RECENT STUDIES ON CARIBBEAN SLAVERY AND THE ATLANTIC SLAVE TRADE

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SEARCHING FOR THE INVISIBLE MAN: SLAVES AND PLANTATION LIFE IN JAMAICA. By MICHAEL CRATON. (Cambridge, Mass.: Harvard University Press, 1978. Pp. 439. $32.50.)

SLAVE POPULATION AND ECONOMY IN JAMAICA, 1807–1834. By B. W. HIGMAN. (Cambridge and New York: Cambridge University Press, 1976. Pp. 327. $9.95.)

PLANTATION SLAVERY IN BARBADOS: AN ARCHAEOLOGICAL AND HISTORICAL INVESTIGATION. By JEROME S. HANDLER and FREDERICK W. LANGE. (Cambridge, Mass.: Harvard University Press, 1978. Pp. 368. $20.00.)

THE COLTHURST JOURNAL. Edited by WOODVILLE K. MARSHALL. (Millwood, N.Y.: KTO Press, 1977. Pp. 255. $12.50.)

SLAVES IN RED COATS: THE BRITISH WEST INDIA REGIMENTS, 1795–1815. By ROGER NORMAN BUCKLEY. (New Haven, Conn.: Yale University Press, 1979. Pp. 210. $17.50.)

THE FRENCH SLAVE TRADE IN THE EIGHTEENTH CENTURY: AN OLD REGIME BUSINESS. By ROBERT LOUIS STEIN. (Madison: The University of Wisconsin Press, 1980. Pp. 250. $20.00.)

THE MIDDLE PASSAGE: COMPARATIVE STUDIES IN THE ATLANTIC SLAVE TRADE. By HERBERT S. KLEIN. (Princeton, N.J.: Princeton University Press, 1978. Pp. 282. $20.00.)

History was once correctly called a "moral science." The past can only be reconstructed from a point of view that is fashioned as much by the historian's values as by his theories and methods. Having said this, however, it must immediately be added that there is a world of difference between a point of view and tendentiousness, between a consistently held perspective and one that is contradictory, above all between historical judgment and moral commitment. The fact that a Jewish historian feels deeply about the genocidal assault on his ethnic group in modern times should not prevent him from asking probing questions about the pattern of resistance of European Jewry during the Second

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World War, or lead him to make heroes of those who betrayed their group in its worst moments.

More than any other area of contemporary history, slave studies runs a constant risk of tendentiousness, moral posturing, and absurd revisionism on the part of both black and white historians. Present realities place too heavy a burden on historical scholarship. Too many black scholars and their readers hope to find disalienation, pride, and the restoration of identity in the exploration of their past. Too many white scholars hope to prove their racial virtue to blacks or put one over on their fellow white scholars by exploding "the myth of the negro past" or "the myth of the broken slave family," or, most recently, by tracking down the hidden haunts of invisible man.

In his work, *Searching for the Invisible Man,* Michael Craton almost ruins what happily remains a major work with his moral posturing, ideological inconsistencies—even while frequently condemning ideologues—and intellectual game-playing with favored and disfavored black readers and colleagues. His claim that "white historians can add few of the intuitions that inform the work of such black West Indian scholars as the poet-historian Edward Kamau Brathwaite," is unctuous. Without in any way discrediting Brathwaite's work, any reasonable and honest student of Caribbean history will agree that the works of the white Jamaican historian Douglas Hall, the white Jamaican anthropologist M. G. Smith, and the white American historical anthropologist Sidney Mintz surpass in sociohistorical intuition and intellectual contribution anything Brathwaite has written on the subject. If statements such as the one quoted were simply superficial lapses one would be inclined to neglect them; however, on numerous occasions they interfere with Craton's analysis. Thus, in clear contradiction to the rich body of data he presents, Craton could not resist the temptation to defend the honor of the Jamaican slave women by asserting gratuitously that: "Further study may well show that black slave women and Africans alike were slower to sexual maturity than modern women, despite the accusations of promiscuity and precocity leveled at them by contemporary writers." Why "accusations"? Why the hypocritical puritanism? What's wrong with sexual precocity? Who ever claimed that Africans were promiscuous? And who, anyway, are these wicked "contemporary writers"?

Fortunately, Craton's work, in spite of these irritating posturings and several analytic flaws, survives as a valuable contribution to the historical ethnography of Jamaica and to the study of slavery in general. Craton paints on a small canvas—his unit is a single plantation in Jamaica—but he paints in exhaustive detail, and, when sticking to his last, he is not only a first-rate historian but an acute observer of contemporary mores.

The work is divided into three parts. Part one is a description of

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the development and structure of the Worthy Park plantation during the period of slavery, especially between 1783 and 1838; part 2 attempts to add humanistic flesh and life to the statistically based analysis of the previous section by a series of biographies of individuals who lived during the slave period; and part three is a study of the plantation from the period of slavery down to modern times using a combination of techniques: family histories, traditional historical methods, and statistical analysis.

Part one presents the considerable body of data on Worthy Park that the Clarke family, with typical imperial viciousness, had deliberately prevented local historians from using. (It is not the case, as Craton claims, that the Clarke family wanted a Jamaican historian to work on these family papers; quite the contrary. It should be added, however, that Craton is in no way to be held responsible for this blatant act of academic and national discrimination.) A major problem that Craton struggles with is the degree to which Worthy Park was a typical plantation. He tries hard to make the case that Worthy Park was indeed typical, but his own data constantly belie his interpretation. The plantation was isolated in the interior of Jamaica, whereas most large plantations were on the coast; its soil was, and remains, fabulously rich, whereas most Jamaican plantations constantly struggled with declining fertility; it has had extraordinarily few owners, whereas most Jamaican plantations constantly changed hands; and it continued to make a profit during the last decades of slavery when most Jamaican plantations were facing difficult, even ruinous, times. Indeed, apart from the lean years after the late 1840s, the estate has been unbelievably profitable. Craton records, without comment, that Worthy Park was even making a profit during the depression years! The truth is that, for its size, Worthy Park was and is perhaps one of the most efficient and profitable plantations in the world. Hardly a typical Jamaican plantation. At the same time, its very lack of typicality may well be its greatest asset to the historian. In struggling against his own data to prove its typicality, Craton fails to make use of a much simpler and more persuasive argument—namely, that Worthy Park is the classic limiting case. As such, what holds for this well-managed plantation with its resident owners must at least hold for all other plantations with respect to the treatment and condition of the slave population.

Although the data presented in part one are both rich and intrinsically interesting, one gets the distinct impression that Craton was, on the one hand, both overwhelmed and overimpressed with the sheer mass of data he had collected, and, on the other hand, highly suspicious of the use of statistical methods in analyzing them. The result is that we are presented with a great many tables and graphs but precious little analysis, and that little is not only purely intuitive but raises more ques-

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tions than it answers. The materials virtually cry out for proper statistical treatment. The most elementary form of multivariate analysis would have resulted in considerable interpretive payoffs. Astonishingly, no such analysis was attempted.

Let me give a few examples of what I mean. One of the great demographic mysteries of the Jamaican (and other Caribbean) slave populations is why was it that the proportion of males fell so rapidly just before and after emancipation? The Worthy Park data present a unique opportunity for answering this question, for the trend was even more marked here than in the rest of the island. Craton makes two intuitively based guesses. The first is that women lived 5 percent longer than men. The most cursory calculation, however, immediately makes clear that the male-female differential in average life expectancy hardly begins to account for so drastic a change in the sex ratio over so short a period. Craton's second explanation is simply irrelevant, namely, that "more men were transferred out of the work force in the reorganization that preceded emancipation" (p. 75). What we have here is a multiplicity of variables—the Creole-African ratio; the age distribution; the ethnic-, age-, and sex-specific mortality rates; and the manumission rate, among others—that simply cannot be analyzed by inspection, for it is clear that they not only influence the dependent variable in question—namely, the overall sex ratio—directly, but interactively. Craton does a beautiful job of presenting the data, both in tabular and graphic form, but to describe is not to explain. He makes the extraordinary claim that: "In the statistical presentation which follows, the forms of the tables and graphs are more or less *self-explanatory*" (emphasis added). Tables and graphs are never self-explanatory, especially when they present unusual, even bizarre, distributions. By refusing to employ appropriate statistical methods of analysis, Craton may well have missed a golden opportunity to solve one of the major riddles of Jamaican demographic history.

Another demographic mystery left unexplained is the declining rate of fertility during the decades before emancipation when the proportion of women of childbearing age was actually on the increase. "What compounds the mystery of the declining birth rate," Craton adds, "is the fact that slave women at Worthy Park apparently reached their peak of fecundity at a later period—some five years—than women in modern West Indian populations" (p. 96). And there Craton lets the matter drop. There is, of course, one very good explanation already in the literature on slavery, namely, that slave women deliberately refused to bear children at anything approaching replacement levels under the oppressive conditions of slavery. It was one of the strongest and most effective forms of protest against the system.[1] Craton, however, would clearly rather see these women as Victorian prissies who did not reproduce because of their excessive sexual restraint (pp. 96–97).

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In his attempt to revise or appear to revise traditional positions, Craton often either neglects previous works or, worse, misrepresents them. Thus, one of the major outcomes of his chapter on employment patterns is the unusually high proportion of women in the fields. Women outnumbered men in the fields and, at the same time, men monopolized what Craton calls the "elite jobs." I must take issue with Craton on two points: not only does he give the impression that this is an original finding—which it is not—but he writes that this finding "runs absolutely counter to the situation predicted (from little actual research) by Orlando Patterson: 'The slave man's sexual difference was in no way recognized in his work situation by the all-powerful out-groups'" (p. 415, note 6). The truth of the matter is that in the *Sociology of Slavery* (London: MacGibbon and Kee, 1967) published eleven years earlier, after a great deal of archival research, on the estate journals of another plantation—Green Park Estate—I concluded my analysis of its occupational distribution with the following remark: "One is struck, too, by the fact that male slaves had a much wider range of occupations to choose from than females; apart from being domestics and field-hands, the latter could only be washerwomen, cooks and nurses" (p. 61). The passage that Craton cites refers to a completely different point that I was making, namely, that in the eyes of the master class, men and women were viewed simply as bodies, as units of production to be exploited without regard to any notions of masculinity and femininity. Men were emasculated and women were equally defeminized. This is not inconsistent with a system that created *for its own purposes*, its own sexual division of labor. The important point is that the correlation of certain jobs with certain sexes was no reflection of any normatively determined sense of what was socially appropriate and honorable for each sex.

Craton's analysis of the occupational structure of Worthy Park during the slave period leaves a great deal to be desired. Clearly, there were good economic reasons why women were so disproportionately represented in the fields and men so disproportionately represented in other occupations. To some extent Craton confuses the issue by his classification of what he calls "the elite jobs of drivers, headmen, and craftsmen and of the specialized occupations of boilers, potters, distillers, stockmen, wainmen, and watchmen" (p. 142). Drivers, whom he places at the top of his list, may have been elite slaves in the eyes of the masters, but they certainly were not in the eyes of the slaves. Here Craton, perhaps too influenced by Genovese's bold but ultimately unsuccessful attempt to rehabilitate the reputation of the U.S. slave drivers, tries to make the same revisionist thesis for Jamaica. It simply does not work.

More than anywhere else, Craton fails to interpret his own data

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properly. One of the most extraordinary features of his table 47, which presents a cross-tabulation of occupation by categories of slaves for selected years, is that right up to the end of the eighteenth century, Africans outnumbered Creoles as heads and drivers by ratios of more than two or three to one. Only after the end of the slave trade, when the Africans began to age rapidly, do Creoles outnumber Africans in these so-called "elite" roles. Craton himself fully supports the established view that Africans were held in contempt by Creole slaves. Why then were Africans given the most "elite" jobs? This fact screams out for interpretation; astonishingly, Craton offers not a word of explanation. I propose that the Worthy Park owners were here not only employing the classic imperial strategy of divide and rule, but more viciously, of placing individuals from groups known to have low status in commanding positions. Such individuals, notoriously, are likely to be the most avid and sadistic in their support of the oppressors' authority, compensating, at the same time, for the indignities they have experienced at the hands of the group that held them in contempt. If this is what was going on here then it casts a wholly different light on the role of the drivers. Far from being members of the slave elite, they were co-opted marginals only too eager to vent their fury on the Creole slaves. Some elite!

The mystery of why so few women were in his so-called elite categories vanishes when the nature of the jobs involved is examined. The job of boiler may have been a skilled one, but it was also very hard, grueling work. Men monopolized it, not because the masters were sensitive to their masculine pride, but simply because they were stronger than women. It is not insignificant that one of the few accidents Craton records concerns a young woman who was killed in 1813 after falling into the machinery at the mill (p. 203). The positions of stockmen and wainmen also called for unusual strength; whatever skills are involved, both of these are men's work for the simple reason that they require frequent and periodic bursts of extraordinary strength.

Part 2 of the work, "Individuals in Slave Society, Selected Biographies," is the most disappointing. Craton promises a humanistic supplement to the statistical analysis of part one by means of a series of biographical profiles of slaves from different occupations and regions. What he actually delivers falls far short of this. He simply takes from the slave ledgers a set of names whose occupations and origins are known; then, drawing on one or two of the better known contemporary sources and equally from modern works—though without acknowledgment[2]—he constructs a profile of what *on average* the bare bones of their lives *might have been*. Only the discussion of the two whites, John Quier and Rose Price, approaches anything that might be called biographical. Of the blacks all we learn in the great majority of cases are their names,

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their price, the dates of their arrival and birth, and the year of their death. Occasionally we also learn the number of children they had.

All Craton has done in fact is to add names to averages, for all his scathing preliminary talk about slaves being "no more average than free men." He has told us nothing about these African and Creole slaves that we did not know already. Ironically, the very juxtaposition of the names and vital statistics he selects—Duke, Clarissa, Raveface, Gamesome, Rebus, etc.—with the composite, generalized knowledge we have, has even the opposite effect of that intended by Craton. Their very anonymity is thereby emphasized. The impression one gets is similar to that of walking in a long-abandoned graveyard, idly reading the tombstones. Not one shred of individuality comes across. We do not know how any of them felt specifically about their condition; we do not know whether they were handsome or ugly, kind or mean, good lovers/fathers/wives/mothers or bad, courageous or cowardly, whether they picked their noses or stuttered or smiled too often or too little. Any claim to a discussion of *individuals* in slave society must surely mean, at the very least, an illumination of persons as individual human beings, as distinctive creatures; and this is most conspicuously lacking in these so-called selected biographies.[3]

If *Searching for the Invisible Man* had closed at the end of part 2 the work would have been judged a failure, useful only for its compendium of poorly analyzed local data. Fortunately for Craton, there is part 3, which is an analysis of the postemancipation era and of a select number of family reconstructions of modern Jamaicans whose roots go back to early Worthy Park. Part 3 is, without qualification, a brilliant performance. Craton's discussion of the transition from slavery to free wage labor is the most factually informed and the most analytically rigorous discussion of this critical period of Jamaican history that I have ever read. Craton reveals himself as a gifted interpreter of complex historical processes; and the postemancipation period was indeed complex. Craton shows that the traditional view, which I also shared, that there was a mass flight from the plantations, has to be considerably revised. While the women might have left in great numbers, this was not true of the men. This, however, implies no nostalgia for the plantation, but rather the harsh reality that wage slavery was hardly much better, indeed in some respects worse, than formal slavery. These valuable new findings are teased out by means of a painstaking, sensitive, and robust analysis of a wealth of data.

This is followed by two genealogical studies: one tracing the descendants of the colored son of a former owner of Worthy Park, John Price Nash (1796–1870); the other, the descendants of the slaves Buddy and Nelson. This is the most absorbing part of the work. Here, at last,

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human beings come alive. We begin to know John Price Nash from his letters, especially the touching one to his sister who married racially and socially out of Jamaica. We sense Nash's deep prejudices and ambivalences. He is all too human in his contradictions, in his total acceptance of the white plantocratic order which, however (and much unlike his sister), involved no attempt to hide the other side of his ancestry.

Craton's tracking down of the present-day descendents of John Price Nash and his account of an evening spent with an arranged gathering of the remaining clan is a sociohistorical tour de force. His observations on the Jamaican middle class are almost novelistic—acute, wry, witty, with a sharp eye for the revealing detail such as Mrs. Iona Pantry's wig, Mrs. Beaulah Myer's eloquence and ignorance about her past, and the subtle correlations and counteracting play of language, color gradations, education, and class.

Beautifully executed too is the genealogical study of the rural proletarian Isaac Brown. Brown is an unusually articulate man and many Caribbeanists, especially native West Indian scholars, will be suspicious not so much of the fact that he is articulate—few rural folk, speaking in their own language, are more articulate than the Jamaican "countryman"—but of the unusual range of ideas and sentiments he expresses. So let me note here that I have in my files hours of taped interviews with him, and can therefore attest to the accuracy of Craton's portrayal.

Isaac Brown is, however, a highly exceptional Jamaican, a fact that underscores the important though negative results of the final chapter. Craton had hoped to gain insight into the past by means of a large number of oral histories from modern Jamaicans. What he found was an extraordinary absence of knowledge about the past, including even knowledge of ancestors, kinship memory rarely going beyond three generations. This, for me at any rate, was an extremely important finding although Craton, understandably in this case, would have preferred more positive results. What his work confirms, however—and from the perspective of West Indian intellectual history it might be the work's most important finding—is the much maligned view that the most critical feature of the West Indian consciousness is what Derek Walcott calls "an absence of ruins." The most important legacy of slavery is the total break, not with the past so much as with a consciousness of the past. To be a West Indian is to live in a state of utter pastlessness, and this is true whether one is a proletarian or a member of the bourgeoisie. It is one of the many ironies of this work that, having set out in search of the invisible man, it ended up confirming the reality of the pastless invisible consciousness.

This is an uneven work. At its worst, it is analytically weak; it fails, especially in part 2, to achieve what it promises; and its author too

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frequently fails to place his work in its intellectual context, to acknowledge his intellectual debts to those who have written on the subject before him and on whom he has drawn. At its best, the work presents a vast body of data on the micro-sociohistorical level that will be of tremendous value to other scholars; it makes a major contribution to the study of the transition from formal slavery to postemancipation oppression in Jamaica; it is an absorbing supplement to Caribbean ethnography and confirms an important, if unpopular, theory of West Indian social consciousness; and, in more general terms, it is a valuable addition to our growing knowledge of the relationship between slavery and capitalism.

Barry Higman's *Slave Population and Economy in Jamaica, 1807–1834* is a macro-socioeconomic study of the last decades of slavery in Jamaica that more than complements Craton's micro-study. Higman employs much the same kind of data as Craton, since he draws on a number of plantation records, but he also makes excellent use of the Slave Registration and Compensation Records, the Accounts Produce Records in the Jamaican Archives, the votes of the Jamaican House of Assembly, and other archival as well as literary data. The data have been meticulously prepared and evaluated and standard techniques of correlation and regression analysis employed in analyzing them. The result is an impressive achievement, a genuinely interdisciplinary work that questions many traditional views on Jamaican slave society and establishes both new explanations and important new issues.

The work's major objective is to examine the structure and changes in the slave population of Jamaica during the nineteenth century and its effects on the nature of the slave economy. Part 1 is a description of the economy concentrating on the distribution of the slave labor force, the structure of agriculture, and the role and pattern of nonagricultural activities. The key issue in the discussion of the distribution of the slave population is the degree to which sugar dominated both the employment of slaves and the economy as a whole. There is no question of course that sugar was the dominant crop, but Higman rightly questions whether it should be assumed that other crops were not of significance in considering the lives of the slaves, and he rightly upbraids me for discussing the economic environment of the slaves solely in terms of the sugar plantation. Mea culpa. I have always been bothered by those pens, coffee plantations, and jobbing gangs and the degree to which they differed from the sugar plantation in imposing constraints on the lives of the slaves. Although his discussion of the problem is new and valuable, I am not altogether convinced that their influence was as great as Higman suggests. At the macroeconomic level Higman claims that sugar directly or indirectly accounts for 60 percent of total national output. This differs significantly from the 80 percent that

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Sheridan claims and that I still prefer, but 60 percent is still a lot of domination and I am not prepared to quibble on the issue. What concerns me much more is Higman's claim, which he vigorously pursues in the chapter on agriculture, that slaves were generally much better off in nonsugar activities, that on the whole the smaller farms and pens as well as the coffee plantations provided greater independence for the slaves. In general, the smaller the holding, the greater the marginal utility of the slave to his or her owner, but this cuts both ways as far as the slave was concerned. The slave may have been better looked after materially, and it is this which Higman emphasizes, but, apart from the 8.0 percent of slaves in the urban areas, it is doubtful whether they experienced greater independence. There was not the advantage of the anonymity of a large plantation. It should not be forgotten that from the slave's point of view the worst thing about being a slave was not the material exploitation, but suffering the indignity, the endless personal assault of being in a relation of slavery to someone else. It is certain, too, as Higman concedes, that slaves in jobbing gangs worked much harder than those on the plantations. The problem is that we do not know just how many of the slaves in nonsugar activities doubled up as jobbers during the slack periods. I suspect that most of them did for a good proportion of their working lives, in which case it is a moot point whether they were even materially better off than those who toiled on the sugar plantation.

Higman's discussion of the structure, growth, and spatial and occupational distribution of the slave population is simply excellent. His use of correlation techniques to isolate the degree of internal migration of the slave population results in valuable new knowledge as does his analysis of the nature and interrelationships of settlement patterns, slaveholding size, age structure, sex ratio, Creole-African ratio, fertility and mortality rates, and population growth. Age structure was found to be the crucial intermediary variable explaining the growth of the Creole population after 1830. Higman speculates that had slavery not been abolished in 1834, the internal cycle of the age structure "might have resulted in self-sustaining population growth or a series of degenerating phases of decline and growth" (p. 98). The latter seems the more likely course, but we simply cannot say for sure, for we do not know how the complex set of variables influencing the age structure would have developed and interacted had slavery continued.

What accounts for the decline of the slave population after 1807? This intriguing issue is valiantly grappled with by Higman. Although the slave population was found to be unusually unfertile, mortality was the "fundamental" factor in explaining population changes. Sex ratio, Creole-African ratio, crop type, occupation, size of holdings, and environmental factors were all examined in order to ascertain their effects

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on mortality and fertility rates. The independent effect of each was found to be small. Only 18 percent of the variation in natural increase was explained, and three variables explain almost all of this variance: African mortality, rainfall, and presence on sugar estates. The results are clearly disappointing, though much of value, if only of a negative sort, is gained by the analysis. There are two reasons for the failure to explain more of the variance. One is substantive: Higman could find no way to measure, let alone observe, the level of brutality and its effect on mortality and fertility rates. More tantalizingly, what little data he brings to bear on the subject strongly support the position that the slave women, especially the Creoles, were engaged in a gynecological revolt against the system. A second possible reason for the explanatory failure may be found in Higman's statistical analysis. The regression technique he employs gives only the direct effects of the independent variables. Path analysis might have indicated both indirect and more complex interactive effects than the simple regression models on which he relied.

Higman's chapter on color, family, and fertility is the most contentious section of the work. His brief discussion of the factors accounting for variations in colored birth both reinforces what we already know about the origins of Jamaican color values and color-class gradations and isolates the significant variables accounting for the rate of miscegenation: the slave sex ratio, the number of slaves per white, and the number of sugar estates.

More controversial is Higman's attempt to make the revisionist thesis that the slave system did not severely disturb normal family life but rather allowed for a situation in which the "nuclear family" was the norm. Not only are the data used to support the contention inadequate as a sample of Jamaican slave society as a whole during the last decades of slavery, not to mention the preceding one hundred fifty years, but a careful study of even this data suggests that they support, rather than undermine, the traditional view.

The interpretation of statistical data in order to gain insights into household and familial composition is notorious for what may be called the "its-half-full; no-its-half-empty" problem. A great deal depends on how the data are grouped and how categories are defined and interpreted. Higman follows the Cambridge Group of historical demographers in distinguishing between the "houseful" and the "household," the latter being the coresident family, and the former including "all persons resident in the house or premises" (p. 157). This may or may not be a useful distinction; more to the point is Higman's failure to explain just what he means by the "nuclear family," since his major claim is that this was the "dominant" form. I take the nuclear or elementary family to mean a residential group consisting of a cohabiting man and woman in a regular relationship and their joint natural or

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adopted offspring. The nuclear family incorporates at least three dyadic relationships: a union between an adult man and woman; a relationship between a mother and a child; and a relationship between a father and a child. Neither the mother nor the father need be the natural parent but it is essential that both perform the respective parental roles (see Richard N. Adams, "An Inquiry into the Nature of the Family," in R. F. Winch and L. W. Goodman, eds., *Selected Studies in Marriage and the Family* [N.Y.: Holt, Rinehart & Winston, Inc. 1968], pp. 44–57). It is noteworthy that there is no evidence whatsoever, in the data presented by Higman, of the existence of two of these three essential dyads: the sexual dyad between the two adults, or the father-child dyad. Higman merely infers from the presence of the males and the age differentials between men and women in the housefuls that some of the men and women were sexually related. There is not even inferential evidence that the males were the natural fathers of the children; indeed, what inferential evidence exists points to the opposite. From what we know of the position of the "outside child" in modern Caribbean society, it is much more reasonable to assume that a male presence, even one involving a sexual relationship with the mother, in no way implies an adoptive paternal relationship.

Another serious limitation of the data is that it is of the census type; that is, it gives the distribution of "housefuls" and inferred unions only at one moment in time. It tells us nothing about the sequential pattern of mating. Higman is aware of this problem, but he underestimates its enormous importance. Modern studies of the West Indian lower-class family, however, have shown unequivocally that a static distribution of union types, revealing a proportion of common law unions as high as 50 percent, may conceal a distribution of sequential unions that is indicative of chronic instability in sexual relationships. This kind of information, however, is only possible when we know the mating history of each individual. The same holds for conclusions concerning the stability of households.[4]

Just as significant are Higman's own conclusions that Creole slaves were more frequently found in households headed by women (p. 160), that the more favored mulattoes showed the greatest sexual and familial disorganization, that it was the younger households that tended to be female dominated, and that the slaves in positions of influence were polygynous. Higman claims that "the rarity of women living without a mate is suggestive of a more stable pattern of unions than Patterson recognizes" (p. 173). On the contrary, judging from the situation on the modern plantations and in urban slums, such "rarity" is indicative of a very high turnover of unions.[5] Women have no "breathing space" between unions, which is why the static data fail to pick up such intervals between unions. Thus, insofar as any conclusions can be drawn

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from Higman's data, it must be the very opposite of his own: the traditional interpretation is reinforced rather than revised.

Higman's discussion of manumissions, runaways, and convicts is important for its finding that during the last decades of slavery urbanism was the most important factor explaining the rate of manumission; it should, however, be cautioned that this does not necessarily hold for most of the preceding period of slavery. On the organization of slave labor Higman's use of multivariate methods results in a far more satisfying analysis than Craton's. Masters allocated slaves to occupations on the basis of their sex, age, color, birthplace and health, but by far the most important factor was age. It is this which mainly accounts for the unusually large number of women in the fields.

Higman's treatment of the problem of productivity and profitability is the least successful chapter of the work. In the absence of data on the cost structure of production, I do not see how any meaningful conclusions can be drawn on the subject of profitability. Higman certainly shows that both productivity and total product increased at a time when the population was decreasing but this most certainly does not disprove "the view that the slave system proved itself inherently inefficient as a strategy of labour organization in the early nineteenth century" (p. 215). What it proves is that the slaves were mercilessly forced to work beyond the limits of endurance during this period and that there was massive depreciation of capital, a position which Higman curiously fails to consider in his eagerness to support the revisionist thesis.

This is an extremely valuable work. Higman places his work squarely within the mainstream of Caribbean sociohistorical scholarship. He never postures nor does he attempt to give the impression that he is some intellectual knight in white armor restoring their lost past to the disinherited West Indians. Higman is at once both less pretentious and more quietly radical in his intellectual style. He has studied the West Indian past under West Indians scholars, among West Indians, in the West Indies. He is no longer another foreign student of the West Indies, but a West Indian scholar who has written one of the most meticulously researched, methodologically innovative, and brilliantly argued treatises on slavery in Jamaica. *Slave Population and Economy in Jamaica* is certain to find its place among the handful of works that all students of slavery in the Americas are required to read.

Over the past twenty years, studies have been published on almost all the major slave societies of the Caribbean. Barbados is the major exception, a puzzling lacuna in view of the fact that Barbados is Britain's earliest colony in the Americas and was pivotal in the British colonization of the hemisphere. For many years Jerome S. Handler has been promising to fill this gap in the modern literature. We have been served

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many academic hors d'oeuvres on one aspect or another of the subject since his 1963 papers on the history of potterymaking in Barbados. Some of them have been of high quality, especially the work on the freedmen, which he coauthored with Arnold A. Sio of Colgate University ("The Freedman in Barbados Slave Society," typescript, 1970) but which, for reasons that are as regrettable as they are delicate, appeared four years later under the sole authorship of Handler (*The Unappropriated People: Freedmen in the Slave Society of Barbados* [Baltimore: Johns Hopkins University Press, 1974]). Handler's latest work, *Plantation Slavery in Barbados: An Archaeological and Historical Investigation*, coauthored with Frederick W. Lange, is, unhappily, not the entrée its title suggests. The work is more an experiment in interdisciplinary method than an original substantive contribution. It combines ethnohistory with archaeology in an attempt to uncover ethnographic details on the life of slaves on a single plantation—Newton, in Barbados. The authors are very conscious of the novelty of their methods, perhaps a bit too much so, since over half of the text, including appendices, consists of discourses and comparisons of method.

Even the most specialized of readers will find his or her patience sorely tested. Reading this work is somewhat like eating a malnourished Jamaican crab: one cracks and breaks and digs and searches, but in the end there is little meat to show for it. The truth of the matter is that the major finding of this work is the negative conclusion that, for all its apparent promise, archaeology has little to offer the study of Caribbean slave societies. The slaves, alas, were simply too poor to indulge in the spiritual luxury of burying their few precious belongings with the dead; what little they might have buried was too organic to survive the tropical heat and humidity of Barbados. Not a single piece of household furnishing was uncovered, nor a single utilitarian item, whether organic or not. All that six months of digging produced were a few beads of uncertain origins, some fragments of pottery and clay pipes, a few copper rings and bracelets, and, of course, a lot of bones. It could not even be ascertained from the archaeological evidence whether the cemetary was indeed a slave burial ground, and even data as elementary as the sex of the skeletal remains remained elusive.[6]

Understandably, the authors concentrated on mortuary patterns as the area of slave life to which they hoped to make their major substantive contribution. The discussion is useful as a summary of the burial customs and beliefs of the Barbadian slaves, but it must be emphasized that nearly every statement of any significance is based on traditional literary sources. The sole exception is the finding that 95 percent of the corpses were positioned on an east-west axis with the head pointing east. The authors do not know what this means but conjecture that since Africa lies east of Barbados the practice may be

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indicative of the well-known fact that African slaves believed in the return of their souls to Africa after death. There was one other find. In "Burial 9" was found the only case of a body buried in a prone position. It is all very mysterious and tantalizing and after a search of the literature on West African mortuary rites our intrepid authors conclude that it must have been a witch. This is very interesting. In essence, what we learn from this chapter is that the funeral customs of the Barbadian slaves were almost identical to those of the other Caribbean societies. African culture influenced these customs but was more marked in the earlier period. In general features, however, by the late eighteenth century, this culture was not "wholly African, but was creolized" (p. 215).

Perhaps the work's main value lies in its background chapters. The overview of Barbadian economy, demography, and history (chap. 2) and the history of the Newton plantation provide us with useful macro- and micro-type data on Barbados, some of which (especially on the plantation level) were not previously available. Students of Barbadian slavery will have to be satisfied with this until Handler delivers his long-awaited major study. Patience is virtuous and a leisurely pace is a scholar's privilege as it is that of a master chef, but some admirers of Handler may well begin to feel that they have had their appetites sufficiently whetted. When the entrée appears, it had better be good.

For those who are starving for something new and substantial on Barbadian slavery, one likely strategy is to raid the kitchen and consume the sources raw. This is exactly what Woodville K. Marshall has done in his splendid edition of *The Colthurst Journal*.

Between 1834 and 1838, Britain experimented with the apprenticeship system as a means of easing the transition from slavery to freedom for the planters and, so it was thought, for the slaves. The experiment, as is well known, was a disaster. The persons most centrally involved were the special magistrates whose task it was "to ensure that the ex-slaves provided 'a fair share of labour' for the estates and that the masters did not employ 'cruel or unjustifiable means' to obtain it" (p. 8). It was a difficult, indeed nearly impossible, task. The remarkable thing is that so many of these magistrates seemed to have made the best of a hopeless situation and performed their tasks reasonably. Largely, this was the result of their careful selection by the Colonial Office. John Bowen Colthurst was one of the most qualified of these special magistrates. A competent and proud ex-soldier, a failed farmer and a moderate abolitionist, he was a scion of the Anglo-Irish gentry who found a good opportunity to combine self-interest with moral integrity in the job of special magistrate.

The job was more than he had bargained for, but he rose to the occasion skillfully and firmly mediated among the "ignorance, irritation of temper and the tyrannical feelings of the masters," the "profound

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ignorance" and "extravagant expectations" of the apprentices, and the "abuse of enthusiastic abolitionists."

More important for us, he spent the last ten years of his life lovingly preparing his journal for publication. All but one lost section is reproduced in this volume. This is one of four such journals kept by special magistrates and, according to Marshall, "arguably the best of the four." The matter is indeed arguable: Madden's account is an absorbingly written, richly documented work, a masterpiece of this special subgenre of published work journals that remains my firm favorite. This, however, is not meant to detract from the enormous value of Colthurst's journal. Though stylistically derivative—Monk Lewis' influence is pronounced, as Marshall indicates—and at times a little too artful, the work will take its place on the required reading list of all students of West Indian history, alongside Lewis' *Journal of a West Indian Proprietor*, Mrs. Carmichael's *Domestic Manners and Social Condition of the White, Coloured and Negro Population of the West Indies, Lady Nugent's Journal,* and a handful of other such classics.

A journal such as this cannot be summarized for it gains its effect by the accumulation of closely observed details. We learn, for example, that some things have simply not changed in Barbados. The day after his arrival in November 1835 was a Sunday, and Colthurst's description of Bridgetown on the Sabbath is not significantly different from what I witnessed when I last visited the place ten years or so ago:

> After breakfast on Sunday morning, I walked out into the town, and found it an exceedingly nice one; but what struck me most forcibly was the order and quiet which prevailed. Scarcely a soul was to be seen in the streets, of any grade or colour. I must say I was greatly pleased with these decent appearances on the Sabbath morning. I afterwards went to one of the principal churches where at least 500 persons were assembled, of every colour in the rainbow, I believe, but azure. To an European eye, these tints among the multitude are exceedingly novel and curious, as if each tint belonged only to a person of some distant nation from that of the person next to him. I declare, these endless shades were sufficient to persuade a man that each had been sent as a specimen of the complexion of his countrymen far beyond the seas. However, I soon found this mixed multitude perfectly alike in one thing, which was, their profound attention to the service. Not a cough, and scarcely a move was made the whole time, and all seemed willing, by their example, to influence their neighbours properly.

But it is in his numerous descriptions of what he calls "the frieks and extravagances of slavery" that the journal is of most value. What comes out clearly is the sheer indignity of enslavement and the capriciousness and emotional viciousness of the master class. Perhaps the most telling instance of this was the planter who terrified and ridiculed two young apprentices then, when called for his defense, "he at once admitted all that was charged; that he considered *all this as a joke; that he was a funny man and liked these things,* and positively seemed to glory in what he had

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done'' (p. 70, emphasis in original). Another instance of this gratuitous humiliation of the slaves was the practice of shaving the heads of female apprentices (p. 100).

I mention these details not simply as a sample of Colthurst's writing but also to make a more general point: that if one's objective is to understand what slavery was really all about there is nothing to match this kind of data. The literary sources, carefully used, are incomparably superior to statistical data when one wants to reach across the decades since the abolition and, as Clifford Geertz would say, ''converse'' with the actors locked in the tragic drama that was slave society.

All students of West Indian slavery are indebted to Marshall for the superb job he has done in editing this journal. His introduction, which is based on considerable research, effectively sets the stage for the text. His short biography of Colthurst is a gem. Nothing more demonstrates the maturity of West Indian historical scholarship than the balanced, detached, and perspicacious assessment of Colthurst's life, opinions, and personality. The textual commentaries are highly informed, but never showy or pedantic; always, they aid our understanding of the text rather than intrude. Marshall confirms with this work his reputation as one of the most respected students of West Indian history.

To students of slavery whose knowledge of the institution is wholly confined to the Americas or, worse, one slave society in this area, the use of slaves as soldiers must always seem like a sociological aberration. And yet, from a world-wide perspective, the military use of slaves is hardly unusual.

In many primitive societies slaves were routinely used in military engagements and in certain tribes, such as the Toradja of the Central Celebes, the war chief on parties against other tribes was usually a slave. In imperial China and ancient Mesopotamia prisoners of war, after a temporary period of servitude, were frequently recruited into the army. In ancient Greece and Rome the practice was normally prohibited, but in times of crisis all scruples on the matter were quickly abandoned though the slaves so employed were usually manumitted. It was, however, in the Islamic world that military slavery was most widely practiced. Indeed it is generally accepted that without military slavery the rise and rapid expansion of Islamic civilization would simply not have been possible. Paul G. Forand in his dissertation on the Abbasid Caliphs, David Ayalon in numerous works on the Mamluks, and B. Papoulia in her work on the Janissaries have, along with many other scholars, fully demonstrated the cardinal role of military slavery in all the major Islamic polities. Indeed, Daniel Pipes has argued forcefully that the systematic recruitment of enslaved aliens for the sole purpose of training them for military service is a peculiarly Islamic practice (''From Mawla to Mamluk: The Origins of Islamic Slavery,'' Ph.D. dissertation,

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Harvard University, 1978). I have shown elsewhere that this claim is unsupportable (*Slavery and Social Death: A Comparative Study* [Cambridge, Mass.: Harvard University Press, 1982], chap. 10). Among the works that partly undermine this claim is Roger N. Buckley's *Slaves in Red Coats*, a work that Pipes read in manuscript and attempted to reconcile with his thesis, though not to my satisfaction.

Buckley's work is the first serious study of a neglected episode in the history of West Indian slavery. Between 1795 and 1808 Britain purchased some 13,400 slaves and formed them into a number of West India regiments. These slaves were overwhelmingly Africans rather than Creoles, and were used effectively not only in the nasty guerrilla wars between France and England between 1794 and 1798, but as a major bastion of support for the slave regimes. Ironically, the regiments were strongly, indeed violently, resisted by the local plantocrats, a collection of hysterical hot-heads who have gone down in history as possibly the most rapaciously short-sighted gang of fools who ever bungled themselves into the role of a ruling class. The metropolitan center not only forced through the innovation—indeed, as Buckley persuasively argues, it was an important element and symptom of the shift of power and control over the colonies from the peripheral elite back to the imperial center—but, characteristically, had the leading Jamaican planter newspaper paying tribute to the regiments by 1818.

And well they should, for without these regiments the entire course of imperial history might have been dramatically altered. White soldiers not only died like flies from the tropical "fevers," but their health, discipline, and effectiveness were also severely affected by their addiction to rum and their incapacity to survive on the local foods.

One valuable contribution of this work is its illumination of Pitt's ambiguous stand on the abolition of the slave trade. Outwardly opposed to it, Pitt apparently worked hard behind the scenes to prolong the trade. His reason, it turns out, was that the slave trade was considered vital for the recruitment of Africans to the West India regiments, the maintaining of which was, in turn, an essential element for victory over France. Only when, in a classically British political stroke, it was decided to secure new recruits for the regiments from the Africans taken as contraband from the slavers who continued in the slave trade was the way paved for the ostensibly virtuous act of abolition. Buckley rightly condemns Pitt as "a man who led a double political life. In deciding between principle and interest, Pitt, like many identically confronted politicos before and since, chose the latter" (p. 61). As the Rastafarian brethren of modern Jamaica would comment: "Seen!"

I cannot agree, though, with Buckley's claim that the imposed equality of treatment of black and white soldiers, the mass manumission of the recruits in 1807 after a period of legal confusion over their status,

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and the efficient performance of these black soldiers and officers had a demonstration effect on the West Indian whites, leading to an improvement in the white view of the blacks. The comparative literature on slavery lends no support for such a generalized effect. What the experience of other slave systems shows quite clearly is that a master class can easily distinguish among different categories of slaves and, having done so, treat one group well while it continues to treat the other brutally. Thus Buckley repeatedly refers to the possible demonstration effect of black officers leading pinioned white deserters through the streets: initial outrage he suggests may have given way to an acceptance of the role of slaves and blacks in positions of authority. Buckley ought to know that for centuries the only police in ancient Athens were slaves, Scythian archers who were apparently aggressively assiduous in the performance of their roles. The free Greeks fully accepted this and just as surely continued to view all slaves, especially barbarians such as the Scythians, with contempt. The contrasting experience of military and farm slaves all over Africa and the Middle East further supports my scepticism regarding the existence of any demonstration effect of these black soldiers. Human beings, not least of all a master class, have an infinite capacity for social divisions especially when they aid the promotion of conquest. The British learned well from their experience with the West Indian regiments, for these co-opted and relatively pampered agents of exploitation from among the oppressed became the model for the *askaris*, the native troops in Africa who were to form an important element in the colonization and underdevelopment of that continent.

Buckley complains about the "academic's neglect" of the men who made up these regiments and their descendants. The neglect is hardly surprising. The co-opted neither incite the moral fervor of the unambiguously oppressed, nor the creative outrage of the wicked and oppressive, nor the vicarious empathy of the revolutionary or heroic. We understand their plight for we see how, in systemic terms, they were as much victims as the masses they were co-opted to control, but we cannot fully restrain our contempt. We recognize their loyalty and, as was often the case, their courage in defending the interests of their lords, but we feel instinctively that their virtues were wasted, were indeed negated by the ends that they served. At best we pity them; at worst we despise them, even against our better judgment. They are essentially ambiguous both morally and socially, and scholars, no less than other human beings, have a natural tendency to avoid the ambiguous. It is to Buckley's credit that he has not. His work is competently written and reasonably well researched, although there is much primary data still to be explored on the subject. It fills a not unimportant gap in West Indian history, and should be of particular interest to those who study slavery in broad, comparative terms.

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The slave systems of the New World were, of course, made possible by the single greatest forced migration in the history of mankind: the Atlantic slave trade. With the exception of the U.S. after the late eighteenth century, all these systems continued to depend on the trade, in varying degrees, right up to the last decades of their existence. A growing body of literature is now devoted to this nefarious traffic and the works of Herbert S. Klein, *The Middle Passage: Comparative Studies in the Atlantic Slave Trade*, and Robert Louis Stein, *The French Slave Trade in the Eighteenth Century: An Old Regime Business*, are among the most recent contributions.

The study of the slave trade has focused on five main problems: (1) its history and character, especially its abolition; (2) the experience of those unfortunate Africans who were its victims; (3) its demography, especially estimates of its volume and demographic costs; (4) its commercial impact, in specific terms, its economic impact on Europe, the New World colonies, and Africa and, more generally, its relationship to the rise of capitalism; and (5) its sociopolitical impact on Europe, Africa, and the slave systems of the Americas.

Apart from the first set of problems, a great many questions still remain unanswered in the other four areas. And even on the first, there is still much that is obscure about the origins of the trade. Scholarship, especially British, has concentrated overwhelmingly on the last years and abolition of the trade and for the most disingenuous of reasons. As Eric Williams has observed with acerbic wit: "The British historians wrote almost as if Britain had introduced Negro slavery solely for the satisfaction of abolishing it." The French, Stein tells us, have been rather more discreet. Their abolition had no great movement that they could use to cloud the real issues. It was not only a messy affair dictated wholly by venality and incompetence, but the prevarication of the revolutionary governments, the treachery of Napoleon, and the manifest inhumanity of all made a mockery of the ideals of their revolution.

While both Stein and Klein touch on most of the issues mentioned above, the bulk of their works emphasizes only a select number. Stein concentrates on the French trade, but deals with a broader range of issues; Klein examines all the major trading nations except the Dutch, but deals in depth with only two of the issues. Of the two works, Stein's is the more readable and more satisfies the historical imagination, but Klein's must be judged the more valuable for students of slavery mainly for the wealth of primary and secondary data it analyzes. Stein begins with a good review of the history of the French trade followed by a discussion of the slaving triangle. One suspects he too readily accepts the traditional conception of this trade: recent studies have shown that the kind of ships used in transporting slaves and the seasonal timing of the colonial trade were such that the use of the same ships for both

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slaving and the transport of goods from the colonies to Europe was impractical. If we are to follow Stein, though, the French pursued the triangular model without deviation, perhaps because of the more centralized nature of their imperial system when compared with the British.

The last third of the work discusses the slaving business in France, its mode of operation, the kinds of persons engaged in it, and the significance of the trade for Nantes, Bordeaux, and France in general. Slaving in France was a very risky business, yielding high profits on a few ventures, but on the whole hardly seeming to be worth the trouble, if we are to take Stein's word for it. There was constant tension between the traders who saw the operation as a business and the planters who saw it as a service. The slavers had a chronic liquidity problem due partly to the reluctance or straight refusal of the planters to pay their debts. This made them highly dependent on the merchants and finance houses. While all this may well have been the case, I find Stein's discussion of the issue of profitability lacking in depth. It is hardly enlightening to be told that: "if the armateur could limit expenses and if the captain could deliver a large number of slaves, profits accrued. The lower the ratio between costs and the number of captives sold, the higher the probability of profits" (p. 145). Nor am I wholly persuaded by Stein's thesis that although there were modern elements in the operations and behavior of the traders, "their deep attachments to traditional forms of business organization, their striving for status, and their apparent apathy in noneconomic political matters, placed them in the tradition of medieval merchants more than of modern industrial capitalists" (pp. 201–2). Being conservative, striving for status, and lacking interest in "noneconomic political matters" would, I daresay, well describe the members of the board of every corporation on Fortune's list of top American firms. And in organizational terms the slave trade was the most destructively innovative operation of its day; in one sense it was capitalism in its most extreme and naked form with not just the labor power but the very bodies of human beings treated as commodities.

As a pioneering study of a neglected subject the work is useful but it is hardly likely to become "the standard work" on the subject as the publisher claims. We need to know much more on the economics of this wretched business and on its socioeconomic role in the development of French capitalism.

Klein's main concern is with the demographic, commercial, and organizational aspects of the trade. The Portuguese trade, especially in Brazil, is treated at greatest length, including the nineteenth-century internal slave trade in Brazil. This is followed by case studies of the British trade to Virginia and Jamaica, the French trade during the nineteenth century, and the Cuban trade between 1790 and 1843. His general introduction on the origins of the trade is rather weak. It offers nothing

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we do not know already, and, by concentrating wholly on demand factors, it neglects the important if controversial contribution of Gemery and Hogendorn that the extremely high elasticity of supply of African labor, itself "explicable largely by reference to the African coastal supply," partly induced the development of the Caribbean slave economies and with it the Atlantic slave trade (see H. A. Gemery and J. S. Hogendorn, "The Atlantic Slave Trade: A Tentative Economic Model," *Journal of African History* 6, no. 2 [1974]:223–46; "Elasticity of Slave Labor Supply and the Development of Slave Economies in the British Caribbean: The Seventeenth-Century Experience," in V. Rubin and A. Tuden, eds., *Comparative Perspectives on Slavery in New World Plantation Societies, vol. 292,* Annals of the New York Academy of Sciences [1977], pp. 72–83). The opening chapter also serves only to highlight a major omission in the work: its deliberate failure to deal with the Dutch trade. It is usually unfair to criticize an author for what he has not set out to do, but in this case one may legitimately complain for, as his cursory review in chapter one clearly indicates, the Dutch played a pivotal role in initiating and expanding the trade right up to the last decades of the seventeenth century.

The subsequent chapters vary in depth of treatment and originality of findings. The importance of the African-based merchants in the Angolan-Brazilian trade and the significance of the intraprovincial movements in the internal slave trade of Brazil during the nineteenth century stand out as the major findings on the Portuguese trade. The chapters on Virginia and Jamaica are useful collations of information already largely available to specialists except for the data on shipping tonnage and its relation to volume of slaves transported. Stein's work on the French trade complements rather than supercedes Klein's findings on this trade, and the chapter on Cuba is extremely useful.

The work is most valuable for what it tells us about the trade in general. First, there was an extraordinary similarity in the organization and functioning of the trade among all the European nations who engaged in it. Mortality rates were equally high among all nations, though there was a general decline in this rate over the course of the eighteenth century. A major finding is that "tight packing" was not, as is commonly thought, an important cause of high mortality but length of time at sea, the local health conditions at the point of embarcation in Africa, epidemics (especially dysentery), and the quality of food and water—the last factors being functions of the length of the voyage.

In attempting to relate his findings to more general themes in the study of New World slavery, Klein makes several questionable assertions. Noting that women were used coequally with men in the fields and that for this purpose the price differential between males and females was not as great as traditionally thought, Klein concludes that the

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greater number of males sold in the trade was not primarily the result of the planter's preference for them, but of supply conditons in Africa. The problem with this argument is that it begins by excluding nonfield slaves on the assumption that they constituted a small minority of the slave populations. But as the works of Craton and Higman, as well as others, have clearly demonstrated, such slaves constituted the majority and the strong preference for males in nonfield operations meant a strong demand pull in accounting for the greater number of males traded. Klein also misinterprets the significance of matrilineality and the role of women in traditional African agriculture in arguing that there was a greater tendency to sell males rather than females in these societies. In the first place, the vast majority of West African societies from which the slaves came were patrilineal and not matrilineal; in the second place, even if most of them were indeed matrilineal the claim that such societies are more likely to sell men rather than women is both empirically and theoretically insupportable. One point of agreement in the growing body of literature on indigenous African slavery is that the vast majority of captives traded were women. (See S. Miers and I. Kopytoff, eds., *Slavery in Africa* [Madison: University of Wisconsin Press, 1977]; C. Meillassout, *L'eslavage en Afrique précoloniale* [Paris: François Maspero, 1975]; and, for an excellent recent review of the literature, Paul E. Lovejoy, "Indigenous African Slavery," in Michael Craton, ed., *Roots and Branches: Current Directions in Slave Studies* [Elmsford, N.Y.: Pergamon Press, 1980 and in *Historical Reflections* 6, no. 1 (Summer 1979):19–61]). To be sure, it could be argued that the Africans might have been more inclined to keep their female captives and sell males because of the importance of women in production. This was true, for example, of the Vai during the nineteenth and early twentieth centuries. However we are here talking about an indigenous system of slavery in which the demand for slaves, of either sex, was small indeed and certainly insignificant compared with the demand on the coast; and we are also talking about the nineteenth century when the trade was over for most of the major European trading nations. Besides, the available data on Africa suggest that male slaves were as desired as females in most of the indigenous systems. The main reason why more women were traded was because they were much easier to capture. The large number of male captives on the coast then could only have been the result of the deliberate preference of the European traders, a preference which reflected that of the planters in the New World. It is unfortunate that Klein should have considered supply factors in precisely the area where they were the least important.

I am mystified by Klein's claim that "determining the specific origins of the African slave migrants to America is still a difficult task" (p. 247). A lot of good work has already been done on this issue employ-

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ing not only the linguistic and cultural data (which admittedly present problems) but the detailed micro-type data such as those studied by Craton, Higman, and, currently, Dunn, as well as the advertisements for runaways. Perhaps the major disappointment of the work is Klein's refusal to make a bold assault on the estimates arrived at by Philip Curtin in his *Atlantic Slave Trade*. Curtin's work was not based on original archival research but was merely a skillful collation of estimates taken mainly from modern and contemporary printed sources. It was a preliminary review, intended to generate research of the type conducted by Klein. It was clearly understood by Curtin that such research would result in drastic revisions of his estimates. I for one have always considered them far too low, an overreaction to the wild estimates in the earlier literature. But a most unfortunate situation threatens to develop in the current study of the slave trade. Instead of using Curtin's work as a point of departure, too many scholars have tended to accept his estimates as the final word.[7] The problem is compounded when, as in Klein's work, Curtin's estimates are used as the basis for the derivation of other estimates. A self-reinforcing pattern of confirmation emerges that is most unhealthy. Recognition should not be confused with reverence; history is not theology, even if, as I began by saying, it is a moral science.

NOTES

1. Incidentally, I was gratified to learn recently from Richard Dunn, who is now completing a study of Mesopotamia plantation in Jamaica which has even richer data in this regard than Worthy Park, that his work supports my thesis that Jamaican slave women deliberately restrained their childrearing capacities as a form of protest against the slave system.
2. For earlier works see: W. J. Gardner, *A History of Jamaica* (London, 1873); Mary Gaunt, *Where the Twain Meet* (London: John Murray, 1922); F. W. Pitman, "Slavery on the British West Indian Plantations in the Eighteenth Century, *Journal of Negro History* 11 (1926); Martha Beckwith, *Black Roadways* (Chapel Hill: University of North Carolina Press, 1929); Winfred M. Cousins, "Slave Family Life in the British Colonies, 1800–1834," *Sociological Review* 27 (1935):35–55. For more modern works see: Edward Brathwaite, *The Development of Creole Society in Jamaica, 1770–1820* (Oxford: Clarendon Press, 1971); Philip D. Curtin, *Two Jamaicas* (Cambridge, Mass.: Harvard University Press, 1968); Douglas Hall, "Slaves and Slavery in the British West Indies," *Social and Economic Studies* 11 (1962):305–18; B. W. Higman, "The Slave Family and Household in the British West Indies, 1800–1834," *Journal of Interdisciplinary History* 6 (1975):261–87; Fernando Henriques, *Jamaica* (London: MacGibbon and Kee, 1960); Sidney W. Mintz, *Caribbean Transformations* (Chicago: Aldine Publishers, 1974); Orlando Patterson, *The Sociology of Slavery* (London: MacGibbon and Kee, 1967); M. G. Smith, *The Plural Society in the British West Indies* (Berkeley: University of California Press, 1965).
3. In *The Sociology of Slavery* I pointed out that there was a *stereotype* of the typical slaves held by whites which the latter called Quashie. I noted that this stereotype was similar to the "Sambo" stereotype held by American slaveholders and was careful to note that this finding not only contradicts Stanley Elkins' claim that "Sambo" was a uniquely American stereotype, but, more importantly, was a part of the political

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psychology of oppression and not a description of how slaves actually were. Many critics of Elkins have cited my argument against him. Craton, amazingly, not only implies that I hold the view of Elkins that all slaves were in fact "quashies" but proceeds to attack this erroneous construction by the specious technique of showing that the seven slaves called Quashie on Worthy Park "demonstrated qualities of skill, adaptability and leadership" (p. 423, n. 10)!

4. On Jamaica see J. Stycos and K. Back, *The Control of Human Fertility in Jamaica* (Ithaca, N.Y.: Cornell University Press, 1964); Orlando Patterson, *The Condition of the Low Income Population in the Kingston Metropolitan Area* (Kingston: Government of Jamaica, 1973). On the Caribbean in general see R. T. Smith, "Culture and Social Structure in the Caribbean," *Comparative Studies in Society and History* 6, no. 1: M. G. Smith, *West Indian Family Structure* (Seattle: University of Washington Press, 1962).
5. See Edith Clarke, *My Mother Who Fathered Me: A Study of the Family in Three Selected Communities in Jamaica* (London: George Allen and Unwin, Ltd., 1957), especially chapters 4 and 5.
6. It was possible to assign the sex of only 39 of the 92 interments (p. 161 and appendix A). In desperation, the authors were sometimes reduced to reporting spurious correlations. Of what use, for example, could it be to even the most assiduous student of West Indian slavery to learn that "The Newton data suggest no relationship between dental pipe wear and the presence of whole pipes" (p. 165)?
7. For examples of archival studies revising these estimates—nearly all, incidentally, increasing Curtin's estimates—see the papers by Roger Anstey, Johannes Postma and E. Philip LeVeen as well as Curtin's response in part 1 of Stanley L. Engerman and Eugene D. Genovese, eds., *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Princeton, N.J.: Princeton University Press, 1975); and the collection edited by Henry A. Gemery and Jan S. Hogendorn, *The Uncommon Market: Essays in the Economic History of the Atlantic Slave Trade* (New York: Academic Press, 1979).

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