a fact for a much wider region (Martin, Rinaldo, Longacre and others 1962: 224-5, Martin, Rinaldo, Longacre and others 1964: 221-5).
Social fission between villages was eliminated at least momentarily. Village endogamy did not change, strictly speaking, but the entry of new groups and the condensation of the population probably enlarged the range of kin groups in any given pueblo. The number of potting and painting traditions would also have increased. This naturally accounts for the increase in internal style variability that we have shown occurring as one gets deeper into the period of cultural adjustment to the environmental shift after A.D. 1000.

Once the shift was adjusted to, however, a maximum and minimum of aggregation were established to meet the varying demands of the new environment. This equilibrium, reached by 1300, produced the same pattern of intervillage isolation as existed earlier. Fission began once again.

After 1300 when a stable adaptation had been reached and populations began to proliferate again, several population fixatives are evident. Hiving off is still employed as a mechanism for expansion. But hiving now probably required budding off a far more enlarged basic economic unit than the nuclear or extended family. This hypothetical model requires the founding of a whole new village of whatever size was required to control an environment for successful agriculture.

Population increase may also have been handled by limiting techniques like infanticide. The large number of infants and children buried under room floors in the big late pueblos may indicate deliberate infanticide, or higher infant mortality rates brought
about by the increased effect of communicable disease when people are gathered in larger numbers. Both the modification of budding as a device for spreading population and use of population fixing mechanisms may indicate a reduction of the absolute number of locations available for successful agriculture, and hence a need to establish a more static population.

Increasing dependence on agriculture is postulated for the period after readjustment to climatic change, and so is increasing social isolation between villages. But during the period of adjustment 1000 to 1300, isolation and dependence broke down, and movement became centripetal. But nothing existed to maintain that trend. None of the alternative corollaries negating the growth of isolation was brought to bear. The number of environments capitalized on was not increased. Water control seems to have been at a rudimentary level. Trade does not seem to have increased spectacularly, nor does pan-village cohesion.

The lone great kiva phenomenon, for example, may be seen as a cooperative device that failed. I would expect to find areas where they occur abandoned relatively soon after their appearance. They represent a trial balloon at a mechanism facilitating cooperation and did not work where the area was either too high or too dry for farming. Where sufficient heat and water did exist in an area, villages had to be enlarged, but not tied to others. Internal great kivas on the other hand were splendid devices to hold together all the hitherto disparate elements now living in one town. There is no evidence for
inter-village ritual. Nor is there evidence of competition with intruders who had a different economic base, nor of farmers fighting with farmers. None of the devices for binding groups together seem to have been present.

There is no evidence of warfare between these great pueblos. Among the inferences one can draw from that is that population levels never reached the maximum carrying capacity of the land. Or if they did, devices like infanticide or high infant mortality rates kept them at a stable level.

I have used three descriptive (Binford 1963: 91-95) and explanatory models in this study up to this point. The first drew upon a descriptive model of population expansion, or budding. I have called the budding process centrifugal. This describes the pattern of population dynamics in the Mogollon area up to about A.D. 900. The reflections of this pattern archaeologically should be two. One is a decrease in the degree of sharing of common attribute elements radially from any point in space. The second is that sharing of attributes between budding units should decrease with time as random selection factors have more time to take effect. The latter hypothesis has been tested positively.

I have explained this descriptive model with the hypothesis that as agriculture dominates, social distance increases. This is not the only explanation for the model, but given the neolithic, I suggest it is the prime one. Given the nature of agricultural economics coupled with a homogeneous environment, social interaction
between settlements falls, producing regional discontinuity. The facts found in the last chapter justify all of this.

The second model is one that is a transition to a more stable dynamic. But it is not stable itself. It is implosive and is called centripetal. It characterizes the Hay Hollow and other areas from about A.D. 900 to 1250. Archaeologically it manifests a higher degree of attribute sharing between sites than occurs in budding situations. Attributes in a site are more variable or more heterogeneous.

Explanation lies in a decrease in dependence on agriculture and behind that a change in weather pattern. This in turn led to decreased social autonomy. Decreased dependence on agriculture in a changed ecosystem led to decreased social distance so that agriculture under a changed ecosystem would remain feasible. This hypothesis, too, has been tested.

The third model is one of an essentially stable population with some budding and some internal fixing devices. It is both stable and slightly centrifugal. It would have archaeological corollaries like a high rate of attribute differentiation in a region. But in a less random fashion than a budding model. In the Mogollon area the reticulation would coincide with a narrower range of ecological zones where earlier it acted far more independently of them.

Explanation for the model lies in an increase in agricultural dependence but in an environment vastly more limited for population expansion. Environmental change yielded fewer exploitable zones,
greater economic cooperation, and more complex social relations in villages, hence a more stable population dynamic.

The first and second models have been tested. They are economical descriptions and sufficient explanation. The third model is incompletely tested at the descriptive level, so the hypothetical explanation is just that, a hypothesis.
CHAPTER 5

THE MOGOLLON: WITHIN LATER VILLAGES

The last two chapters have dealt with the economic and social changes occurring between villages in the Hay Hollow Valley. The changing pattern of interaction between Mogollon villages from A.D. 400 to 1300 is constructed as a function of economics and a reflection of social organization. It occurred to me, however, that what could be inferred for inter-community relations might also be extended to intra-village relations.

For the work in testing this hypothesis and for some of the consequent explanations, credit is due to Charles Oliver (1967) of the Oriental Institute, University of Chicago, and Charles Vanasse, University of California, Los Angeles. They were participants with the Expedition in the summer of 1967 and much of the effort in this chapter is theirs.

The original idea for measuring regional interaction belongs to Robert Whallon. He accomplished his task by measuring an in-village pattern and inferring a between-village relationship from that. It seemed reasonable, then, that if a measurement of internal variation could be coupled with what we know about residence units in prehistoric pueblos, it would be possible to measure the relationships between units within a given village. This would be a more complete statement of regional interaction as well as one that implied
the nature of economic and social ties within a village. This information would be especially valuable because it would be pertinent to solving the problem of why villages maintain a certain size in the face of general regional fission. Fission is the undeniable pattern as we have traced 900 years of prehistory in this location. If we assume that the family is the minimal self-sufficient economic unit, then why do villages, which clearly contain more than a single family, exist?

To address these aims an experiment was tried using the data and consequent reconstructions done by William A. Longacre (1963) and James N. Hill (1965). William Longacre was responsible for the definition of residence units at Carter Ranch pueblo; James Hill performed the same kind of analysis for Broken K pueblo. Carter Ranch dates from about A.D. 1050 to 1250. Broken K ranges from about A.D. 1150 to 1300. These two late pueblos consist of 45 and 96 rooms respectively. Neither was dug completely. Both were sampled. Carter Ranch was sampled on the basis of informed archaeological intuition and long experience. Broken K was sampled randomly, a table of random numbers being used to select the rooms for digging after the outline of all the pueblo's walls had been exposed on the surface.

For the statements on residence at Carter Ranch and at Broken K, design element clusterings were argued to be relevant data by both Longacre and Hill. Painting styles, it was suggested, were taught in a family line; ethnographic analogy suggested a female line. If that line was localized, so would be the design fragments.
characteristic of it. Elements were defined for Carter Ranch. Their relationship to rooms was plotted and a multi-variate analysis showed clearly that there were clear clusterings of groups of the style elements in parts of the pueblo. Some elements fell over the whole village, but some were very clearly localized in what happened to be the two obvious architectural divisions of the ruin. These, it was inferred, were the residence units of localized matriline. The issue of descent and its archaeological expression is not relevant here. It is true that that particular inference is less than secure, but the inference of residence seems to have stronger support, both from the data and those squinting at them.

Broken K was done somewhat differently. The design elements once again were found to be arranged in spatial clusters in the pueblo, this time in five groups as opposed to the two at Carter Ranch. To back up the validity of the argument that the clusters of design elements matched residence units other data were sorted which would, it was reasoned, support the inference that the clusters were created by localized groups. Fire pit styles, pottery types, storage pits, types of choppers and animal bone all sorted out on the same spatial divisions created by the clusters of ceramic design elements. None of these clusters could be explained as functionally specific areas for economic tasks (Hill 1966: 17, 20). From all of this the five residence units seem, then, to be validly inferred. Moreover, on the basis of style similarity the five groups can be divided into two larger groups. One of these contains two of the five groups,
the other three. These larger units obviously reflect some aspect of social organization. Two clans composed of two and three extended families respectively have been suggested with all cautious tenta-

tiveness (Hill 1965: 153). For the moment I am not concerned with inferences beyond residence for these data. I am willing to let the inference on residence stand on its merit and call it solid.

It became obvious that since we wanted to say something about culture change in this valley, changing relations between residence units in a village was one way to do so. This was all the more obvious since two pueblos had already been dug and analyzed so successfully. The point of culture change we had in mind for testing was that internal units in a village undergo the same fission that characterizes the breakdown in intercommunication between villages.

The original hypothesis postulated a consistent, positive relationship between two variables, dependence on agriculture and endogamy. Given previous cautions, this relationship seems to be the case when relations between towns are looked at through time. However, what happens in a village? If economic independence is fostered by reliance on farming, and if this independence is reflected in dissolving social bonds between villages, then the minimal economic unit, the extended or nuclear family, has no economic reason for maintaining ties with any other similar unit. It seemed reasonable that families in a village would become increasingly distinct and that fission, if it were indeed based on the independence of the minimal economic unit, would not recognize village walls as a boundary.
The basic, if simple, device we used to measure changing relations between residence units in a village was to compare how many design elements of all those used in a village were shared by all the groups, against those not held in common. The design element inventories had been previously defined for each pueblo by the original investigators (Martin, Rinaldo, Longacre and others 1964: 160-1; Hill 1965: 65, 67). From these inventories we were able to extract the frequencies with which each element occurred throughout each section of the respective ruins. For the experiment we used only rooms, not trenches or burials. We used the residence units as already defined. These were the basis for comparison. They are the residence unit for some kind of social group, probably extended families, but only probably.

At first we were only concerned with one pueblo or the other, although comparison between the two and within them through time was the goal. The morass of problems involving method, both statistical and comparative, is too large to enter here with profit, although our mistakes do make interesting and sometimes amusing telling.

There were 96 elements used in the analysis of Carter Ranch. We listed these and then saw where they fell in the residence units. If they did not occur in any rooms they were eliminated. This happened infrequently. They were counted if they fell in both residence units, or in only one. Elements occurring five times or less per room were considered too statistically unreliable to use. The figures resulting follow in Table 9.
Table 9. Frequency of element occurrence in residence units at Carter Ranch.

<table>
<thead>
<tr>
<th>Elements occurring six times or more</th>
<th>Elements occurring ten times or more</th>
</tr>
</thead>
<tbody>
<tr>
<td>Common</td>
<td>Distinct</td>
</tr>
<tr>
<td>62%</td>
<td>38%</td>
</tr>
<tr>
<td>(39)</td>
<td>(24)</td>
</tr>
</tbody>
</table>

Real numbers of elements are in parentheses.

Table 10. Frequency of sherd occurrence in residence units at Carter Ranch.

<table>
<thead>
<tr>
<th>Sherds occurring six times or more</th>
<th>Sherds occurring ten times or more</th>
</tr>
</thead>
<tbody>
<tr>
<td>Common</td>
<td>Distinct</td>
</tr>
<tr>
<td>77%</td>
<td>23%</td>
</tr>
<tr>
<td>(571)</td>
<td>(172)</td>
</tr>
</tbody>
</table>

Real numbers of sherds are in parentheses.
Common means the element was shared by both units; distinct, of course, means only one unit used the element. For reasons discussed below it was felt that this tabulation technique was both unrepresentative and unreliable.

The second method used involved counting number of sherds having various elements. It was reasoned that the larger numbers involved would ultimately yield a statistically more significant result, as well as reflecting distribution reality more accurately. This method yielded the results in Table 10. Common and distinct mean the same thing here as previously. The results are obviously very different, however. But this method too was found to be unsatisfactory.

To make this recitation of method as short as possible, I will discuss the most satisfactory technique used to compare what the units of residence had in common. This third comparative method assumes that although many or most elements of style may be shared by the groups in a village, some may use part of the set of elements more intensively than others. This may seem a diluted way to measure degree of isolation, but it seems an adequate measure of sharing. This method not only uses presence and absence, but also degree of presence, i.e., degree of sharing. The technique was suggested by Fred Plog of the Expedition staff and yielded quite satisfactory results.

A ratio of sharing between residence units was constructed for each design element. Each half of the ratio was listed in a column; the columns were totaled and percentages created of the
sums. A ratio of 50:50 is complete sharing; one of 90:10 approaches the reverse. Every element occurred with a greater and a lesser frequency when compared against the residence units. Elements equally shared by both units are expressed as 50:50. When the column with "lesser" occurrences was totalled and the same was done for that labeled "greater" and the sums of the columns were converted to percentages, the result for Carter Ranch was a ratio of "lesser" to "greater" reading 29%:71%. This shows a large imbalance between the units of the pueblo in terms of shared design elements. Remember that 50:50 is perfect sharing, or perfect commonness.

None of these three methods for measuring degree of comparability between units contradicts the others in terms of the general distinctiveness of the living units involved. It will become clear, however, that the last method is the preferable one for valid comparison with other sites.

The same routine was carried out for Broken K where 179 style elements were used in Hill's analysis. It had been suggested by Hill that the five residence units he was able to define were comparable to the two Longacre had shown at Carter Ranch (Hill 1965: 153). To achieve a result comparable with that from Carter Ranch, we at first counted as "distinct" those elements occurring in only one of the residence units. If an element showed up in two or more units it was counted as "common." The results of using this first method appear in Table 11.
Table 11. Frequency of element occurrence in residence units at Broken K.

<table>
<thead>
<tr>
<th>Elements occurring</th>
<th>Elements occurring</th>
</tr>
</thead>
<tbody>
<tr>
<td>ten times or more</td>
<td>six times or more</td>
</tr>
<tr>
<td>Common</td>
<td>Common</td>
</tr>
<tr>
<td>Distinct</td>
<td>Distinct</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Common</th>
<th>Distinct</th>
</tr>
</thead>
<tbody>
<tr>
<td>60%</td>
<td>40%</td>
<td>50%</td>
</tr>
<tr>
<td>(23)</td>
<td>(15)</td>
<td>(28)</td>
</tr>
</tbody>
</table>

Real numbers of elements are in parentheses.

Table 12. Frequency of sherd occurrence in residence units at Broken K.

<table>
<thead>
<tr>
<th>Sherds occurring</th>
<th>Sherds occurring</th>
</tr>
</thead>
<tbody>
<tr>
<td>ten times or more</td>
<td>six times or more</td>
</tr>
<tr>
<td>Common</td>
<td>Common</td>
</tr>
<tr>
<td>Distinct</td>
<td>Distinct</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Common</th>
<th>Distinct</th>
</tr>
</thead>
<tbody>
<tr>
<td>63%</td>
<td>37%</td>
<td>70%</td>
</tr>
<tr>
<td>(514)</td>
<td>(210)</td>
<td>(480)</td>
</tr>
</tbody>
</table>

Real numbers of sherds are in parentheses.
The inappropriateness of this arrangement is obvious. It treats elements occurring in two, three, four, or five units as "common" to all and consequently gives a most unreal picture of unit inter-connections.

When sherds per element were tabulated and turned into percentages, the results of this second method were those in Table 12. Neither of these methods shows any flagrant contradiction. But the second procedure does not change the method used in Table 11.

The ratio of sharing suggested by Plog corrected for the shortcomings of the two previous systems. It compared every one of the five subunits against every other, thereby producing a series of validly comparable ratios of elements shared for the whole of Broken K. The schedule of ratios by unit follows. The Roman numerals refer to the two major groupings in Broken K that Hill found using design element similarity. A, B, or C following these numerals refers to and identifies the smaller residence units initially discovered at Broken K and which are believed to be comparable with the two units at Carter Ranch (Table 13).

These require discussion and clarification. But it is essential to present one further set of figures first. The two larger groupings at Broken K were compared by the ratio of degree of sharing technique and the results appear in Table 14.

**Changing Patterns of Integration**

In Broken K the five residence units seem generally to be sharing less than is the case with the two similar units at Carter
Table 13. Ratio of elements shared between the residence units in Broken K.

<table>
<thead>
<tr>
<th>Unit</th>
<th>Smaller Residence Units</th>
<th>Elements occurring ten times or more</th>
<th>Elements occurring six times or more</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Common</td>
<td>Distinct</td>
</tr>
<tr>
<td>Unit I</td>
<td>IA : IB ::</td>
<td>36</td>
<td>64</td>
</tr>
<tr>
<td></td>
<td>IB : IC ::</td>
<td>21</td>
<td>79</td>
</tr>
<tr>
<td></td>
<td>IC : IA ::</td>
<td>29</td>
<td>71</td>
</tr>
<tr>
<td></td>
<td><strong>Average</strong></td>
<td>28</td>
<td>71</td>
</tr>
<tr>
<td>Unit II</td>
<td>IIA : IIB ::</td>
<td>25</td>
<td>75</td>
</tr>
<tr>
<td>Units I</td>
<td>IA : IIA ::</td>
<td>31</td>
<td>69</td>
</tr>
<tr>
<td>and II</td>
<td>IA : IIB ::</td>
<td>30</td>
<td>70</td>
</tr>
<tr>
<td></td>
<td>IB : IIA ::</td>
<td>22</td>
<td>78</td>
</tr>
<tr>
<td></td>
<td>IB : IIB ::</td>
<td>22</td>
<td>78</td>
</tr>
<tr>
<td></td>
<td>IC : IIA ::</td>
<td>29</td>
<td>71</td>
</tr>
<tr>
<td></td>
<td>IC : IIB ::</td>
<td>24</td>
<td>76</td>
</tr>
<tr>
<td></td>
<td><strong>Average</strong></td>
<td>26</td>
<td>74</td>
</tr>
</tbody>
</table>
Table 11. Ratio of elements shared between larger units in Broken K.

<table>
<thead>
<tr>
<th>Elements occurring ten times or more</th>
<th>Lesser</th>
<th>Greater</th>
</tr>
</thead>
<tbody>
<tr>
<td>38%</td>
<td>62%</td>
<td></td>
</tr>
<tr>
<td>(219)</td>
<td>(359)</td>
<td></td>
</tr>
</tbody>
</table>
Ranch. If anything can be concluded, it is that the five units at Broken K are sharing less and are more isolated than the two socio-logically comparable units at the earlier Carter Ranch. Before this can be held as conclusive of any social trend, it should be noted that Hill, the anthropologist responsible for the analysis of Broken K, considers that in its later stages the village was composed of groups immigrating in from surrounding territory in the process of amalgamation peculiar to this era (Hill 1965: 240). The regional style diversity which has already been demonstrated to exist in the area as a whole is then transported to fewer locations, like Broken K. This consequently accounts for the increase in number of style peculiarities at the level of the matrilocal residence unit. Each residence unit has actually become more idiosyncratic than was the case at Carter Ranch. The suggested explanation for this is the recent agglomeration at Broken K of groups, hitherto scattered and independent. These were brought together under debilitating environmental conditions.

At Broken K the contrast of these five smaller groups with the larger two into which the five can be arranged is suggestive. The ratio of sharing between the larger units is 38:62. This is quite different from both the ratio of sharing of the smaller units that make them up and from the 29:71 ratio of sharing produced between the two units at Carter Ranch. It shows, in fact, a move to greater internal sharing which, if you will, reflects less social isolation between the units of the pueblo.
Hill has suggested that the two larger groups may be clans (1965: 153), or at least serve similar functions in drawing lines of marriage, descent, residence, and ritual participation. Membership in these larger residence unit groupings has been suggested by Hill and the breakdown of the five units has been described. The effective welding together of these units by the overarching "clans" is adequately shown in the figures presented by the ratio of sharing between these larger units: 38:62. This ratio shows a large amount of style elements held in common. Perfect sharing is represented by the ratio 50:50. The change over the Carter Ranch ratio, 29:71 should be noted.

Should it have been possible to show a developed increase in farming between Carter Ranch and Broken K, we would certainly have expected a continuation of the trend toward social isolation and disparateness. Such a trend has been demonstrated to be the case in the period between A.D. 400 to 800. But social isolation between A.D. 1000 and 1250 is not the case at all. The very reverse has occurred, and it has occurred within village walls as well as between villages. The economic conditions for the era have already been outlined. They have demanded more cooperation to insure survival using either farming or hunting-collecting. Economic isolation was directly contradicted as a trend and as a viable survival adaptation. Social isolation declined visibly as has been shown in this and the preceding chapter.

There is some evidence for changing patterns of ritual integration. At Carter Ranch there are two kiva; one is a great kiva outside the pueblo. The function of great kivas in the Southwest has
been unsolved for years. Since their advent coincides with the environmental change beginning around A.D. 1000, people have naturally assumed there was a relation. The great kiva was in its way a social device for maintaining cohesion between groups who never before had to cooperate economically and who consequently needed reinforcement in the social and ritual regions of organization. Great kivas appear both within villages and as lone kivas with no villages directly attached. The former type is later and is connected with the very large post-Mogollon pueblos of the thirteenth, fourteenth, and fifteenth centuries. The lone kivas occur earlier in the Mogollon region and in the Anasazi area to the north.

It is reasoned that the great kiva at Carter Ranch is one of the social and ritual foci of activity between that pueblo and the others it was cooperating with (Martin, Rinaldo, Longacre and others 1964: 209). Neither the level nor type of cooperation is safely inferred, but it may be suggested that if there were economic cooperation, it was important enough to be backed by the obvious amounts of energy expanded on a large ritual chamber.

Broken K does not have a great kiva. It has several small ones, each evidently belonging to one of the smaller residence units (Hill 1965: 190-3). These are all small and incapable of holding a group of any size. The village does have a plaza, however, and there is archaeological evidence that it was the center for social and ritual activities of a communal nature (Hill 1965: 146). The pueblo
is so arranged that the central plaza is enclosed on four sides by the
dwelling units. There are two connecting alleys with the outside.
Altogether the arrangement of space for maximum enclosure and conse­quent ritual intensification seems an effective means for welding
people into a cooperating social unit.

The original problem addressed was fission: fission between
social units whether they were villages, or groups within a village.
For this chapter, I was interested in the problem of what becomes of
units, presumably extended families, when they become increasingly
independent of one another economically. If the economic unit capable
of maintaining itself with greatest facility is the extended family,
as is the case among some agriculturists, then why do villages with
more than one family exist? They clearly do.

As dependence on agriculture increases, matrilocality becomes
intensified. Endogamy also is increased. This was the burden of
Chapter 3. Endogamy compels a community to marry within itself.
Wives must come, or husbands in alternative cases, from other families
who are considered to be within the social limits of the group. En­
dogamy carries within it the automatic necessity for plural groups
within the social confines. One cannot marry with one's own close kin
in any society. "Close" is a relative word, of course, but it always
exists and no matter how it is defined from society to society it de­
mands existence of a group who are either non-kin, or more likely
among primitives, more distant kin. Among these one marries, or bet­
ter, extracts a mate. Ultimately this is the reason that minimal
economic units, if they exist and if they really are individual ex-
tended families, are usually not found alone. This, too, is the
answer to the question, why the village in the first place? The ex-
tended family may be the minimal economic unit, but the multi-family
village is the minimal social unit in societies with localized en-
dogamous units.

Fission, then, rarely proceeds below the level of the two-
family village in the society and area these chapters are about up to
A.D. 1000. I have used increasing stylistic differentiation among
villages to infer increasing village endogamy. Increasing differen-
tiation of localized groups reflects the effects of endogamy. As en-
dogamy increases so does the need for more precise definition of whom
one may and may not marry. As these lines are more finely drawn and
observed, the families occupying the residence units also will become
more and more differentiated. Simultaneously there are two tendencies
working to increase the separation between the matrilocal units. The
most obvious is economic independence. To what degree this operates
is not perfectly obvious, but it is not really possible to deny that
it is an increasing tendency. The second is that as villages become
more independent from each other, as they do, they are thrown back on
themselves for mates and so must define the lines across which they
may and may not marry. These two processes, especially the latter,
narrow the range of women and traditions women are enculturated in
and are permitted to use when making craft items. The degree to which
any of this stylistic differentiation is conscious or unconscious is
up for argument. It cannot be settled here, nor is it necessary to
do so. A caveat should be issued too, on endogamy, since male mo-
bility is poorly controlled when the inference is made.

In the era used earlier in this chapter, A.D. 1000 to 1250,
styles at villages become increasingly heterogeneous. This means that
villages become more similar to each other during this era in this
place. This is demonstrated implicitly by measuring the increasing
degree of variability at any one site and inferring from it regional
movement of traditions. One infers that greater intercommunication
and hence sharing of style comes with mobility. This may or may not
be a fallacy. Agglomeration had indeed occurred, styles had become
more heterogeneous, but there is no evidence for matrilocal groups
dissolving. If Broken K demonstrated anything it was that the matri-
local residence units were every bit as distinct from each other as
was the case at Carter Ranch. Family units have not broken down, a
very interesting fact in itself, and a comment on both the degree of
environmental disturbance and resultant adjustment. It is also a
comment on the commitment of these pueblo building peoples to farming
as the principal fraction of the economic base.

The advent of the overarching clan or clan-like organization
as a device to tie these newly agglomerated family units together did
facilitate a heightened degree of communication. This is seen in the
fact that the five residence units at Broken K can be joined into two
on the basis of style similarity, and that the ratio of sharing be-
tween these is heightened over any pair from which they are drawn and
over a similar ratio from Carter Ranch.
I have been interested in measuring the covariation of two variables, dependence on agriculture and endogamy. I have assumed that if they do covary, the former moves first and is causal. By experiment I have shown that they covary positively. In a village this means that social units become increasingly well defined as agriculture predominates. It also means that as the circumstances under which farming is practiced become more difficult, endogamy will lessen as inter-village cooperation is increased. This does not necessarily cause the blurring of family lines, however; it may just as easily lead to extending the fictive definitions by which families are united or separated. I am not assuming that the family in any form is a naturally defined social unit because of a biological base. I am, however, suggesting that its limits can be extended by consanguineal, affinal, or fictive ties. Fictive ties can take ritual form, or descent from the same mythical ancestor, "blood" brotherhood, or even feudal vassalage. Clans are a usual device among preliterate groups to accomplish this.

This extension is the point of clanship. It broadens the marriage base, as certainly would be necessary at a village like Broken K. Broken K is probably just as endogamous as Carter Ranch, maybe even more so. However, in one sense it is far more exogamous. It is not more socially isolated, but less. This may be the case insofar as the people with whom the residents of Broken K had to marry to complement economic cooperation came from a region larger than previous economic and social demands warranted. This wider region
was brought within the village walls and hence presented as a single community. Exogamy became endogamy when agglomeration took place and villages grew fewer in number and larger in size. As long as endogamy at the village level is required, family lines will not become obscured; they may even become extended. Compromised economic autonomy created increased social cooperation. This took the form of a regional centripetal movement making the range of potential mates wider while maintaining village endogamy. Previous caveats about the nature of the demonstration of endogamy should be recalled at this point.
CONCLUSIONS

Caveats

To this point there have been few qualifications put on the objectivity or verifiability of what has been presented. And yet it is necessary to ask to what degree the data provide a positive, or even firm test for the hypothesis. To this end caveats must be attached to the arguments of relevance for the data measuring dependence on agriculture, the data for measuring female mobility, the logic in place of data for measuring male mobility, and even the statistical device that was used to do the measuring. The strengths and weaknesses of all these matters contribute to the final assessment of the validity of the test. At any point in the testing process the observer may want to stop and reserve judgment because the degree of rigor he demands cannot be found. Observers usually vary in where they draw the line of credibility, and I will not presume to draw that line for them.

I would like to make several areas of varying trustworthiness clear, however. The worthiness of the hypothesis as a hypothesis and as a valid object of investigation can be attacked. This is an issue of relevance. That it matters not one whit in the modern world whether or not we know about the validity of the hypothesis may disturb some people. That the hypothesis may be an already acknowledged
fact may bother others. To answer the last objection first, I do not think it was a fact before I began this study, but it was certainly recognized as a probability by Childe decades ago. I further acknowledge that even this study does not raise the hypothesis to the status of a fact. And to answer the more difficult first objection, I can say that if the hypothesis could be put to no use on any but dead data, this study would indeed have been stupid. It is not that extinct phenomena are irrelevant; it is that living phenomena are so much less so. However, a demonstration of the pertinence of all of this to anything alive will have to wait.

Three classes of data were used to measure degree of dependence on agriculture. Declining variability in types of manos, scrapers, and projective points is reasoned to indicate a more specialized economic adjustment. This specialization is agriculture. But declining plurality of types of manos may not result from a declining scope of wild seeds used. Agriculture may require a finer degree of processing maize into its various utilizable forms. And scrapers may not be scrapers, but some class of tool in which decreasing variation means successful and efficient adaptation to some natural food source. And so on, until reasonable doubts are raised or quieted about the measure of agricultural reliance. The fact that the data do not come from the same sites or even the same immediate area as those measuring social distance is also a just cause for worry. It is a cause that I cannot calm, except by saying that the environmental shift that affected the Hay Hollow Valley affected the
whole area (Martin, Rinaldo, Longacre and others 1962: 191-4), an area which is culturally and environmentally very homogeneous (Kirchhoff 1954: 533).

My efforts at measuring female mobility have been thoroughly described. So have the difficulties in method. The experiment with plain pottery leaves me unsatisfied, but for reasons I cannot be wholly sure of. The number of categories measuring variation in color, and hence a kind of style variation, is very few. This makes a difference in the coefficient produced when the same formula is used to measure both this earlier variation and that coming from later sites with 70 and more categories. The difference that variation in number of categories makes is not controlled.

I, of course, rely heavily on arguments developed by others that permit social inferences to be drawn from quantified style variability. Any weaknesses in those arguments are present in my own.

The biggest single source of discontent comes from not having data to control male mobility. A research design for alleviating this source of embarrassment is in Appendix 1. Logic has been substituted for facts in this matter until I can obtain concrete data. The logic has flaws, both in terms of cross-cultural facts and systematic completeness in the presentation of supportive arguments. In cross-cultural terms, my argument stumbles when faced with the case of some groups of Thai (A. Thomas Kirsch, personal communication). Some groups of Thai farming villages, having no apparent economic ties with their neighbors or with any of the exterior world, are nevertheless
not endogamous. Husbands are brought in from a wide region, the reason for this lying in ritual and ideological requirements. More facts would have to be controlled for this to be a genuinely upsetting case, but it serves to highlight the difficulty of relying on cross-cultural logic, especially without an elaborate survey.

The problem of logically supporting arguments is that they can never deal adequately with all exceptions. But the chief problem is being divorced from data. The conclusions one comes to using logic are in reality hypotheses, not facts. They await empirical referents. The world of what ought to be, logically arrived at, is not always the world that is. A hypothesis tested with logic is a hypothesis tested with a different degree of reliability.

The problem of statistics is a variable one. I didn't invent the formula for measuring validity, and do not fully understand its shortcomings. That is a fault, but archaeologists who know more statistics than I do suggest that it will serve my purposes, though not perfectly. Binford, Whallon, and Plog have all worked on it, and a certain amount of faith will have to be placed in their judgment. Also, tests for statistical significance have not been used where the results seemed sufficiently conclusive.

There are other problem areas, although all of lesser concern; those just recited serve to warn the gullible reader that the bigger the step the scientist wants to take, the less reliable his results inevitably are. So, when glancing at the graph describing the inter-relationship between lines labeled "dependence on agriculture" and
"social distance" all the above caveats should be held in mind. Otherwise decisions about what they mean may be less than secure.

Extensions of the Hypothesis: The Southwest -
the Limitations of Natural Abundance

For a tested hypothesis to prove its worth, it is required to order data from a greater area than it was tested in. In other words, it must be fertile not only in provoking other hypotheses, but it must bring order to analogous but as yet unexamined data. To this end I would like to review briefly some other relevant data from the American Southwest, from the pre-Columbian Maya, and from Mesopotamia. I offer the following statements as hypotheses. They are not tested. But testable situations are outlined and results suggested.

One of the ancient problems of Southwestern archaeology is why the area never went beyond the proto-urban complexes we see in the Hohokam and Anasazi and later Mogollon areas. If the area could reach the threshold of urbanism, why couldn't it cross it? Why isn't the Southwest a little Mesoamerica? The answer does not lay in a single cause. But I would like to suggest that part of the explanation lays in the nature of the agricultural adaptation in the Southwest. I suggest that at no period in the entire history of the aboriginal Southwest was there either a natural or cultural reason for cooperation between those economic units which were the minimum necessary to make agriculture work. In the Southwest the evidence seems to indicate that this economic unit is the independent farming village for almost the entire prehistoric period. I cannot extend
this statement with any precision among prehistoric cultures beyond the Mogollon. And even here it can be done only within the horizons detailed. But I would hypothesize that the condition existed among the classic Anasazi, and with modification, among the Hohokam.

Agriculture provided a subsistence pattern which produced self-sufficient units over a tremendous area. These had no economic and probably no social reasons for interaction. This is the archaeological situation, and as a hypothesis I suggest it to have been the ethnohistoric and ethnographic situation as well among the Puebloan peoples (Beals and Siegel 1966: 50). The single ethnohistoric example of pan-village unification for a common goal was the Pueblo revolt of 1680. During a twelve-year period beginning in 1680 and ending with the Spanish reconquest, the level of intervillage cooperation passed everything that preceded and succeeded it. Even today the All Pueblo Council has members, the independent villages, which are autonomous (Spicer 1962: 180).

Agricultural cooperation between the modern villages has been slim, or nonexistent among Pueblos. However, social and ritual ties between the villages are stronger. Both inter-village marriage and ritual cooperation exist (Dozier 1961: 166-172). How old these are and under what circumstances they developed are questions of history. The answers to them might not contradict the hypothesis that the circumstances of economic independence ought to be paralleled by the circumstances of social independence. On the surface, however, the facts tend to. The modern Pueblo situation neither supports nor
sufficiently detracts from the hypothesis to be of huge value as research stands. What may seem like economic independence at the village level on one hand, and social links between villages on the other may be a function of growing economic reliance on non-agricultural pursuits. By extending the economic base beyond any given pueblo, the ground may have been set for wider social interaction between the pueblos.

One may point to ritual links and cooperation between villages (Beals and Siegel 1966: 50). But ritual falls as a category within ideology and my hypothesis does not deal with any kind of ideological phenomena. It does not do so purposefully because, while economics and sociology may have concentric dimensions, I have not suggested that the same is true for ideology.

Economically autonomous villages that are exogamous indicate that either the hypothesis is wrong or the facts are incomplete. But it is not incorrect to stress both the potential self-sufficiency of agriculture and the homogeneous nature of the environment over this large region. Agricultural self-sufficiency and lack of environmental differentiation combined to eliminate any major trend to counterbalance the fission produced by ecological adjustment. No primary economic resources had to be brought into the region, and those that were needed seem to have been evenly enough distributed within the area so that commerce in essentials was slight at best (Bluhm 1957: 175). This assessment involves those items essential for economic success at agriculture. It therefore does not consider exchange in
shell, turquoise, pottery (although we do not know what was in the pots), and stone artifacts not directly related to accomplishing economic tasks. Salt expeditions may have helped contradict regional fission, but hardly effectively (Simmons 1942: 232-246; 252-255).

I would hypothesize that trade in items not directly related to economic success is accomplished by a mechanism closer to itinerant traders than to exchange centers in the prehistoric Southwest. I would like to establish a dichotomy between exchange centers and mobile trade. In the latter model the suppliers do the redistributing. There are no middle men, no exchange centers, simply itinerants traveling circuits. I suggest that in ecologically homogeneous regions having no need to import primary economic goods, trade will be mobile as in the latter model; exchange centers will not exist. In ecologically diverse regions, exchange centers will exist regardless of whether the diversity occurs naturally or by keying in to external areas by trade. In the latter case trade actually creates the ecological diversity and that diversity is parcelled out by an exchange center.

The model regarding mobile trade, if demonstrated, would clarify the case of a trade network not counterbalancing regional fission. The need for cooperative contact would not have been raised as long as the exchange mechanism was mobile, rather than settled at a given spot as was the case in Mesopotamia and Mesoamerica. Here, too, is a clue to the absence of patronizing state religions in the Southwest, and to the absence of any central authority built on wealth
or power accumulation. Mobile exchange both permitted fission and prevented the growth of those institutions fostered by the existence of exchange centers.

One key to the existence of fission is intervillage warfare and witchcraft. Both set up an opposition between villages, or whatever social elements are involved. Warfare, of course, serves for aggrandizement, but also as a means for social intensification. Economic competition may produce friction as among the Dugum Dani, as described in the ethnographic film Dead Birds. These groups are economically autonomous and have no social bonds aligning them. They war with each other on alternate weeks. Since the area occupied is maximally exploited, it is important that territorial lines be drawn and maintained against poaching. But it is equally important that intra-group morale be maintained. Warfare serves to do this by providing a device focusing attention in such a way that group identity is intensified while vital territory is protected.

Witchcraft directed at outsiders may serve the same function of internal intensification. Inter-village acrimony may develop between groups who are spatially close but who are bound neither economically nor socially. This obviously is more likely to occur between groups competing with each other over some segment of the environment. Warfare and its less overt form, witchcraft, is not highly developed among the Pueblos. But histories of warfare do occur (Parsons 1939: 905-6). This is taken as an indication of lack of inter-village cooperation which may stem directly from the self-sufficient nature of the village economies.
The economic autonomy of the village was never effectively contradicted by any mechanism enforcing or necessitating cooperation in the Anasazi, Mogollon, or Puebloan Southwest. The factors explaining the absence of counterbalancing mechanisms are many, and added together they are the explanation for the continual fission aboriginal history is witness to.

If fission is the inevitable result of depending on agriculture, then how does urbanism develop? How, in fact, does anything beyond a big village come into existence? Can it be hypothesized that some agricultural areas never experience fission at the village level because some natural fusing agency exists militating cooperation? Or is it more fruitful to suspect a basic fissive tendency in all neolithic cultures at the village level which is only contradicted by certain economic adjustments? Probably both hypotheses could be tested positively. I suspect that a fissive tendency may never have existed in Mesopotamia until proto-literate and proto-urban redistribution centers became self-sufficient. But in any case, the two questions are not antithetical, but rather the first is a specific version of the second.

The Maya: Civilization on Borrowed Resources

Before I raise some hypotheses about Mesopotamia I would like to discuss an aspect of the Maya briefly. American archaeology's favorite lost civilization is in danger of being permanently misunderstood by a class of investigators who think cultural problems are solved by invoking either historical or ecological explanations.
Whether invasions or environmental change upset the Classic Maya is not really at issue. This is not to say that such information is not useful, but an anthropological explanation for collapse lies in an explanation of the nature of a type of economic system and the social order built upon it.

To put my general hypothesis in Maya specifics, I would suggest that an initial occupation of the lowlands around 1000 B.C. was dependent on agriculture. Probably even at this era some need existed to关键 into the resources of other areas outside the lowlands. The scarcity of obsidian for making blades and all volcanics for grinding in the lowlands is well known (Thompson 1966: 25, 213). If there ever were a trend to village isolation, this was contradicted from earliest preclassic times by the necessity to cooperate both with immediate neighbors and external sources to insure the raw materials essential for economic success at agriculture. In other words, ecological dependence on external areas was a prime factor behind the destruction of any potential fission between farming units.

However, by extending the environment and capitalizing on external ecological diversity, a society is removed only to that level of economic and social organization necessary for self-sufficiency in its region. Once the self-sufficient unit is attained, the normal tendency to fission can be expected to recur until something interferes and creates a different size unit as the most efficient economic base. The redistributive center, the ceremonial center, of which there are hundreds if not thousands in the lowlands, is the
natural candidate for the economically autonomous unit and consequently the socially isolated. Were they? We do not know.

Ecological interdependence counterbalanced any movement to isolation for lowland maize farmers. Trade as an extension of the lowland environment required an elaborate system of redistribution. This system organized both whatever material goods were shipped out of the lowlands and the large ranges of resources brought in. Ecological niches like the Guatemalan Highlands, the Atlantic and Pacific seashores, the Tabasco-Vera Cruz area, British Honduras and indirectly the Mexican Highlands all were milked for resources needed to implement a maize economy and the convoluted social network built upon it and making it go.

The balance between external ecological niches and the Maya heartland is a useful hypothesis. But one obvious corollary to this idea is the result of upsetting the balance. Should it have become difficult to key into external niches for economic necessities, then an entire structure built on environmental extension would begin to collapse. Collapse it did, and the population of the lowlands was reduced to pre-ceremonial center levels, levels which, it might be suggested, probably resulted from minimal exchange with the outside. When the centers collapsed, the farmers who relied on them for essentials dispersed. The Guatemalan department of El Peten, which is most of the lowland area, can sustain very few farming units by itself. In short, the autonomous farming unit in the Peten can scarcely exist; the real unit was the exchange center with its connected maize
agriculturalists. This was determined by a combination of economics and ecology. With the centers' fall, there necessarily came decreasing reliance on agriculture which meant, not a decrease in social distance in the usual sense the hypothesis would predict, but population migration and abandonment.

Noteworthy here, too, is the present population level of the Department of the Peten, 20,000 (Encyclopedia Britannica 1967:8: 302). The Tikal area alone is conservatively estimated to have had a resident population of 12,000 at its height (Willey and Bullard 1965: 373-4). Infferred safely, then, is a pre-Columbian Maya population in the Peten vastly larger than today's. The present Peten population is isolated from the outside except via expensive air transport. It is interesting that the most effective means of breaking down this isolation is electronic communications systems. Radios America, Havana, and Moscow are more familiar to the Petenero than is any other means of communication. The native of the Peten knows more about what is news in Southeast Asia than he does about news in the Guatemalan Highlands. The present population is as divorced from ecological niches outside the Maya area as were the pre- and post-Classic Maya. The modern Guatemalan government evidently either has not learned or does not care to learn the lesson of the Classic Maya's economic success. This success was dependent on extending the environment, an environment which is far narrower in the Peten today than it was for the Classic Maya.
The Maya are not only a potentially positive test for the hypothesis, but their situation promises to be far clearer in its light. The Maya were always dependent on agriculture. They were also always dependent on non-agricultural and extra-village sources to make agriculture work. Hence the redistribution center, not the farming village, was the minimal economic unit for the Maya. Village fission either never existed or was contradicted early by economic necessity. This forced links with other villages and the outside generally. Also contradicting fission was ecological redundancy. Unlike the ecological homogeneity of the Southwest, however, the lowlands did not provide equal amounts of natural resources to all; they provided equal need to capitalize on the outside for all. The minimum unit capable of realizing this was the economic and ceremonial center. The economically autonomous unit, then, is this center, and I would hypothesize that these are the socially autonomous units as well.

Increasing dependence on agriculture leads to increasing social distance, or fission, between the units needed to make agriculture successful. This is the hypothesis. Minimal economic units in the Maya lowlands were two. Initially there may have been a light population of farmers, as along the Usumacinta drainage. These may or may not have existed and may or may not have been independent units. The second and concrete example of a minimal economic unit in the lowlands is the ceremonial center. In order for the latter to come into existence, trade had to extend the environment to create
the ecological variation not existing locally. Agricultural dependence was thereby made successful, and I would suggest that as the system became more efficient the social distance between the efficient units grew apace. Hence, increasing dependence on agriculture may have led to increasing social distance between those minimal units needed to make agriculture work.

The minimal economic unit was dependent on ecological diversity imported from the outside. When the sources of diversity were cut, the economic unit ceased to function. I would reduce the rise and fall of Maya civilization to this hypothesis.

Mesopotamia: Ever-Declining Autonomy

Mesopotamia has the potential for being one of the most thorough tests for the hypothesis. But that potential cannot be exploited here. I would, however, like to use what we know of the economic base of early settled Mesopotamia to illustrate the nature of the hypothesis that increasing economic diversity is accompanied by increasing social diversity.

The earliest economic base in Mesopotamia is described by Robert M. Adams (1966: 48) as being fourfold: The fishes and birds of estuaries, the dates harvestable from palm groves, domesticated grains, and domesticated pastoral herds. These closely juxtaposed natural and domesticated food resources were the natural circumstance for economic exchange. Exchange occurred between the families or villages depending either equally on all, or primarily on one, but of necessity utilizing all of the available foods. The necessity to
exchange, plus the necessity to trade in the material for cutting implements, chiefly obsidian, combined to demand the very early appearance of exchange centers. These are the temple centers where, under the aegis of ritual specialists, exchange was organized. These centers were the foci for two of the basic economic mechanisms in any society, production, and distribution. Production may have been a secondary development after capital building had taken place in the ritual centers. The reasons for the growth of exchange centers in the guise of ritual centers is unclear. But regardless of their ideological or capital building aspects, the centers homogenized the economy, in fact, probably made it work.

There is some doubt about whether or not fission below the level of the exchanging groups ever existed in Southern Mesopotamia. I presume that economic interdependence in the area comes either with initial settlement in earliest neolithic times or shortly after these, when the limits of domestic production as well as the limits of natural resources would have taken effect. In either case, the minimally effective and efficient economic unit, I suggest, was the group of mutually interdependent producers relying on an exchange center. These producers included not just villagers growing domesticates, but marsh dwellers, and herders, and whatever scion filtered in obsidian and other foreign essentials. This naturally cooperating unit, the economic perimeters of which would naturally tend to expand as population grew and wealth increased, was the permanent mechanism for contradicting any trend to village isolation. The establishment of
this naturally cooperating unit did, however, create an analogous situation. By about 4000 B.C. Southern Mesopotamia was populated by dozens of independent, growing, and economically successful centers. What appeared in the American Southwest as autonomous villages, in Southern Mesopotamia took the form of independent proto-urban centers each with attached and mutually reciprocating production bases. These economically autonomous units were socially autonomous, also, I would suggest.

The rise of Kish and then of Agade signal the rise of the pan exchange-center state. The growth of kingdoms from fiefs and the rise of empires from kingdoms culminated in the emergence of the first true national imperial state, the Persian Empire. One after another, through a period of almost four millenia, the range of proliferating boundaries connecting villages, provinces, and states grew. I cannot construct the ancient history of Mesopotamia to show that continually decreasing economic autonomy on the part of all economic units, villages, towns, and regions, led inevitably to an extending of social ties between cooperating units. This is probably the case, but would take years of research to demonstrate. The earliest period of this ancient history does, however, seem most provocative as a test for that hypothesis.

Competition between the autonomous centers in Southern Mesopotamia can be postulated to have begun when arable land began to get scarce. This was due to population growth and salinization of land. Competition between these units naturally reinforced internal bonds
and served to emphasize any center's collective identity. Later, when Mesopotamia became a center of urban civilized society with all of those items and virtues that are enticing to neighboring have-nots, the competition and conflict between these two different economies resulted in further reinforcing internal bonds. Ties between centers would have had useful economic, political, and military consequences. I would suggest that one of the factors acting in the rise of pan-city political units was the external pressure forcing larger cooperating units for protection within Mesopotamia. External pressure does not refer solely to Elamites or Assyrians, but includes nomadic groups inhabiting the non-arable areas between the sown. These natural predators on settled agriculturalists became a potential threat as soon as balanced exchange between them and the settled proto-urbanites became unbalanced. This must have happened very early, and was exaggerated as soon as the sedentary groups actually began to accumulate an economic surplus which was inevitably displayed ostentatiously on temples and deities and ritual functionaries.

The economic factors giving rise to states and empires were based in part on the need to preserve stable trade networks that guaranteed raw materials, markets, and royal and ritual revenues. The extended economic base evolved a concentric political system to protect and augment it. It is entirely likely that as a region's economic autonomy declined, so did its social autonomy.

Neolithic fission at the village level may never have existed as a stage of development in Mesopotamia. Ecological diversity
creating differential abundance and scarcity of essential resources produced cooperating units centered on a redistribution center. The first minimal economic units in Mesopotamia were the proto-urban centers. I would expect the social links of the proto-urban centers not to extend beyond these centers except as economic exchange required it. This is the specific hypothesis and it is not to be regarded as tested.

The size of the minimal economic unit in Mesopotamia was always in a state of disequilibrium. The size of the unit was always dynamic and was continually extended. It was not extended due to increasing dependence on agriculture necessarily. Only the creation of the initial minimal economic unit is to be constructed as a product of agricultural dependence. All later extensions come as a result of internal and external pressures to extend an economic base containing more than agriculture. There is very little doubt, however, that Mesopotamia can provide a testing ground for both the specific and general hypotheses, and that among the alternatives testing could reveal would be the ever-declining scope of economic and social autonomy in Mesopotamia. The ever-increasing size of the minimal economic unit would be accompanied by an expansion of its social ties as well.

The burden of this chapter has been suggestive rather than affirmative. To that end I want to suggest that the two variables, dependence on agriculture and social distance, can be replaced with analogous variables and the resulting hypothesis will describe the interdependence between economic base and social organization in any
society. This is so because the agriculture-specific hypothesis is merely a version of the general hypothesis: increasing economic autonomy leads to increasing social autonomy. Another hypothesis which is a specific version of the general hypothesis is increasing industrialism leads to increasingly fine divisions of the social structure. This hypothesis does not seem either startling or new. Most social scientists really do think that the state of a society's economy is reflected in its social organization. To say, then, that the commonest economic base of the twentieth century is accompanied by fine vertical and horizontal social divisions is to play with the almost obvious.

Modern American Culture: The Decline of the Future?

However obvious a hypothesis may seem, it usually has its value demonstrated when tied to so many specific facts. The specific facts I find fascinating in this instance are from contemporary American culture. If the hypothesis about industrialism is correct, we ought to currently see in modern American culture thorough-going changes in our social structure to accompany those we know to be taking place in our economic base.

Automation has the effect of making any individual in an automated system more dependent on every other and on the system itself. This is opposed to the case when simple specialization and an assembly-line, interchangeable-parts technology defines industrialism. Division of labor in our society has gone beyond the point where every individual was dependent on every other member of the system because
one made the handles for the other's pots and pans, or mined the bauxite for the aluminum refineries which did thus-and-so for other industries in a continual chain. Automation has produced a much more finely intermeshed as well as finely specialized economic base. Systems engineering is a reflection both of the complex nature of any given industry and of the fantastic production interconnections all industries have with one another. By eliminating skill as a requisite for accomplishment and substituting controlled interchangeable parts to facilitate huge production, interdependence was fostered. When specialization in labor was further destroyed with automation, interdependence was even more fully emphasized.

With the end of the last century the systemic nature of the American industrial complex became evident with the rise of trusts, holding companies, and condominia. These were formed by capitalists who saw and wished to safeguard the dependent relations they had with one another. The increasingly large number of seemingly independent company names masked what was really an increasingly fine homogenization of the capitalizing stratum. At a different level the whole union phenomenon can be seen as a reflection of the need to systematize social contacts among workers. The number of organizations proliferating among both owning and working classes served to divide up both of these strata more finely. In the course of this, the strata were brought into higher relief and placed in clear opposition to one another. The resulting subdivision of the strata proliferated voluntary associations, kinship, and especially government activities as
integrating devices in the United States at the turn of the last cen-
tury. Unions crosscut labor, and trusts and kinship crosscut owner-
ship. The Federal Government with its new Department of Labor and new
anti-trust laws crosscut both strata vertically. As industrialism
grew, so did the social devices to handle it. And the effect most of
these had was to create more ties throughout the population.

The changing nature of our economy's industrial base can be
safely accepted. Consequently, a look at the state of the major tying
institutions in U.S. culture ought to reveal both glaring disharmonies
and potential alternatives to augment or replace older ones. The
state of bureaucracies and of voluntary associations, sodalities, etc.
is interesting in this regard. Modern industrial nation-states are
tied together internally by bureaucracies and voluntary associations
at the sociological level (Service 1962: 181). The health of these
major "integrating mechanisms" might serve as a clue to the state of
change in the economy.

The point of a governmental bureaucracy is to order and facil-
itate flow of information and opinion. A government bureaucracy fa-
cilitates communication between governed and governing. Not only are
the clamors against bureaucracy deafening today by vast groups of
people from states-rights parties to fighters for individual freedom,
but it is becoming obvious that the base of popular contact essential
to an effective bureaucracy is weakening. What is taking its place?
Two phenomena are suggested: opinion polls and the broadcast talk
show (James F. Downs, personal communication). How many government
decisions are made without reference to public opinion polls? These opinions are gathered with statistical accuracy, not by bureaucrats but by national poll takers. And consider how frequently the President is supposed to watch television.

The incredible rise of the opinion poll phenomena is an institutionalized way of circumventing the growing inefficacy of standard bureaucratic information handling. Poll takers have become a part of government and increasingly fewer governmental decisions are made without consulting them. Circumventing this gulf, too, Downs suggests, is the television and radio public conversation with any Everyman who wants to complain about any aspect of this ailing world that particularly bothers him. When this happens before several million people, coast to coast, information about some tacky problem is spread around fast. It is presumably spread to those who are sympathetic as well as to people who have power to do something about it. Communication is consequently enhanced. Television and radio have become one of the broadest tying devices in American culture as a result of spreading so much information to so many people.

Voluntary associations or sodalities had been homogenizers long before industrialism became an economic base. What is curious is their current status. Americans seem to be joining fewer now and enjoying that less (Hausknecht 1962: 111-125). We not only belong to fewer associations today, but we are members in any one for a shorter length of time now than has ever been the case. What is replacing the voluntary associations?
If major failings can be seen in the ability of bureaucracies and voluntary associations to fulfill their supposed functions, then can we assume that our society is progressively falling apart, or can we assume that some other tying device is beginning to be utilized?

The best and simultaneously most outrageous answer is provided by Marshall McLuhan in his book Understanding Media. A completely new way of thinking and of conceptualizing phenomena is being created among us by our own culture. This is neither mysterious nor mystical, but should be seen as the adaptive response to living with so systemized and interconnected an economy.

McLuhan at his least temperate suggests we are all becoming "retribalized." What he refers to can best be described as a harkening back to Redfield's "little community" where everyone did essentially think alike. We are being reintegrated by having our thought processes changed -- really those of our children, since most of us are too old to have our minds reshaped. Rather than the linear thought patterns familiar to us that come as a result of being enculturated via media that arrange messages linearly, the generation below us is being enculturated by television principally. This medium forces a kind of instantaneous, synthetic selection of information from the mass presented by television. This refers, says McLuhan, not to television content but to the way the tube works -- presenting millions of changing dot patterns from which the individual viewer must select a portion in order to understand anything at all. This selection and self-synthesis process forces the brain into certain new
patterns which might best be called cybernetic. Whether or not better thoughts will be produced awaits demonstration. But the real issue is not better minds, but minds that consistently react in a new way. A pattern of thought is being bred which depends on the single individual as its base. These thought processes coupled with certain uniform values and uniform information can serve as a basis for reintegration. McLuhan thinks the process involved is one of media shift. I would maintain that shift in message-bearing media is only one variable in a hypothetical process, the other variable of which is economic. Specifically, the economic variable is the transformation from a fragmented to an automated industrialism. As earlier, I would suggest that the hypothetical relationship is one of decreasing autonomy of economic units accompanied by a finer breaking down of social units. Both the economic and social systems are increasingly homogenized.

The reason for such shifts lays in economics. And if the current homogenization of the economic base will allow prediction of anything, it will dictate continued efforts at social homogenization like civil rights legislation and ecumenical Christianity. Trends like these smash economically antithetical practices by eliminating now useless social divisions. Ultimately, of course, the removal of such coarse divisions is leveled to the point where the unit of division is the individual. McLuhan suggests precisely this eventuality. "Electronic media are subtly and constantly altering our perceptual senses." The new vehicle is tribalizing and decentralizing. "Man
now lives in a global-sized village, and is returning to the values and perceptions of a preliterate culture" (McLuhan, Understanding Media 1966: i).

The reflection of economic shift on social organization is thorough-going. The method of inculcating values is affected. A switch in media changes the depth and thoroughness with which values are taught. Media do not necessarily change values themselves, although there is a high likelihood of value change being involved in media shift. The process then is fairly clear. But the future is not. The mechanism by which things will change is the process of media reformulating thinking processes at the level of the individual to match the finely homogenized economic base.

Social homogeneity is to be brought about by media, especially television, McLuhan suggests. Of the cultural devices available to affect social homogeneity, extensions of bureaucracy, voluntary associations, fictive kinship networks, and media, it is relatively easy to see, once pointed out, the efficacy of media as opposed to the others. Nothing else reaches so many people so consistently and constantly. Nothing else affects thought patterns as well as the content of them. And nothing else so well complements the cybernetic path the economy is taking.

Predicting the content of the media is quite another matter. It seems that by a fairly intimate knowledge of American culture, McLuhan has picked the most likely alternative to perform the reintegrative task. In order to control the use of the new medium to
affect public thought processes, one is reduced not to controlling content but to controlling presence and absence of the tube. At best whole ranges of subjects could be removed from presentation on television. Once it is clear that television presentation will affect public reaction to an issue or subject in a way some sponsor, public or private, does not want, the only alternative is not to change content but to eliminate form. This involves either not using television as a presenting medium, or drastically changing the image the subject takes on television.

A discussion of prediction and control inevitably raises the fact that no evolutionist can predict in the specific case. In fact the most successful sciences predict only probabilistically. The evolutionist, however, can predict generally, if he has his hand on a real process. He cannot, however, predict that in a particular case where a well understood process is occurring the future will have a given form. This is so because the alternatives among which change can occur are not usually known. And if they are, the particular culture's attitudes to them are not. Studies in specific evolution in anthropology are, therefore, ineligible for consideration as successful science. They do not and cannot predict.

However, given the United States today and the need for sociological readjustment, as well as the vehicles that can accomplish it, the prediction about the dominance of television as a homogenizing device seems at least permissible. Given the need to predict content in this medium, préciser specification is needed. On this point one
can say that American culture is and has always been characterized by a specific and finite number of "themes." These are best illustrated by studies done on American literature. These themes can be expected to permeate the content of television thus insuring stability in subject matter, values, and basic structural patterns of American thought. These are hierarchically arranged values and basic forms of reference which are used by Americans to order most phenomena and almost all the problems arising from that ordering. The themes will ultimately dictate all media content. They will always be present.

These patterns of categorization, or themes, take the form of concepts like the frontier, a fundamental American ambiguity to science, a use of reason in approaching most problems, a conceiving of America as "Promised Land," home of the "American Dream." American innocence, the decadence of Europe, and American disillusionment are major keys to American reaction to almost any issue today. The place of nature in our conception of reality has a peculiarly novel American development. Nature is both apart from and responsible for the world of reality. An early view of the probabilistic nature of the universe and consequently an innate suspiciousness of cause and effect relationships has characterized American culture for generations.

But even this enumeration would not give Hollywood and New York broadcasters much help in predicting what viewers will and will not watch. It would not, because it is not combined with other variables that undoubtedly govern viewing taste. But with these
patterns in mind and knowledge of a process like economic and social homogenization, one might expect to do a better job of prediction regarding both media shift and content of them.

The effects on our culture of a new vehicle for integration are probably endless. One can easily predict disharmonies, but controlling them is something else again. I want to suggest some problems that are obvious. Communication in our culture is becoming increasingly oral. That much seems obvious, but in academics the consequences of this fact are both curious and sometimes appalling.

Students and younger faculty today cannot spell. Almost across the board, poor spelling characterizes the whole generation of undergraduates, graduate students, and new professionals. This is merely a reflection of the fact that written communication outside of academia has become close to irrelevant. Certainly handwritten communication outside the educational network is irrelevant. Since academicians live in both worlds, and since the professional world, too, is increasingly unconcerned with the need for self-composed materials without their being edited first, spelling has decayed.

The ability to compose and be grammatical has suffered also. English orthography is naturally chaotic, but when ability to communicate in written form degenerates to that so frequently seen in social scientific journals, it becomes obvious that research results are being circulated some other way. IBM can advertise in Time (1967: 82-3) that "It is now vital that American businessmen forget how to write." It is inefficient compared to the other media available.
The same will soon be admitted in academics. Written communication will soon be irrelevant.

Stanford psychologist Nevitt Sanford predicts that before the end of the century, "the most prestigious colleges will forbid their professors to publish until they have been on the faculty five or ten years." He suggests that publication by television should be the only exception. A scholar "who has something important to say goes before cameras to say it in plain language to the general public." Time refers to this idea as "birth control for books" (1968: 71).

The pathetic part about the shift in use of media is evident in academics when people taught in one medium, the oral, are examined in another, the written. This switch will not create chaos so long as the student knows beforehand what he is going to have to write about. But surprise him with unfamiliar questions, and the result will be catastrophic. Those people most thoroughly at home in our culture's chief media will do poorly and those less familiar with communicating orally will do well. This will create a division, putting the brighter on the bottom of the performance scale and the less bright on the upper end. This is hardly the way to insure progress.

From the preceding discussion it is clear that our economic base has become more homogeneous. Our social organization has also become more finely intermeshed. Communications networks facilitated the former case and were the vehicle for the latter. All of this homogenization naturally eliminates variation as it fosters integration. Evolutionary potential does not reside here since adaptation
to change depends on the existence of variation. Is this a matter for real concern? Or can the worry be alleviated by citing or long-term national commitment to preserving as much variation as exists? There is undeniably a trend toward homogenization. Does it foretell the decline of the future?

This entire chapter should be regarded as containing hypotheses. It is suggestive, not affirmative. And it is incomplete. If any part of it can be constructed as of current interest, then archaeology is not the science of the past.
APPENDIX 1

RESEARCH DESIGN FOR MEASURING MALE MOBILITY

In order for a hypothesis about endogamy to be testable, it is necessary to measure both female and male mobility. Female mobility was dealt with in the preceding chapters. The appropriate ranges of facts have been presented and the inferences argued. But male mobility has been dealt with more casually. Logic and argument have stood in place of collected data. There is a certain inherent weakness in this, consequently this appendix is an effort to suggest those ranges of data useful for measuring what has been previously handled impressionistically.

The earlier argument on female mobility centered on style variability in female-produced craft goods at any site. As females became more localized through time, the style variation women produced at any one site decreased because the number of manufacturing traditions present at that site decreased. As women married within their natal villages more and more frequently, stylistic variation measured at any one village grew less and less. The outside simply ceased being represented.

I would like now to suggest the arguments of relevance for data that can measure the results of male mobility. What this means, in effect, is the examination of the idea that at no time in the
history of the aboriginal population did males marry out of their natal groups. Neither among hunter-gatherers nor among agriculturists were males siphoned off to or recruited from other groups on a consistent basis in the specific region being investigated.

In order to test this variable in the hypothesis, a measurable range of stylistic variation on male-produced craft goods could be examined. Then it would be predictable that if an unchanging pattern of male mobility were the case, through time, the coefficient measuring style variation would remain relatively constant. Or, if male mobility increased or decreased through time, the variability in style at any site would climb or go down accordingly. This rests on the former argument that variation in style is correlated with number of style traditions present. The number of these is, of course, a measure of the number of microtraditions brought to a single site, and consequently the mobility of males.

The crux is, of course, obtaining that class of male-produced artifacts that will measure style variation. Fortunately, much work has been done recently with variation in lithic artifacts. The general assumption behind this work is that chipped stone artifacts are assumed to be male made. And consequently style variation in them, if it can be isolated, would suit the present purpose of measuring male mobility.

Edwin N. Wilmsen in two different papers (1965, 1967) expanded and utilized a metrical technique for measuring two specific angles on chipped stone flakes. He demonstrated convincingly the existence of
variation in flaking pattern between a series of Paleo-Indian sites in North America. Wilmsen attributes the real variation in angle range to differing ecological adjustments from site to site across North America (1967). Therefore, variation is based in economics and technology. There is some suggestion, however, in his 1965 paper that Wilmsen attributes variation to what might be called cases of cultural drift in the Paleo-Indian tradition. This latter would involve variation coming as a result of isolation of units of the larger Paleo-Indian tradition. Differences in flaking pattern would represent stylistic variation in an essentially homogeneous culture rather than primary economic adjustment.

Having raised the possibility that variation in flaking angle can measure style variability, it is natural to suggest that this device be applied to a class of artifacts available for analysis in the Hay Hollow Valley. We have at least a 2000 year spectrum of sites which can be or have been dated. Lithic collections from these are available. For the experiment to work, a class of artifacts would have to be used that insured the elimination of flaking angle variation due to functional variation through time. Thinning flakes might serve this purpose. Also, flakes all utilized for a general function like cutting soft material or fleshing or skinning might be used. These latter categories which can be singled out with some assurance of prehistoric use could guarantee homogeneity in function across time.
One assumes, in the latter cases, that all groups across time would have had to perform tasks like fleshing or scraping regardless of the group's basic economic pattern. Tools whose basic functions were of this sort can be identified because we know that certain activities produce specific wear patterns on stone. At a more general level, tools used for pushing versus tools used for pulling can be readily identified. And if it can be assumed that certain processing techniques require specific flake characteristics in order to function, then some aspects of morphological variation can be held constant over time. This would allow the variation in measured angles to more fully represent style fluctuation.

Quite obviously some experimentation is necessary before an appropriate artifact class can be singled out for relevant analysis. And advice from lithic experts will be sought here. But given success at isolating a suitable artifact class, it would then be necessary to measure the angles defined by Wilmsen. A sample size of 200 or 300 flakes of the required type would be collected from the surface of several single component sites. The sites would be spaced temporally and would include already collected data from previously excavated sites in the Hay Hollow Valley. Among these are the County Road site (ca. 500 B.C.), the Hay Hollow site (ca. 100 B.C. to A.D. 100), and the Hatch site (ca. A.D. 750-800), and the Broken K site (ca. A.D. 1100-1250).

After the collections had been selected and relevant angles measured, means and standard deviations of individual site collections
would be taken. These then would be compared statistically using the Student's t-test. The results of this comparison would state with defined confidence limits whether or not any two collections were similar enough to be considered part of the same population. Similarity of samples to one another would mean very little variation in flaking traditions between these sites, and would tend, under normal circumstances, to back up the statement on the static position on male mobility through time in the Hay Hollow Valley.

Another, though perhaps more convoluted, way of measuring variation between site collections involves defining categories of angle variation for any collection. All the measurements for one angle's variation at one site can be divided into arbitrarily defined and equal categories. The categories, each of the same metric interval, would proliferate until all measured variation was included. The number of flakes falling into each category would then be totaled and the whole would be described by a single coefficient arrived at by using the formula in Appendix 2. The coefficients can then be compared and the mobility of the male population described accordingly.

The coefficients will vary between 0 and 1. Zero is complete variability, or equal representation for all categories, and one is all variability occurring in one category. If the coefficients rise to one, variability decreases and male mobility goes down. If the coefficients decline to zero, male mobility increases. If the coefficients do not change or show no marked direction, male mobility may be inferred to be relatively constant.
APPENDIX 2

STATISTICAL FORMULA USED IN THE ANALYSIS

The formula used to create the coefficients of variability in this study is:

\[ C = 1 - \frac{2(\Sigma c - P)}{P(n_o - n_t)^2} \]

- \( c \) = cumulative percentages of the attributes.
- \( P \) = total number of artifacts in a sample.
- \( n_t \) = number of nominal categories in the universe of attributes.
- \( n_o \) = number of nominal categories in a particular sample.

This formula is a version of one used by Robert Whallon (1966: 121). Whallon's formula was thought to have certain shortcomings which Binford (personal communication), Plog (personal communication), and Redman (Howe, Menkes, and Redman 1967: 24-29) attempted to remedy. The formula described above is the result they all largely agreed on. It has been used throughout this work.

The formula is a mathematical attempt to represent the area under a curve on a graph showing cumulative percentages. If all the variability in an artifact class at a site were reduced to percents and these were represented in cumulative fashion on a graph, a connected series of straight line segments would result. Since minimum
variability would be a nearly vertical line, and since maximum variability would be a diagonal, the area between the two is easily calculated. All curves in fact fall between these two lines, and it was determined that the measure of variability of any curve is the area between the empirical curve and the permanent diagonal representing maximum variability.

The formula for arriving at coefficients of variability is an effort to accurately measure the area between the diagonal on the graph and the curved line created by the accumulated percents for any class of variation for any site.

The use of 20 as a constant is to standardize the formula for use in comparing analogous classes of attributes from varying sites. Since number of attributes varies from site to site, and since only slope of the curve alone was to affect the coefficient, an arbitrary constant was chosen to avoid dependence on the fact that numbers of attributes do vary from site to site. Whallon originally chose 20 because it was intermediate between the number of attribute categories he used from site to site. Some sites needed more than 20 attribute categories to adequately account for the ranges of variation in them, some required less. Twenty was retained for use in my analysis because it seemed suited to represent an average middle ground between the "earlier" and "later" sites. It is doubtful that a different constant would markedly change the results of measuring variation, but that is not completely certain.
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