Appendix A

Note on Statistical Methods

In order to make statistically informed statements about the nature of slavery for the entire range of conditions in which it occurred, I have employed the sample of 186 world cultures developed by the eminent cross-cultural anthropologist George P. Murdock. Over the course of a long career it became apparent to Murdock that "what is needed in cross-cultural research is not samples of great size, nor rough approximations to representative samples, nor random samples drawn from the total universe of known cultures, but rather 'stratified' samples carefully adapted to the facts of ethnographic variation and distribution."[1]

Although I cannot consider them all here, formidable theoretical and methodological problems are posed in adapting this philosophy to my specific venture. However, it is important to note that there were three major objectives of Murdock's sample: first, to represent in 186 cases the entire range of known cultural variations; second, to do so while eliminating "as far as possible the number of cases where similarities are presumably due to the historical influences of diffusion or common derivation"[2] (Murdock employed the most advanced techniques available in coping with "Galton's problem," as this is usually called); third, to select societies on which reliable ethnographic and historical data exist.

A major advantage of using this sample, apart from the fact that it is the product of more than half a lifetime of research by a major scholar assisted by a large research team, is that other scholars have also used it for their own specific researches and have made their codings available. In addition, Murdock and his team have prepared a code of general ethnographic data for all 186 societies.[3] To these I have added my own codings. The previous nine sets of data prepared by Murdock and other scholars constitute a pool of literally hundreds of variables against which other variables can be cross-tabulated. I hope to do this in another study; my main concern in this work was the sample itself. All research materials and codings employed in this work are, with the exception of one table, entirely my own.

From the list of 186 societies I set about selecting my own sample. Murdock had

already indicated the subset of slaveholding societies (column 37 of part 2 of his *Ethnographic Atlas*). The relevant variable was coded by Murdock as follows for "Type of Slavery":

(0) Insufficient information
(1) Absence or near absence
(2) Incipient or nonhereditary
(3) Reported but type not identified
(4) Hereditary and socially significant.

I retrieved all the societies listed in categories (2), (3), and (4).

My first task involved a preliminary search of the most readily available data on the retrieved list of societies that were coded in the Murdock atlas as (2) or (4), plus a few cases that had been coded (3), "Reported but type not identified." Often, because of my specialized interest in slavery, I was able to locate much richer data than Murdock. (The more general sources Murdock used sometimes did not provide sufficient data to enable him to code a society with any specificity.) The result of this initial work was the sample of sixty-six slaveholding societies listed in Appendix B.

My next research task was a more thorough familiarization with the ethnographic and historical sources, after which I drew up a preliminary questionnaire schedule. This questionnaire I pretested with a subsample of the slaveholding societies, found it to be far too ambitious in light of the available sources, and accordingly trimmed the size and revised the categories. Even with this amended schedule, it was not always possible to code all the variables. The final version was a forty-three-item schedule. All but one of my variables were of the nominal (divisible into class or categories) or ordinal (also capable of being ranked or ordered) type. The single exception was a question on the size of the slave population. Significantly, the response rate for this variable was so poor that I had to drop it from the analysis. Travelers and field anthropologists, like the authors of journals and archival documents on which historians draw, rarely counted. In three or four cases the available data on slavery were so meager or of such poor quality that only about half the variables could be coded.

The questions attempted to categorize information in six main areas: demography; origins, means of enslavement, and acquisition of slaves; main uses of slaves and methods of organization; legal and social status of slaves; frequency of manumission and status of freedmen; and frequency and type of warfare in the society. A typical question from the demographic section is, What was the sex ratio of the slave population? The precoded options for response were

(1) More men than women
(2) About even
(3) More women than men.

On socioeconomic issues I asked questions such as the following: How was the status of children determined? The choices here were

(1) Free if mother free
(2) Free if father free
(3) Free if either parent free
(4) Free only if both parents free
(5) Always free.

A final example taken from the questions dealing with the means of enslavement is

the following: Rank order the following seven methods of enslavement using code 1 to indicate the most important method of enslavement, code 8 the absence of that method, code 9 the fact that the method was used but could not be ranked.

The next task involved coding the sixty-six slaveholding societies, using the revised questionnaire schedule. Three coders did the first coding of the data, after which I recoded each society to ensure that all of the variables (with one exception) were coded twice. Where my own interpretation differed from that of the first coder, I carefully reviewed the available data and made a final decision. The single variable that was coded only once was that on warfare. I did not decide to include such a code until after my coders had completed their work, so it was coded only once, by me. No statistical use was made of this code in the present work.

The data were coded in 1974 and 1975, and the first set of analyses conducted in 1975 and 1976. Since that time there has been a virtual explosion in studies of slavery all over the world by scholars in many areas, especially in the Americas and Africa. I have responded to this growing mountain of data by periodically recoding and reanalyzing the materials. Fortunately, in most cases the new information has supplemented the old, so that what has been primarily involved has been the insertion and coding of previously missing information. In a few cases new studies have meant a radical reinterpretation of the traditional view of slavery in a given society. The most dramatic case in point is the Lozi. When we first coded this society, the classic work of the late British anthropologist Max Gluckman still dominated our view of the Lozi past. To be sure, there were indications that Gluckman's view of Lozi traditional society was both too static and too idealized, but in the end we accepted Gluckman's view that slavery among the Lozi was a minor and thoroughly benign institution. Work that has become available since 1974 shows that this view could not have been farther from the truth. The Lozi, as indicated in Appendix C, had a large-scale slave system with an unusually harsh system of exploitation when assessed in African terms. Fortunately, I have not encountered any other case where recent reinterpretations have been as extreme as this. Nevertheless, I should be very surprised if, ten years from now, we did not have to change our coding of several cases in the light of new studies. (Incidentally, the capability for recoding without exorbitant expense is another major advantage of using a small sample.)

Two further points should be emphasized. First, the statistical analysis was always regarded as a *supplementary analytical device.* Even those societies that were coded were studied in the traditional manner also. In the process of recoding the sixty-six societies I became fully immersed in the available literature, and my notes were used in the essentially illustrative and humanistic analysis that dominates this work.

Second, the societies listed in Appendixes B and C do not exhaust the number of cases studied. Many specialist studies of slavery do not appear in either list—for example a number of case studies in the collections edited by Miers and Kopytoff, Meillassoux, Watson, and Lovejoy. Nor do the many Hispanic slaveholding societies of South America, which do not qualify as large-scale slave systems; they nonetheless provide interesting comparisons.

Although many research facilities were used in the various stages of research, coding, and analysis, I must draw special attention to two of them, without which this work would have taken even longer than it did. One is the Human Relations

Area Files (HRAF) in New Haven. We made three main uses of this facility. First, the bibliographic resources were invaluable for our preliminary researches. We were able to go straight to the major ethnographies on each of the sixty-six societies studied. In almost all cases the HRAF bibliography was adequate for the necessary background material. However, in only a small number of cases did the bibliography provide us with *all* the information available on the specific problem of slavery. The core HRAF bibliography, then, was followed up with a specialist search for works dealing specifically with slavery.

Our second and third uses of the HRAF resources had to do with the files themselves. In my pretesting of the questionnaire schedule, the files proved to be invaluable. At that point, however, the limits of the HRAF resources had been reached, and I used them for only one further purpose: to consult a number of essential ethnographies written in Asian and less well known European languages that are available in translation only in the HRAF files. In addition, I took advantage of several HRAF manuscript ethnographies that are accessible nowhere else.

For the final coding of the questionnaire schedule, and in my own notes for the humanistic analysis of the data, I went to the original works referred to in the files, not to mention the specialist literature on slavery that our own group generated. In this regard we were fortunate in having another major research facility at our disposal: the library of Harvard's Peabody Museum of Archaeology and Ethnology (which halfway through our studies moved to new quarters and became known as the Tozzer Library). For comparative ethnographic and ethnohistorical research, Tozzer must certainly rank as one of the best libraries of its kind in the world. Just as valuable as its virtually exhaustive collection of ethnographic materials is its index of authors, books, *and* journal papers. Months, possibly years, of research effort were saved by making full use of these extraordinary facilities.

The reader not acquainted with statistical methods may wonder how these data were analyzed. Without attempting a short course in statistics, I do want to make one or two general remarks. In recent years there has been a great deal of talk about quantitative history. I fully support this development, and not simply because discretion urges support for the inevitable: such studies complement rather than threaten the interpretive approach to history and comparative sociology.

Several statistical techniques are available for handling nominal and ordinal variables and, in elementary form, it is these that I have used in analyzing the coded data. Cramer's V is a common measure of association between nominal variables. It ranges from a score of 0 (no relationship) to 1 (perfect association). Gamma (G) and Spearman's rho are both symmetric measures of association for ordinal variables ranging from minus 1 (perfect negative association) through 0 (no relationship) up to plus 1 (perfect positive association). Any standard elementary textbook in statistics will explain the rationale and mathematical bases of these measurements. The chi square and probability statistic (p) are measures derived from inferential statistics; purists hold that they are relevant only if one's units constitute a random sample. It is rare, however, to find a genuinely random sample in the social sciences. We usually assume that our sample approximates the demands of randomness. The probability statistic assesses the degree to which the observed and measured association is due to chance; more properly, the degree to which we may confidently infer that the particular association observed in the sample holds for the parent popula-

tion. Thus if $p = 0.05$, it means that there are five chances in a hundred that the observed association is just a fluke; if 0.005, the chances are five in a thousand; if 0.5, the chances are one in two. It is up to the researcher and his reader to decide where he or she is going to draw the line. In this work I have accepted as statistically significant only relationships at the 0.05 level or better. Put another way, whenever I report a relationship as significant I mean that, at the very worst, there is only a 5 percent probability that the reported relationship could have been due solely to chance.

In recent years a powerful new technique has been made available to scholars who analyze categorical data, that of the so-called log-linear models. Unfortunately, I did not have access to an economically feasible program at the time I conducted the major part of my analysis; nor had I acquired full competence and confidence in the theoretical underpinnings of the new method. By the time this situation changed, I had completed the final draft of this book. Nevertheless, with the assistance of one of my programmers, Hiroshi Ishida, I reanalyzed my statistical data using the new methodology. Happily, the log-linear modeling technique fully supported the findings arrived at with the use of the more traditional methods.