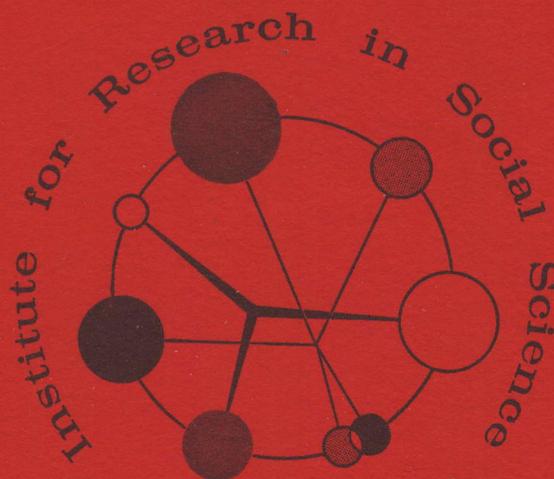


# A NATURAL ORDER OF CULTURAL ADOPTION AND LOSS IN TRINIDAD

F. T. CLOAK, JR.

Working Papers In Methodology  
No. 1



UNIVERSITY OF NORTH CAROLINA

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CHAPEL HILL

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BY

F. T. CLOAK, JR.

WORKING PAPERS IN METHODOLOGY

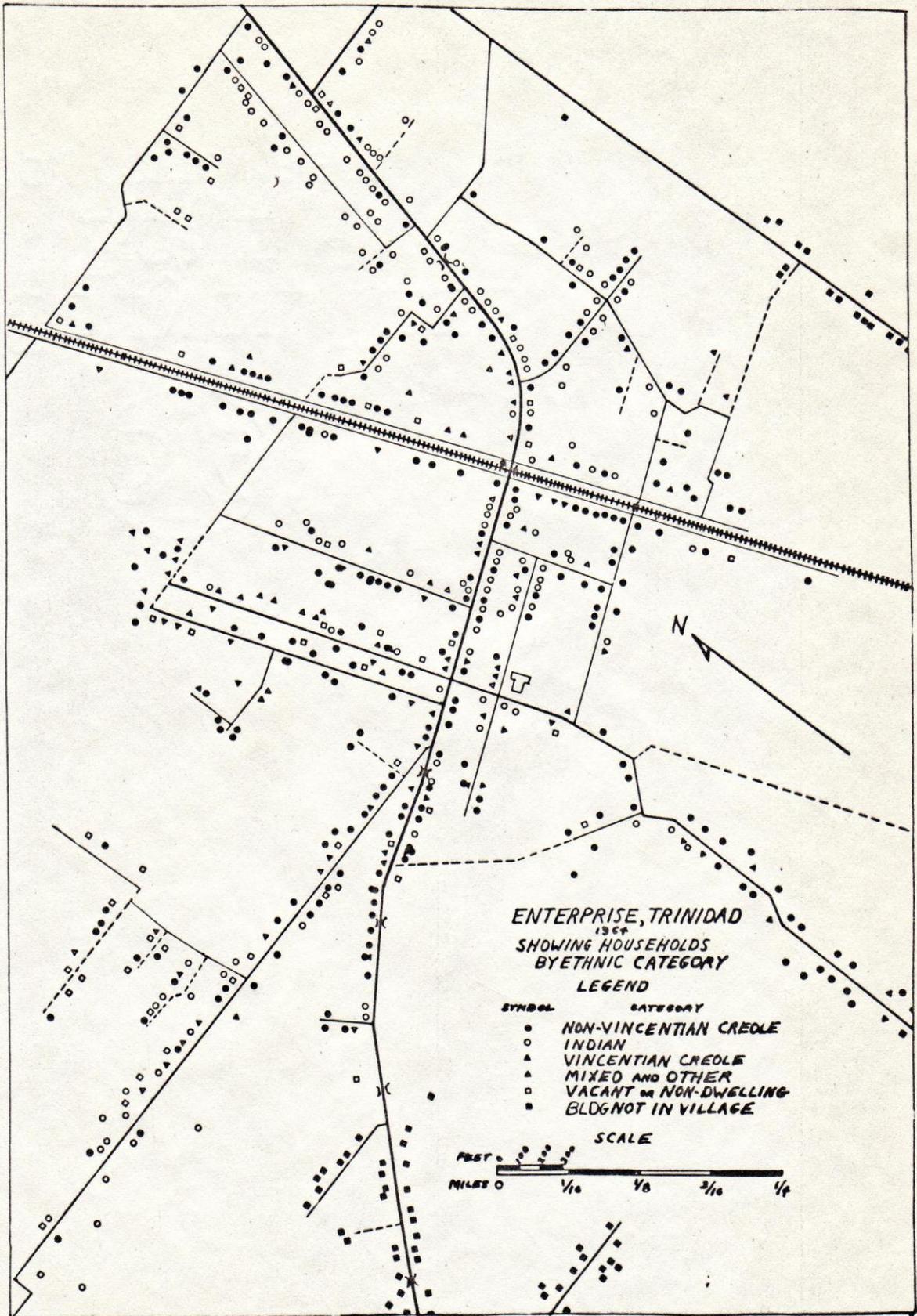
No. 1

INSTITUTE FOR RESEARCH IN SOCIAL SCIENCE

University of North Carolina

Chapel Hill

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1966



St Vincent Calypso King Competition (Coke-oh-co)

by Lord Blakie

Last Calypso Season, Lord Blakie went St. Vincent  
Last Calypso Season, Lord Blakie went St. Vincent  
So they had a competition  
To crown "King Calypsonian"  
When they crown the calypso king  
This is the song the fellow sing:

Chorus

Morning . . . cock ah crow, co-ke-oh-co, Morning .  
. . . cock ah crow, co-ke-oh-co,

Wake up girl and put on your frock,  
Wake up girl, you nah hear de cock, Wakeup  
and come leh we go,  
Morning . . . cock ah crow.

Is now ah start to grind,  
I was so sure that the crown was mine.  
Ahsing like a lark,

They still give the crown to Lord Mauby-Bark.  
Ahwatch the judge and ah shake my head,  
So, a Vincentian girl turn round and said,  
Youcould grind like a corn-mill  
Nah for we prize . . . nah leaving here still.

Well friends, ah feel so funny  
' Cause they didn't give the prize to Lord Blakie,  
Ah say, "Well look at my cross, ah leave Trinidad and come quite  
here to loss."

They make me feel so nice, they hand me a consolation prize,  
Open the prize ah blow like a flute,  
Is ah piece of fish, three pieces of breadfruit.

So ah turn to my girl-friend Verro and tell her "Darling let' s go"

She say Blakie wait a little longer  
Let we see the prize you sing so hard for.  
When she see it the girl start to laugh.  
Laugh till she burst she brassiere in half.  
Friend, you couldn't believe all the time  
Ah was singing for a pitch-oil pan of chataigne.

(Runner-up for Road March, Trinidad Carnival, 1964)

## FOREWORD FOR NON-ANTHROPOLOGISTS

While the primary purpose of this monograph is to contribute "to the understanding of culture and how culture works" (p. 1), it appears that the methods developed herein may have wider application in the social and behavioral sciences.

The methods are designed for the discovery, establishment, and analysis, respectively, of developmental sequences. In the study reported here, I had data on the presence or absence of some 3,666 behavioral and artifactual features in 28 households in a village in Trinidad. On the basis of general anthropological theory, I had reason to believe

1. that at least some of these presences or absences resulted from adoption or loss of the features through time;
2. that these adoptions and losses were causally related;
3. that, hence, they had occurred in a developmental sequence or "natural order;" and
4. that the several households differed in the extent to which they had passed through this sequence.

First, then, the method of discovery was applied to order the households, from the one which had undergone the fewest adoptions and losses to the one which had undergone the most.

While this was being done, the order of adoption and loss of the features was determined (pages 38-42, 57-63, 65-68, 136).

Secondly, the method of establishment was applied to the result of the first process; an estimate was made of the probability of making, from random data, an arrangement as orderly as the one constructed from the empirical data (pages 42-45, 63-65, 68-80, 140-143),

Finally, the method of analysis was used to extract those features which conformed most closely to 'the discovered order, so that these features, and the relations between them, could be studied further (pages 136, 143-164, 170-172) .

The methods, then, are perhaps most effectively used in situations where the following conditions hold:

1. There is good reason to believe that one is dealing with multiple examples of a developmental sequence, and one wishes to discover, establish, and/or analyze that sequence.
2. One has only contemporary results of the sequences to work with; i.e., the materials to be examined are not already chronologically ordered, either relatively or absolutely. (Where the materials are already ordered, the methods can be used, and the sequence thus produced checked against the known chronological sequence.)
3. The materials are qualitative in form; i.e., they are concerned with the presence or absence of specific features.
4. There is a considerable amount of information, on the order of

30 (or more) sets of data (individual cases) with features numbering in the hundreds or thousands.

5. It can be assumed that the sets of data, or the features, or both, are either total populations or random samples thereof. When this cannot be assumed, the methods of discovery and analysis can be applied, but the method of establishment cannot. (Guttman's Coefficient of Reproducibility [1944] might be applied instead.)

There are many problems in the various disciplines to which these methods could be applied; they might be used, for example,

In archaeology, to establish a sequence of non-stratified sites, either in cases where the amount of material is too limited for standard quantitative seriation techniques or as a supplement to seriation.

— In economics, to establish a sequence of addition and subtraction of various specifications in certain kinds of contracts.

— In epidemiology, to establish a developmental sequence for certain conditions of health or disease, either in individuals or families or in geographic areas.

—In history, to study mediaeval city charters.

— In political science, to study factors reported to be influencing voting behavior in a survey of various precincts.

—In sociology, to study the evolution of street gangs or informal work groups.

I hope, then, that the methods will be used by other social and

behavioral scientists; even more, I hope that they will be found useful enough to stimulate efforts to improve them from their present weak and inelegant state.

F. T. Cloak, Jr.

Chapel Hill, North Carolina

February 20, 1967

## PREFACE

In July, 1958, when I had completed my first year of anthropology courses, I took my family to Parker, Arizona, for six weeks' field work on the Colorado River Indian Reservation. Professor Milton L. Barnett, who had first encouraged me to take up anthropology, and who had arranged the financing for that field trip, met us at Parker and showed us around the reservation. Before he left he handed me

a copy of William H. Sewell's "Scale for the Measurement of Socio-economic Status of Oklahoma Farm Families," remarking that I might find it useful.

Taking Mr. Barnett's advice perhaps a little more literally than he had intended, I proceeded to draw up a sample of Mojave and Chemehuevi households and to interview them, using a modified Sewell scale accompanied by a handful of questions concerning use of Indian language, dress, and so forth. Then I brought the results home to Madison and put them in a file for two years, getting them out from time to time and trying to decide how to use them in an

M. A. thesis.

Meanwhile my course work continued; as one would expect, most of it consisted of general anthropological training, but a few segments

stand out as having influenced the peculiar direction of my thought within the general framework of anthropology:

In 1958-59, in an archaeology course with David A. Baerreis, I learned that "culture-trait" is not necessarily a dirty word, and first began to realize that the culture-trait, if properly defined, might be the empirical unit of culture and culture-change.

During the summer of 1959 I read and re-read Kroeber's Anthropology. Only now, as I read it yet again, do I realize how many of the ideas I have about culture have been derived from that one book. This work, for example, is in a very real sense simply an expansion of and an attempt to use operationally a few of the ideas Kroeber saw so clearly and explained so well.

Robert J. Miller, in a theory course, put me on to the concept of culture as superorganic, as a causal force rather than as merely a descriptive concept, and pointed me toward Leslie A. White's The Science of Culture. The result of this encounter was a real intellectual crisis for me. I soon realized that while both White and his detractors were offering profound insights into the nature of culture, they were unable to bring their arguments to the point of empirical resolution. The reason for this seemed to be a sometimes incredible naivete, on both sides, with respect to the logic and nature of scientific method. It was mainly in order to find something to resolve this controversy and thus reduce my intellectual tension that I returned to the study of the

logic of science, taking my Ph. D. minor under Haskell Fain of the Department of Philosophy, University of Wisconsin.

Finally the bits and pieces garnered during these various intellectual excursions began to fit into a sort of pattern, and I was able to turn my field data into a master's thesis. In it I demonstrated that among the Colorado River Indians, the people who had lost the most aboriginal and early reservation period culture were the people who had acquired the most modern day Anglo-American culture (using the Sewell scale as a measure of the latter), and that the specific data items for loss and for adoption could be ordered into a pair of Guttman scales.

Soon after the master's thesis was completed I began planning to replicate it by a much more intensive study in Trinidad. I was intuitively aware that the notion of a culture as an integrated system of some sort would logically lead to the prediction of certain regularities in culture change, and I was eager to try this idea out in the field. After a two-year wait I was able to secure support for eighteen months' field work in the form of a pre-doctoral training fellowship from the Social Science Research Council.

I arrived in Trinidad with my family in August, 1963. After two months in the St. James section of Port-of-Spain, the capital, we moved to Enterprise, Chaguanas ward, where the field study was carried out. We returned to Madison in March, 1965, and I spent six months there preparing the data for computer processing and processing it, using the

facilities of the University of Wisconsin Computing Center, funded by the University of Wisconsin Faculty Research Committee (through my advisor, Robert j. Miller). During that spring and summer I was supported for three months by an extension of my S. S. R. C. fellowship, and for one month by a Graduate School summer fellowship. The writing of the monograph was done during the academic year 1965-66, while teaching at the University of North Carolina.

I should like to express my appreciation and gratitude to all the individuals and institutions mentioned above, and also to John Gonsalves and Horace Fifi, for helping us adapt to Port-of-Spain; to Henry Nebblett and to Lionel E. Punnett and to Mrs. Ambrosine Walters, for continual help and encouragement in Enterprise; to Edgar F. Borgatta and to Gene V. Glass for suggestions which led to the method of data processing; to Joshua Chover for explaining the facts of life about the current status of knowledge about binary matrices; to Robert Schacht for his constructive suggestions while programming; to the staff at the University of Wisconsin Computing Center for many little courtesies and favors; to Joan Rubin for editorial suggestions; and to Robb and Ouida Taylor for typing, etc.

My wife, Jane Cloak, should not be mentioned here. Her name should appear on the title-page as co-author, not only because she did a substantial part of the field work but also because she has listened

creatively for nine years —to every idea that went into this effort and to a  
great many more that were (fortunately) left out.

Finally, this book is dedicated to the people of Enterprise, collectively  
and individually.

Chapel Hill, North Carolina

May 4, 1966

## TABLE OF CONTENTS

CHAPTER	Page
I. INTRODUCTION .	1
What this study claims to accomplish	2
Notes for Chapter I	5
II. THEORY: A CULTURE AS A FUNCTIONALLY INTEGRATED WHOLE .	7
A. The two concepts of "A Culture"	7
B. The general nature and the general empirical consequents of the d-culture as a functionally integrated whole (FIW) .	11
C. The language of description of x-cultures	17
D. The culture trait: the unit of x-cultural change	23
E. The Natural Order of Cultural Adoption and Loss: theoretical considerations	26
F. The Natural Order of Cultural Adoption and Loss: empirical problems .	29
G. The household as the social unit .	33
H. Studying cultural change synchronically	36
I. Testing the hypothesis synchronically .	42
Notes for Chapter II .	46
III. METHOD: ARRANGING THE DATA AND TESTING	
THE GENERAL HYPOTHESIS . . .	57
A. Introduction	57
B. Processing the data-matrix: computer aspects	65
C. Evaluating the best possible matrix	68
Notes for Chapter III .	81

CHAPTER	<u>Page</u>
IV. A FIELD TEST OF THE HYPOTHESIS IN ENTERPRISE, TRINIDAD.	86
A. Introduction	86
B. Social description and selection of the study population	87
C. Similarities and differences among the ethnic categories in Enterprise .	95
1. Occupations and employment	95
2. Household composition	103
3. Religious groupings .	107
4. Inter-group relations .	108
D. The sample of Vincentian households .	113
Notes for Chapter IV .	119
V. FIELD WORK METHODS AND TECHNIQUES.	124
Notes for Chapter V .	138
VI RESULTS .	140
Recapitulation.	164
Further Research	165
Note for Chapter VI	170
REFERENCES CITED	173
CULTURAL DATA .	Supplement

## LIST OF TABLES

TABLE	<u>Page</u>
1. Summary of the Household Register	92
2. Rates of Employment by Ethnic Category	96
3. Frequency of Employment by Ethnic Category	97
4. Place of Employment by Ethnic Category'	98
5. Computation of Total Occupation Units with Kind Known	99
6. Types of Occupation-Units by Ethnic Category .	101
7. Composition of Households with Children, by Ethnic Category. .	105
8. Composition of All Households, by Ethnic Category .	105
9. Summary of Life History Data of the Sample of 28 Vincentian Households	115
10. Selected Culture Traits, by Cutting-Point: An Approximation of the Natural Order of Cultural Adoption and Loss	147

LIST OF FIGURES

FIGURE		<u>Page.</u>
I.	Schematic diagram of a cultural change . . . . .	25
II.	Distributions and means of two sets of hypothetical values of UTOT	79
III.	Random and empirical values of UTOT superimposed	141

. theory about human [cultural] evolution remains at a stage comparable to that in biology before the development of genetics.

— Margaret Mead (1958: 486)

Students of human life who pride themselves on being "scientific" and upon their rigor still tend, consciously or unconsciously, to hold the view of "science" set forth in Karl Pearson's famous Grammar. In other words, they not only take physics as their model but specifically nineteenth-century physics. Here problems of measurable incidence and intensity predominate. Such problems also have their importance in anthropology, but the most difficult and most essential questions about culture cannot be answered in these terms. As W. M. Wheeler is said to have remarked, "Form is the secretion of culture." Form is a matter of ordering, or arrangement, of emphasis. Measurement in and of itself will seldom provide a valid description of distinctive form. Exactly the same measurable entities may be present in precisely the same quantities, but if the sequences or arrangements of these entities differ, the configurations may have vastly different properties. Linguistics, which is, on the whole, the most rigorous and precise of the cultural sciences, has achieved its success much more by configurational analysis than by counting.

— Kroeber and Kluckhohn (1952 : 161)

Perhaps a better way of putting the problem would be to say that as yet we have no full theory of culture. We have a fairly well-delineated concept, and it is possible to enumerate conceptual elements embraced within that master concept. But a concept, even an important one, does not constitute a theory. There is a theory of gravitation in which "gravity" is merely one term. Concepts have a way of coming to a dead end unless they are bound together in a testable theory. In anthropology at present we have plenty of definitions but too little theory.

— Kroeber and Kluckhohn (1952 : 181)

CHAPTER 1  
INTRODUCTION

In this monograph, I hope to make a contribution to the understanding of culture and how culture works by showing, in part, what a culture is and how it changes. I will not deal with broad, sweeping long term changes in culture, but rather with very short, very slight changes in the culture of one social group in fact, I intend to discover, and utilize, the smallest empirical unit of cultural change. I further intend to show that microchanges in culture can be studied synchronically, by comparing the cultures of parallel subunits of the social group being studied.

This study will, it is hoped, lead to an understanding of some of the mechanisms by which culture changes at the microscopic level. If this can be done, perhaps these mechanisms may, in turn, be useful in explaining cultural change in the historical sense of the term, in the same way that we can understand developments in the evolution of man through concepts developed in short-term experimental studies of populations of fruit-flies, English moths, and human beings in malarial regions.

### What This Study Claims to Accomplish

1. It develops a general hypothesis about culture that includes both integration and change without sacrificing either. This has not been done previously,'

2. It provides specific techniques for studying a culture as an integrated changing thing. These techniques are completely straight--  
forward. They are not confused by the use of terms denoting substantive items of culture in theoretical statements or methodological instructions; thus they are completely general. No one has heretofore spelled out a complete methodology in such detail,

3. It can lead to a general theory of culture and culture change that can be tested, anywhere culture exists, by using the techniques mentioned above. No one has as yet written such a theory.<sup>2</sup>

4. This general theory, if supported by replication elsewhere, can be refined, added to, improved, even completely rewritten, as a result of the replicative study; but it will still be the same theory, In other words, the theory will be corrigible not only as a whole but in detail. Again, no one has as yet written such a theory.

5. In the process of developing procedures for validating the general hypothesis, we present a new method for constructing Guttman-type scales. With this method and a high-speed computer, we can

a. construct double scales as well as single scales; 1.  
e., scales with a parallelogram as an ideal pattern as well as  
scales with a triangle as an ideal pattern

b. construct scales from very large amounts of data, and

c, construct scales where the number of ' attributes' is many  
times the number of ' individuals,' to use Guttman' s terms (1944).

6. We develop a new measure of the closeness of empirically derived  
scales to the ideal pattern; this measure is adapted from Mann and  
Whitney' s "U" (1947), Used in conjunction with the scaling process  
mentioned in paragraph 5 above, the new measure is, in effect, an  
expansion of "U" into two dimensions.

7. The problem of determining the probability of getting a certain  
value of this new measure is explored, and a computer technique for  
estimating this probability is offered, using Monte Carlo procedures, No  
one has yet successfully presented a method for estimating the probability  
of a Guttman-type scale.<sup>3</sup>

8. The field work technique developed for and used in this study is not new  
or unique, having been used, for example, in the California Cultural Element  
Surveys in the thirties, and more recently by  
John M. Roberts (1951). It has, however, never before been

used to this extent and with this degree of precision in the study of a single village or social group. I refer here to the use of interview schedules and material culture checklists to gather comparative cultural data from parallel subunits of a society,

## NOTES FOR CHAPTER I

<sup>1</sup>Kroeber and Kluckhohn touch on this problem when they say:

On the other hand, the inescapable fact of cultural relativism does not justify the conclusion that cultures are in all respects utterly disparate monads and hence strictly noncomparable entities, (Footnote: As a matter of fact, cultures may share a large body of their content through historical connection and provable derivation and yet have arrived at pretty diverse value systems. If we could recover enough ancient and lost evidence, it is expectable that we would be driven

to the admission that every culture shares some of its content, through derivation, with every other on earth. This historic interconnection leaves any monadal view or talk of the noncomparability of cultures without basis. Possessing coancestry, they must be comparable.) If this were literally true, a comparative science of culture would be ex hypothesi impossible. It is unfortunately the case that up to this point anthropology has not solved very satisfactorily the problem of describing cultures in such a way that objective comparison is possible. Most cultural monographs organize the data in terms of the categories of our own contemporary

Western culture: economics, technology, social organization, and the like. Such an ordering, of course, tears many of the facts from their own actual context and loads the analysis. The implicit assumption is that our categories are "given" by nature an assumption contradicted most emphatically by these very investigations of different cultures. A smaller number of studies have attempted to present the information consistently in terms of the category system and whole way of thought of the culture being described. This approach obviously excludes the immediate possibility of a complete set of common terms for comparison. Such a system of comparable concepts and terms remains to be worked out, and will probably be established only gradually.

(1952: 175-6)

<sup>2</sup>It may be argued by some that such theories do exist, that hypotheses have been offered, and tested, that explain and predict one cultural change on the basis of another, and that these hypotheses do

reflect a developing agreement on the nature of culture and cultural change; in other words, that there does indeed exist a theoretical framework, albeit a rather implicit one.

I agree. But I feel that such hypotheses are concerned with what one might call macro-change , that is, with large-scale changes of state (e.g., with jumps from one level of socio-economic integration to another, from being a sedentary culture to being a horse-culture, from being apathetic to being messianic). I submit that such macro-changes are the end product of a series of minute changes occurring systematically through time and that a global explanation of a macro-change fails completely to take this sequence of micro-changes into account

The analogy to our growing awareness of the nature of biological change is apparent; studies of macro-evolution have led merely to descriptions of temporal sequences of forms, and such quasi-explanations as "irreversibility of evolution," "trend toward increasing complexity," and "increasing ability to capture and utilize energy from the environment" Studies of micro-evolution, on the other hand, have led to, and been aided by, explanations in terms of change in allelo-types of populations, natural selection expressed mathematically, the notion of balanced polymorphism, and other sophisticated concepts, See Kroeber and Kluckhohn 1952: 182.

Some archaeologists have also been working on micro-change. Compare the following:

It would seem, therefore, that the types serve best for reconstructing broad-scale historical relationships, whereas the modes are more sensitive indicators of changes in culture within small regions and over short periods of time. (Rouse 1939: 14<sup>1</sup>)

<sup>3</sup>Professor Joshua Chover, Department of Mathematics, University of Wisconsin: personal communication.

## CHAPTER II

### THEORY: A CULTURE AS A FUNCTIONALLY INTEGRATED WHOLE

#### A. The two concepts of "A Culture"

We open our discussion with an extensive quotation from Kluckhohn and Kelly, to which we will refer repeatedly in the next few chapters:

". . . there are four variables in the determination of human action: man' s biological equipment, his social environment, his physical environment, and his culture. Let us designate those as a, b, c, and d. But a given system of designs for living is clearly the product of a, b, c, and d. In other words, it is quite clearly different from ' d' alone, so let us call it ' x' . It would seem, then, that anthropologists have used the same term ' culture' to cover both 'd' and 'x' . This is enough to make a logician' s hair stand on end.

". . . Perhaps, in practice, the confusion has been mitigated by the tendency to use ' culture' for the analytical abstraction ' d' and 'a culture' for the generalizing abstraction ' x' . But it is all too true that anthropologists and other scholars have frequently treated 'd' (the explanatory concept) and 'x' (the descriptive concept) as synonyms or equivalents. Having given a sound abstract description of 'group habits,' the anthropologist then unthinkingly employs this (' x') a an explanatory concept, forgetting that ' x' must be regarded as the joint product of ' d' and three other determiners.

" 'X' [sic] is much closer to observable 'reality' than ' d' ' D' is, if you will, only an hypothesis —though a highly useful

hypothesis. X', however, is an abstract representation of central tendencies in observed facts."

—Kluckhohn and Kelly 1945, quoted in Kroeber and Kluckhohn 1952: 90.<sup>1</sup>

Since we are going to be talking exclusively of particular social groups and their cultures, we will refer from here on to "the d-culture of social group y," "the x-culture of social group y," "d-cultural change" (i.e., change in the d-culture), "x-cultural change," and so forth. In other words, as far as we are concerned there is no such thing as "culture," but only cultures, x- and d-

But we are interested not so much in what produces an x-culture, or in how a d-culture controls, influences, or determines its x-culture; we are more interested in how d-cultures (and x-cultures) change. For this reason we are going to have to introduce some ideas and concepts additional to those of Kluckhohn and Kelly.

a, b, and c (biological equipment, social environment, and physical environment) are, from the point of view of d, all external factors; for the present discussion we will treat them as being either constant or essentially unpredictable, and lump them together under the term habitat. (In justification of this, it might be pointed out that to Kluckhohn and Kelly, 'social environment' seems to refer to demography, not to the determinants of social interaction-patterns, which they would include under 'd'.)

in addition, if we are to discuss d-cultural change, there is an important factor not mentioned in the Kluckhohn and Kelly discussion. This is the collection of behaviors and products of behaviors exhibited by neighboring societies, which are available for borrowing by society y. This will vary, of course, from place to place and from time to time, and variation in it will be very important in determining changes in the d-culture and the x-culture of society y. Let us refer to it as the cultural environment of society y.

It goes almost without saying that it never happens that the entire cultural environment is borrowable by a society. For one thing, the habitat may be unsuitable for certain items. More important for this study, certain items may not "fit in" with the d-culture of the society. In other words the d-culture acts to control, selectively, diffusion or borrowing.<sup>2</sup> One way to view the role of the d-culture, in fact, would be to see it as a filter, which determines the content of the x-culture by controlling borrowing.

But the d-culture does not operate in a random, arbitrary fashion. Its actions have a form, or pattern, underlying them.

Cultures are systems (that is, are organized) because the variables are interdependent. All systems appear to acquire certain properties that characterize the system qua system rather than the sum of isolable elements. Among these properties is that of directionality or "drift," There is a momentum quality to cultural systems.

(Kroeber and Kluckhohn 1952: 189)<sup>3</sup>

For the kind of systems d-cultures are, we are going to use the term ' functionally integrated whole, abbreviated . This expression will be made more explicit in the next section. We are not going to take this lastmentioned notion about culture as an assumption, however; we will, rather, regard it as an hypothesis to be tested experimentally.

We are not claiming, furthermore, that the d-culture is an unchanging changer, that it has the character of, say, a genotype that once established can be modified in its effects but not in its nature. A d-culture is seen as more like an allelotype of a population, which is modified by the process of the population' s adapting to its habitat. In other words, we claim that when the x-culture is limited or modified by habitat-factors, these limitations or modifications are "fed back" in some fashion to the d-culture, and limit or modify it, thus altering its further operation as a determiner of the x-culture.<sup>4</sup>

Finally, it would be well to make explicit two more basic assumptions, which really need no defense: first, that individuals learn their culture through precept or example, mainly from older members of their own social groups (persistence) but also, occasionally, from members of other groups (diffusion or borrowing); second, corollary to the first, that a d-culture can properly be viewed as the social heritage or tradition of a social group.<sup>5</sup>

B. The general nature and the general empirical consequents of the d-culture as a functionally integrated whole (FIW)

We will put two main questions to ourselves: First, can we show by empirical means that a culture is indeed an FIW —that is, not only that certain broad features of an x-culture tend to vary together but that integration, like traditionality and learnability, is an essential characteristic of a culture? Second, in the process of showing that a culture is an FIW, can we use the empirical evidence to get some indication of what kind of an FIW a d-culture is, what some of the mechanisms involved are; in other words, of how a d-culture, as an FIW, works?

To answer these questions, we must find a mechanical procedure by means of which we can feed in behavioral data from a culture-bearing group (1, e., feed in the group's x-culture or a sample of it), turn a methodological crank, and be informed whether the d-culture of that group is an FIW or not.

Then, if the procedure reveals that it is indeed an FIW, we want to be able to arrange the data in such a way that we can examine it and get some insights into the nature of that FIW, and thus of d-cultural FIWs in general.

To begin with, let us assume, for purposes of exposition, that we know that a d-culture is an FIW. (Later we will drop this assumption.) We will not assume anything about the specific nature of cultural FIWs,

We will not assume, for example, that the ecological or technical areas of culture are central, or even that every culture has an ethos, or a focus, or a set of themes or postulates, around which it is organized.

What, then, are the general, essential characteristics of an FIW, cultural or otherwise?

1. An FIW is a piece of a universe. That is to say, the things which go to make up an FIW are of a kind. For instance, a living animal is made up of cells, a chemical formula is made up of valences, a genotype is made up of alleles or of DNA molecules (but not both). We don't know what a d-culture is made up of, yet, but we can say that it is made up of one kind of (cultural) things.

2. An FIW is a piece of a universe. In other words, not all of the kind of d-cultural things mentioned above are in any single culture; things can be added and subtracted.

3. A change in a part of an FIW causes a change in the whole.<sup>6</sup> This is the central defining statement of what an FIW is, and requires a good deal of explication. It is not meant to be a trivial truth, a truth inherent in the definition of the whole-part relation (as is, say, the truth that a change in the number of shirts on a laundry list is a change in the list as a whole). It is meant, rather, to be a factual statement of cause and effect involving, at least, a difference in time between the first change and the second.

4. The preceding paragraph introduces the notion of "part of an FIW," but we lack any basis for distinguishing one part of a d-culture from another (just as we lack knowledge of d-cultural things). We have noted, however, that a change in a part is a change in the whole (by definition of the whole-part relation), so we can rephrase paragraph 3 to read simply: One change in an FIW causes another.

5. The statement is perhaps too strong, since it implies a continuous, never-ending series of changes, given any change at all. To avoid such an implication, we will rephrase the statement again: The changes in an FIW are causally related.

6. The notion of 'cause' is not very clear, but we need not solve the problem of its explication here. For our purposes, we can simply reinterpret statement 5 thus: Antecedent changes in an FIW are sufficient conditions for subsequent changes.

7. The epistemological import of 6 is: If we fully understood the principles or rules governing the operation of a certain FIW; and if we knew its history, i.e., the changes that have occurred in it up to the present moment; then we could predict the next change that will occur in it (or that there will be no further change). In other words, by knowing what has happened in an FIW, we can in principle predict what will happen next,

8. Suppose that a certain FIW is in stasis. Then a change occurs

(whose cause, presumably, lies outside the FIW). From 7 it follows that we can, in principle, predict a sequence of changes to follow.

All the above statements can be summarized into two:

1. An FIW is a piece of a universe.
2. If an FIW is in stasis, and a change occurs in it, subsequent changes fall into a predictable (in principle) sequence.

We have now explicated the concept of a functionally integrated whole (FIW), to the extent that we know the general rule of change in an FIW.

Let us reiterate that it is in the nature of an FIW that changes in it are predictable in principle. What we mean by "in principle" is that if we had sufficient theoretical knowledge of how FIWs work, and if we knew the history of a given FIW, or perhaps its present state alone, we could, without reference to anything outside the FIW, predict what is going to happen to that FIW next; always provided that no event occurs outside the FIW which influences its internal workings.<sup>7</sup>

Now a thoroughgoing philosophical determinist will say that every event is, in principle, predictable. In our terms, then, he is claiming that the entire universe is a functionally integrated whole (FIW). The difference between him and us is that he merely asserts his claim; we aim to subject our claim to empirical test.

In order to make such an empirical test, we must find a way to

express changes in an FIW. This is essentially a problem of describing the FIW, fully or partially, and then changing the description.

We do not know what the terms for describing a d-cultural FIW might be, just as we don't know what the 'parts' are or what the 'things' are (see above). We do know, however, that changes in Kluckhohn and Kelly's d-culture determine changes in x-culture. Thus we can use changes in x-culture as indicators of changes in the d-cultural FIW and, hopefully, we can study sequences of the former for clues as to sequences of the latter.<sup>8</sup>

At this point we want to make another temporary assumption, in order to make the implications of this relationship clear. We assume that the d-culture is not only known to be an FIW, but that it is also omnipotent and all-inclusive. That is, the d-culture and only the d-culture controls x-cultural change.

Now we have the problem of describing changes in the x-culture.

Any description of a named thing can be said to consist of a number of sentences joined by 'and'. The subject of every one of the sentences is the name of the thing, in this case 'the x-culture of social group y.' The predicate of each sentence consists of a term or phrase or clause describing some characteristic or quality.

If we required a complete description of the thing we would be obliged, obviously, to write all the descriptive sentences; the predicate of the final sentence might be something like "nothing else." Or

else we could write every possible sentence, with the word ' not' in the predicates of the sentences that were not applicable.

Fortunately, however, do do not have to perform this infinite, exhaustive, and Herculean task, as we only want to describe changes in the thing. To do this we can simply write a certain sentence for a certain time, and write the same sentence, but with the predicate negated, for a certain subsequent time, thus:

John is not old at time  $i$ .

John is (not not) old at time  $i + 1$  .

Thus we describe a change in John. For an x-culture:

The x-culture of  $y$  has characteristic  $z$  at time  $i$  .

The x-culture of  $y$  does not have characteristic  $z$   
at time  $i + 1$  .

Now if the "thing" in question is, as we are assuming for an x-culture, determined in its changing by an omnipotent and all-inclusive FIW, it follows from Summary Statement 2 (above, page 14) that if an x-culture is in stasis and we write a description of a change in that x-culture, we could in principle then proceed to write further descriptions of changes in that x-culture in the correct sequence. The reason that we cannot do this at the present time is not that culture change is inherently unpredictable but that we don't understand d-cultural FIWs (and how they control x-cultures) well enough.

Or we can forget about the d-culture and simply look at the x-culture through time. Given our (temporary) assumptions, if we observe changes

in an x-culture, and describe them as they occur, we will have written them in the correct sequence, the sequence determined by the FIW'; in other words, we will have written the changes in the sequence we would have written ante factum had we had sufficient knowledge both of the determining d-culture and of how d-cultural FIWs work.

### C. The language of description of x-cultures

We have said above that descriptions of changes consist of changes of descriptions, by negating their predicates. (It can easily be shown that this statement holds for quantitative changes, i. e., changes along a continuum, as well as for clearly qualitative changes.)

Descriptive statements about an x-culture are statements of presences or absences of culture-traits, so descriptions of change in an x-culture consist of time-tagged negatings of such statements. Culture traits, in turn, are located by applying certain procedures to statements inferred from observations of the behavior of members of the social group whose x-culture is being described. These procedures are described as follows:

The lowest-level descriptive term describes a behavior-element; a behavior element consists of a stimulus-element and a response element.

Behavior-elements exist a priori in the lexicon of the observer. That is to say, a field worker observes someone behaving and writes

down as simple and as extensive a description or what he has observed as possible, This description will include notations of movements, gestures, etc., and also verbatim quotations of the speech of the individual in question and of others.<sup>9</sup>

In general, the report will consist of a description of the situation, including what other people may have said or done, and of the action of the person in question. The field worker will use his intuition to determine which part of the total situation is stimulus for a particular response and which part of the action is a response to a particular stimulus.

(This intuition will be derived from common sense, from study of the field descriptions and analyses of other anthropologists, and from specialized training in techniques of anthropological field research. It will be refined and re-refined as observation and analysis proceed.)

The behavior-elements, then, are words and phrases used in these reports of behaviors. They are very roughly analogous to phones, symbolized in the International Phonetics Association alphabet, as used by someone gathering textual materials for a later linguistic analysis.

When a body of such descriptive elements is in hand (and as it is added to), one begins to analyze the body into behavior-types. A behavior-type is, quite simply, an ordered set of behavior elements (first stimulus-elements, then response-elements) that seem frequently

to occur together in that order. in other words, the ethnographer judges that there exists a propensity in the society for (at least some) people to produce a certain ordered set of response-elements when confronted with a certain ordered set of stimulus-elements.<sup>10</sup>

In an ideally simple society, with a very small population and no status-differences at all, a description of the x-culture would consist of nothing more than a list of behavior-types, and a change in x-culture would be expressed by negating a behavior-type predicate. To express an adoption, for example:

S U B J E C T	P R E D I C A T E	T I M E
the x-culture of y does not include behavior-type w at time i		
the x-culture of y      does include behavior-type w at time i + 1		

If, however, a society does have more than one status, we have to analyze a corpus of behavior-type descriptions further, utilizing behavior-traits as the analytical unit. A behavior-trait is a predicate consisting of a behavior-type plus a status-designator, for example:

	STATUS DESIGNATOR
culture of y includes behavior-type w by <u>young adult male</u> at time i	

More than one status-designator may be required; for example: . . .

behavior-type w by young adult male toward adolescent female

Statuses are discovered empirically by correlating behavior-type descriptions with personal names or other individual-designators. If certain persons have exhibited certain behavior-types while other persons have not, one suspects a status-difference. If analysis shows that there is a set of behavior-types associated with and only with one group of persons, this set of behaviors defines a status. In all probability the defining behavior-types will include a ready-made status-designator in the indigenous language, e. g.

#### STATUS DESIGNATOR

when asked "Why don't you hunt?" Old Man says "I am an old man."

If no indigenous status-designator is forthcoming, the ethnographer will coin one."

Finally, in a society with more than an absolutely rudimentary social system, we will discover sets of behavior-traits which hold for pairs, triplets, or quadruplets of statuses, e. g. , husband and wife, maternal uncle and nephew, the various statuses involved in a puberty ceremony. Such sets of behavior-traits define institutions. An institution is, in effect, a set of statuses which occur together in space and time and are defined in part by patterns of interaction among them.

The reader will no doubt have realized that these various "steps" in the analytical description of an x-culture will not in actuality occur separately and independently, but rather that, as one begins to suspect

the existence of an institution, one will postulate statuses and look for defining behavior-types. He will then, of course, examine his

raw" observation data for overlooked similarities and differences in behavior-element sets to conform to these rough hypotheses. The analogy to linguistic analysis need not be labored. It is also obvious that what I have attempted to do here is to provide a sort of rational reconstruction of what ethnographic field workers have been doing all along as they "work up" their data.

The next step in analysis is, however, novel. But before going on, we want to deal with the problem of time and the problem of propensity both terms having been used above without discussion of their meanings.

A behavior-trait is not, properly speaking, the response of a holder of a certain status to a certain stimulus in a certain institutional context, but rather a propensity for such a response. Now, we can state that so-and-so has or hasn't such a propensity only if we see him holding that status and presented with that stimulus in that institutional context. But the stimulus may occur only at a certain time of the year or the institution may be instituted only once every ten years or, for a long time, no individual may be eligible to hold a certain status. In such cases the variation through time we observe constitutes behavioral change but not cultural change. If we want to observe cultural change we have to wait for the next year or the next time the institution is instituted or for another person to grow into the status.

Only then can we negate the predicate and say that culture change has occurred, because only then can we be sure that the old propensity is gone or a new propensity is added.<sup>12</sup> Regular time, as measured by clocks and calendars, we will call here 'behavioral time.' The time measured by 'i' , ' i +1' , etc., in one of our predicate-negations we will refer to as ' cultural time.'

We are now going to observe x-cultural change under our temporarily assumed ideal conditions - i.e., assuming that d-culture is an FIW and is omnipotent and all-inclusive. This assumption about d-culture, if interpreted strictly, includes conditions that may seem quite surprising.

It is almost universally agreed that the vast majority of changes in any human culture have been due to contacts with and borrowings from other cultures. Thus, if the x-culture we are studying is to undergo much change at all, it must have a cultural environment, that is a source of behaviors to borrow. But, if that cultural environment is in any way limited, the d-culture cannot be an omnipotent and all-inclusive determiner of x-cultural change. So omnipotence and all-inclusiveness imply that the cultural environment includes every possible human behavior, so that any trait that fits in can be borrowed. For similar reasons, d-cultural omnipotence and all-inclusiveness also entail a universal habitat, one in which every possible human behavior can be realized.

**a** The culture trait: the unit of x-cultural change

Assuming universal cultural environment and habitat and imagining ourselves to be omniscient ethnographers, let us proceed with our cultural analysis. First, we turn on the behavioral clock and get a complete behavior-trait inventory for social group  $y$ . Call this inventory Inventory A. To have a complete description of the x-culture of group  $y$ , we next write out a complete inventory of behavior-traits not exhibited by group  $y$ , Inventory B.

We then advance the cultural clock one unit of cultural time, run the behavioral clock through a complete cycle, and compare our inventories, time  $i$ , with our inventories, time  $i + 1$ . Each behavior-trait predicate from Inventory A which has been negated describes a culture trait which has been lost. Each behavior-trait predicate from Inventory B which has been negated describes a culture-trait which has been gained or adopted. The concept 'culture-trait' is thus defined, roughly speaking, as the empirical unit of cultural change (i. e., the unit of x-cultural change). While such a conception may be unusual, I believe it does no violence to common usage.

(Note that analytical feedback is again required; for example, what we had thought was a behavior-trait may have been lost or adopted only in part, in which case we must conclude that it was, at time  $i$ , actually two behavior-traits highly correlated, and we must alter our original inventories accordingly.)

(in so doing, we are also solving what has been at times a vexed problem in the study of diffusion; namely, the problem of reinterpretation, of the borrowing of form without the borrowing of function, and so forth. Our culture traits are built up from behaviors, and expressed by verbs or verbals; they describe only what people do (including their verbal deeds) and, ideally, everything they do. If the form of a behavioral complex is borrowed, but not the function, this will be reflected in the description of the borrowing. As a matter of fact, the function of a trait can be discovered only by analysis of the institutional setting into which it is borrowed and of its place in the sequence of borrowings.

(See, for instance, the example presented below, kissing at an office party. One can see from this example that "the office party" would never be the name of a trait; traits might be such things as drinking at an office party, kissing at an office party, etc. , each further qualified by who does it and under what specific conditions. The name of the trait does not in any way imply its function, but only its form; i. e., the specific behavior in question.)<sup>13</sup>

A schematic diagram of a description of a cultural change, then, might look something like this:

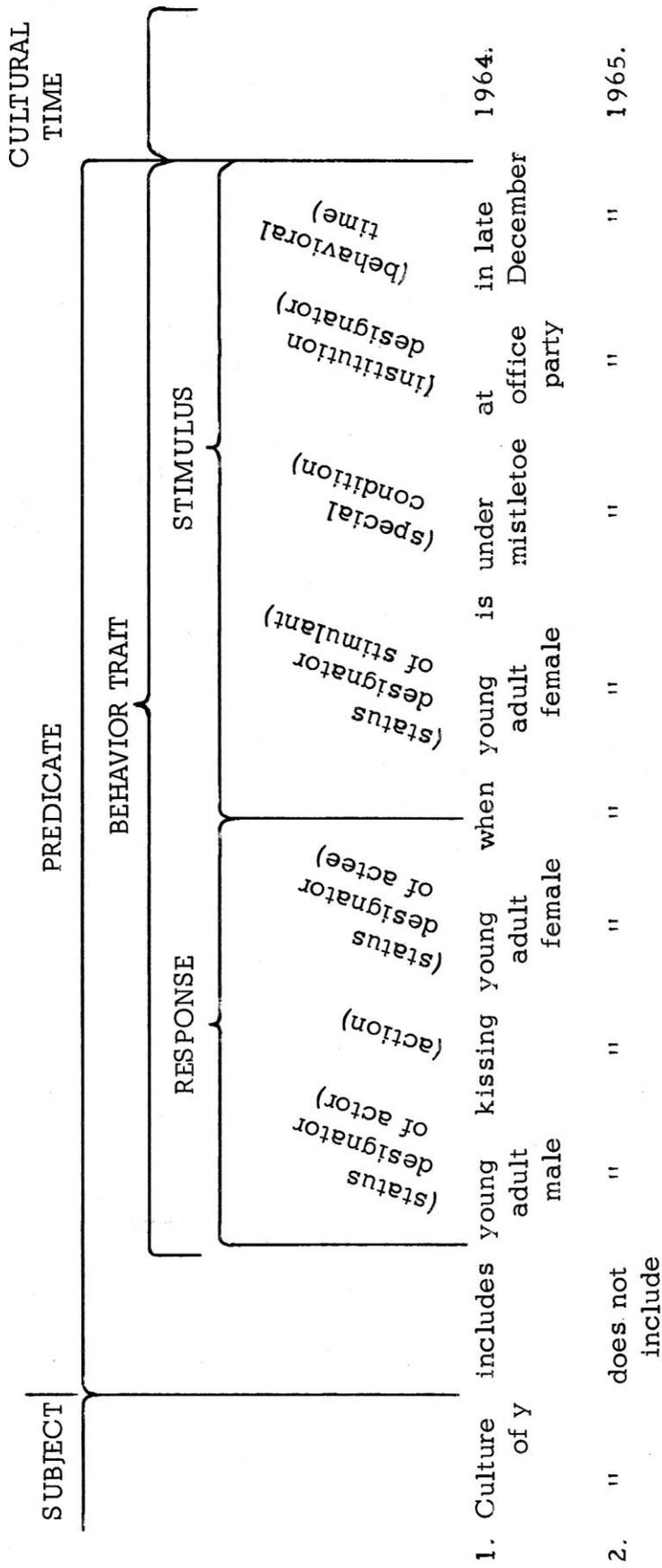


Figure I. Schematic diagram of a cultural change.

Thus might an ethnologist studying American culture denote such a change; to wit, the loss of a culture-trait. At some time between December 1964 and December 1965, the propensity of young adult males for kissing young adult females under the mistletoe at office parties in late December has been lost.

#### E. The Natural Order of Cultural Adoption and Loss: theoretical considerations

And so we go on, advancing the cultural clock and cycling the behavioral clock, listing the cultural traits and changes as they occur. When we have a goodly list of such traits and changes, we can examine this list in an effort to discover the mechanisms in the d-cultural FIW which caused the changes to occur in that order. Since we know that all changes in the x-culture were caused by the operation of the FIW, we can call the order represented by this list a Natural Order of Cultural Adoption and Loss of the culture of group y .

Let us now relax our assumption of d-cultural omnipotence in a series of steps and examine the consequences of each step in turn.

First, we will reduce the last-mentioned entailments of d-cultural omnipotence, and let the cultural environment and habitat be limited; that is, not every possible behavior-trait will be available for borrowing, nor will the habitat permit the entire range of cultural expression. We will not, however, allow them to change ; we will continue to hold cultural environment and habitat constant through cultural time.

The consequence of this is that, while not every change that the d-culture would determine can occur, any change that does in fact occur is determined by a change in the d-culture and, as before, the d-culture's being an FEW guarantees us a Natural Order of Cultural Adoption and Loss, but with gaps. (A corollary consequence may be that because certain changes cannot occur when called for by the d-culture, the whole process may come to a halt. It could be set in motion again only by a change in cultural environment or habitat.)

Next, we give up d-cultural all-inclusiveness and allow normal variation in cultural environment and habitat. Such changes are, of course, inherently unpredictable; that is to say, no amount of knowledge of d-culture-as-FIW would enable us to predict them. At the same time, any one of these "external" changes can start a sequence of changes in the d-culture; such a sequence would in turn produce a Natural Order of changes in the x-culture until that order was upset by another "external" change. So we are confronted, empirically, with a series of sequences of changes in the x-culture; with fragments, as it were, of Natural Orders; but we can't really tell when one Natural Order is broken or when the next Natural Order begins. We have both gaps and foreign elements to contend with.

(We can, however, try to pick a society where drastic changes in cultural environment and habitat are not apparent —where rapid acculturation isn't being forced, for example, by political and/or

military means, or by recent contact with a society recognized as worthy, for prestige or economic reasons, of intense emulation. Thus we can hope that FIW-changes will be revealed in the x-culture in relatively extensive sequences of Natural Order.)

At best, unfortunately, we will have some foreign elements, and such will surely confuse us if we attempt to use this broken order to make inductions about the nature of the d-cultural FIW.

We must solve this problem, then, of sorting out changes in the x-culture due to changes in the d-culture, from changes in the x-culture due to cultural-environmental and habitat changes.

We can do this if we make the following assumption: Changes in the cultural environment and in the habitat are, in the main, unpredictable. What we really mean by this is that changes in the cultural environment and the habitat are determined by many different factors, simply related neither to each other nor to the d-culture. Thus there appears to be no temporal pattern in the behavior manifestations of these changes.

If we make this assumption, we can observe several social groups with similar cultures, similar habitats, and similar cultural environments, in the manner described above for studying a single group; i. e., we can replicate our study. Barring radical change in the habitat or cultural environment of one or more of these groups, we would end up with roughly similar sequences of adoptions and losses

(predicate-negatings). Those adoptions and losses that were present in all (or most) lists, and in the same (or nearly the same) order, would be the ones resulting from changes in the d-cultural FIW. Thus we could attempt to infer the nature of the d-cultural changes from changes in x-culture. Of course, we would still have discontinuities and gaps, which would make the task more difficult.

At this point we can discard our last remaining temporary assumption, the one we made first, namely that we know that a d-culture is an FIW. That is to say that, instead of stating it as an assumption, we state it as an hypothesis.

It can now be seen that the very procedure which we have outlined above, a procedure to investigate d-cultural change through its x-cultural effects, actually is also a test of this hypothesis, for this reason:

The d-culture can be said to be an FIW only if similar x-cultures, under the stated conditions, do indeed go through parallel sequences of adoptions and losses; that is, only if the same Natural Order of Cultural Adoption and Loss can be demonstrated to exist in several parallel social groups can we assert that the x-culture of those groups is controlled by an FIW.

F. The Natural Order of Cultural Adoption and Loss: empirical problems At this point it may be asked how we can locate parallel culture-bearing social groups for this kind of comparative study. We want to

be able to do this a priori , that is before we begin describing x-cultures and x-cultural changes. What criteria can we use to determine that two or more social groups a) have similar cultures, and b) occupy similar cultural environments and habitats?

To take the second part of the question first: The groups can be said to occupy similar habitats and cultural environments if they occupy the same habitats and cultural environments; i. e., if they exist at the same place and time. But, it may be asked, how can they occupy the same habitats and cultural environments, when each will then be a part of the other' s habitat and cultural environment? The answer is that we don' t need identical environments and habitats, we need similar ones, and since the groups' x-cultures are to be similar they will have similar effects on the habitats and environments of the others. Then, too, it can be said that such effects of a group' s x-culture are part of its own habitat and environment, so groups occupying the same place and time really do have practically the same habitat and environment.

As for cultural similarity: Two groups are similar culturally if they are daughter-groups of the same parent-group. This statement is true in the light of the notion of a culture as a tradition, or social inheritance, of a people. It is our intention to apply this parent-daughter analogy quite literally.

(To take a similar analogy, if a biological population splits up into two groups we would expect the allelotypes of the two daughter

populations, one generation hence, to be about the same; and we would say the daughter populations are racially similar, without having to describe their allelotypes. Severe cases of genetic drift must, of course, be excepted.)

Of course, it is hardly likely that we can find in nature two daughter societies of one parent society, still less a large enough number of societies to replicate our experiment often enough to suit us. That is, it is hardly likely that we will find several distinct, complete, whole societies only a generation or two removed from the parent society and occupying the same habitat. Actually, total distinctiveness is not only empirically a will o' the wisp, it is not theoretically necessary. Diffusion between daughter societies need not be ruled out, since each of the social units would, in the case of diffusion, be acting simply as a part of the cultural environment of the others; and each's d-culture would, hypothetically, be controlling its own borrowing.

Likewise, it is not necessary that each of the social units be a complete society, i. e., that it provide for all the biological, social, and cultural needs of its members. For example, several bands of a single tribe would meet our criteria well enough. Each band is a social unit, presumably being a sub-unit of the tribe on an equal basis with every other band. Although a part of the cultural life of the people occurs in inter-band interactions, still a large part of it occurs strictly intra-band, For example, day-to-day subsistence behavior,

behavior toward one's co-bandsmen and women, self-curing practices, and so forth can be looked at from the perspective of each band separately.

Furthermore, much of general socialization and enculturation occurs at the band level, so that many attitudes toward extra-band situations, along with the behaviors expressive of these attitudes, are learned as part of band-culture, not as part of tribe-culture. On the other hand, the practices learned at tribal initiation rites cannot be considered as band-culture, although band-cultural differences might help to explain differences in, say, the interpretation an individual would give these practices.

So a series of bands might serve as daughter-societies, in a sense, with their daughterhood being re-established in every generation by band exogamy coupled with tribal endogamy. Assuming, then, that the bands are not ranked or specialized within the tribe, that they occupy similar habitats, and that they have the same degree of contact with the same alien peoples —to the extent that we can assume all these things, to that extent we can expect each band to go through precisely the same sequence of micro-changes in x-culture if there is a d-culture and that d-culture is an FIW.<sup>14</sup>

To put the above in more specific, practical terms: if we have separate ethnographers living with the several bands, and noting their cultural changes over several years, we would expect to find similarities between their sequences of cultural changes significantly

greater than chance alone would explain. If we so find, then we proceed to claim that the differences in the sequences were due to such imponderables as accident, habitat-differences, differences in alien cultures in contact and, perhaps, minor differences in the nature of the d-cultural FIW due to earlier accidents, habitat-differences, etc.

It should be noted at this point that on the one hand, we don' t claim that the micro-changes will occur simultaneously; a certain change may occur in one band a whole generation before it occurs in another. We argue only that if a culture is an FIW, then if changes occur in a certain sequence in one social unit they will occur in the same sequence in every other social unit bearing that culture.

On the other hand, our basic hypothesis gives us no grounds for predicting which of the social units will be farthest along in the sequence, which the second, third, etc. Presumably this will be determined by extrinsic factors —which is the first unit to be struck by an external change-causer: a change in habitat due to drought, a change in cultural environment (and habitat, perhaps) due to contact with Europeans, etc. Another factor might be differences in intensity of contact with regularly-present aliens.<sup>15</sup>

#### G. The household as the social unit

It is only a short leap from bands of the same tribe as social units to households of the same village as social units. Again, certain

behaviors occur only in a household context, and households are generally the main enculturative milieux, so that differences in many basic attitudes may be traced to differences in household cultures.<sup>16</sup>

Since households are generally even smaller than bands, however, they are subject to an important kind of what might be called "cultural drift." If a "normal" household for a given society includes occupiers of the statuses of, say, Father and Husband, Mother and Wife, and Child, a household that doesn't have bodies to occupy those statuses is going to be behaviorally defective, not on cultural grounds but on habitat, in this case demographic, grounds. Similarly, if the same person who is Father and Husband is "normally" also Head of Household, but it happens that the wife's mother is present and is the owner of the homestead, the Head's behavioral inventory may be rendered defective; or, putting it another way, the roles of status Head may be split among two people.

What we must try to do, then, is select households with similar demographic characteristics for comparison. Ideally, they should not differ in social class within the village, nor should they differ in occupational specialty, particularly if available specialties include home industries involving more than just the Head.

As for access to resources, i. e., differences in the physical part of habitat, it should be possible to show that differential use of resources is due to cultural preferences and not to extrinsic causes.

This may, of course, be extremely difficult to determine in an actual case.

It cannot be emphasized too strongly, however, that any errors we make in determining similarity will have the effect of reducing the likelihood of getting a Natural Order, not of increasing it , because differences in habitat, as we stated on pages 8 and 28, are "essentially unpredictable" ; in other words, nothing we do in selecting our households can lead to bogus confirmation of our hypothesis, but only to mistaken rejection of it.

One way to improve the probability of getting similar cultures is to use households that are linked by kinship ties --again, a literal use of the "daughter" metaphor. If the ties of marriage and kinship are fairly close, the likelihood of class differences between households is somewhat diminished, for instance.

So, now we can select a matched sample of households from a village, assign a field worker to each, and note microchange over the years. If we get similarity of sequences, we declare the basic hypothesis confirmed; we then throw out or explain away the variations from the general pattern, write down the general pattern, and begin to use it as a source of hunches for a general theory of micro-cultural change. After several such studies, hopefully, we can elevate the hunches that work out to the level of hypotheses, and subject each to theoretical and empirical test.

## H. Studying cultural change synchronically

Unfortunately, we are still talking in terms of ideal field situations; we have not yet finished making concessions to the practical. We do not have many field workers (we have two); we do not have a generation or two to work (we have eighteen months). The first of these limitations means that we cannot observe each member of every household under every relevant stimulus; instead, we must simulate a series of relevant stimuli, and record simulated responses, and also record artifactual responses such as house types and furnishings.

The second limitation is even more confining. Eighteen months is a minimum of behavior-time; it cannot encompass more than an instant of cultural time. This means, in the first place, that only a tiny bit of cultural change could take place in any one household during the course of the study, and such a change would be very difficult to separate from random variations in responses. In the second place, the second limitation may well make inter-household comparison impossible, since it could very well be the case that no two households are within one and one-half years of each other in their common culture-change sequence.

These limitations in time and personnel create a further difficulty; namely, it is impossible to denote adequately the cultural environment, to list every behavior-trait that might be observed, at one time or another, by members of the households being studied. Indeed, it may

well be that to do this would be impossible even with unlimited time and personnel, when dealing with a population that has access to mass-media: books, newspapers, radio, motion pictures, and television.

In short, we cannot specify exactly either the base-line behavioral inventory nor the sources available for borrowing. All we can do is record some of the differences in behavioral inventory between households at one point in time. We cannot specify with certainty, on any a priori basis, whether a difference with respect to any particular trait is due to a gain by those who have the trait or a loss by those who lack it.

Now, is it still possible, with all these limitations and difficulties, to identify and specify a Natural Order of Cultural Adoption and Loss, if there is one, and thus to confirm the general hypothesis that a d-culture is a functionally integrated whole (FIW)?

Further, and more important, is it possible to invalidate that hypothesis; i. e., is the general hypothesis corrigible in the light of the limitations listed? The answer is yes, and the means of validation lies in the prediction of the pattern of distribution of the behavioral traits throughout the households. The way this works is that each household, if the hypothesis is correct and a Natural Order exists, will be located by its most recent cultural change somewhere along that Natural Order; thus there will be an ordered sequence of households ranging from the most progressive to the most conservative.

When we have completed our behavioral inventories of all of the households, we will have a list of behavior-traits, e. g., "Trait i. . . Wife and Mother, when asked question #25, ' would you allow your teen-aged daughter to go to the movies with a young man?' replies ' only if I know his whereabouts.' " Each of these behavior-traits is, in either its affirmative or negative form, a predicate of every household, and can be so listed. For example, using "1" for affirmative and "0" for negative, and letting capital letters stand for the names of households:

	A	B	C	D	E
trait i	1	0	1	1	0

Now, if the general hypothesis holds, we know that A B C D E cannot be the correct order of households. Suppose trait i is a trait being borrowed by the culture borne by A, B, C, D, and E. In that case, B and E must be conservative and A, C, and D progressive, and a correctly ordered list must group them as such, e. g.:

	B	E	A	C	D
trait i	0	0	1	1	1

We have reordered the households according to the distribution of the trait. Now we examine the distribution of another trait, j . Our original list shows

	A	B	C	D	E
trait i	1	0	1	1	0
trait j	1	0	0	1	0

and our reordered list shows

	B	E	A	C	D
trait i	0	0	1	1	1
trait j	0	0	1	0	1

Again, we see that the households are not in the correct order: households A and C must be reordered

	B	E	C	A	D
trait i	0	0	1	1	1
trait j	0	0	0	1	1

Suppose our next trait, k, has distribution

	A	B	C	D	E
trait k	0	0	0	0	1

which we now reorder as

	B	E	C	A	D
trait i	0	0	1	1	1
j	0	0	0	1	1
k	0	1	0	0	0

Again, we must reorder and we can reorder, but this time we see that if i and j are traits being borrowed, k must be a trait being lost:

	E	B	C	A	D
trait i	0	0	1	1	1
j	0	0	0	1	1
k	1	0	0	0	0

If the general hypothesis were true and omnipotent, we would expect that additional traits would allow us to improve our ordering of the households, until we got a set of distributions something like this:

	E	B	C	A	D
i	0	0	1	1	1
j	0	0	0	1	1
k	1	0	0	0	0
l	1	1	1	1	0
m	1	1	0	0	0
n	0	0	0	0	1
o	1	1	1	0	0
p	0	1	1	1	1

These distributions have one thing in common: under household order EBCAD, in every one of them, either all 1' s are before all 0' s, or all 1' s are after all 0' s. Further, of all the possible household orders, this condition obtains only under E B C A D or its exact opposite D A C B E; any other household-order would cause 1' s and 0' s to be mixed together for at least one trait (actually two traits). D is either the most conservative household or the most progressive (we cannot tell which on the basis of the synchronic distributions alone), A the second most conservative or progressive, and so on through E.<sup>17</sup>

For purposes of exposition, let us stipulate that D represents the progressive end of the scale. From this it follows that traits with 1' s to the right of 0' s are traits being borrowed, and that trait n , which is held by only the most progressive household, is the latest adoption. Similarly k , which is held by only the conservative E , is the earliest loss. Likewise p , held by everyone but the conservative E , is the earliest adoption; and so on. We can thus reorder the traits :

	E	B	C	A	D	
k	1	0	0	0	0	
p	0	1	1	1	1	change one
m	1	1	0	0	0	
i	0	0	1	1	1	change two
o	1	1	1	0	0	
j	0	0	0	1	1	change three
l	1	1	1	1	0	
n	0	0	0	0	1	change four

There, then, is our Natural Order of Cultural Adoption and Loss for the culture borne by households A, B, C, D, and E

1. k lost, p adopted
2. m lost, i adopted
3. o lost, j adopted
4. l lost, n adopted

(At this point, it would be well to note that more than one behavior-trait might have a given pattern of distribution over the households.

(It would also be well to state emphatically that nothing here implies that culture change is a simple matter of the substitution of one culture-trait for another. On the contrary: If two of the households happened to be separated by just one moment of cultural time, we would find a difference between them of either an adopted trait or a lost trait, but not both. And it is perfectly possible for several successive moments of cultural time to be taken up by adoptions only or by losses only.)

One point to be remembered here is that, while we can't know which end of the sequence is earlier, we can know the sequence. In

this example it is either the one shown above or its exact opposite, namely

1. l adopted, n lost
2. o adopted, j lost
3. m adopted, i lost
4. k adopted, p lost

and no other. In practice, determining the direction of change is easy, using historical materials, obvious linguistic borrowings, individuals' recollections of changes, etc.

So we have demonstrated that a synchronic study can be used to identify and specify a Natural Order of Cultural Adoption and Loss.

#### I. Testing the hypothesis synchronically

We now turn to the more important question of corrigibility; can the hypothesis that x-cultural change will show a Natural Order of Cultural Adoption and Loss, and thus the general hypothesis that a d-culture is an FIW, be invalidated by synchronic methods?

For purposes of exposition, let us suppose that we are still claiming that the general hypothesis is not only true but omnipotent. If we make such a claim we can be easily confuted empirically. Suppose, for instance, that in our example above we found not only the eight trait-distributions presented but also a ninth:

	A	B	C	D	E
q	1	1	0	0	0

Put in with the other eight on the chart on page 40, it would appear thus:

	E	B	C	A	D	
q	0	1	0	1	0	

Here is a distribution that cannot be reconciled for, if we reorder the households to suit that distribution, some other trait or traits would show a scrambled distribution.<sup>18</sup> Thus, we would have to admit that we have a behavior-trait that is being borrowed or lost out of order. This means that unpredictable changes are occurring in the x-culture; a certain change has occurred which has been followed in some cases by this one, and in some cases not. But we have shown that our general hypothesis entails a predictable Natural Order of change. Hence we must abandon our claim to both truth and omnipotence. So, of course, we abandon the claim to omnipotence and retain the claim to truth.

We might next swing to the opposite extreme and state that we will continue to assert our hypothesis if we are able to find any order at all, in other words we will abandon it only if the distributions of traits seem to occur completely at random.

There are several cogent arguments against taking this stand; I will mention two of them. First, if the effect of culture-as-FIW on culture change is so weak that we have to be satisfied with bare detection of it, it would seem a waste of time to pursue its study, since

there are so many other factors that are known to have relatively strong effects on culture change. It would be much more useful to turn our attention to these more salient factors, and leave cultural integration to the more poetically inclined among us.

Secondly, there are many minor functional relationships among different behaviors that have little or nothing to do with culture-as-FIW. Some of these relationships are pretty obvious: no one who lacks electricity will have an electric refrigerator, in all probability; others may be less obvious. But all would tend to show up in an analysis like that proposed and thus perhaps give a bogus indication of order, or at least woefully obscure the nature of a weak FIW-effect; which, in turn, would make inferences from the empirical order to the nature of the d-cultural FIW extremely hazardous, to say the least.

Ergo, we must try to find a way of judging whether or not our hypothesis is worth going on with, and we must look for it between the extremes of too difficult and too easy a test.

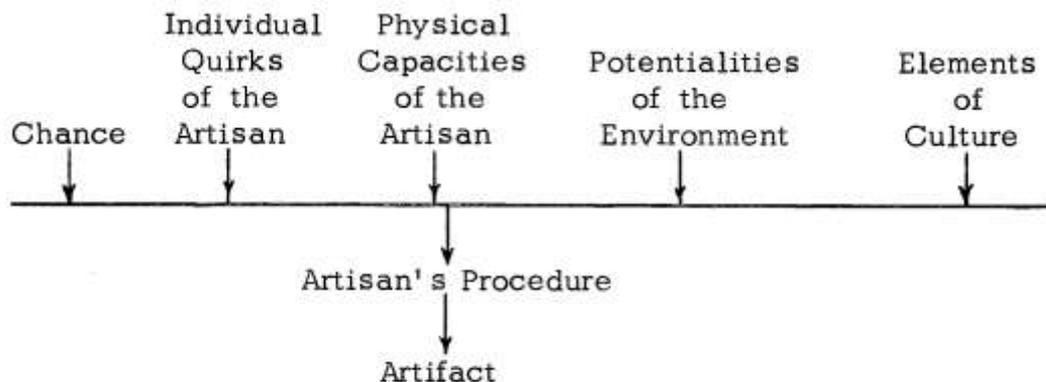
A middle ground, here, requires us to develop and utilize some quantitative techniques. When we have our data arrayed, we must arrange it into the best possible pattern. We must arrange our households in the way that yields the most better distributions and the fewest worse distributions. Then we must ask, and answer, the question "What is the probability of getting a pattern this close to the ideal, or closer, by chance alone?"

To do this, we will have to invent a measure of closeness to the ideal, and then calculate the distribution of the values of that measure over all the possible patterns, from the best to the worst. The probability-figure we seek can then be found by dividing the number of closer and equally close patterns by the total number of possible patterns.

## NOTES FOR CHAPTER II

<sup>1</sup>Although he does not use the same terms, Rouse's analysis of the various factors entering into an artisan's behavior, and hence into an artifact, parallels the Kluckhohn-Kelly analysis very closely:

To speak in physical terms, elements of culture are analogous to forces which react upon the individual artisan, causing him to behave in a certain way, i. e., to make artifacts of certain kinds. They are not the only forces which react upon the artisan and affect his procedure, however. Chance, peculiar elements of his personality, differences in his innate ability, and environment all may have affected the artisan and moulded his procedure. In fact, the procedure itself may be considered to have been an equilibrium, into which all these factors were able for a moment to enter. The artifact is the result, somewhat of a fixation, of the equilibrium. These relationships may be diagrammed as follows:



(Rouse 1939: 17-18)

<sup>2</sup>In view of the known eclecticism of cultures, the problems inherent in a theory of diffusion are mainly the mechanical ones of time, space, and means of transport of materials or ideas. It is recognized that the nature of the traits and the attitudes of the donor and recipient groups also enter importantly.

into the successful transmission of cultural elements,  
(Rands and Riley 1958: 274)

Cf. also Eggan:

. . . the larger organizations or configurations of culture may well be limited to a considerable extent in terms of types. We do know that these patternings or organizations have an important influence on innovation and borrowing and hence on cultural change. (Eggan 1955a: 493)

Social structures are no more subject to ready borrowing than are linguistic structures, whereas cultural elements, like linguistic vocabulary, may be more easily taken over. The patterning or configurational aspects of culture may offer parallel resistances to borrowing, as Benedict has demonstrated. (Eggan 1955a: 493-4)

Murdock, and Eggan again, emphasize how a portion of the d-culture, namely social structure, may resist, and thus influence selection in, borrowing:

A people borrows from its neighbors only what its cultural base is prepared to accept and, among such elements, only what its members have reason to feel will satisfy their wants better than existing practices, and, among such, only the elements which actually prove, after trial, more satisfying under the enviroing conditions. In actual fact, the presence of other peoples with differing cultures in the vicinity is reacted to as is any other aspect of the environment, as a source to be selectively drawn upon for innovations which may bring superior adaptation.

(Murdock 1960: 185)

Obviously borrowing has taken place on a large scale throughout the Pueblos — and beyond as well — in terms not only of technology and material culture but of ideas, attitudes, and concepts as well. But in the field of social structure as with linguistic structure the borrowing of patterns of organization is relatively rare, except where there is close and intimate contact accompanied by intermarriage or bilingualism. The borrowing of clan names is possible, but the essentials of a clan system are difficult to grasp; matriloca residence can be comprehended readily, but the principle of grouping on a

lineage basis is not easily formulated; kinship terms can be borrowed, but the system of organization is largely unconscious. (Eggan 1950: 319)

<sup>3</sup>This is, of course, a very common idea. Divers authors have offered divers schemes for understanding the nature of the underlying patterns of culture; e. g., Benedict's and Mead's "configuration," Opler's "themes," Hoebel's "postulates" :

Nor are the basic postulates in a culture necessarily perfectly consistent among themselves. However, the measure of consistency between basic postulates, and between the postulates and the specific selected behavior patterns, will be the measure of integration of the culture. Integration, it is assumed, must be held above a minimal level if any society is to continue to function effectively. Integration is achieved by selection in accordance with a body of basic postulates that are not overly inconsistent with each other. Every society maintains a social system.

As a result, the behavior of the individual members of any society (or of subgroups within a society) reveal considerable similarity in response to specific stimuli. These frequently recurring patterns we call norms or ways. (Hoebel 1954: 14)

Note also how Eggan contrasts social integration and cultural integration:

The concept of integration likewise has a somewhat different meaning with reference to social structure and to culture. Social structure may differ in terms of the number of individuals bound together and in the character and complexity of the ties uniting them with one another. Cultural integration, on the other hand, refers to the degree to which beliefs, attitudes, and behavior patterns are mutually adjusted and form a system without contradictions or loose ends. This process takes place in the individual but may be generalized for the group. Social integration and cultural integration not only are measured by different criteria but can be measured independently of each other, (Eggan 1955a: 494)

See also the definitions of ' culture' and the following discussion under "Structural" in Kroeber and Kluckhohn (1952/63: 118-24); Kroeber's

discussion of "Integration —Inherent or Induced" (1948: Section 122); Oliver' s section on "Function and Structure" (1964: 41-2); and Benedict' s well-known Patterns of Culture (1934/46).

<sup>4</sup>Cf. Kroeber and Kluckhohn on Sapir:

As Sapir showed for language, there are "configurational pressures" which bring about both parallel and differentiating changes. Every particular cultural structure through its emphases, its tendencies toward disequilibrium in certain sectors, its lack of development in particular areas, favors evolution in some directions and not in others. And as Sapir further pointed out, "it is more than doubtful if the gradual unfolding of social patterns tends indefinitely to be controlled by function." (Kroeber and Kluckhohn 1952a: 183)

Hoebel makes this more explicit, in the framework of his "postulates":

Once a culture gets under way there are always some criteria of choice that govern or influence selection. These criteria are the broadly generalized propositions held by the members of a society as to the nature of things and as to what is qualitatively desirable and undesirable. We prefer to call these basic propositions "postulates." [Others, Hoebel points out, call them themes or values.]

New patterns are accepted, rejected, or modified with reference to the basic postulates. New patterns may be ideologically rejected as incompatible with the pre-existing postulates, but once present they may persist though "officially" banned, and in time may influence a change in the postulates themselves so that ultimately their general acceptance becomes possible. Or they may be found acceptable in terms of some of their aspects and without awareness of the potential implications of other aspects that are incompatible with existing postulates. Then when such implications are finally realized, the behavior may be so well established as to cause a modification or even overthrow of the original postulate. (Hoebel 1954: 13)

<sup>5</sup>For the learnability of culture, see the definitions in Kroeber and Kluckhohn 1952/63, pp. 111-14; for the traditional nature of culture, see the definitions on pp. 89-94.

<sup>6</sup>In other words, an FIW is a "K-Gestalt, a system of functional interdependence" (Madden 1953: 561).

As we have seen, Koehler stresses interaction in the sense that a change in one element causes alteration in all areas of a system. This is essentially what is meant by saying that a K-Gestalt is a system of functional interdependence.

(Madden 1953: 566)

<sup>7</sup>For example, a chemical reaction, as expressed in a formula, is an FIW. Knowing the nature of electron bonds, etc., and knowing what elements have been put into the reaction, in what order and under what conditions, the chemist can predict the sequences of chemical changes that will occur; provided that no events extraneous to that formula occur which affect the course of the reaction. Such extraneous events could range from unsuspected impurities in the input chemicals to, say, an earthquake destroying the laboratory.

<sup>8</sup>This is not meant to imply that there is necessarily a one-to-one relation between changes in the d-culture and changes in the x-culture. To put that another way, it does not necessarily follow that the terms used to describe changes in the d-culture will all be interpretable into terms describing changes in the x-culture. To use a chemical metaphor, terms such as "electron bonds," "atom," etc., are not interpretable into terms describing what the chemist sees when he looks into his test-tube or measures the acidity of its content at different stages in an experiment.

<sup>9</sup>Gardin suggests essentially the same basis for working with the raw materials of archaeology:

Archeological data can indeed best be described by varying combinations of elementary terms to which unique and stable meanings can be assigned. These meanings conform sufficiently to the pan-human universalities of sense perception so that — with the aid of drawings — speakers of different languages will nevertheless assign the same features to the same categories.

(Cardin 1958: 338-9)

<sup>10</sup> When systematically gathered comparative data are being used, this process is referred to as coding. Cf. Rouse' s procedure for coding attributes into modes (1939: 26-27).

<sup>11</sup>Cf. Linton 1945: 55-82. Our analysis here has been inspired by Linton' s discussion of status and role. We use ' status' in precisely the sense he uses it, and the set of behaviors defining a status corresponds closely to his notion of ' role.'

<sup>12</sup>For more on the problem of the logical status of propensity, see Hempel' s discussion of the problem of disposition terms (1952: 24-29).

<sup>13</sup>Cf. Kroeber and Kluckhohn' s discussion:

Uniformities. Most anthropologists would agree that no constant elemental units like atoms, cells, or genes have as yet been satisfactorily established within culture in general. Many would insist that within one aspect of culture, namely language, such constant elemental units have been isolated: phonemes, and morphemes. It is arguable whether such units are, in principle, discoverable in sectors of culture less automatic than speech and less closely tied (in some ways) to biological fact. We shall present both sides of this argument, for on this one point we ourselves are not in complete agreement.

One of us feels that it is highly unlikely that any such constant elemental units will be discovered. Their place is on lower, more basic levels of organization of phenomena. Here and there suggestions have been ventured that there are such basic elements: the culture trait, for instance, or the small community of face-to-face relations. But no such hints have been systematically developed by their proponents, let alone accepted by others. Culture traits can obviously be divided and subdivided and resubdivided at will, according to occasion or need. Or, for that matter, they are often combined into larger complexes which are still treatable, in ad hoc situations, as unitary traits, and are in fact ordinarily spoken of as traits in such situations. (1952: 162-3)

<sup>14</sup>This use of sub-units of a single society is clearly carrying Eggan's "Method of Controlled Comparison" to its logical conclusion:

My own preference is for the utilization of the comparative method on a smaller scale and with as much control over the frame of comparison as it is possible to secure. It has seemed natural to utilize regions of relatively homogeneous culture or to work within social or cultural types and further to control the ecological factors so far as it is possible to do so. Above all, it is important to control the historical framework within which comparison takes place. While I share Radcliffe-Brown's vision of an ultimate science of society, I think that we first have to cultivate more intensively what Merton has called the "middle range" of theory, and I suggest the method of controlled comparison as a convenient instrument for its exploration, utilizing covariation and correlation and avoiding too great a degree of abstraction. (1955a: 499)

See also, of course, Eggan 1954.

<sup>15</sup>Before the present study was undertaken, a pilot analysis was performed on the data from the California Cultural Element Survey of the Northern Paiute, by Omer Stewart (193-). The eleven bands of the Paiute were found to serve quite well as social units for this kind of analysis.

<sup>16</sup>Cf. Levi-Strauss:

It seems that both the reality and the autonomy of the concept of culture could better be validated if culture were, from an operational point of view, treated in the same way as the geneticist and demographer do for the closely allied concept of "isolate." What is called a "culture" is a fragment of humanity which, from the point of view of the research at hand and of the scale on which it is being carried out, presents, in relation to the rest of humanity, significant discontinuities. If our aim is to ascertain significant discontinuities between, let us say, North America and Europe, then we are dealing with two different cultures; but should we become concerned with significant discontinuities between New York and Chicago, we would be allowed to speak of these two groups as different cultural

"units." Since these discontinuities can be reduced to invariants, which is the goal of structural analysis, one sees that culture may, at the same time, correspond to an objective reality and be a function of the kind of research undertaken. Accordingly, the same set of individuals may be considered to be parts of many different cultural contexts: universal, continental, national, provincial, parochial, etc., as well as familial, professional, confessional, political, etc. This is true as a limit; however, anthropologists usually reserve the term "culture" to designate a group of discontinuities which has significance on several of these levels at the same time. That it can never be valid for all levels does not prevent the concept of "culture" from being as fundamental for the anthropologist as that of "isolate" for the demographer.

(Levi-Strauss 1953: 535-6)

<sup>17</sup>In our claim #5 (pages 2-3 above) we mentioned that we would be constructing a "Guttman-type" scale. If the items in the chart on page 40 are reordered properly, a perfect Guttman scale appears, thus:

	E	B	C	A	D
n	0	0	0	0	1
j	0	0	0	1	1
i	0	0	1	1	1
p	0	1	1	1	1
l	1	1	1	1	0
o	1	1	1	0	0
m	1	1	0	0	0
k	1	0	0	0	0

The reader is advised that the work leading up to this dissertation was inspired, in part, by the idea of the Guttman scale. The writer's M.A. thesis (1961) included the use of the Guttman scale as a measure of "acculturation status" ; but even then I realized the exciting possibility that a Guttman scale, applied to cultural materials, might be seen as a static representation of culture change through time.

Robert L. Carneiro got the same idea at about the same time (personal communication, 1962), and showed how Guttman scale technique could be applied to the study of macro-cultural change; i. e.,

of cultural evolution in the historical sense (Carneiro 1962).

In 1963, Ward H. Goodenough published an article in which he discussed and criticized Carneiro's applications, suggesting that Guttman analysis could be used to show whether there is, indeed, a single path of cultural macroevolution, all other changes being essentially random, or whether the presence of non-scale types would reveal that there are several paths. (At about that time I was working on Stewart's Paiute materials [see above, footnote 15]. Using data of the Cultural Element Survey type strikes me as being somewhere in between Carneiro's interests and those of the present study — perhaps we could refer to it as the study of mesoevolution.)

In the same article, Goodenough applied a scale to data collected from a single Trukese informant, as a device for ordering normative statements to determine whether these lie along one or more dimensions. In this he follows Pauline Mahar Kolenda's study of ritual distance among castes in India (Mahar 1959, cited by Goodenough [1963: 243]). Both his and Mahar's studies use the method for the same purposes Guttman originally did (1944).

Paul Kay followed this lead in analyzing Tahitian consumer behavior (1964), claiming as his contribution the rather nice distinction between attitudinal (Goodenough) and behavioral (his own) data,

Meanwhile Carneiro has been working on carrying out his program of studying macroevolution, with the help of a student (personal communication); and James W. Bosch, a graduate student at Stanford University, is independently replicating my 1961 study, using data he collected from Navaho (personal communications, 1964-65).

The present work presents many striking contrasts to the work of others (and my own earlier work) cited above. So many and so striking are these contrasts, in fact, that I do not regard this work as "an application of Guttman scale analysis" at all. To use anthropological

terminology, the form of my ideal pattern is the same as that of a Guttman scale, but the function is quite different.

The Guttman scale was originally designed, and has been used by all the above writers except Carneiro, to establish dimensions of empirical measurement. We ask, "Is ethnic prejudice a single dimension in Anglo-American culture?" Then we apply Guttman techniques to several sets of expressed attitudes which we have decided, a priori, fall into the general category of ethnic prejudice. (We ask a series of respondents, "Would you want a member of your family to marry an Italian? a Jew? a Negro? a Turk?" ; "Would you want an Italian [Jew, etc.] to move into your town? Your neighborhood? Your block? Next door?" ; and so forth.) On the basis of the scalability ("reproducibility") of the data, we decide whether or not ethnic prejudice is a single dimension, empirically. If it is, we have a measure of ethnic prejudice, and thus we can, for example, seek the causes of ethnic prejudice, using scale scores as the dependent variable and such things as income, education, rural-urban differences, etc., as independent variables.

Because the data are selected, however, it is impossible to work out a probability distribution for the measure used to decide whether a scale actually exists or not; we can't say, for example, "If the answers to our questions were unrelated, we would expect to get a scale with a Coefficient of Reproducibility of .93 or better only 5% of the time"; in other words, we cannot specify a confidence level for our statement that a single dimension, ethnic prejudice, exists.

The present effort is not an attempt to develop a measure of anything; it is, rather, an attempt to test the general notion that a culture, any culture, is in toto a functionally integrated whole. (Goodenough points out that Carneiro's work is an effort to test the general notion that cultural evolution is unilineal [1963: 246] .) For this reason we do not have to select our data; every culture-trait should scale, not just

those we have labelled 'ethnic prejudice' traits, or those we have labelled 'consumer traits,' etc. While we cannot claim that we have been able to list all the culture-traits (or to sample them), we are able to list several thousand. Further, we do so without any preconceived notions of which traits are being lost, which being gained, or in what order; our mechanical procedure will sort these out as we go. Thus a probability distribution exists, and we feel we can use random techniques to estimate where our result lies in that distribution. From this estimation we can state our confidence in our hypothesis in mathematical terms.

There are two less important differences between our method of analysis and the standard Guttman method. First, we use a different measure of "scalability"; this measure, based on the Mann-Whitney "U" (1947), is more precise than Guttman's Coefficient of Reproducibility, in that it not only counts errors, it weighs them. Second, we throw out all universal traits and all traits not present in any household. Such traits are known a priori to be errorless, and thus tend to make the data appear more scalable than it actually is, whatever the measure.

So we must conclude that while the Guttman-scale form is present, the function is very different; and that the methods introduced here are quite different, both in general and in detail.

<sup>18</sup>As a matter of fact, no distributions other than the original eight can be reconciled with them, and there are  $2^n = 2^5 = 32$  possible distributions, where  $n$  = number of households.

In the actual study being reported here, there are 28 households. There can thus be  $2(n-1) = 2(27) = 54$  perfectly compatible distributions out of  $2^n = 2^{28} =$  approximately 268.5 million possible distributions.

CHAPTER III  
METHOD: ARRANGING THE DATA AND  
TESTING THE GENERAL HYPOTHESIS

A. Introduction

In this chapter we will discuss

1. the technique for processing the data to reveal the Natural Order of Cultural Adoption and Loss, if it exists, and
2. the technique for evaluating the processed data in order to test the hypothesis that the Natural Order does indeed exist.

In the remainder of this section we will describe these two techniques in general terms. Then, in the following two sections, we will treat each of them in detail, describing computer procedures and giving statistical rationales.

When the data have been gathered and have been coded in terms of presences and absences of behavior-traits (IIC and IIH, above), they must be processed in a computer to form a matrix as closely approximating the ideal pattern (II I, above) as possible.<sup>1</sup> This matrix will be referred to from now on as "the best possible matrix."

We start with the raw data, arranged in the form of a binary matrix. In this matrix the households are listed across the top in some arbitrary

order (e.g., in alphabetical order by surname of husband-father), and the behavior-traits are listed down the left-hand side in the order in which they were elicited. In every cell in the matrix there is either a 1 or a 0 , denoting whether or not the household represented by that vertical column exhibited the trait represented by that horizontal row.

Transforming the original data-matrix into the best possible matrix is a matter of rearranging the columns, i. e. , the households. If a Natural Order of Cultural Adoption and Loss exists, each household will be located at a certain point in that Natural Order; that is, it will have adopted and lost traits up to a certain point in the order, and its next adoptions and/or losses will be predictable, once the Natural Order is known. Thus the households can be ranked, according to their position in the Natural Order, from the most conservative (those which have undergone the fewest changes) through the most progressive (those which have undergone the most changes).

Let us suppose for a moment that we are dealing only with traits being adopted. In that case, determining the order of the households will be quite easy —the household having the most adopted traits will be the most progressive, that with the second most traits the second most progressive, etc. Next, let us suppose that we have two lists, one of traits being adopted and one of traits being lost. Again, ranking the households will be easy, but this time we count both the number of traits-being-adopted which a household possesses and the number of traits-being-lost which it lacks.

But now let us turn to the actual situation we will be confronted with: we will have both traits being adopted and traits being lost, in a single list; we won't know whether any given trait on that list is being adopted or is being lost. Of course, if we had our households in the correct rank order, we could easily determine the adoption-or-loss status of each trait: If a trait predominated among households at the progressive end of the list we would know that it is being adopted; if it predominated at the conservative end, we would know that it is being lost. But of course we don't have the correct rank order of households; that is what we are trying to determine. So we appear to be in a circular impasse: We need to know the rank order of households to establish the adoption-or-loss statuses of the traits, and we need to know the adoption-or-loss statuses of the traits in order to establish the rank order of the households.

We can break out of this impasse, and determine simultaneously both the rank order of the households and the adoption-or-loss statuses of the traits, by using what are known to computer scientists as iterative procedures. We can start out with an arbitrary (e.g., alphabetical) ranking of the households, and determine which traits would be traits being adopted, and which would be traits being lost, if that were the correct ranking of households.

Now we ask, given that assessment of the adoption-or-loss statuses of the various traits, what is now the correct ranking of

households? That is, what household appears to have undergone the most adoptions and losses, the second most, . . . , the fewest? The answer to that question, of course, is in the form of a new rank order of households.

We compare this new rank order with the previous one. If they are different, we discard the old rank order, and tentatively adopt the new rank order. Then, on the basis of this new rank order, we re-assess the adoption-or-loss status of each trait and, again, determine a new tentative rank order of households. And so we continue until we arrive at a tentative rank order which coincides exactly with the previous rank order. Then we stop. We have now found the rank order of households, and the sorting of traits by adoption-or-loss status, which yields the best possible matrix. We have done so by using a set of mechanical procedures to detect a structure in our raw data; this process is directly analogous to that used in factor analysis, latent structure analysis, and cluster analysis.

At this point it would be well to discuss more fully the idea of "best possible matrix." We defined this expression above as the "matrix as closely approximating the ideal pattern as possible." The ideal pattern, of course, is one in which every single row has all its Ps at one end and all its 0's at the other end, with no interpenetration whatever. In our best possible matrix there will be many rows which are not perfect, i.e., which have 1's and 0's interpenetrating. An

imperfection, or error, in a row represents, e.g., a case where a household has adopted a trait out of order, say prematurely; the farther out of order the adoption is, the more serious the error.

Let us imagine an imperfect row in a matrix. Obviously, we can correct the errors in that row by reordering the columns. But that means reordering the columns for all the rows. If we have the best possible matrix, reordering the columns will always produce more (and/or more serious) errors than it corrects. This is, in fact, an operational definition of "best possible matrix."

In our discussion of the use of iterative procedures to construct the best possible matrix, we spoke as if the procedures would ultimately determine which traits were being adopted and which lost. This is not exactly true. When the procedure is finished, the traits will have been sorted into two groups, one of traits being borrowed and one of traits being lost. But the problem is that the procedures will not tell us which group is which. By the same token, the households will be correctly ordered, but the procedures will not tell us which end of the ranking represents the most conservative household and which the most progressive. The reason for this is that we started with an arbitrary ranking; if we had started with the same arbitrary ranking, but had reversed it, we would have ended up with the same final ranking, but reversed; and, of course, the adoption-or-loss statuses of all the traits would have been reversed.

So we must use some means, extrinsic to the raw data and the mechanical procedures described, to decide which end of the household ranking is which. Suppose, to take an analogy, we were given a list of thirty Americans, ranked according to their annual incomes, but we weren't told which end of the list had the higher incomes. It would not be difficult for us to make the determination by observing the dress and speech of a half dozen individuals taken from opposite ends of the list.

Similarly, if our processed matrix really does represent a Natural Order of Cultural Adoption and Loss through time, we should have no trouble identifying the conservative and progressive ends of our ranked order of households. For instance, it will probably be observed that the rank-ordering of the households correlates roughly with a ranking by age of husband-father; in that case, the older households presumably are the more conservative.<sup>2</sup>

Another way of solving this problem is to look at the patterns of distribution of certain traits which are known historically to be being adopted or lost by the community as a whole. It would be rather obvious among American Indians, for example, that ability to speak the native language is being lost and ability to speak English is being adopted.

If we want to anticipate the which-end-is-which problem and have an excellent chance of solving it before we begin our iterative procedures, we can do so. Instead of using an arbitrary list of households

at the start, we can make up a ranking which we feel will approximate the final ranking; for example, we could list the households in descending order of age of husband-father, left to right. In all probability, then, the final household ranking will put the most conservative household on the left. Using a tentative ranking of this sort will have another advantage; namely, it will reduce the number of iterations required, and thus will save computer time.

When the data processing is completed, and we have the best possible matrix our original data-matrix can be made to yield, we can examine each row in order to determine its cutting-point. The cutting-point of a row is the place where, as one reads across, 0's end and 1's begin, or vice-versa. In cultural terms, the cutting-point separates the household which most recently changed with respect to the trait in question from the household which will be next to change. The position of the cutting-point of a trait, then, enables us to determine the position of that trait in the Natural Order of Cultural Adoption and Loss. Traits whose cutting-points are to the left of its cutting-point are adopted or lost before it; traits whose cutting-points are to the right of its cutting-point are adopted or lost after it. Thus to list the traits in the order of their cutting-points is to list their Natural Order of Cultural Adoption and Loss.

The next question to be dealt with is that of evaluating the best possible matrix to determine whether it is close enough to perfection so

that we can conclude that a Natural Order exists and, thus, accept the general hypothesis that the cultural change we are observing is under the control of an FIW.

The problem and our attempted solution will be mentioned here only briefly; we will go into greater detail in section C. In our best possible matrix there will be a certain number of errors of varying degrees of seriousness; that is to say, 1's and 0's will be out of place, i.e., they will interpenetrate in some of the rows. If we could calculate how many matrices of the same general type as ours there are, and how many of those matrices could be processed into matrices not less perfect than our best possible matrix, we could divide the latter figure by the former and get an answer in the form of a decimal fraction between 0 and 1000. That fraction would represent the probability of getting a data matrix not less perfect than ours by sheer chance — say by flipping a coin to determine the contents of each cell. If that probability is low, we can conclude that we have a Natural Order of Cultural Adoption and Loss, and accept the hypothesis of an FIW.

Unfortunately, however, there is no method existing at present for calculating the numerator of the probability-fraction. For this reason, we will resort to what computer scientists call "empirical techniques"; we will generate a number of small matrices at random, process them, and compare the results with those obtained by processing a sample of small matrices drawn at random from our empirical matrix. From this

comparison we will be able to make a rough estimate of the probability we seek.

#### B. Processing the data-matrix: computer aspects

In order to save computer time and attempt to insure that we will end up with the progressives on the right (and the conservatives on the left), we make up a tentative order of households, assigning the household we suspect will be the most conservative the number 1 and giving successive numbers to the second most conservative, etc. The  $n^{\text{th}}$  household, then, is our candidate for most progressive.

The computer then examines the distribution of each of the  $m$  traits in accordance with the initial ranking, and determines for each trait whether that trait would be being lost or adopted, if the initial ranking of households was correct.

The method we chose for doing this was to calculate a rank-biserial correlation coefficient.<sup>3</sup> If the 1's and 0's in the row are balanced, or distributed evenly over the ranked columns, there is no correlation and the value of the coefficient is at or near 0. If the 1's tend to fall toward the upper end of the ranking, this value will be a positive decimal fraction, and if the 1's tend to fall among the lower-ranked columns, the value will be a negative decimal fraction. Thus traits with negative coefficients are deemed, tentatively, to be traits being lost, since they preponderate among the more conservative

households, and, conversely, those with positive coefficients are deemed, tentatively, to be traits being adopted.

The computer now tags each "negative trait," in order to remember which they are, and then complements it — that is, it changes all the 1's in that trait's row to 0's and all the 0's to 1's. (What it has done, from the point of view of culture, is this: It has changed the meaning of a '1'. A 1 formerly marked the presence of a behavior-trait in a household; now it notes the fact that that household has changed with respect to that trait — adopted it if it is a trait being adopted, or lost it if it is a trait being lost.)

As a result of this complementing procedure, all the rows (except those with a correlation coefficient of 0) now have 1's preponderating among the higher ranking (more progressive) households. If our tentative order of households happened to be the correct one, and if a perfect matrix were possible with our data, each and every row would now have all its 1's on the right and all its 0's on the left, with no interpenetration. Let us examine the implications of this more fully.

There are some changes which have taken place in only one household; a row representing such a change would have only one 1 in it. If we had the correct order and perfect data, the 1 in every such row would now have to be in column  $n$ . If a change has taken place in two households, the row representing it would have its two 1's in columns  $n$  and  $n-1$ . Likewise, a row representing a change that had taken

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place in all but one household would have its only 0 in column 1 . In fact, column 1 would contain nothing but 0's and column n would contain nothing but 1's (non-existent and universal traits are thrown out, because they can throw no light on culture change).

Furthermore, in a correctly-ordered perfect matrix, as the ranks of the households go from 1 to n, the number of 1's in the columns representing these households increases monotonically (going from 0 to m , the total number of traits).

Similarly, in a correctly ordered imperfect matrix, the number of 1's in the columns will increase monotonically (although it may neither begin at 0 nor reach m).

So the computer next counts the number of 1's in each column, and compares the ranking determined by the resulting column-sums with the original tentative ranking. If one or more households were not correctly ordered in the initial ranking, the column-sums will not increase monotonically as we go from column 1 to column n ; one or more decreases (drops) will be noted. In other words, the tentative initial ranking and the ranking by column-sum will be different.

Noting this discrepancy, the computer proceeds to do two things. First, it reconciles the two rankings by generating a new tentative ranking that parallels the column-sum ranking exactly. But this means that the earlier assessment of traits-being-gained and traits-being-lost will be upset, at least as far as some traits are concerned. So,

secondly, the computer recalculates each rank-biserial coefficient, tags each negatively-correlated row again and complements it, and repeats its column-sum check. And, of course, if the column-sum check again reveals a discrepancy, it continues to repeat, getting a closer correspondence each time, until the column-sum check reveals that the two rankings coincide. At that point, since recalculating the coefficients would only produce the same signs and values of them, the computer quits computing and prints out the final, best, order of households. It also can be programmed to print out the final best possible matrix, noting which traits are being lost and which gained, and giving also the last rank-biserial correlation coefficient of each row, as an indicator of which traits had a powerful influence in determining the final order of households, and which a relatively weak influence. Another useful bit of information that can be calculated and printed is the "cutting-point" of each row; that is, the point at which, if the row were perfectly correlated with the final rank-ordering, the 0's would leave off and the 1's begin. In cultural terms, the cutting point denotes the line between the household which most recently made the change and the household which we would expect to make the change next.

### C. Evaluating the best possible matrix

Now that we have organized our data into the best possible matrix, we want to determine just how good it is. We ask, is this matrix close

enough to a perfect pattern to justify going on with our analysis? Does it support our central hypothesis, that an x-culture is controlled, in part, by a d-culture which is an FIW? Putting it another way: Can we reject the null hypothesis that a matrix this good (or better) could very well have been constructed by reordering a matrix of 1's and 0's which was originally generated by some random process?

Before we can even begin to answer this question, we have to make an assumption and devise a measure.

The assumption, necessary for any situation where an estimation of probability is to be attempted, is one of randomness. We have to be able to state that there is no built-in bias in our selection of data that would result in an unjustified appearance of a low probability. (As far as the actual study here reported is concerned, the households amounted to 28 out of the 31 households which met certain standards, none of them behavioral [see below, p.113] in short practically a total sample. The stimuli for the behavioral traits were selected by various criteria, none of which had anything to do with whether they appeared likely to yield a well-integrated set of responses or not. The resulting responses were selected only from the point of view of frequency, and even then only when necessary; e.g., so as to avoid having literally hundreds of responses which occurred in only one of the 28 households.

The measure is UTOT. This is derived from the U of Mann and Whitney (1947) which, like a rank-biserial coefficient, is a measure of

whether a row of x's and y's, 1's and 0's, +'s and -'s, black balls and white balls, etc., is structured or random or where it is in between those extremes.<sup>4</sup> The value of U for a given row can be arrived at by simply counting the number of times a 1 precedes (is to the left of) a 0, or vice-versa, whichever is less. In fact, this is the definition of 'U.' The mean of U,  $\bar{U}$ , is calculated from the number of 1's ( $n_1$ ) and the number of 0's ( $n_0$ ;  $\bar{U} = \frac{n_1 n_0}{2}$ ), and when the arrangement is random  $U = \bar{U}$ . When all the 1's follow all the 0's, or vice-versa,  $U = 0$ .

The important thing about U is that, for any  $n_1$  and  $n_0$ , the frequency distribution of U can be calculated; thus, if we know  $n_1$  and  $n_0$  we can calculate how many different arrangements of 1's and 0's will give each value of U, from 0 through  $\bar{U}$ , and thus we can figure the probability of getting a certain degree of orderliness. If we have a row of six 1's and four 0's, thus

0 1 0 0 1 1 0 1 1 1

$U = 5$ ;  $\bar{U} = \frac{n_1 n_0}{2} = \frac{6 \times 4}{2} = 12$ ; from Mann and Whitney's tables (1947: 52), we read that the probability of getting U not larger than 5, when  $n_1 = 6$  and  $n_0 = 4$ , is .086, or a little less than one in ten.

It should be pointed out immediately that the use of 1's and 0's is arbitrary. The distribution of U is exactly the same for four 1's and six 0's as for six 1's and four 0's. Furthermore, if we change all the 1's to 0's and vice-versa, in any given row, U remains precisely the same.

Thus it makes no difference to  $U$  that, in processing our matrix, we have complemented some of the rows (it does make a difference to  $U$ , of course, that we have reordered the columns). In fact, we can define  $U$  for our purposes as the number of times a 1 precedes a 0 in a row after processing. And now we further define  $UTOT$  as the sum of the  $U$ 's of all the rows or, putting it another way, as the total number of times a 1 precedes a 0 in the entire matrix.

Before we leave  $U$ , one more thing should be pointed out; namely, that if  $n_1$  or  $n_0$  is greater than 7, the distribution of  $U$  is essentially normal (Mann and Whitney 1947:51). Its standard deviation can be calculated and its probability can be figured by area-under-the-normal-curve, without calculating its actual frequency-distribution.

To summarize:  $UTOT$  represents the number of times a 1 precedes a 0 in the processed matrix. In a perfect matrix, no 1 precedes any 0, so  $UTOT = 0$ . We cannot say, however, that in the worst possible matrix all the 1's precede all the 0's, because this can never occur, due to the automatic complementing of rows where 1's predominate to the left.

Rather, we should remind ourselves that the worst possible row is one where the 1's and 0's balance perfectly, i.e., where  $U = \bar{U} = \frac{n_1 n_0}{2}$ . In theory, then, the worst possible matrix is one where each and every row has  $u = \bar{U}$ . This is academic, however, because in most cases, when the  $U$  of many of the rows approaches  $\bar{U}$ , the column-sum check will fail and the columns will be reordered to get a better matrix and a

lower value of UTOT

The outcome of all this is that first, we cannot specify what the worst possible value of UTOT will be; second, we don't know how to calculate the frequency distribution of the values of UTOT. These drawbacks are caused by the fact that we don't start with some a priori order of columns and simply calculate U for each row and add them up to get UTOT; instead, we rearrange the columns to get the lowest possible value for UTOT .<sup>5</sup>

In effect, we play the rows against one another. If enough of the rows "agree," more or less, upon an order, and the orders "proposed" by the rest of the rows are more-or-less scattered, random, unable to "agree" on an alternative order, then the rows that do have a "consensus" will carry the day.<sup>6</sup> Thus, we have loaded the dice in our favor when we come to calculate how well we've done, and we must somehow compensate for this loading to get an honest result.

The processing method is new and therefore, unfortunately, there are no existing statistical gadgets for compensating for it; and this writer has neither the time nor the training to try to develop a compensator. Computer science, however, provides us with a way out. We can use a Monte Carlo process<sup>7</sup> to estimate the distribution of UTOT over matrices which have certain characteristics in common with our empirical data matrix.

If we can use the Monte Carlo process in a straightforward manner,

we will first state the defining characteristics of the matrix; then we will use the computer to generate at random a few dozen matrices with those characteristics, process each to get the best possible matrix, and calculate the UTOT of each; and finally, by observing what proportion of the randomly obtained values of UTOT were equal to or less than the UTOT of our empirical matrix, we will estimate the probability we seek. The problem of choosing the defining characteristics of the universe is an interesting one and we will examine it here.

We are trying to estimate the distribution of UTOT over a universe of matrices, so we must define that universe before we sample it. First, of course, we limit ourselves to binary matrices, those in which there are two and only two possible occupants of each cell, because UTOT measures only such matrices. Secondly, we limit ourselves to  $m \times n$  matrices, those with the same over--all dimensions as the empirical matrix; otherwise we would have to include all sorts of bizarre matrices, e. g. ,  $1 \times 1$  ,  $1 \times 00$  ,  $00 \times 00$  , in our universe.

Although I don't know how to prove it, it doesn't seem to me that leaving the bizarre matrices out will influence the shape of the distribution of UTOT or the position of the empirical value in that distribution. To put that another way, I don't see how we could, by adding or subtracting columns or rows (households or traits), alter the probability of the resulting value of UTOT

Going on, we remember that rows with only 0's or only Ps must

be thrown out before processing. Hence these rows must be eliminated before the dimensions are determined. So, as a third step in defining our matrix-universe, we specify that every row must have at least one 1 and at least one 0 in it.

Let us continue our examination of the rows. We note that in our empirical matrix, before processing, there are so many rows with one 1, so many with two 1's, and so on. Let us refer to the number of 1's in a row as its row-type. Again, the number of rows with each row-type seems to be irrelevant to the probability of UTOT. Further, we have observed that the distribution of U for a row depends not on how many 1's there are in that row, but on the ratio of 1's to 0's or vice-versa; that is, the distribution of U for a row with  $n_1 = k$  is the same as for a row with  $n_0 = k$ . Since the processing of the matrix includes complementing some of the rows, it would seem better to define rows in the unprocessed matrix in terms not of the number of 1's, the row-type, but in terms of the number of 1's or 0's, whichever is the smaller. Let us call this characteristic the row-class of the row. Since complementing a row does not change the value of U for that row, it does not change the value of UTOT for the matrix that row is in. Thus, as a fourth step in defining our universe of matrices, it seems proper to limit it to matrices with not only the same dimensions, but with the same number of rows of each row-class.

To summarize, then, we define our universe of matrices as all

those binary matrices which are the same as our empirical matrix (before and after processing, since none of these characteristics changes in the processing), with respect to

1. over-all dimensions, and
2. the number of rows of each row-class.<sup>8</sup>

We then, as stated above, generate a random sample of these matrices, process them and get their respective UTOTs, and estimate the probability of our empirical UTOT from the distribution of these UTOT-values.

If our matrix is large, however, to generate and process the required 30 or so random matrices will take an inordinate amount of computer time. (The matrix of the actual study [ $n = 28$ ,  $m = 3666$ ], even with a fairly accurate initial ordering of the households, took more than fourteen minutes just to process. Each random matrix would take longer to process, because we would have to start with an arbitrary ordering of columns. And it would require additional time to generate each matrix.) So, instead of using the UTOT-value of the whole empirical matrix, and generating full-sized matrices to match it, we will use a different UTOT-measure and a sampling procedure.

A matrix can be sampled by taking a certain number of its columns and a certain number of its rows. For example, we could sample a matrix with dimensions  $m = 6$  x  $n = 3$  to get a matrix  $m = 3$  x  $n = 2$ :

Our ideal would be to get the distribution of the mean-UTOTs-of-sample-matrices of the universe of large matrices with the same characteristics as our empirical matrix; i.e., the universe we discussed above (p, 73-75). But to do so we would have to generate all, or a random sample, of the large matrices and then sample each of them, in turn, to get an estimate of the mean-UTOT-of-sample-matrices of each, and this would be nearly as burdensome as estimating the distribution of UTOT in the first place.

But we observe that the random generating of a matrix, large or small, boils down ultimately into a coin-flipping operation. We take each row as a unit, and flip a coin for each cell: heads it's a 1, tails it's a 0. After filling each cell in the row, we determine the class of the row (counting the 1's or 0's, whichever is less). If our quota for that class is unfilled, we then put the row into our matrix; if the quota is full, we discard the row or save it for the next matrix. (Remember again that we can complement rows without any effect on the results.)

Thus to generate a large matrix and then sample it is a very inefficient way to get a small matrix; we can get precisely the same results just as well by generating a small matrix at random. Hence to estimate the distribution of the mean-UTOT-of-sample-matrices of a sample of randomly generated large matrices, we can simply generate at random a number of such small matrices<sup>10</sup> (with the same characteristics as the sample-matrices drawn from the empirical matrix) and observe the

distribution of the UTOTs of these small matrices.

At this point we will have two sets of UTOT's of small matrices. First, we have the UTOT's of small matrices drawn at random from our large empirical matrix; we estimate the mean-UTOT-of-sample-matrices for the empirical matrix to be the same as the mean UTOT of this random sample.

Second, we have the UTOT's of small matrices constructed at random; i. e., drawn at random from the entire universe of such small matrices. We estimate the distribution of mean-UTOTs-of-sample-matrices for the whole universe of large matrices to be the same as the distribution of this random sample.

What experience we have with these distributions indicates that they are roughly normal (Clark 1953:74-7). Thus, in the case where the general hypothesis is to be confirmed, the distribution and means of the two sets of UTOT's would appear something like Figure 2.

We are now confronted with the problem of interpreting these results. Unfortunately, there are at present no conveniently available methods for doing so (Joshua Chover, personal communication, 1965).

Besides simple inspection of the curved (or histogrammed) distributions, there is a rather rough-and-ready method of estimating our desired probability. This is to assume that we have an accurate estimate of the mean-UTOT-of-sample-matrices of our empirical matrix; this is, of course, derived from the random sample of sample-matrices

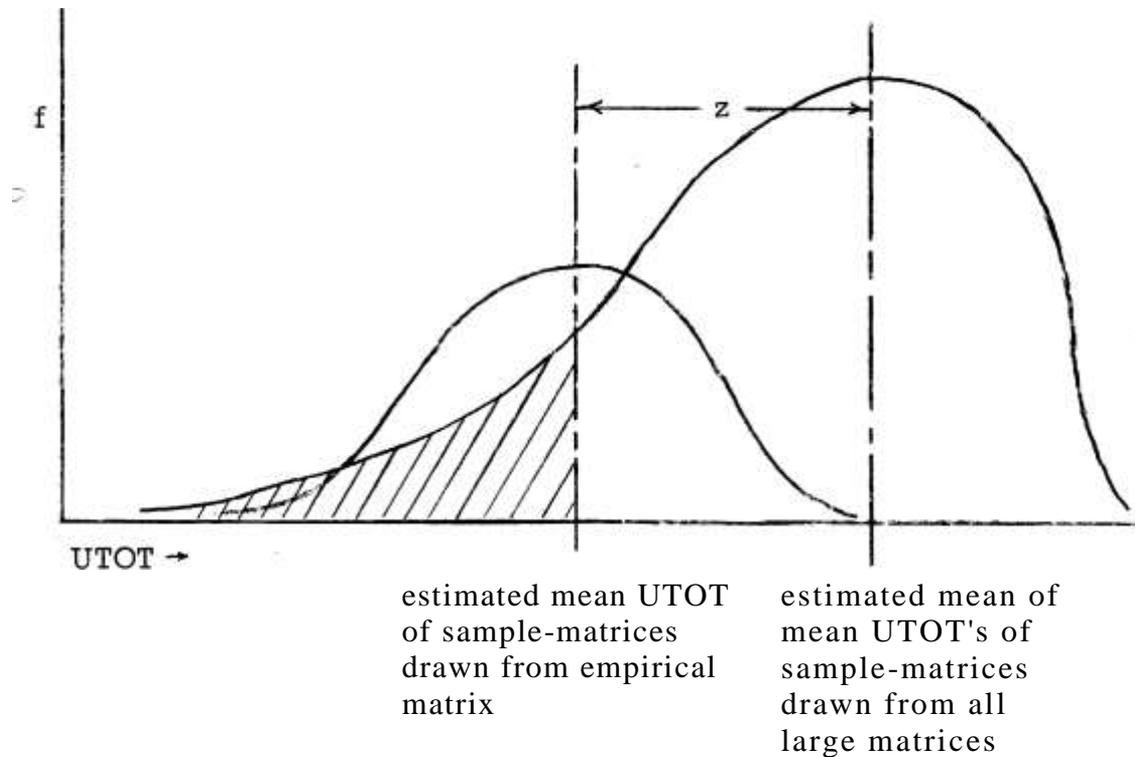


Figure II. Distributions and means of two sets of hypothetical values of UTOT

drawn from that empirical matrix. We then calculate the mean and standard deviation of the UTOT's of the small matrices generated at random. Then we ask how many standard deviations away from that mean of mean- UTOT's our mean-UTOT is; from this figure,  $z$ , we read off the area left under the normal curve (shaded area in diagram) from any standard table, and this is our required probability, very roughly.

It is of the greatest importance that improved methods for evaluating processed matrices be devised. These methods could involve an analytical method for estimating the distribution of UTOT, or empirical methods better than those just described, or perhaps an entirely

different approach, using a measure other than UTOT. Until such methods are available, however, we can work quite comfortably with those outlined here. If a great deal of subsequent analysis is to be based on an acceptance of our general hypothesis, as it is, we should expect confirmation of that hypothesis to be unequivocal and obvious by even the crudest methods.

### NOTES FOR CHAPTER III

<sup>1</sup>The program used to reorder the data was written for me by Mr. Robert Schacht, an undergraduate student at the University of Wisconsin.

<sup>2</sup>This presumption is based on the fact that the older persons will have been enculturated at an earlier period, and will thus be more likely to have learned behaviors now being lost, and less likely to have learned behaviors now being adopted, than younger persons.

<sup>3</sup>The coefficient used was  $r_{rb}$  developed by Gene V. Glass of the Experimental Design Laboratory, Department of Educational Psychology, University of Wisconsin (Glass 1965). The formula for

$r_{rb}$  is:

$$r_{rb} = \frac{2}{n_0} \left( \bar{Y}_1 - \frac{n+1}{2} \right) \quad \text{OR} \quad r_{rb} = \frac{2}{n_1} \left( \frac{n+1}{2} - \bar{Y}_0 \right)$$

where

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<sup>4</sup>U and  $r_{rb}$  are relatively straightforward functions of one another. <sup>5</sup>I

don't know how to go about proving that the process of determining the final order of columns is at the same time a process for ensuring the lowest possible value of UTOT. I think, however, that the following observation gives strong intuitive support to that assertion:

The quickest way to calculate UTOT for a given processed empirical matrix E is this. First, calculate the column-sums for the

ideal matrix I with the same row-types as E. (By this we mean that I has the same width [n] as E, that it has the same number of rows with  $n_1 = 1$  as E, the same number of rows with  $n_1 = 2$  as E, and so forth up to the same number of rows with  $n_1 = n - 1$ .) Arrange these column-sums of I in a row, and place the analogous column-sums of E two spaces beneath each. For example ( $n = 5, m = 15$ ):

column-sums of I	0	3	8	12	15
	(space)				
	(space)				
column-sums of E	4	5	7	10	12

Start at the right. Subtract the column-sum of column n of E from that of I, writing this figure in the customary Place. Now move to column n - 1 (col. 4 in the example) and add the figure just obtained to the column-sum of I; then subtract the column-sum of E as before. Repeat until all columns have been done. To continue the example:

column-sums of I	0	3	8	12	15
	4	6	5	3	
	4	9	13	15	
column-sums of E	4	5	7	10	12
	0	4	6	5	3

Now add up the figures in the bottom row, the results of the successive subtractions. The result of this addition is UTOT for matrix E. (In the example,  $UTOT = 18$ .)

It would seem then, that any other ordering of E would produce a higher value of UTOT; and, since the ordering of E was done in the first place by ranking the column-sums and UTOT is calculated by means of the column-sums, it would seem that the column-reordering method will always produce the lowest possible UTOT.

<sup>6</sup>It might be pointed out here that the rows don't carry equal voice in this decision. The number of votes a row has depends, in effect, on the relative number of 1's and 0's in it. This is because a row with only one 1 or 0 in it has a maximum value of U when

$U = \bar{U} = \frac{n_1(1)}{2}$  or  $\frac{1(n_0)}{2}$  (or the next lower integer if those figures include fractions). For example, if  $n = 28$ ,  $\bar{U} = \frac{27(1)}{2} = 13.5 = 13$  (the next lower integer). Such a row, then — one with a relative imbalance of 1's and 0's — can't have too much weight in determining the value of UTOT. On the other hand, a row with  $n_1$  and  $n_0$  equal, or nearly so, carries a great deal of weight. If  $n_1 = n_0$ , then  $\bar{U} = \frac{n_1^2}{2}$ . If  $n = 28$ ,  $\bar{U} = \frac{14(14)}{2} = 98$ ; one of these has, in effect,  $7\frac{1}{2}$  times as much weight in determining UTOT as has a row with  $n_1 = 1$  or  $n_0 = 1$ .

This same relative weighting of rows applies in the ordering of the columns by means of the process described above. When the column-sums are being added up to check and alter the column order, a row with  $n_1$  close to  $n_0$  can make differences, for better or worse, between many more pairs of adjacent columns than can a row with only a few 1's or a few 0's. That this same weighting applies in the column-ordering process and in the calculation of UTOT should come as no surprise (see Note 5).

<sup>7</sup>When a statistical procedure is so complicated that its properties cannot readily be discovered by analytic methods, empirical methods can be used. Random numbers are used to construct hypothetical data that conform with the statistical model. The statistical procedure in question is applied to many sets of such data, to provide estimates of the reliability of the results. With a digital computer to do the routine labor, the procedure can be tested in many different situations to give a comprehensive picture of its properties.

Computations based on random numbers are called Monte Carlo computations, as a tribute to the patriarch of random processes, the roulette wheel. (Green 1963:155-156.)

<sup>8</sup>A digression: In theory, it would be possible to use empirical computer procedures to figure the exact probability of our value of UTOT. To do this, we would generate and evaluate not a random sample of the universe of matrices with the same characteristics as the empirical matrix, but the whole universe.

This is a practical impossibility, of course, because if the matrix is of any size there are literally billions of matrices with those characteristics. This number could be reduced somewhat if we remember that the positioning of the rows is irrelevant; i.e., if two matrices have identical rows they are the same matrix. Since there are  $m!$  possible arrangements of rows, we could reduce the size of the universe by that factor; except that when two or more rows within a matrix are identical we would have to multiply again by the number of possible arrangements of those identical rows, since the identity of those matrices has already been taken into account, so to speak.

While these observations are at present academic, they might be useful later on to someone attempting to develop, by analytical methods, a way of calculating the distribution of UTOT .

<sup>9</sup>It would be well to have the characteristics of the sample-matrices bear some proportional relation to those of the empirical matrix; to take a metaphor, the small matrix should be a stratified sample of the large matrix. This can be done by establishing analogues between the row-classes of the large matrix and those of the small matrices. (In the present study, for example,  $nn = 28$  and  $n = 12$ ; we chose to make large-matrix row-classes 1 and 2 analogous to small-matrix row-class 1 ; 3 and 4 to 2; 5 and 6 to 3; 7 and 8 to 4; 9, 10 , and 11 to 5; and 12, 13 , and 14 to 6. ) Then the number of rows of each row-class in the sample-matrix is made proportional to the number of rows in the analogous row-class in the large matrix. (In the present study the ratio was 1/10.)

<sup>10</sup>The techniques for drawing the two samples the sample of small sample-matrices from the large empirical matrix and the sample of like matrices from the whole universe of small matrices —will not be discussed here. They are relatively straightforward computer

techniques involving, on the one hand, putting the empirical matrix into storage, sampling the rows and columns at random, and processing the resulting matrix and calculating UTOT ; and, on the other hand, generating all the possible rows for a small matrix, ordering these according to row-class, drawing the specified number of rows at random from each class and processing and calculating.

## CHAPTER IV

### A FIELD TEST OF THE HYPOTHESIS IN ENTERPRISE, TRINIDAD

#### A. Introduction

In Chapter II we concluded that households in a single village would be suitable social groups to compare for the purpose of detecting a Natural Order of Cultural Adoption and Loss. Earlier in that chapter we referred to the social group whose culture was under study by the general term "social group y," and invariably included it as part of the subject in representative sentences of cultural description, so: "the culture of social group y includes [predicate: behavior trait + cultural time]."

The reader may legitimately inquire whether or not there is anything to be said about social group y other than the totality of these cultural predicates. Strictly speaking, the answer to this question is no; that is to say, in order to detect and describe the Natural Order of Cultural Adoption and Loss we can use only behavior traits. In other words, we use as our realm of observation not social group y but the x-culture of social group y.

On the other hand, when the time comes to try to understand the Natural Order, that is, to construct an explanation of it in theoretical

terms, it will be very useful to know certain facts, not strictly cultural, about the social group whose culture is under study. Who are they, i.e., do they have an identity, a name? Where are they? Where did they come from? What do they do for a living? How are they related to one another? With bearers of what other cultures are they in contact? How frequent and how intense is that contact?

Of course, none of these questions is entirely culture-free. What people do for a living, to use the most obvious example, is partly determined by what they know how to do and what they are willing to do, as well as by factors external to their culture such as availability of jobs of various kinds, availability of agricultural land and other capital requirements, world economic conditions, and so forth. Similar cultural factors are at work in the situations to which the other questions are directed. The point being made here is that these are questions about the people, about their history, about external factors that may be determining their cultural environment, their habitat, and the rate of their cultural change. The purpose of this chapter, then, is to describe the social and historical background of the households selected for field study.

## B. Social description and selection of the study population

The island of Trinidad, in the West Indies, was chosen as the place to look for a site for this study. Trinidad was chosen because

of its well-known multi-cultural character, its social pluralism, and because there appear to be no intense pressures for acculturation by any of the socio-cultural groups which comprise its population (Crowley 1957, Klass 1960). Specifically, I planned to use the methods described in Chapter II to study acculturative change between Trinidadian Creoles (i.e., Negroes), and Trinidadian Indians (the descendants of indentured laborers brought from India). Since two studies which had been recently made described the acculturation (or lack thereof) of Trinidadian Indians toward Creole culture (Klass 1961, Niehoff and Niehoff 1960), I planned to concentrate on the acculturation of Creoles toward Indian culture. For this purpose I looked for a predominantly Creole village in a predominantly Indian area. The Director of Community Development advised me that County Caroni, the "sugar belt" of Trinidad, had the highest proportion of relatively unacculturated Indians in Trinidad.' At his suggestion I asked the County Caroni Community Development Officer to suggest a predominantly Creole village, and thus I arrived in Enterprise, Chaguanas Ward, County of Caroni.

My first contact in the village was the secretary of the Village Council. He confirmed my information that the village was predominantly Creole. He also showed me where the Council considered the boundaries of the village to be. On the basis of this information I made a map of the village, showing every house.

Enterprise is by no means an isolated village. It is situated on the Southern Main Road and thus, until a by-pass route (the Princess Margaret Highway) was completed in 1953, all road traffic between Port-of-Spain and San Fernando, the two largest cities of Trinidad, perforce passed up or down the main street of the village. There has been regular bus service through the village since the 1920's, and dozens of route taxis ply the Southern Main Road. The taxi fare to "town" (Port-of-Spain) is 35 ¢ W. I., about 21¢ U. S. The fare to Chaguanas, the market-center three miles down the road from Enterprise, is 15¢ W. I. These prices make it possible for any Enterprisean with a steady job to commute to work in the city.

When the village map was complete, I went over it with a series of informants, making up a household register. This household register shows, for each household:

1. its location (address, keyed to a map)
2. the name of its head, if known<sup>2</sup>
3. its racial character (Negro, Indian, Mixed Negro-Indian, etc.)
4. its composition (number of adults of each sex, older children, school children, and infants)
5. length of time the household (or its members) has been in Enterprise and/or its environs, and where the adults lived previously
6. occupations and regularity of employment of adults and older children.

Meanwhile, I began constructing a genealogical chart for the village. I began with an old lady, a resident of the village all her life, as Ego. When the limits of her considerable recollection and current information were reached, I extended the chart by using her most distant relatives<sup>3</sup> as informants. In the course of the genealogical work I was able to fill some of the gaps in the household register. The genealogical chart and the household register were keyed to each other.

At this point I began to realize that the nucleus of the village, the "old livers," belong to a rather limited number of related family lines, many of which were founded around the turn of the century by immigrants into Trinidad from the Lesser Antilles, principally from St. Vincent.<sup>4</sup>

After the genealogical chart had been extended as far as possible, I filled in the gaps in the household register by visiting the hundred-odd Creole households on which information was lacking.<sup>5</sup> These visits turned up a half dozen or so additional fairly old Vincentian households, unrelated to the others; but in the main they confirmed that at least a third of the present Creole households are recent arrivals in the area, and that Creole households outside the one large loose genealogical network already mentioned are almost entirely unrelated to each other. In other words, there is only one genealogical network in the village of any size at all; it is a network of older families,

primarily; it contains about one third of the present Creole households (ca. 120); and the village's households of Vincentian extraction are vastly over-represented in it.

In most cases, it appeared that the Creole newcomers were using Enterprise as a dormitory, spending most of their waking hours elsewhere; the refrain heard again and again was, "I doesn't belong to here, you know." Many of the newcomers have fled the slummier sections and suburbs of Port-of-Spain, to build rough houses on cheaply rented land well away from the main road and the village center (the intersection of the Main Road and the railway line, see the map). Most of them either are of Trinidadian grandparentage or else are very recent immigrants from Grenada. There are also a few middle-class Creoles who have built rather substantial houses in Enterprise simply because they want to live in the country; in the United States we would call them exurbanites. They, too, use Enterprise simply as a dormitory.

When the procedures outlined above were completed, the household register could be summarized as shown in Table 1.

The next task was to select the population of households for study. The prime criterion for this selection was that the households should be, as far as possible, bearers of the same cultural tradition.

First, then, I eliminated the non-Creole households from consideration. Next, I eliminated the short-term residents; first, because

TABLE 1

## Summary of the Household Register

---

Non-Creole Households:		
Indian		141
Mixed Indian-Creole		21
Other ethnic categories		<u>4</u>
Total Non-Creole		166
Creole Households:		
Short-term residents (less than 10 years)	94	
Long-term residents:		
Non-Vincentian:		
Trinidadian only	46	
Other small island (at least one adult of at least 50% non- Vincentian small island ancestry)	48	
Total Non-Vincentian	94	
Vincentian (at least one adult of at least 50% Vincentian a n c e s t r y )	<u>79</u>	
Total long-term residents	<u>173</u>	
Total Creole known and classified		267
Creole, unclassified (provenance and/or length of residence not ascertained)		<u>97</u>
Total Creole		364
GRAND TOTAL		530

---

they are of very heterogeneous origin, and second, because I felt that they have not been in the area long enough to have developed any definite observable patterns of adoption and loss with respect to the local cultural environment.

Turning, then, to the long-term residents, I examined the information I had on the households of Trinidadian ancestry (meaning that neither adult had more than one grandparent from abroad). It soon became apparent that while a dozen or so Trinidadian families have deep roots in the village, the vast majority are first-generation Enterprisers, most of them included in the long-term resident category solely because of the arbitrary 10-year cutoff point. Furthermore, they or their immediate ancestors have lived in practically every part of Trinidad, and thus can be supposed to have been subject to a very heterogeneous set of cultural influences prior to moving to the Enterprise area. Finally, very few of these households are related to each other; when they are, it is almost always by a distant affinal relationship established through one or more Vincentian households, in the main genealogical network.

An examination of the information on the other-small-island households showed almost exactly the same conditions existing as for the Trinidadian households.

In the case of the Vincentian households, however, one can more closely approximate the ideal of a single common cultural heritage.

First, every Vincentian household has, by definition, from one-fourth to all of its immediate ancestors coming from the small island of St. Vincent; further, almost all of them can trace their roots to one area of St. Vincent, the vicinity of Georgetown. Second, many of the Vincentian households are directly or indirectly related in the main genealogical network, and when they are indirectly related it is usually through yet another Vincentian household. Third, even when a Vincentian household is not related to the main Vincentian group, they are generally in Enterprise because the main group is there; that is, individuals, while still in St. Vincent, learned about Enterprise as a place where many Vincentians lived, and came to Enterprise to be with their own people.<sup>6</sup> Fourth, and closely connected with the last fact, the Enterprisean Vincentians, unlike many other-small-islanders, came directly to Enterprise from St. Vincent, without stopping for several years or for a generation in Port-of-Spain or its suburbs. Fifth, some of the Vincentian households in the kinship network have deep roots in the village, going back to 1898, when the village had only five houses.

For all these reasons, then, I determined to make the Vincentian households the subjects of my study.

For our convenience in the following sections, the household register summary can now be restated thus:

Indian	141
Mixed Indian-Creole	21
Other ethnic	4
Non-Vincentian Creole	285
Vincentian Creole	<u>79</u>
Total	530

### C. Similarities and differences among the ethnic categories in Enterprise

Continuing our description of the village and its population, we will now discuss several aspects of ethnic differentiation, comparing and contrasting the Indian, Non-Vincentian Creole, and Vincentian groups or, more properly speaking, ethnic categories. We will also characterize briefly the relations between these categories.

#### 1. Occupations and employment

Table 2 indicates roughly the rates of employment of the three ethnic categories.

For households for which employment information is available, then, the frequency of total unemployment for the three categories is virtually identical.

In many of the households there is more than one person employed; indeed, some individuals have more than one occupation, often a full-time and a part-time occupation. For this reason, we will not use the household as the unit for the balance of our analysis of occupations and employment; rather, we will speak of occupation-units. In other

TABLE 2

## Rates of Employment by Ethnic Category

	Indian		Non-Vincentian Creole		Vincentian	
	No.	(%)	No.	(%)	No.	(%)
Someone in household employed	88	(79)	208	(78)	60	(78)
No one in household employed (no occupation, unemployed, or pensioned)	24	(21)	60	(22)	17	(22)
Total households whose employ- ment status is known	112	(100)	268	(100)	77	(100)
Households whose employment status is unknown	29		17		2	
Totals of households	141		285		79	

words, if a Vincentian man is a full-time carpenter and he and his family tend an extensive garden, we will count this as one Vincentian building-trades unit and one Vincentian self-employed agriculture unit. The rationale for doing this, besides its convenience, is that from the cultural point of view we are looking at two sets of skills, attitudes, willingnesses, etc., just as if these two sets resided in two different men or in two different households; these are two things Vincentians do, in spite of the fact that one Vincentian participates in both.

Taking up the matter of frequency of employment, then, we find the pattern presented in Table 3.

TABLE 3

## Frequency of Employment by Ethnic Category

---

	Indian		Non-Vincentian Creole		Vincentian	
	No.	(%)	No.	(%)	No.	(%)
Units of regular employment	85	(88)	178	(81)	73	(87)
Units of intermittent employment	5	(6)	38	(17)	10	(12)
Units of seasonal employment	6	(6)	5	(2)	1	(1)
Total units <sup>2</sup>	96	(100)	221	(100)	84	(100)

---

The variations among the ethnic categories do not appear especially striking. The relatively high frequency of intermittent employment for non-Vincentian Creoles, and thus their relatively low frequency of regular employment, is the only thing unusual. This is probably largely a matter of the frequency of employment of self-employed skilled tradesmen. It is very difficult to divide such people into regular and intermittent.

Next we turn to place of employment. This is important to use because a man who works away from the village, especially if he works in Port-of-Spain or San Fernando or in the oil areas, is actually spending a major part of every day in a different cultural environment from

TABLE 4

## Place of Employment by Ethnic Category

	Indian		Non-Vincentian Creole		Vincentian	
	No.	(%)	No.	(%)	No.	(%)
Local	22	(26)	26	(15)	17	(23)
Area	53	(62)	99	(56)	33	(45)
Elsewhere	10	(12)	53	(30)	23	(32)
Totals	85	(100)	178	(100)	73	(100)

that which we had anticipated. Table 4, above, concerns only units of regular employment. "Local" refers to units of employment in Enterprise and its immediate environs. "Area" refers to the space within a radius of three miles of the village center, in effect embracing work-places which can easily be reached on foot or by bicycle; it also includes some units about which it was known only that they are outside the village. "Elsewhere" means anywhere else in Trinidad; the majority of these units are in Port-of-Spain or its suburbs (11 - 19 miles), and most of the rest are in Couva (8 miles) and points farther south (Point-a-Pierre, 15 miles, San Fernando, 18 miles).

The similarity in the frequency of Indian and Vincentian local

units, and the lower frequency for non-Vincentian Creole local units, is perhaps a reflection of the fact that many of the former have been in Enterprise and its environs for some time, while the latter includes many newcomers. More significant as an indication of cultural differences, however, is the disparity in frequency of "elsewhere" units between the Indian category, on the one hand, and both Creole categories, on the other. This can be explained by the types of occupations Indians tend to have, as we shall see below.

TABLE 5

Computation of Total Occupation Units with Kind Known

	Indian	Non-Vincentian Creole	Vincentian
Total occupation units	96	221	84
LESS units with kind of work unknown	14	48	18
Total units with kind known	82	173	66

The tabulation in Table 5, above, presents the occupation-units of the three ethnic categories according to type. Here we ignore the frequency of employment, combining regular, intermittent, and seasonal

units. We will base our frequencies on the total number of units, for each ethnic category, whose occupation can be typed; that is to say, we will leave out those occupation units where we knew where the person worked and/or how often he worked, but not what kind of work he did. The figures in the last row of Table 5, then, are used as the denominators in calculating the frequency of each type of occupation for each ethnic category in Table 6. The reason that these frequencies will total more than 100% is that some occupation-units fit into more than one type; e.g., a taxi owner-driver unit will be included in both the semi-skilled operative type and the medium-capitalization business type. Types whose frequencies vary strikingly among ethnic categories are marked with an asterisk (\*), and some of these variations will be discussed.

To summarize the subject of occupations and employment: we have found that the Vincentian category is like the other two ethnic categories in percentage of households with someone employed, in regularity of employment, and in frequency of certain types of occupation-units. We have found that the Vincentian category differs from the non-Vincentian Creole category but resembles the Indian category in its relatively high frequency of local occupation-units and in its low frequency of policeman-units. We have found that the Vincentian category differs from the Indian category but resembles the non-Vincentian Creole category in its high frequency of units of employment

TABLE 6

## Types of Occupation — Units by Ethnic Category

	Indian		Non-Vincentian Creole		Vincentian	
	No.	(%)	No.	(%)	No.	(%)
Other skilled trades (butcher, shoemaker, joiner, baker, 1. Skilled trades (self-employed, auto mechanic, welder, tailor, building trades (esp. mason, dentist, Calypsonian, black- carpenter), self-employed, smith, jockey, radio repair, welder, etc.)	1	(1)	33	(19)	13	(20)
whidwright trades, other employed	4					
TOTAL SKILLED	5	(0)	9	(5)	1	(2)
building trades, total	1	(1)*	42	(24)	14	(21)
			14	(8)	4	(6)
		(6)*	56	(32)	18	(27)
2. Semi-skilled operatives						
motor vehicle drivers, tractor, etc. <sup>8</sup>	17	(20)*	20	(12)	4	(6)*
other	0	(0)*	15	(9)	5	(8)
TOTAL SEMI-SKILLED	17	(20)	35	(22)	9	(14)
3. Unskilled labor						
canal cleaning	3		0		0	
field labor	9		3		0	
other	15		15		3	
TOTAL UNSKILLED <sup>9</sup> LABOR	27	(33)*	18	(10)	3	(5)

	Indian		Non-Vincentian Creole		Vincentian	
	No.	(%)	No.	(%)	No.	(%)
4. Unskilled light work	6		7		1	
literacy required (esp. sales)	0		6		3	
literacy not required (e.g. watchman)	<u>0</u>		<u>4</u>		<u>1</u>	
domestic service	6	( 7)	17	(10)	5	( 8)
TOTAL UNSKILLED LIGHT WORK						
5. Other occupations (not totalled)	2	( 2)	9	( 5)	3	( 5)
white collar, trained (esp. teachers)	1	( 1)	2	( 1)	3	( 5)
religious specialist	0	( 0)	9	( 5)*	1	( 2)
police	6	( 7)	12	( 7)	11	(17)*
self-employed agriculture						
6. Business:						
amount of capitalization no or low (e.g., "parlour," fish peddler)	10	(12)	14	( 8)	6	( 9)
medium (e.g., requiring shop or vehicle)	<u>1</u>	( 1)	<u>2</u>	( 1)	<u>3</u>	( 5)
heavy (bakery, house rental, light mfg.)	30	(37)*	31	(18)	12	(18)
TOTAL BUSINESS UNITS						
7. Authority & responsibility	2	( 2)	3	( 2)	8	(12)*
supervisor level (incl. builders who employ others)	<u>1</u>	( 1)	<u>8</u>	( 5)	<u>3</u>	( 5)*
foreman level	3	( 4)	11	( 6)	11	(17)*
TOTALS						

outside the area, in its high frequency of skilled trade occupation-units, in its low frequency of unskilled units, and in its relatively low frequency of business units. Finally, we have found that the Vincen-tian category differs from both of the other categories in its low fre-quency of motor vehicle operator units, its high frequency of self-employed agriculture units, and its high frequency of non-business units requiring authority and responsibility.

It is clear that some of these similarities are closely related. The fact that Indians tend to be laborers and businessmen would explain why they have such a high frequency of local units, the frequency of skilled units in the two Creole categories would explain their high frequency of distant units, etc. Some of the differences in occupation and employment reflect differing cultural preferences, and others re- flect differences in the histories of migration of the three groups.

## 2. Household composition

Because of the interest in Caribbean family structures re- cently displayed by some anthropologists,<sup>2</sup> we will summarize here the section of the household register dealing with the demographic compo- sition of the households. In organizing the data for presentation, we make a basic dichotomy between households with children and house- holds without children. Without going into a lengthy rationale for this, we will point out two things: First, the presence of children in

the world is more or less fortuitous. The problem that interests us as anthropologists concerns differences in the arrangements made by bearers of various cultures for the nurturing and enculturation of the children; for this reason we should compare households that actually have children. Second, we have in Enterprise many households of adults living alone, or in the company of another adult of the same sex (usually a relative), or in a non-mated relationship with an adult of the opposite sex (e.g., a brother and sister or a young man looking after his grandmother). The frequency of these "adults alone" households varies among ethnic categories. But this variation reflects not differences in nuclear family structure, as such, but rather differences in the institutional modes for dealing with unmated adults, particularly the aged: Do they live alone or do they retain (or acquire) membership in the relatives', especially their children's, households?

First, then, we present the households with children (Table 7).

The most striking fact here is that the so-called matricentral or matrifocal family (type c) is by no means modal among the Creole families in Enterprise. In fact its frequency for Vincentian households is slightly lower than that for Indian households. The mode for all ethnic categories is the "normal" family household, that with at least one adult of each sex present.

Table 8 shows all households, giving frequencies for several types of childless household.

TABLE 7

## Composition of Households with Children, by Ethnic Category

	Indian		Non-Vincentian Creole		Vincentian		Mixed
	No.	(%)	No.	(%)	No.	(%)	
Households with one or more children, and							
a. one adult of each sex	66	(75)	119	(69)	44	(72)	14
b. as above with additional adult(s) <sup>13</sup>	4	(5)	9	(5)	8	(13)	0
SUBTOTALS:							
"normal" family units	70	(80)	128	(74)	52	(85)	14
c. female adult(s) only	15	(17)	42	(24)	8	(13)	1
d. male adult(s) only	3	(3)	3	(2)	1	(2)	0
TOTALS:							
households with children)	88	(100)	173	(100)	61	(100)	15

TABLE 8

## Composition of All Households, by Ethnic Category

	Indian		Non-Vincentian Creole		Vincentian		Mixed
	No.	(%)	No.	(%)	No.	(%)	
All households whose composition is known							
a. households with children (from Table 7)	88	(77)	173	(62)	61	(77)	15
b. households without children							
couple only	19	(17)	51	(18)	5	(6)	3
couple + additional adult(s)		(0)	1	(0)	1	(1)	0
adults alone (not couple)	8	(7)	54	(19)	12	(15)	3
TOTALS	115	(100)	279	(100)	79	(100)	21
Households whose composition is unknown	26		6		0		0
GRAND TOTALS	141		285		79		21

There are two things which might be noted about the "couple only" type. First, the frequency of this type in Indian households seems high, as high as that for non-Vincentian Creole households. It might possibly reflect a breaking down of the Indian extended or joint family, reported by Klass (1961: 44-5, 131) and Niehoff and Niehoff (1960: 101) to be still functioning strongly in "Amity" and in Penal. If the Indian extended family were still strong in Enterprise, one would expect the married son to stay with his parents at least until a child was born to his wife.

Second, we observe that there are relatively few Vincentian households of the "couple only" type. This is perhaps because Vincentian couples generally want, and are able, to acquire unofficial foster children if they have none of their own.<sup>14</sup> The wanting is undoubtedly cultural, and may be shared with non-Vincentian Creoles; the ability is probably a result of having access to kinsmen from whom to acquire foster children.

The remaining point of note in the above table is that Indian households are low in the "adults alone" type. This suggests that, whether the Indian joint family is losing its force or not, it still has not become the custom among Enterprisean Indians for unmated adults to live alone.

### 3. Religious groupings

No formal census data were acquired on the subject of religion, so this section is based mainly on casual observation. In general, then, it would appear that

— Indians are mainly Hindus, with a tiny minority of Muslims and a slight minority of Christians. The frequency of actual practice is high, as attested by the large number of Indian households displaying prayer (puja) flags.

—Trinidadian Creoles are primarily Roman Catholic and Church of England. A few are or have been active in the African Saints (so-called "Shango") Cult.

—Non-Vincentian "small islanders" are mostly Church of England and Wesleyan Methodist.

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—Vincentian data are in the Supplement (Q. 075). See page 139. The A. M. E. Church has a church in nearby Montrose, as has an American Christian revivalist sect, whose local minister is an Indian but whose congregation is predominantly Creole. In Enterprise proper there are six Spiritual Baptist churches (one abandoned), one Wesleyan church, and one Hindu shrine. There are Roman Catholic and Anglican churches in Chaguanas (three miles south) and Cunupia (three miles north). In Enterprise there are two small and intermittently active "Shango" centers; there is an important center just outside the village.

#### 4. Inter-group relations

Attitudes of members of the Vincentian sample toward various ethnic categories, including Vincentians, Indians, Trinidadian Creoles, and other-small-islanders, are presented in detail in the Supplement (Questions 023, 024, 028, 029, 033, 037, 048 -051, etc.). The following remarks are based on observation (including participant-observation) and informal interviews, and are intended simply to delineate the social context of the attitudes.

As the map shows, there are sections of Enterprise which are predominantly Indian and sections which are predominantly Creole. (Of the latter, there are sections which are predominantly Vincentian.) There are, nevertheless, many cases of Indians and Creoles living side by side. In such cases, it is my impression that ordinary neighborly relations are carried on; that is to say, a given Creole household is as likely to be on speaking terms with their Indian neighbors on one side as with their Creole neighbors on the other. If they are on speaking terms, their members will greet each other upon meeting for the first time each day, there will be a certain amount of calling back and forth between the women as they go about their daily tasks, and there will be gossip at the property line or the water "pipe" (public hydrant) if any topic of interest should come up. It is my impression that in such gossip, public events will be dwelt upon and the private affairs of third parties will be passed over quickly, in an effort to

avoid the charge of "talking other people business." Home visiting among neighbors is not common, unless the neighbor happens to be a personal friend as well.

These neighborly relations are very brittle, and may be broken off upon the least real or imagined slight; frequently a failure to greet or return a greeting is all that is required ("We doesn't speak, you know.") Again, however, such ruptures are at least as likely to occur between Creole neighbors as between Creole and Indian neighbors.

In more formal private situations, however, ethnic differentiation is quite marked. One rarely sees Creoles at an Indian wedding or Bhagwat (Hindu religious festival) except as onlookers. Likewise, one sees only an occasional Indian at a Creole house-fete (private party), christening, wedding party, or wake. At the latter, of course, Indian neighbors and acquaintances may stop by to pay their respects, but they are not likely to stay to sing or drink or play cards.

The above observations hold mainly for non-Creolized Indians. For Indians of varying degrees of Creolization exceptions have to be made; this suggests that absence of Indians from the lives of Creoles is a voluntary thing on the part of the Indian rather than a rejection by the Creole on racial grounds. For a striking example, we once attended a christening party given by an Indian who had recently moved to Enterprise from a suburb of Port-of-Spain. The parents regarded themselves as Hindus, but had the child baptized Roman Catholic,

with Creole godparents, because they thought "it might help him in school." Both Creoles and Indians attended the party at the parents' house afterwards. One young Indian told me he was from St. James, Port-of-Spain: "I'm completely Creolized, man." At the same time, Indian ladies were enjoying themselves beating a drum, singing Indian songs, and dancing in the kitchen. My wife asked one lady where she'd learned a certain step. She replied she'd learned it from watching Indian movies in Chaguanas.

Along with an undetermined but certainly small number of households like this one, counted in the household register as "Indian," we also have the 21 households listed as "Mixed Indian and Creole." That such households exist is, of course, interesting. Whether they indicate a trend toward social and cultural amalgamation or assimilation is doubtful. In my opinion, these mixed households are simply exceptional.

There is one other cross-ethnic situation that sometimes develops, when an Indian or mixed Indian-Creole child or youth is adopted by a Creole family. The result is that the child is considered a Creole and will live his adult life with Creoles as a Creole. I know of no instances of Creole or mixed children being adopted by un-Creolized Indians; I would hazard the opinion that this would be unlikely, because Indians are much more concerned with kin-relationships than are Creoles, and would not want to raise and care for someone else's

child. The general propensity for Creoles, particularly Vincentians, to adopt children has already been invoked above as an explanation of the infrequency of childless couples among them.

In spite of the exceptions noted, it can be stated that, in general, an Indian-Creole dichotomy exists in the social structure of private life in Enterprise, and it is strong. This dichotomy is carried over into public life, as well. The Indians of the village seem to believe that the Village Council, for example, is a closed corporation, in effect a private club, run by Creoles. This seems to be true in spite of the fact that Indians have been urged to attend the Council's meetings (at which any villager may speak and vote), and in spite of the fact that one member of each of two well-known Indian families has actually participated actively in the Council's affairs, both having been elected to office. On the other hand, the Enterprise Tenants' Association, whose sole purpose is to resist the eviction of its members from certain lands in the village, includes both Creoles and Indians as active members. Its executive body is primarily Creole. The area chapter of another special-purpose group, the Trinidad Islandwide Cane Farmers' Association, is represented by a Creole, although the majority of the chapter members are Indians. Again, the Agricultural Credit Societies, formed to take advantage of a government loan program, are mixed in membership, although usually one ethnic category or the other predominates. The two sports clubs in the village, on the other

hand, are almost entirely Creole in membership, while there is a cricket club in nearby Cunupia which is entirely Indian. The steel-band is entirely Creole, while the Indian Orchestra is entirely Indian.

As far as relations between Vincentians and other Creoles are concerned, it is probably safe to say that ancestry or immigrant status is practically irrelevant. In fact, it was some weeks before I became aware that the Vincentian group, as such, existed at all. We can say that there is total social integration among the various groups making up the Creole community. One does occasionally hear the stereotype that "Bajan too intelligent" (i.e., natives of Barbados are tricky businessmen), or the stereotype that Grenadians are clannish and uncivilized, but I know of no stereotype applied to Vincentians qua Vincentians. I have heard the Spiritual Baptists attacked as being a group devoted more to their own advancement than to the common weal, and of course most Baptists are Vincentians. This kind of attack has to be understood in a certain light, however. Individuals in the village seem prone to attack any group they see as organized — Indians, the Village Council, the Baptists, etc. In fact, it almost seems a general cultural trait in Creole Enterprise to view with suspicion any organization or, for that matter, any individual, who shows many signs of success.

#### D. The Sample of Vincentian Households

The criteria for selecting a sample of households as social sub-units for comparative purposes have been spelled out in Chapter II (pp. 34 - 35). We want households that contain persons occupying each of a set of essential statuses (Husband-Father, Wife-Mother, and Child), and that are not too different in social class or occupational specialty. All this is to make it more likely that we shall get households of similar d-culture.

We have suggested that another way of increasing the likelihood of getting households of similar d-culture is to use households that are related. So in selecting the sample in Enterprise, I first turned to the genealogical chart, and selected the households there that included a mated couple and at least two children, and in which at least one of the adults was of at least 50% Vincentian ancestry. When those ran out, I chose the most Vincentian households outside the genealogical network that met the composition-criterion, until I had a total sample of 31. With the exception of a young couple who came directly from St. Vincent to Enterprise seven years ago, all the households (or their adult members separately) have been in the area ten years or more. The Wife-Mother in one household died before she could be interviewed, and two households were finally dropped from the sample when it became apparent that it would be extremely difficult to interview the Husband-Father successfully. Thus we interviewed and inventoried a total of 28 households,

The first step in the formal interviewing of the adults in each household was the taking of a brief life history, being sure to account for each year of the respondent's life.

Table 9 summarizes the life history data and some census data on the 28 households, The households are listed in the order in which they were arranged by the computer (operating on cultural data alone, not using the life history data): The most conservative households are on the left. (This same order will be followed in all subsequent presentations of data.) In the L D. number, "M" refers to the male head of the household (Husband-Father) and "F" refers to the female head (Wife-Mother). "Freq." (frequency) refers to the number of times the fact obtains in the sample of 28; each "X" stands for a presence and "0" for an absence.

"CTGPT" (cutting point) is calculated (by hand) for a few life history items that show striking agreement with the household order, This figure represents the point in the household order where, ideally, presences of the item end and absences begin, or vice versa, A "C" or a "P" in the next column indicates whether presences of the item occur at the conservative (left) or progressive (right) end of the household spectrum, respectively, Consider, for example, MLH00004. Here we see that the male heads of the most conservative six households tend to have been born before 1900, but the male heads of the other 22 households do not; hence CTGPT = 6.



Table 9, continued. Notes

ID numbers are abbreviated to include only the significant digits.

8. In other words, were not brought or sent or sent for by relatives.
27. Spent first fifteen years of his or her life in Enterprise, Longdenville, Chaguanas, or Cunupia; i. e., within three miles of Enterprise.
29. MOMO = mother's mother
9. CMI = Canadian Mission Indian; one of the many schools established in Trinidad by Canadian Presbyterian missionaries to the then still indentured Indians.
11. Standard IV is about the equivalent of the sixth grade in the U. S.
16. MECH = mechanic; PIPEFTR = pipefitter, generally in the oil fields.
17. WHLWRT = wheelwright.
- 23-4. SPRVSRY CAP = supervisory capacity.
24. %NOT EST  $\square$  = (not estate).
35. Has been independent farmer, gardener, or cocoa contractor.
- M30. WMN BFR = woman (i. e., wife or "keeper") before.

Z indicates how closely the pattern of X's and O's for the item corresponds to the rank order of households. Z is a measure based on Mann and Whitney's U, representing the distance between the actual value of U and the mean value of U ( $U$ ), and expressing this distance in standard deviations.<sup>15</sup> The probability of U being that far or farther from the mean can be determined from any table of areas under the normal curve (two-tailed test). (For example, if  $Z = 1.96$ , the probability is .05.) Some data which might have been included in Table 9, such as religious associations and some employment information, have been included instead with the cultural data in the Supplement (see page 139).

When one looks over this life history material, so arrayed, two facts stand out clearly. One is that the rank order of households is correlated quite closely with the rank order by age of male head (and female head as well), as we anticipated (p. 62). The other is that the households are a pretty heterogeneous lot, with respect to past cultural influences and habitats, in spite of our efforts. At the time the fieldwork was going on, it seemed that this was a good thing, since it would ensure that the households would be broadly scattered over the whole Natural Order; I feared too much sameness, too many responses discarded because universal or non-existent. I believe now I would have done better had I sought a group that was more isolated and more inbred, biologically and culturally, than the

Enterprise Vincentians. I think there would still have been plenty of variation; that a greater proportion of that variation would have been due to differences in position along the Natural Order of Cultural Adoption and Loss; and that thus a smaller proportion would have been due to past and present differences in cultural environment and habitat. All this is not to say that a Natural Order could not be found in the Enterprise data; it could and it was. It just wasn't as clear-cut as it might have been. My situation is analogous to that of a chemist who finds that some of the chemicals used in a critical experiment were not as pure as he had thought. The experiment worked anyway, but the results aren't as close to the predicted results as they would have been had the chemicals been purer. Or so he likes to think.

## NOTES FOR CHAPTER IV

<sup>1</sup>For a more detailed analysis of the distribution of ethnic groups, see Augelli and Taylor 1960,

<sup>2</sup>These informants, all of them young Creole men, often knew the occupant of a household only by some nickname. Only rarely could they give both surname ("title") and Christian name ("name"). I found also a certain reluctance on the part of many Creoles, older people especially, to refer to neighbors and acquaintances by name. I am quite certain that this had nothing to do with supernatural ideas of any kind, e.g., with the spirits of the dead. I believe, rather, that it reflected an aversion to getting involved in someone else's affairs, perhaps a fear of being accused of gossiping (possibly with a latent fear of retaliation through sorcery). A speaker would refer rather vaguely to "that girl livin' by the [railway] line," and only gradually would one become aware that the speaker knew the person in question very well and certainly knew her name.

<sup>3</sup>In constructing the chart, I construed the term "relatives" in a very broad sense indeed. Consanguineal links, of course, were included. Affinal links were considered to be established not only by marriage, but also merely by the fact that the man was known to recognize a child born by the woman as his own.

<sup>4</sup>One elderly informant described coming to Enterprise from St. Vincent at the age of 12, after the island was devastated by the hurricane of 1898. (Other Vincentians came after the eruption of Mt, Soufriere in 1902,) His parents had come to take up a cocoa contract with the local estate; that is, they were granted a number of acres of land and provided the cocoa-tree seedlings which they were to plant and bring to fruition, a matter of three

years or so. When the trees bore, they were paid one shilling per tree; meanwhile, they had land on which to grow a garden. They also could find jobs on the estate, supervising the Indian laborers and performing other responsible services. At the time this informant came to Enterprise there were but five houses, all clustered near the railway crossing. All were occupied by Creoles; the Indians were confined to barracks on the estate, except for a few who had completed their indentures and elected to remain in Trinidad.

It should be pointed out that no cocoa has been grown in the Enterprise area for some years. The lands have changed hands several times. The village is now flanked along its entire western edge by the cane-fields of a huge British sugar firm; the rest of the estate lands have been sold off for residential use. As one informant put it, "Nothing grows in Enterprise but house-grass." This is not, of course, strictly true; quite a few people in Enterprise have vegetable gardens, and several grow cane in small patches for sale to the British firm's factory.

<sup>5</sup>Actually, there were about 200 such households. Information already in the household register, however, showed that many of these households were unsuitable for my research because of their composition; i. e., they did not include a male head, a female head, and two or more children (see page 34). In addition, at several houses I was unable to find anyone at home despite repeated visits, and there were two outright refusals.

<sup>6</sup>The question of whether the Enterprise Vincentians have a concept of group identity as such is interesting. As we shall see in the cultural data, some of them definitely claim to prefer to deal with other Vincentians, but most do not. A few even go the other way, saying they want their child to work for a Trinidadian "as he [the child] is a Trinidadian."

Several older people referred to St. Vincent as "my country," as

did some recent immigrants among the young. This most emphatically does not mean, however, that they identify themselves with other Vincentians in Enterprise. On the contrary, in at least one case one gets the distinct impression that the informant means to say "in my country we didn't have to associate with the kind of Vincentian people we have to associate with here." In short, some of the people feel that they have been de-classed by coming to Trinidad; the discovery of this fact, late in the field work, makes us question whether our sample of households is as homogeneous with respect to social class as we had assumed.

In any case, however, it is true that many Vincentians came to Enterprise because other Vincentians —relatives, friends, or just acquaintances were there. We have several personal accounts to establish this.

<sup>7</sup>One is tempted to compare the three ethnic categories with respect to number of occupation-units per household. This would not be correct, however; our information being more complete on the Vincentian households, they would of course appear to have a higher number of units per household.

<sup>8</sup>The propensity of Indians for occupations involving motor-vehicle driving has been noted by Niehoff and Niehoff (1960: 44, 46-48). The attitude of many Vincentians is practically the opposite. (See Supplement, Questions 017 and 019; see page 139.)

<sup>9</sup>Canal cleaning and field labor are listed separately because they are regarded as especially undesirable by certain Vincentians (see Supplement, Question 015). It is very likely that most of the units where the kind of work is unknown are of the unskilled labor type. If this is true, the numbers of unskilled labor units, with their frequencies expressed as a percentage of all occupation units, known and unknown, are as follows:

Indian	Non-Vincentian Creole	Vincentian
No. (%)	No. (%)	No. (%)

As above, the asterisk indicates a type whose frequency varies strikingly among ethnic categories.

<sup>10</sup>A parlour is a small store, run by a woman or a retired man from a little shed in front of the house. Small amounts of staple items, such as ground provisions (root crops, obtained from the Chaguanas market or locally grown), rice, salt fish, and perhaps cheese are sold, in addition to candies, cigarettes, little cakes, and soft drinks. The latter two items may be home-made or manufactured.

A shop is more often a full-time proposition, involving a building constructed of hollow clay or concrete blocks, fully plastered, but still situated in front of (perhaps attached to) the owner's house. The selection of manufactured goods is greater, including clothing, cooking utensils, dishes, etc. The main distinguishing feature, however, besides size and volume of business, is that a shop has a license to sell rum and beer.

<sup>11</sup>This section overlaps the preceding (business) section with respect to but two units (one builder who has a small van, and one manufacturer who employs non-relatives). The relatively high frequency of Vincentian units here may be traditional; that is, the ancestors of several of the older Vincentian families had been supervisory personnel on estates in St. Vincent, and several of them were founded by people who had come to Trinidad for the express purpose of taking charge of planting on cocoa estates in the Enterprise region.

<sup>12</sup>Keith F. Otterbein (1965) has recently reviewed the work done in this area.

<sup>13</sup>It should be noted that in coding the data, we occasionally failed to distinguish between types a and b; that is, sometimes a household consisted, e. g., of an adult man, his wife, their child, the child's spouse, and the children of child and spouse. In such a case we put the household in type a, although a case could be made for putting it in type b. At any rate, using the subtotals for "normal" family units eliminates this problem.

<sup>14</sup>For example, while we were in Enterprise one Vincentian lady in her late seventies sent to relatives in St. Vincent for a child to mind. She wanted the child to keep her and her enfeebled husband company. The young mother in another Vincentian household remarked that she had made babies for several of her childless sisters; the stepfather of one of these children, at the child's birthday party, told a gathering of adult males how he had "begged and begged" for the child.

<sup>15</sup>For the method of calculating U and Z, see Mann and Whitney 1947, pp. 51 and 55, respectively.

## CHAPTER V

### FIELD WORK METHODS AND TECHNIQUES

In the preceding chapter we have been preoccupied with presenting certain facts about Enterprise and its inhabitants, especially its Vincentian inhabitants. In doing so, we have made reference, from time to time, to the means by which these facts were ascertained; in most cases this has been by interviewing informants as to census data, genealogical relationships, etc.

In this chapter we are going to outline briefly the general field methods we used to gather material that would aid us in making up good questions, and then discuss the preparation for, and conduct of, the cultural interviews and inventories themselves.

One of the first problems encountered by any ethnographer is that of fitting himself into the social structure of the people he is studying, in other words, of establishing a status and exhibiting the appropriate role behavior. Some anthropologists have been obliged to take on a status already existing in the society under study; e. g., by being adopted into a family as Eldest Son. This course has its advantages, in that opportunities for participant-observation are maximized. On the other hand, it may be difficult, if not impossible, to step back and observe without being observed, to "become part of the woodwork," and it may

be difficult to observe events that holders of the assigned status do not customarily observe.

In other cases, it may happen that there already exists a status for non-members of the society into which a stranger may automatically be put, and from which he may find it difficult to extricate himself fully.

To the people of Enterprise, there were several statuses into which an anthropological field worker and his family might be put. The members of the Village Council saw me in the role of White Well-wisher or Social Worker. (In Trinidad, a Social Worker is not a government official but is rather a private citizen who volunteers his time for community uplift projects.) To them I was, initially at least, an honored and learned guest whose advice was sought and who was expected to speak at meetings urging the rank and file to cooperate with the Village Council and work on community projects.'

As time went by, I tried to convert this status into that of Faithful Ordinary Member of the council; that is, I made an effort to attend all meetings, to do my bit on work projects, and to contribute my share when subscriptions were taken. This status was not congenial to me, and I soon found myself taking a more active role than proper for the status — suggesting projects, helping to plan the remodeling of the community center, drafting a new constitution, and so forth. When elections were held, I had to decline firmly a nomination as general secretary of the council; I accepted a place on the Executive Committee.

It soon became clear that, from the point of view of those familiar with council affairs, I had found an acceptable social niche; I know this because several times I was compared, not unfavorably, with certain other outsiders who had lived in the village earlier and who had been able and willing to assist the council.

It is my belief that my fitting into this status helped us enormously in building and maintaining rapport with the village as a whole, or at least with that portion of it that I wanted to learn from. As long as a person we were meeting for the first time knew that I occupied this status, and that we "gave everybody ' right' " (greeted everyone), we could count on a cordial reception and remarkably cheerful endurance of our lengthy interviews. This reception contrasts sharply with the various "White Man" treatments I almost invariably received upon casual contact with people, both Creole and Indian, who didn't know "who I was." I believe that if such a high but neutral status can be found and adopted, the ethnographer is far better off than if he develops very close friendships with a few individuals but by so doing risks the enmity of others. I say this because very often two households which were both very cordial to me were not on speaking terms with each other.

The reader may have noted the lack of any statement about an occupational status. The question of occupation did not come up as frequently as one might think, perhaps because so many villagers worked outside the village and perhaps because, for this and other reasons,

people are not defined by their occupations to the extent they are in U.S. society. When the question did arise I answered truthfully, saying that I was an anthropologist interested in the way people' s customs and ideas changed when they lived close to people different from themselves. This seemed to be an acceptable explanation, and no one ever demanded that I go farther, except that middle-class people sometimes wanted to know what was in it for me, who was paying me, and so forth. These people were satisfied when told that I was working for an advanced degree and a private foundation was supporting me; middle-class Trinidadians put a very instrumental value on scholarship.

From the start, of course, the plan was to prepare for formal data-gathering procedures —interviews and household inventories. The map-making, genealogy-taking, and census-taking outlined in the previous chapter were part of this preparation, as was the establishment of status discussed above. While these tasks were being accomplished, and after they were finished, my wife and I were gathering information and understanding necessary for the formulation of meaningful questions and for the intelligent interpretation of the responses. I used no special techniques for this, nor did I seek out certain key informants or prepare specific questions in advance. In other words, for this part of the field work I was learning to be a participant and was acting as a relatively passive observer. At a Village Council meeting I observed how others addressed the chair and then, when it seemed appropriate for me to speak,

I used the same phrasing or modified it to suit myself, always, of course, observing its effect. When someone called me into a rumshop to have a drink, I observed how he and his companions went about drinking, and then followed their lead. When I fell in with a lime (a group of people, generally young males, lounging somewhere along the main road and enjoying themselves by demonstrating the virtuosity of Trinidadian English), I gave everybody "right," and sometimes stopped to listen and perhaps chat briefly (unfortunately, it never seemed appropriate for me to join in, perhaps because my status was too similar to that of Elder).

One of the most important accomplishments of the early period was becoming more-or-less attuned to Trinidadian speech. This speech is distinct from the American varieties I am familiar with first, in the phonetic values assigned to phonemes, second, in stress and intonation patterns, and third, in semantics—in the meanings assigned to morphemes and in idioms. I learned the speech well enough to be able to understand most utterances directed at me, although I never reached the point where I could understand every utterance in an overheard conversation. As for my own utterances, I never consciously tried to adopt phonetic values or stress/intonation patterns, except when I wanted to produce a comic effect. On the other hand, I tried very hard to make the morphemic content as idiomatic as possible without seeming to make fun.

To mention some more situations in which background information

was acquired: We were often invited to fete or to sup at people's houses, where we learned about food and drink preferences and habits, variations in etiquette and seating, behavior expected of children, and, through conversation, attitudes about sports, valued and disvalued personality traits, etc. On other occasions a private chat with a man or woman would lead to his opening up and revealing attitudes which would be, to say the least, embarrassing for him if bruited about the village. We could then attempt to include a question in our schedule which might elicit the same attitude, if it were present, in a respondent in the private (or semi-private) interview.

One source of general information was the attending of various religious ceremonies, especially those of the Spiritual Baptists and the African Saints Cult people. After learning as much as possible about the practices and beliefs of these people we could then plan to test our respondents for the extent of their knowledge and possession of these practices and beliefs. We also got (we think) a certain amount of insight into the way Trinidadians and Vincentians, respectively, and Creoles in general, look at the natural and supernatural worlds.

Since several informants were building tradesmen, and since quite a few new houses were built in the village while we were there, I was able to learn a great deal about methods, techniques, and materials of building construction, both traditional and recently adopted. This learning was facilitated by actual participation in the rebuilding of the

community center. Similar information was similarly acquired in the field of automobile repair and maintenance.

Information of health, ethno-medicine, childbirth, and associated attitudes was also gathered informally, much of it by my wife. Attitudes toward doctors and hospitals became clearer as we carried sick and injured people and expectant mothers to the district medical officer and/or the government hospital in Port-of-Spain. And our understanding of the relative position of Enterprisers in the life of Trinidad as a whole was enhanced by our own contacts with government officials, doctors, educators, artists, folklorists, merchants, and others in the more urbanized and more cosmopolitan strata of Trinidadian society. Even a simple taxi-ride to town often provided a remarkable amount of grist for our mill.

The result of all this preliminary informal spade-work was that the time for constructing schedules for interviewing and inventorying seemed to appoint itself. We began to list our ideas for questions on cards, each with a more-or-less definite idea in mind —that is, an idea of what responses might be expected to each question and what the cultural significance of such response might be. These questions were supplemented by using ideas from Herskovits and Herskovits (1947). Since we were still looking for Creole borrowing of Trinidadian Indian patterns, Klass (1960) and Niehoff and Niehoff (1959) were used to augment ideas gathered from Indian informants within and without the village.

At this point my emphasis shifted from concentration on the expected responses taken singly to concern for producing a field-workable schedule, that is, a schedule that would put questions in an order that would seem natural to interviewer and respondent alike and thus would be most likely to produce a steady flow of responses. I decided to begin by censuring the household, if we had not already done so.

The schedules for the interviews proper were constructed using two general guide-lines: First, the over-all trend would be from superficial, relatively non-sensitive questions that called for a factual answer, to deeper, more sensitive questions that called for the expression of an inner state of some sort. (For example, each interview would begin with a year-by-year life history and would end with a question calling for a self-description and an evaluation of the respondent's life to date.<sup>3</sup>) Second, within a block of questions on a single topic, we would move from more open-ended, circumstantial questions to more forcing, hypothetical questions, again working from "fact" questions to opinion questions. The household inventory would follow a roughly similar set of patterns, going from the yard to the sitting-room, to the kitchen, and examining the bedrooms last of all.

The actual interviews and inventories followed the expected pattern to a remarkable degree. Although the interviews were long and tiring (as long as six hours), the data continued to come. Some people were bothered a bit by the hypothetical questions, but this in itself is a datum;

similarly, some were embarrassed by questions testing their knowledge of affairs on a larger scale than village life; still others were perhaps a bit insulted by certain questions with respect to the supernatural; but practically all came up with a thoughtful answer to every question, or, when they lied, did so obviously and/or in ways that were clearly culturally patterned. In short, most questions were, at least to some extent, meaningful to the respondents. It would not add anything to describe each interview in detail; we could make a few brief points, however. Most interviews were conducted in the respondent's home, although a few were conducted in a sort of office at my house. The first was a mistake; too often there were other people present, if not in the very room then on the other side of a thin partition. In more than one case the Wife-Mother heard (and listened to) part of the Husband-Father's interview before being interviewed herself.<sup>4</sup>

Most of the interviews with Wife-Mother, done by my wife, were accomplished at a single sitting; almost all the interviews with Husband-Father required two meetings, and a handful extended to three or more. In only one case of the latter was difficulty encountered in completing the interview; this was finally overcome by sheer persistence (I waylaid him at work).

The questions were typed on standard 5 X 8 cards, as many to a card as it would hold, and the cards were punched for a small three-ring binder, thus providing an easy way of carrying the questions and of

flipping through them as the interview proceeded. For each interview and inventory a standard schoolchild's "copybook" (like an extra-thick bluebook) was prepared, with a copy of the census-data on the household pasted on it for quick reference at appropriate points in the questioning. As each question was asked, then, the interviewer would write down its number in the copybook and then write down the response verbatim (or as nearly verbatim as possible). The household inventories were taken in much the same fashion.

As the interviewing got under way, some changes were made in the questions, usually involving rewording the question; two or three questions were dropped after the first couple of interviews. Some of these changes are noted in the discussion of the questions and responses in the Supplement (see page 139).

When the 56 interviews and the 28 household inventories were completed, we began coding the responses. In the case of the interviews, we followed the paradigm outlined in Chapter II, in that a response was seen as something characterizing a household, not an individual. Thus the interviews with Husband-Father ("male interviews") were coded separately from those with Wife-Mother ("female interviews").

The actual procedure of coding was something like this: First, all the answer-books were opened to the responses to the same question. Then, taking the booklets in alphabetical order, the responses in the first book were entered on the code sheet and 1's entered after them in

the first column, the column for that household. Then each of the other books was taken in turn; when a response in it seemed to be the same as one encountered before, a 1 was entered in the appropriate column in the row already labelled with that response. When a novel response was encountered, a new row was labelled and a 1 entered there. Each response was assigned an eight-character ID number: three letters designating the source of the response (e. g., MIN for male interview), a three-digit number representing the question, and a two-digit number representing the specific response.

Occasionally, in the course of coding we discovered gaps in our data and, since we were still in the field, we were able to revisit the respondent and fill the gaps. I feel that a more general benefit accrues from doing the coding in the field; namely, the material is still fresh, one's "ear" for cultural nuances is more in tune, one's own circumstances are more nearly the same as those of the respondents — in short, the results of the coding will be more natural, more in accord with the classifications actually pertinent to the culture-bearers themselves, than classifications devised later on from the bare descriptions and quotations alone. There were real difficulties in coding, and it is almost certain that responses were often miscoded; every time that happens, of course, the Natural Order of Cultural Adoption and Loss is further obscured. We utilized certain techniques to lessen this effect: One technique was to look for certain striking phrases that were used

practically verbatim by several respondents. For example, while many people said they grew, among other things, corn, peas, and cassava, a certain number of women began their answer to the "what do you grow in your garden?" question with "Corn, peas, cassava (falling intonation and end-of-utterance juncture)"; then they went on to name other things. So the "corn-peas-cassava formula" was coded as a single response, additional to the separate mention of each of the three vegetables.

Another technique used to lessen the randomizing effects of miscoding was the use of the "recap" response; that is, several responses, which might or might not have been coded separately, were recapitulated as a single response.<sup>5</sup> For an example, see the coded responses to the question on carnival activities (Supplement, Question 070).

Back in the U. S., the code sheets were keypunched onto Hollerith ("IBM") cards, one card for each response. The card showed the ID number of the response, a brief (35 character) description of the response, the frequency of the response, and its distribution of presences and absences over the 28 households. These cards were processed by means of the procedures explained in Chapter III. The final result of processing was the production of a new deck of cards containing the same information but with the households in rank order of descending conservativeness, with X' s rather than 1' s representing presences and with the following information also punched into the card:

1. The cutting-point for the trait; that is, the best estimate of the place in the household-order where presences of that trait end and absences begin, or vice-versa. The computer determines the cutting-point by finding the place which produces the fewest "errors", i. e., in the case of a trait being adopted, the place that produces the lowest total count of X' s to its left plus 0' s to its right.

2. An indicator of whether the trait is being adopted (A) or lost (L), if either. If X' s and 0' s are perfectly balanced, or if the cutting point is either before the most conservative household (CTGPT = 0) or after the most progressive (CTGPT = 28), a 0 is entered in this column. In such cases, the adoption-or-loss status of the trait simply cannot be determined.

3. A figure, Z, representing the number of standard deviations, away from the mean, of the value of U for that particular response. This figure, when translated into a probability by the use of a table of areas under the normal curve (two-tailed test), tells us how closely the distribution of that response corresponds to the rank order of the households, which rank order in turn has been determined by the distributions of all the responses taken as a whole. We interpret the probability associated with this figure, then, as an index of the relative importance of that particular response in the determination of the final matrix. A response whose distribution has a low probability of having occurred by chance is one which interacted well with certain other

traits in determining the reordering of the columns. Therefore it seems logical to presume that such a trait must be a good indicator of the Natural Order of Cultural Adoption and Loss, and thus will be worthy of study in any attempts we might make to develop insights into the nature of the d-cultural

The information of these final data cards has been reproduced in a Supplement to the present volume, together with the actual questions and some explanatory materials pertinent to the individual responses. This Supplement is available, in various forms, to interested readers.<sup>6</sup>

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## NOTES FOR CHAPTER V

<sup>1</sup>The expected role, then, was very similar to that of an official of the Community Development Department or other agency of the Central Government —an official from Port-of-Spain, that is, not one of the county officials. My impression is that high officials are thought to be remote and powerful and just, but ill-informed about local problems; county officials, on the other hand, are thought to be upwardly mobile and hence are suspected of holding office merely to line their own pockets. It is felt that only by appealing directly to Port-of-Spain can the villagers get their share of government largesse and justice. (All financial aid must come from the Central Government, since only the Central Government has the power to tax.)

<sup>2</sup>I don' t mean to claim that adopting such a status would result in such universally cordial relations for a local man; on the contrary, it was a very common experience for me to hear someone I met on the road "mauvais langue" my colleagues on the Village Council, many of whom had done and were doing far more for the village than I.

<sup>3</sup>For women, it was discovered that life history was a sensitive area, because of early sexual liaisons which commonly resulted in what we came to know as "sociological parthenogenesis" —the birth of a child who is raised by the girl' s mother, aunt, or grandmother. This causes little embarrassment in Creole circles, but my wife soon found that the "White Lady" was not expected to take such news calmly. Thereupon the life history was shifted to the end of the interview, and the interview opened with the gardening questions.

<sup>4</sup>An interesting methodological problem arises here: Given that we want to present each respondent with the same stimuli under the same conditions, but given that we want these stimuli to simulate natural stimuli as closely as possible, which is better: to interview every respondent in an identical but unfamiliar setting chosen by the ethnographer, or to interview each in his own home, subject to all sorts of stray and uncontrolled conditions, but in a sense the natural setting for his cultural behavior? One possible way to escape this dilemma would be to conduct the interview in an artificial environment but one designed to concentrate the informant's mind on a limited set of culturally relevant stimuli, for instance by the use of slides, films, recordings, etc., as well as the voice of the ethnographer.

<sup>5</sup>This would have an anti-random effect, however, only if an ordering is possible on other grounds. If the "recap" is mistaken, it will obscure the order just as any other "mistake" in coding or eliciting responses.

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<sup>6</sup>The Supplement consists of a six-page introduction, 80 pages of computer-printed data (like that on pages 148-157, but arranged by question-categories), and 60 pages of notes to the data, including the questions verbatim.

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The Supplement is available at cost in the following forms: (1) on microfilm; (2) Xeroxed on 8½ x 11 sheets; (3) introduction and notes, Xeroxed; data, on I. B. M. cards (two boxes). For information and prices, write to the author, c/o Institute for Research in Social \_\_\_\_\_  
 Science, University of North Carolina, Chapel Hill, N. C. 27514 .

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## CHAPTER VI

### RESULTS

After the households had been reordered, giving the processed empirical matrix on cards (see pp. 135-137), the next step was to evaluate the matrix according to the methods presented in Chapter III. The characteristics of a sample-matrix were defined in such a way as to make the sample-matrices stratified samples of large matrices (i.e., like the empirical matrix) (see note 9, Chapter III). Forty such sample-matrices were drawn at random from the empirical matrix, processed, and their values of UTOT calculated. Then 100 such sample-matrices were constructed at random, processed, and their values of UTOT calculated. The results of these two sets of operations are shown, in the form of superimposed histograms, in Figure 3. Means were calculated for each set of values of UTOT . The mean of the larger sample (the constructed sample) was used to estimate the mean value of UTOT of the universe of small matrices, and thus of the universe of samples of small matrices drawn from the universe of large matrices (see Chapter III). This mean value of UTOT was 2539, and its associated standard deviation was 114.

What we are saying, then, is that if one generated a large number of large matrices (i.e., those with the characteristics of our empirical

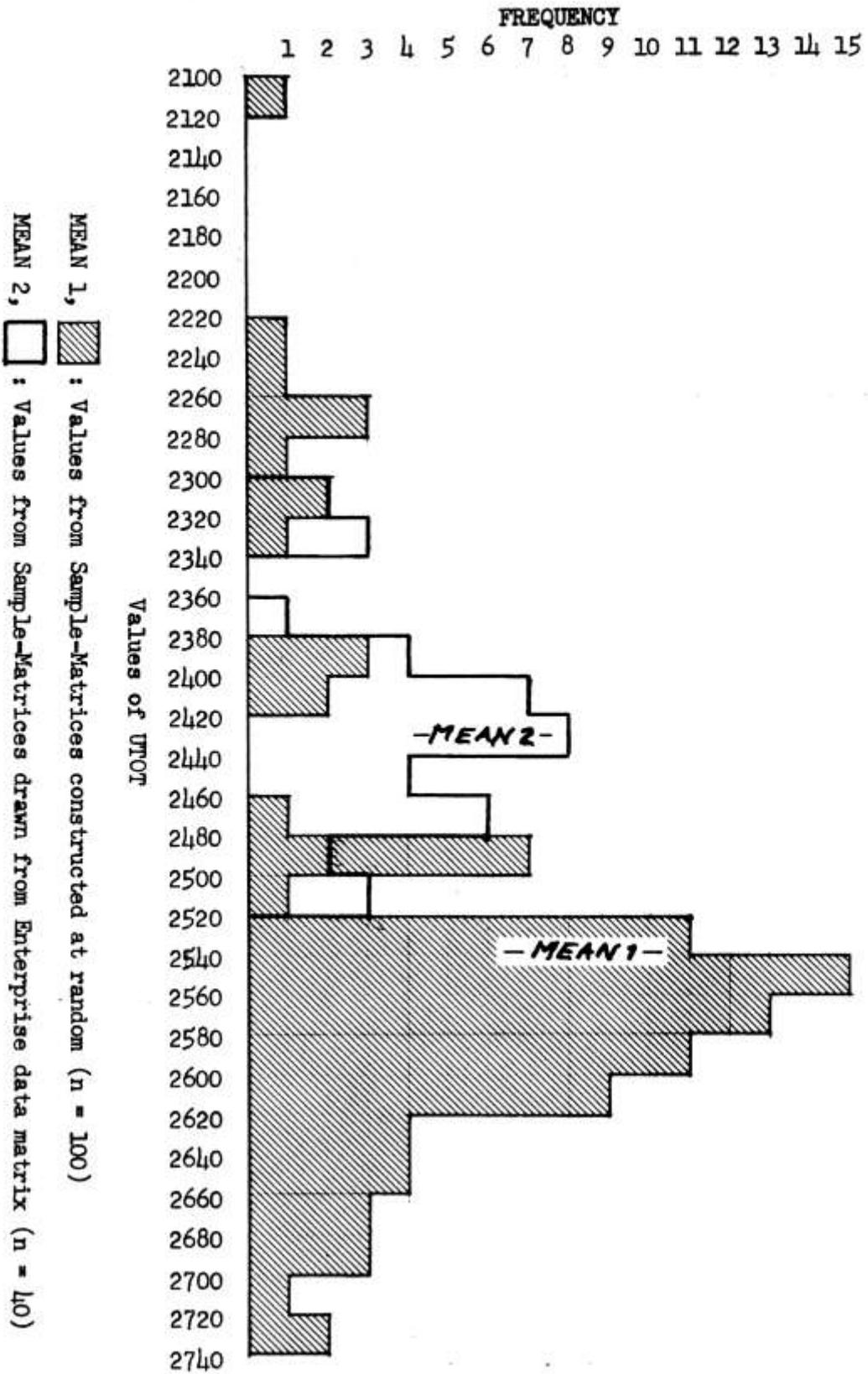


Figure III. Random and empirical values of UTOT superimposed.

matrix) at random; then from each of these drew a sample of small matrices at random; then calculated UTOT for each of the small matrices; then averaged the UTOT's for all the small matrices drawn from each large matrix; and then averaged the results from all the large matrices; and if one did this several dozen times, then one would find that the results clustered around the figure 2539, with a standard deviation of 114.

The next question is, how does our empirical large matrix compare with the universe of large matrices? We have a sample of 40 UTOT-values of small matrices drawn from the empirical matrix, and the mean of these values is 2425. We use this value to estimate the mean of the values of UTOT of all the sample matrices which could be drawn from the empirical matrix. The difference between this value and the expected value is  $2539 - 2425 = 114$ , which makes it, by coincidence, exactly one standard deviation less than the expected value. The probability of a value being one or more standard deviations less than a mean value is .159 , or slightly less than 1/6.

Before making a final interpretation of this result, it is necessary to enter a qualification. Late in the analysis, a re-examination of the computer printouts from the operations generating and evaluating the two sets of sample-matrices has revealed an apparent flaw in the logic of processing. Because of this flaw, the computer seems to have stopped processing a few of the sample-matrices before the lowest

possible value of UTOT for those matrices was achieved. This seems to have occurred in the processing of both sets of sample matrices, so that any errors may well have cancelled each other, but we cannot be sure. Correction of the flaw is a simple matter, but reprocessing all the data would be extremely expensive in both time and money.

With that caveat in mind, then, we can interpret our results as follows: If we were making up large matrices by random processes, only about 1/6 of them would be as orderable as the empirical matrix consisting of the Enterprise data. Putting that another way, we can say with about 84% confidence that the data reordering we have accomplished could have been accomplished only if there were a certain factor or factors operating to give our data an innate orderability. We have called that orderability-factor a Natural Order of Cultural Adoption and Loss, and predicted its existence on the basis of a general hypothesis that the constitution of an x-culture is, in part, determined by a d-culture which is an FIW.

While the confidence level is not highly satisfactory, I feel that we can regard our hypothesis as tentatively confirmed: there is a Natural Order, and it is controlled by a d-cultural FIW. With the hypothesis accepted, we can proceed to examine the data to find the traits which best illustrate the Natural Order.

In selecting the portion of the data which we will regard as a first approximation of the Natural Order of Cultural Adoption and Loss of the

Enterprise Vincentians (or, rather, that portion of the Natural Order spanned by our sample in 1964), we utilized the following line of reasoning:

Some behavior-traits have had more influence than others in re-ordering the households. These traits have "cooperated," so to speak, and the final rank order of households is the end result of their combined influence. We assume that the reason these traits have combined to produce that influence is that they, in turn, most closely reflect the influence of the d-culture in controlling culture change.

To put that another way: We have accepted that the d-culture exists and that it is an FIW. This FIW has had an important effect in controlling the adoption and loss of behavior-traits by the Vincentian households studied. There are also many other extrinsic factors at work, however, which operate in an unpredictable fashion and thus introduce a certain amount of indeterminacy into our observed results. But these factors do not operate with the same strength on all behavior-traits. Some traits are so strongly influenced by extrinsic factors that their distributions, when compared with the rank order of households determined by the Natural Order, approach randomness. On the other hand, there are traits whose distributions conform very closely to the rank order of households. This conformity is taken to be due to those traits' being relatively free of extrinsic influences; in other words, to their distributions' being controlled almost entirely by the d-cultural

FIW. If the d-culture were omnipotent, and we had made no mistakes in data-collecting or in coding, all traits' distributions would correlate perfectly with the rank-ordering of households. The well-conforming traits, then, represent most closely the Natural Order of Cultural Adoption and Loss that would stand revealed if the d-culture were omnipotent, and can be taken as a first approximation to that Natural Order. In so taking them, we are treating them as the empirical units of cultural change; i.e., as the culture-traits, properly speaking (pp. 23-26).

To identify the culture-traits and arrive at a first approximation to the Natural Order, we have recourse again to the Mann-Whitney U-test (p. 69). We have utilized the fact that the distribution of U is roughly normal, when  $n = 16$  or more, to compute a value, for each trait, of  $Z$ .  $Z$  represents the number of standard deviations the U-value for that trait is from the mean of all possible U-values, i.e., from complete randomness, and thus it can be used to calculate the probability that the degree of conformity of the behavior-trait in question is due to chance alone.

To give us a convenient criterion, we have selected behavior-traits whose probability of being as well ordered as they are by chance alone is .05 or less, i.e., whose Z-value is 1.96 or higher, for further analysis. (We recognize, of course, that about one in every 20 of the traits so included is included by chance.) These traits are

considered candidates, as it were, for the status of culture-trait.

Now we take the selected traits and arrange them for convenient inspection as an approximation of the Natural Order of Cultural Adoption and Loss by cutting-point in ascending order, by adoption-or-loss status within the cutting point, and then in descending order of Z-value -- and present them on the following pages (Table 10). If we could assume that this group of traits did, in fact, exactly represent the Natural Order, we could say that the adoption (A) or loss (L) of the traits with cutting-point 1 was a necessary condition for the adoption or loss of the traits with cutting-point 2 and all higher cutting-points, and so forth. This is because ideally speaking there exists no household which has undergone the changes at cutting-point 2 , but which hasn't undergone the changes at cutting-point 1 .

To take a concrete example, using the first trait listed with cutting-point 1 and the first with cutting-point 2: An Enterprise Vincentian apparently has to lose the propensity to make odious comparisons between Christianity and Indian religions before his wife can stop believing that A. N. R. Robinson is the owner of Woodford Lodge Estate. If this particular sequence seems hard to explain, it may be because there is in fact no connection between the two traits; but it may be because we don't know enough about how d-cultures in general, and this d-culture in particular, operate.

We will shortly look at a few culture-traits, their cutting-points,

TABLE 10  
Selected Culture Traits, by Cutting-Point:  
An Approximation of the Natural Order  
of Cultural Adoption and Loss





















and their sequences. First, however, let us examine the selected list of traits, the pro-tern Natural Order, to find general trends in Enterprise Vincentian acculturation.

It is glaringly apparent, right away, that our original assumption, that the Vincentian Creoles were adopting Indian culture, was totally wrong. Knowledge of Hindi, for example, is lost at cutting-point 4. (Hence forth, we will use the code of Table 10 for loss/adoption status and cutting-point, e.g., "Knowledge of Hindi, 4L". The reader may wish to refer to the Supplement for further information on specific traits.) It appears that the conservatives look more favorably upon interaction with Indians (prefer rent house to Indian, 2L), and have picked up a few Indian culture traits that progressive people lack (M likes meat "a little," 5L; doesn't like beef, 9L; likes Indian music; M , F would listen to Indian music, 23L - 24L), but disapproves of some specifically Indian values and practices, especially in the religious sphere (odious comparison, 1L; disapproval of prayer flags, 2L). More progressive people, on the other hand, are able and willing to express what we might regard as ethnographic knowledge and attitudes concerning Indian culture (prayer flags, 14A, 18A; has been to Ramdilla, 22A; B. S. Maraj is head of Maha Sabha, 26A). In this respect it is interesting to note that the most conservative ladies learned to make roti by watching a hired Indian woman make it for them to serve at a Baptist prayers (3L), while the most progressive ladies

learned it at home, from their mothers (26A). In other words, roti-making as a specifically Indian trait is almost lost, but it is being adopted by the most progressive Vincentians, now, as a general Trinidadian trait. Far from having Vincentian Creoles adopting Trinidadian Indian culture we see, rather, that we have Vincentian Creoles losing what little Indian culture they picked up at an earlier time as they adopt Trinidadian Creole culture, of which a small part, in turn, is ultimately Indian in origin.<sup>1</sup>

An examination of Table 10 reveals that, while most of the adoptions and losses do indeed seem to represent cultural change, a few seem to represent not culture change but change in age-status. The most obvious and direct example is that some men at the "conservative" end of the household-order regard themselves as retired (6L). A set of such adoptions and losses occurs in the area of housing; the most conservative families have not only the largest (3L) and highest (4L) houses, they have the most modern and up-to-date houses (walls of hollow clay block, 15L; walls plastered, 9L), while the most progressive families still live in the old-fashioned tapia houses (25A). It would appear that the older people have been saving their capital, in money or in land, and have at last decided to spend it on building a house in which to spend their declining years. The younger people, on the other hand, are living in much older houses which they rent or have purchased on rented land or which, in a few cases, they have

learned it at home, from their mothers (26A). In other words, roti-making as a specifically Indian trait is almost lost, but it is being adopted by the most progressive Vincentians, now, as a general Trinidadian trait. Far from having Vincentian Creoles adopting Trinidadian Indian culture we see, rather, that we have Vincentian Creoles losing what little Indian culture they picked up at an earlier time as they adopt Trinidadian Creole culture, of which a small part, in turn, is ultimately Indian in origin.<sup>1</sup>

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built in the much cheaper old-fashioned way. This impression is borne out by the data in the Supplement (answers to Questions 004 and 005), and by material in field notes.

In spite of the fact that they have smaller and less modern houses, it is interesting to note that the younger, more progressive women have made shift, if necessary, to have a kitchen fully inside the house, in a separate room (18A), and that the most progressive of these have arranged some built-in counter space (22A). By contrast, several of the conservative women cook in dirt-floored detached or semi-detached tapia sheds, even when a separate kitchen-wing is available in their recently-built upstairs houses (see Supplement, 

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Question HH C01; see above, p. 139) .

In general, then, the pattern of the traits has the expected direction as long as we are dealing with features that are within the means of everybody; but some modern conveniences, which everyone would like to have, are out of reach of the more progressive but younger, and hence poorer, families, and thus appear to contradict the general trend. Such behavior-traits cannot be said to be culture-traits; they are being adopted or lost not through cultural time but through behavioral time (p. 22), in this case behavioral time whose period is the life-cycle. Since age can generally be expected to correlate well with the rank order of households produced by the Natural Order of Cultural Adoption and Loss (p. 62), we must be prepared to encounter such life

cycle changes in future analyses of other x-cultures.

So much, then, for general trends revealed by scanning the Natural Order. It had been my intention to attempt a complete analysis of the pro tern Natural Order, a kind of circular process which would have tentatively selected certain traits as bona fide culture-traits and established their exact cutting points, then examined their sequence (the tentative true Natural Order) in order to leap to the construction of an elaborate theoretical model, representing the d-cultural FIW and derivable from a limited set of theoretical hypotheses; this theoretical model, in turn, would be predictive of a Natural Order of Cultural Adoption and Loss which would then be compared to the existing tentative true Natural Order. The process could be repeated until a fairly close correspondence is reached. Then plans could be made for testing this model, and thus the hypotheses, in a replication of the present study.

Perhaps this can still be done, using the very pro tern Natural Order we have here in Table 10. But the fact is that the task will be laborious and time-consuming in the extreme. One must admit, sadly, that a clear pattern does not leap out at one from the pages; several hunches have been followed through without success, and analogies which have occurred to the writer from which to build the theoretical model have failed. This temporary failure is not, after all, unduly surprising; if the underlying FIW worked according to our common

sense understanding, so that adoptions and losses followed one another in a common-sense order, it would have been discovered long ago, and would have been a rather uninteresting discovery at that. It would have been as if the laws of chemical bond-formation had followed common sense conceptions of what happens when different substances are mixed together; Lavoisier, Priestly, and others would have had no difficulty in discovering these laws, the theory of valence would never have been invented, and our understanding of atomic physics would have been to that extent impoverished.

So we will defer the attempt to make a complete theoretical analysis, and simply call the reader's attention to a few features of the Natural Order that are clearly revealed in the table.

The most striking feature occurs at cutting-point 13. Here we see the loss of the propensity to give a response containing the words "Baptist" or "Spiritual Baptist" to the question, "Do you belong to a church or religious organization? What church or religious organization?", and this loss is very definite (with  $Z = -3.72$ ,  $p = .0002$ ). If we look back through the collections with lower cutting-points, we see the loss of other Baptist traits (e.g., Baptist gift: is a Mother, 7L; if sudden money would give a prayers, 7L; and a whole series at 4L). But 13 is the point where self-identification as a Baptist is finally and completely lost. At the same point, we find a series of responses being adopted, responses indicating attitudes of acceptance of various

secular pleasures. It is as if the d-cultures of the first thirteen households contained a germ, a factor, expressed as self-identification-as-a-Baptist, which inhibited, or otherwise prevented the expression of, acceptance of cards, movies, rum, the idea that a wife might be adulterous, and so forth. Or possibly it's the other way around: Adoption of a "pleasure" factor has eliminated, or silenced the expression of, a "Baptist" factor. Again, at cutting-point 21, we see the adoption of another "pleasure" factor, this time a propensity for active participation in Carnival. Perhaps this is associated with loss of the perception of the environment as Baptist at the same point; but we would have to recognize that, in that case, the Wife's loss of the latter perception was accompanied by the Husband's adoption of the former propensity.

To examine the situation from another point of view, let us follow the career of a particular folk-belief, namely the belief that drinking rum and eating fig (banana), simultaneously or successively, can cause illness or death: First the ability to name specific varieties of fig that are to be avoided is lost (3L). Then flat agreement with the whole proposition is lost (9L); this is the most clear-cut shift ( $p = .0004$ ). Flat agreement is replaced immediately by the non-committal "I hear (but never see)" (9A). Finally an open indication of disbelief takes over (19A).

One has a definite impression of sequential turnings-on and

turnings-off of different propensities to respond to the rum-and-fig question. Again, it is as if a certain behavior-determining factor is present up to a point, and then is lost; or, conversely, that a factor previously not possessed is acquired. If this notion is correct, then such factors are the "things" of which a d-culture is made (p. 12). Changes in the d-culture consist of acquisitions and losses of these factors, and one gain or loss is causally related to a subsequent gain or loss. Another way of putting that is that the presence or absence of one factor or group of factors in the d-culture may determine whether or not a certain other factor will be adopted or lost. What we must work to discover now are the mechanisms by which such determinations are made. This study has indicated that the mechanisms exist and has provided a means for empirically testing our ideas about these mechanisms as they evolve.

To recapitulate:

We have shown, in Chapter II, that if the widely-held view that a culture is functionally integrated is true, we should be able to find a certain pattern in the distributions of behaviors over various households of a single social group. After specifying our method of analysis (Chapter III), describing our field situation (Chapter IV) and method of eliciting behavior (Chapter V and Supplement), we have shown that the predicted pattern can indeed be discerned, with a 16% likelihood that we are wrong (p. 142). We have next selected the traits most

representative of the discerned pattern (Table 10) as a first approximation to the predicted Natural Order of Cultural Adoption and Loss (and have shown how this order can give us an over-all characterization of culture change in Vincentian Enterprise [Note 1]). We have tried to indicate how some of these culture-traits seem to reflect the presence of discrete factors, presumably in the d-cultures of the households, factors which are to culture-traits as Mendelian genes are to phenotypic biological traits. Finally, turning back to our original theoretical discussion in Chapter II, we have raised the question of causal relations between the presences and absences of the various factors. Answering this question will require the construction, and testing, of an elaborate theoretical framework, and for that reason is deferred.

#### Further Research

The present study is by no means intended to be definitive. On the contrary, it serves primarily to attest to the feasibility of a new approach to the study of culture change, to outline the methodology of the new approach, and to put forth some specific techniques. What is suggested here, then, is that the present study can be regarded as a pilot study for an extensive program, or perhaps a number of programs, of research in culture change. Such continued research is required on three levels: theoretical, methodological, and empirical.

As stated in the preceding section, theoretical research has been

deferred for this study. Perhaps the building of theory on the basis of the method of analysis presented will have to wait for additional field studies, so that we can look for cross-cultural similarities in Natural Orders of Cultural Adoption and Loss. Perhaps, on the other hand, re-analysis of existing data will prove theoretically fruitful as new methodological techniques are devised.

Among the new methodological techniques required is an improvement in the method of processing a matrix (Chapter III). As noted on pp. 142-143, the existing method has an unfortunate habit of stopping, occasionally, before its job is done. This habit can be corrected by providing an instruction to the computer to re-shuffle the households arbitrarily the first two or three times it stops and start re-ordering again, making sure it arrives more than once at the one and only rank order that produces the lowest possible value of UTOT.

As badly needed, and much more difficult to acquire, is an improved technique for evaluating matrices once they are processed. What is really required is a means of calculating the probability of UTOT (or some other measure) based on theoretical knowledge of the distribution of that measure for matrices of a specified size and shape. Such a technique would make it possible to evaluate large matrices directly, instead of having to sample them.

As these techniques are developed, they can be applied both to new field data (if available) and to existing data. Besides simply

re-processing the existing raw data along the lines already followed, certain other approaches to the existing data might be fruitful of theoretical insight. For one, it may have been noticed by the reader that certain responses are coded both for males and for females (see Supplement). Those responses could be selected as a separate data set, and a new analysis made using the 56 individuals, rather than the 28 households, as the subjects.

Another approach would involve "improving" the data. Once the raw data has been processed and evaluated as it is, and it has been decided to accept the existence of a Natural Order; one can look among the answers to a single question to see whether they might not be combined. That is, two responses which were originally coded separately might be re-coded as a single response, if that yields a higher value of  $Z$  for the combined response than either gets separately. The justification for this would be that the two different responses were actually variants of the same response; i.e., a single underlying factor controls both. Similarly, one could go back to the original data-books and see whether some coded responses ought not to be broken down into two or more separate responses; this would not be as fruitful as the former procedure, however, because our rule while doing the original coding was "when in doubt, code separately."

When the "improved (i.e., re-coded) data is processed, it should produce a clearer-cut Natural Order and thus, hopefully, regularities

in relationships between the various adoptions and losses should be more apparent, leading to an improved possibility of theoretical insight.

Finally, more and better field studies are indicated. While the basic field techniques outlined in Chapter V are sound, they certainly can be refined.

The most important single improvement that should be made is in the choice of a study population. Before commitment is made to a particular village or other social group, an intensive study should be made of the history of the people in an effort to ensure their cultural homogeneity in terms of social class, for example. Unfortunately, this can mean that weeks of work spent in taking a census and obtaining genealogies and life histories may end in one's deciding to do one's study elsewhere. Furthermore, the village studied should be more isolated than Enterprise, and thus less subject to differential cultural influences on its members (i.e., differences in cultural environment). On the other hand, the social unit studied must be, like Enterprise, culturally stable and viable; it is difficult to specify exactly how one determines this, but at any rate a society that is undergoing extremely rapid change, which is being overwhelmed by alien customs to the point where intergenerational communication is jeopardized, must be avoided.

Another area in which improvement can be made over the present

study is in the pre-testing of the interview schedule. Before beginning the actual survey, the schedule should be gone over with a good informant, and he should be asked for his interpretation of each question. If his interpretation doesn't match the ethnographer's, the informant should be asked to help re-write the question. This will not guarantee that every respondent will understand the question, but at least one can be sure that every question will be meaningful to at least some respondents, a condition that was reached in the Enterprise study only after several interviews. Then, ideally, a few trial interviews should be run, and perhaps re-run, to eliminate any remaining "bugs" in the schedule; the results of these interviews would not be used in the analysis.

Finally, it is the very strongly held opinion of the writer that future studies must be carried out, as was this one, entirely by anthropologists. This must include the actual interviewing. Rapport of the kind that anthropologists seek as a matter of course is absolutely essential; the relationship between interviewer and respondent must approximate the traditional relationship between ethnographer and informant as closely as possible. Only if he has carried out the interviews himself can the anthropologist hope to code the responses properly, to understand the pro tern Natural Order when the computer gives it to him, and, most important of all, to build a fruitful theory of culture change from the Natural Order of Cultural Adoption and Loss.

## NOTE TO CHAPTER VI

If we examine the Order further, and augment this examination with information from the life-history table (Table 9), from additional cultural data in the Supplement, and from field notes, we may be able to detect some additional over-all trends, such as those shown in the table below, (The table shows changes along several dimensions of culture in three stages. We do not mean to imply by the use of this trichotomous form that our sample of households could be broken down into three distinct cultural groups one having all the qualities in the first column, another all those in the second, etc. In the first place, most of the dimensions are continuous; the adjacent descriptive categories intergrade gradually into one another. In the second place, far more important, even where the lines between adjacent categories are abrupt, they do not necessarily occur between the same two households from one dimension to the next. The table is presented, rather, as an example of a set of descriptive generalizations that could be made about Enterprise Vincentian acculturation, based primarily on a summary of the data systematically produced by the computer. The reader is invited to compare the summary with its immediate source, the pro-tem Natural Order (Table 10), and to check their congruence for himself.)

Cultural Dimension	Trend	
	From	To
Time and Place Reference	Rural Small Island, ca. 1900-1930	Central County Caroni, Trinidad, 1930-1945
Sense of belonging	St. Vincent	Enterprise area
Economic position reference	Responsible, relatively well-to-do, agricultural	Non-responsible, very poor to poor, laborer or operative
Social position reference	Top of lower caste; frequent, highly patterned, accommodative relations with small upper caste	Lower class; few, unpatterned relations with upper caste
Family of procreation model	Patriarchal, structured. Distinct sex, parent-child statuses.	Matrifocal; Father status weak; little expected of children.
Relations with adult kin	Responsibility if morally deserving, but no favoring over non-kin	Weak and few
Religious group	Spiritual Baptist	Anglican
Nature of religious belief	Devout	Nominal
Attitude toward secular pleasures	Rejecting	Accepting
Aims of child-rearing	Moral character	None; utter permissiveness
		Trinidad as a whole, including cities, 1945 - present
		Independent, poor, skilled tradesman
		Working class and/or lower-middle class in essentially casteless society
		Partnership; small-family ideal; aspirations for children.
		Non-responsible, selective, instrumental
		Roman Catholic
		Secular and instrumental
		Embracing
		Success

Cultural Dimension	From	Trend	To
Folk-beliefs: provenance	Small island (apparently)	Trinidadian	Trinidadian
Folk-beliefs: strength	Strong, disguised from white interviewer	Moderate, manifest	Weak, rational, folkloristic
Home curing: centers about	Bush tea	Rum	Aspirin
Attitude toward Indians	Accepted as lower class people	Avoided, rejected as higher class, exploiters	Accepted on ideological egalitarian grounds
Attitude toward Indian culture	Rejection of religious aspects; some direct borrowing of secular aspects	Relative ignorance	Ethnographic interest, indirect borrowing via Trinidadian Creole culture

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