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The True & Tragical History of 'Time on the Cross'

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Slavery and the Numbers Game: A Critique of Time on the Cross
by Herbert G. Gutman
University of Illinois Press, 192 pp., \$2.95 (paper)

"A Symposium on Time on the Cross"
edited by Gary M. Walton
Explorations in Economic History, Vol. 12 pp.

Reckoning with Slavery: Critical Essays in the Quantitative History of American Negro Slavery
by Paul A. David, by Herbert G. Gutman, by Richard Sutch, by Peter Temin, by Gavin Wright, with an introduction by Kenneth M. Stampp
Oxford University Press, 352 pp., \$4.50 (to be published next spring) (paper)

Anyone who recalls the uncritical enthusiasm that greeted the publication of *Time on the Cross* a year and a half ago will be shocked by the three volumes of criticism under review. Their combined effect is devastating. A study of slavery that at first seemed exceptionally important, if contentious, now appears at least to be severely flawed and possibly not even worth further attention by serious scholars. This is hardly the fate one would have predicted for a book that the Harvard historian Stephan Thernstrom called "a remarkable achievement," "absolutely stunning, quite simply the most exciting and provocative book I've read in years." Or which inspired the Columbia economist Peter Passell, in his review for *The New York Times*, to declare: "If a more important book about American history has been published in the last decade, I don't know about it." It has, he said, "with one stroke turned around a whole field of interpretation and exposed the frailty of history done without science."

The enthusiasm of the book's initial reception and the intensity of the attack now being mounted against it leave one uncertain what questions to ask about *Time on the Cross*—should we ask how such an important book can be so severely flawed, in spite of its importance? Or how a book with such deep flaws could ever have been thought important? To understand these wildly contradictory judgments we must return to the puzzling book itself.

T*ime on the Cross* was written by two scholars trained primarily as economists, Robert W. Fogel and Stanley Engerman. The book advances two major themes. The first dramatically revises the history of slavery; the second is a polemic on behalf of scientific method in history.

Fogel and Engerman contend that slavery in the United States was far more successful economically and far less vicious in its impact on the personality and culture of blacks than most historians have thought. Their view contrasts sharply with the interpretations formulated by such conventional historians as Kenneth Stampp, Stanley Elkins, and Eugene Genovese. These scholars differ markedly among themselves, but each finds in the history of the peculiar institution ample grounds for both white guilt and black rage.

Although the authors of *Time on the Cross* grant the immorality of slavery, they depict it as a rational business enterprise in which the interests of master and slave often converged. Precisely because the master was a rational businessman and the slave his valuable property, there could exist no general incentive for abusive treatment. The authors condemn harsher views of slavery as a "perversion of the history of blacks" that serves to "corrode and poison" race relations by making it appear that blacks were deprived of all opportunities for cultural development for their first two and a half centuries on American soil. Like Genovese's *Roll, Jordan, Roll*, and most other recent contributions to the history of slavery, *Time on the Cross* is intended to soften the stark image of the concentration camp that Stanley Elkins so subtly superimposed over the plantation in *Slavery* (1959). But it overshoots the mark.

To support their own benign interpretation, the authors present masses of computer-digested quantitative evidence purporting to show, for example, that slaves were rarely whipped, seldom sold, and usually able to maintain a stable family life. Food, housing, and medical care, they claim, were good by contemporary standards. Under these favorable conditions slaves became imbued with the work ethic and adopted the prudish sexual mores of the Victorian era. The typical slave as represented by *Time on the Cross* is reminiscent of a Horatio Alger figure in his eagerness to rise from field hand to artisan to slave driver or even overseer. These were the major steps in an occupational hierarchy within slave society that the authors describe as being remarkably well differentiated and open to talent.

Inspired less by the whip than by positive inducements and the values favoring achievement they had internalized, the typical slave, according to *Time on the Cross*, worked so diligently that the South was 35 percent more efficient in agricultural production than the North. Without denying the injustice of slavery, the authors calculate that the economic benefits of the plantation's large scale and elaborate organization were so great that blacks actually received more income as slaves than they would have as free farmers. And although a share of their earnings was expropriated by the master, his share was only about 10 percent, "well within the modern tax rate on workers." The master's modest rate of expropriation did not prevent him from making a healthy profit or the South from being among the most economically advanced regions of the world.

In short, Fogel and Engerman contend that the slave experience is a fitting object of modern black pride—not merely to the limited extent that slaves resisted or evaded the coercions of the system, but more importantly because slavery was an unusually efficient productive system in which slaves willingly participated, feeling they had a genuine stake in its success.

How could previous historians of slavery have failed to see these things? How could the historical profession have so misled the American public about what is, after all, the most fateful chapter in the nation's past? The methodological argument of *Time on the Cross*, which at times threatened to dominate the book, emerged in response to this often repeated question, with its arch connotations of disciplinary competition and institutional rivalry. The answer was simple: previous students of slavery, Fogel and Engerman said, exaggerated the severity of the system, in part out of neo-abolitionist sentimentality and in part out of a covert racism that refuses to attribute the work ethic to non-whites. But mainly their methods failed them. They erred because they employed the unscientific methods of conventional historical scholarship.

The authors of *Time on the Cross* are proponents—missionary is the more apt word in Fogel's case—of our generation's version of scientific history, known as "econometric history," or the "new economic history," or, more broadly, "cliometrics." As exponents of the cliometric approach, Fogel and Engerman characteristically (though not uniformly, as we shall see) disdain impressionistic judgments based on diaries, correspondence, newspaper accounts, and other merely literary sources that are the mainstay of conventional history. Instead they and dozens of their research assistants scoured the country to gather information on slave life from manuscript census returns, probate records, and other sources of quantitative data. These data were then fed into computers and analyzed by means of sophisticated statistical techniques and the elegant mathematical equations of modern econometric theory. The authors claim to have amassed more data on slavery and dealt with it in a more scientific manner than any previous investigator.

Appearances to the contrary, the principal feature of the cliometric approach is neither its preference for hard, quantitative data nor its reliance on computer technology and mathematics. Rather, it is the effort to specify explicitly the usually implicit assumptions about causation that underlie and make possible any explanation of human behavior. When formally stated and systematized, these theoretical assumptions constitute the cliometrician's "model," and in the ideal case the model should be clear enough to express in the perfectly unambiguous form of an algebraic equation. The mathematical aspect of the method derives from this effort to specify assumptions rigorously. [1](#)

Conventional historians tend to dismiss all this painstaking specification of assumptions as misplaced precision, or, worse, a futile aping of scientific method. If a cliometrician were to write the history of the crucifixion, according to a current historical joke, he would begin by counting the nails. But the cliometricians are on strong

ground when they reply that conventional historians often get away with fuzzy thinking by leaving their theoretical assumptions implicit, or, as is often the case, simply unexamined.

On the surface, cliometrics is an austere and rigorous discipline that minimizes the significance of any statement that cannot be reduced to a clear empirical test ("operationalized"). But beneath the surface one often finds startling flights of conjecture, so daring that even the most woolly-minded humanist might gasp with envy.

The soft, licentious side of cliometrics derives, paradoxically, from its reliance on mathematical equations. Before the cliometrician can use his equation to explain the past, he must assign an empirical value to each of its terms, even if the relevant empirical data have not been preserved or were never recorded. When an incomplete historical record fails—as it often does—to supply the figures that the cliometrician's equations require him to have, it is considered fair play to resort to *estimation*, just so long as he specifies the assumptions underlying his estimates. And although cliometrics requires that these and all other assumptions be made explicit, it sets no limits at all on the *number* of assumptions one may make, or how high contingent assumptions may be piled on top of each other—just so they are explicit.

Pyramiding assumptions in this manner is an art-within-a-science whose finest practitioners achieve brilliant results without forgetting the ethereal nature of the medium in which they work. But inherent in the art is a temptation to see how high one's assumptions can be stacked before they begin to topple. That temptation brings out the daredevil in all cliometricians occasionally, and the Evel Knievel of the profession is Robert Fogel. His now classic "counterfactual" study of the railroad perfectly exemplifies the daredevil impulse that lurks just beneath the austere surface of cliometrics. It is precisely this impulse that has gotten *Time on the Cross* in such deep trouble.

Fogel's aim in *Railroads and American Economic Growth* (1964) was austere: to test empirically the widespread notion that the coming of the railroad contributed greatly to the industrial growth of the United States. But to carry out this aim Fogel shifted into his daredevil mood: he concocted a railroadless United States in 1890. After all, to say that the railroad helped to cause economic growth is to say that if the railroad had *not* existed, economic growth would have been retarded. So Fogel built a counterfactual world by estimation: he estimated how much it would have cost in the absence of railroads to expand the canal system to its maximum potential; how much more shippers would have paid if they had been forced to rely on wagons and boats alone; how much income would have been forgone by steel manufacturers and other suppliers of the railroad if they had lost their best customer—and so on at great length.

All historians bemoan the incompleteness of the historical record, but Fogel's problem was quite different: he needed data that had never existed and could only be estimated. This is a game for which the rules have yet to be written. Each estimate introduced a new chain of assumptions—all explicit, of course, to anyone who cared to burrow through them. The difficulties of making reliable estimates were compounded by his choice of the year in which to test the railroad's impact, 1890, six decades after the railroad's introduction, when the entire society had adjusted to its presence in the most subtle ways. No surgeon struggling to excise a malignant growth from a living body ever faced so difficult a task.

The result of this painstaking projection was a fabulous pyramid of assumptions and estimates that was plausible (as well as highly debatable) in its parts, but exceedingly fragile as a whole. Fogel crowned his careful conjectures with the conclusion that the railroad contributed less than 5 percent of the gross national product in 1890. Thus he rejected the "myth" of the railroad's indispensability to economic growth. Out of a series of conjectures, it seemed, he made an argument of empirically warranted scientific exactitude.^[2]

Although *Time on the Cross* is not a counterfactual exercise, it relies on chains of assumption and estimation that are no less daring than the ones Fogel employed in the railroad study. But there is a difference. In the railroad study the conjectural elements of the work are plainly exposed to view, as cliometric doctrine requires. In *Time on the Cross* they are hidden from all but specialists. Most readers of *Time on the Cross* see only the silk purse of apparent scientific exactitude; the authors spared them the sight of the sow's ear from which it all came.

This neat trick the authors accomplished by splitting *Time on the Cross* into two volumes, one for general readers, the other for specialists. Volume one is written in a brisk, declarative style that achieves terrific argumentative force by eschewing the tedious tasks of qualification and substantiation. It has no footnotes, much less extensive specification of assumptions. It "announces" findings in the way an astronomer might announce the discovery of a new planet, and it heaps scorn on previous interpretations, especially those of Kenneth Stampp of Berkeley, as if they were astrological gibberish written by men unequipped with a telescope. But the tiresome qualifications, discussion of evidence, and specification of assumptions—the things that give cliometrics its rigor—were relegated to volume two.

Or at least they were supposed to be. Volume two is indeed jammed with tables and equations and cryptic descriptions of procedure, but in fact it often fails to substantiate the story told so simply in volume one. One cannot even find in volume two the citations for quotations presented in volume one. The authors have been apologetic about this failure in documentation which they explain as a result of the haste with which volume two had to be thrown together at the last minute—they originally planned to publish the undocumented popular volume alone!^[3]

Of course the authors' motive in adopting the unprecedented two-volume format was not to deceive, but to popularize. They evidently believed that their topic was so sensitive and their reinterpretation of it so dramatic that the book might be used to advertise cliometrics, bringing to public attention for the first time a discipline which is in constant need of money for computer time and research assistants, but whose literary products the average undergraduate is not eager or able to read. Fogel did not hide this motive: he told a reporter, "We're using the book like a red flag to get attention."^[4]

The same aim was pursued after publication by arranging for the first review to appear in the *Wall Street Journal*, by setting up a debate between author Fogel and black psychologist Kenneth Clark on the "Today" show, and by arranging numerous interviews. It was no accident, one supposes, that the starry-eyed *Time* magazine reviewer came away from his interview believing that "traditional 'impressionistic' historians persistently wrote about American slavery in delusive and polemical stereotypes," and that "historians who do not have these [cliometric] tools could grope for another hundred years in subjective confusion and never be able even to evaluate or rebut the work of the cliometricians." Popularization made *Time on the Cross* a weapon of methodological and institutional rivalry.

Now a year and a half has passed since the publication of *Time on the Cross*. No one is surprised that the book has generated controversy, but what is startling and ironic in view of the authors' aims is the identity of the book's critics. *Time on the Cross* is being torn apart by cliometricians. The conventional historians to whom Fogel and Engerman threw down the gauntlet have hardly been able to get their word in. That this would be the likely pattern of the book's downfall became apparent late last October at a much publicized conference held at the University of Rochester.

Most of the leading scholars in the field of slavery and many prominent historians, economists, and sociologists in other fields—nearly one hundred participants in all—gathered at Rochester to subject *Time on the Cross* to careful scrutiny. Participants included Eugene Genovese (who as chairman of the Rochester history department was co-organizer of the conference with Engerman), Kenneth Stampp, Stanley Elkins, David Brion Davis, C. Vann Woodward, Winthrop Jordan, Oscar Handlin, Albert Fishlow, Peter Laslett, and R.M. Hartwell. Few black scholars attended because the conference coincided with the meeting of the Association for the Study of Afro-American Life and History in Philadelphia.

Whatever the ultimate fate of *Time on the Cross*, Fogel and Engerman will be justified in feeling a certain pride in the Rochester conference. For a single study to rivet the attention of an entire field of scholarship is rare enough, and it is even more astonishing considering that *Time on the Cross* had been published only six months before.

But after paying *Time on the Cross* the scholar's highest tribute of undivided attention, a great many of those who attended the Rochester conference felt ready to consign the book to the outermost ring of the scholar's hell, obscurity. Rarely has such an important book proved so vulnerable, not only to attack—the best books provoke

attack—but to dismissal.

Fogel and Engerman set out to save history from the historians, but at Rochester they were waylaid by a campaign to save cliometrics from Fogel and Engerman. The great *Methodenstreit* that the authors had hoped to generate was a bust, because *Time on the Cross*, judged on its own premises, proved to be too severely flawed to sustain any sort of profound controversy. At Rochester serious methodological questions were crowded offstage by such prosaic queries as "Have Fogel and Engerman gotten their facts straight?" and "Can this formula even in principle answer the questions they ask of it?" The conventional historians generally sat in bemused silence as the most telling blows were struck by Fogel and Engerman's cliometric colleagues.

The most devastating criticisms leveled against *Time on the Cross* at Rochester are now reproduced in the three volumes under review. All but one of the contributors to these volumes were participants at Rochester and two of the essays (by Richard Sutch and Gavin Wright) were originally prepared for the conference. Of the seven contributors, five are trained econometric historians whose professional standing is as high as that of Fogel and Engerman. The other two contributors are Kenneth Stampp, author of *The Peculiar Institution*, probably the most widely read history of slavery, and Herbert Gutman, whose two-volume history of the black family is scheduled for publication next year and whose work uses much quantitative data even though he is not known as a cliometrician.

Gutman's book, *Slavery and the Numbers Game*, first appeared last winter as an issue of the *Journal of Negro History*.^[5] It shows some signs of hasty and angry preparation, but it inflicts great damage on the nontheoretical parts of *Time on the Cross*, those concerned with labor incentives, slave work habits, sexual mores, slave sales, and the slave family. It is all the more striking because Gutman shares Fogel and Engerman's aim of illuminating the "record of black achievement under adversity." Yet he concludes, shockingly, that *Time on the Cross* "really tells us nothing of importance about the beliefs and behavior of enslaved Afro-Americans."

"A Symposium on *Time on the Cross*" is a special issue of *Explorations in Economic History*, one of two major organs of the new economic history. The most important piece in the collection, by the Berkeley econometrician Richard Sutch, is a lengthy criticism—almost as long as Gutman's—of Fogel and Engerman's novel claims about the treatment of slaves. Sutch's paper was the sensation of the Rochester conference because it exposes, as we will see, blunders so obvious that they would make a sophomore blush with shame. Sutch's conclusion is as damning as Gutman's: "*Time on the Cross* is a failure."

Reckoning With Slavery is an imposing collaborative effort which consolidates the closely related criticisms of Gutman and Sutch and brings them together with the more theoretical econometric work of Paul David of Stanford, Peter Temin of MIT, and Gavin Wright of the University of Michigan. David and Temin, in two jointly written essays, expose many conceptual confusions in *Time on the Cross* and also do much to clarify the inherent limits of cliometric method and its proper relationship to conventional history.^[6] They predict that *Time on the Cross* "will be remembered as an unsatisfactory and profoundly disappointing book...[because it] pressed behaviorist social science to its natural limits, and then beyond."

Gavin Wright's important essay in *Reckoning With Slavery* goes beyond the particular failings of *Time on the Cross* to locate the real cause of the prosperity and apparent efficiency of the southern economy in consumer demand for cotton, rather than the virtues of slave labor cited by Fogel and Engerman. Wright contends that the booming demand for antebellum cotton was an "inherently impermanent foundation" for prosperity which, in fact, collapsed in the 1860s, for reasons essentially unrelated to the Civil War. His rigorously cliometric evidence undermines *Time on the Cross* at many points and injects new life into the old thesis, especially dear to Eugene Genovese, that the slave regime was destined to face a severe economic crisis even without the Civil War.^[7]

The authors of *Reckoning with Slavery* showed their respect for conventional historical scholarship by inviting Kenneth Stampp to write the introduction to their volume. He admires some cliometric work and concedes that his book on slavery would have benefited from computer analysis of some of the data; but he remains skeptical of any methodological approach that is intolerant of the ambiguities of human experience.

To convey in a short space the reasons for the disenchantment felt by those who participated in the Rochester

conference, we can begin with a sampling of the most egregious blunders made in *Time on the Cross* and then turn to the fragility of the book's central argument, the claim that slaves were more efficient workers than free men. Although many of Fogel and Engerman's errors are attributable to nothing but carelessness, others suggest an extreme overindulgence in the heady art of pyramiding assumptions.

Consider first Fogel and Engerman's discovery that "the houses of slaves compared well with the housing of free workers in the antebellum era.... The 'typical' slave cabin of the late antebellum era probably contained more sleeping space per person than was available to most of New York City's workers half a century later." Upon trying to reproduce this finding, Sutch discovered that it was based on a comparison of whole slave cabins to the bedrooms of workers' tenements. It is also of passing interest that the free worker's living space was measured in 1893, not just a random year, but at the lowest point of one of the country's worst depressions prior to the 1930s.

Far worse, the authors exaggerated the size of the average slave cabin, according to their own sources, by about 50 percent. They also understated the size of the average worker's tenement (bedroom) by presenting figures drawn from a study that expressly set out to find the very worst slums in all of New York City. Using data drawn from neighborhoods containing the most downtrodden 1.8 percent of the city's population, Fogel and Engerman concluded that slave dwellings compared favorably with those of "most of New York City's workers."^[8]

Another error that caused much discussion at Rochester was Fogel and Engerman's contention, which came as a great surprise to historians of the South, that most plantation overseers were black slaves. This is an important point for the authors because the black overseer neatly symbolized their theme of an essential convergence of interests between master and slave, and also strengthened their portrait of a full occupational hierarchy within slave society by showing that there was room for slaves even at the top. When the shouting was over it was clear that *Time on the Cross* had gone astray again. First, the authors had virtually no positive evidence about the racial identity of overseers. They surmised that at least 70 percent of overseers were slaves on the basis of strictly negative evidence: if no free overseer was listed for a plantation in their sample of census data, they leaped to the conclusion that (1) there must have been an overseer, and (2) he must have been a slave. They discounted the likelihood, especially great on small holdings, that the master or his sons supervised the plantation. They ignored other census data in which thousands of free whites listed their occupation as "overseer."

Still worse, the authors failed to recognize that the census data they used did not even record the presence of an overseer unless he happened to live in the same *house* as the plantation owner—hardly the usual arrangement. The authors conceded at Rochester that their estimate of the number of white overseers needs to be raised by about 100 percent, but even this radically revised estimate is ephemeral, because it is still based on strained assumptions about negative evidence.^[9]

Fogel and Engerman's unfamiliarity with the pitfalls awaiting users of nineteenth-century census data is reflected in many more of their claims, for instance that there were no slave prostitutes in Nashville in 1860. They adduced this arcane fact to support their larger thesis that masters were not likely to exploit their female slaves sexually. But Gutman shows that their source was a census that recorded the occupations only of the free population, so even if the census taker had been surrounded by slave prostitutes he would not have recorded their presence.

Even when Fogel and Engerman have their facts straight, their deceptive presentation breeds distrust. Sutch discovered through correspondence with the authors that their sweepingly revisionist portrait of an elaborate occupational hierarchy within slave society is based on information from only thirty plantations, all of which were located in a single, decidedly untypical sugar and rice parish of Louisiana. Gutman argues on the basis of a larger collection of data that Fogel and Engerman overestimated the number of slave artisans by at least half, and perhaps much more. Of 20,576 blacks who enlisted in the Union Army of Kentucky, for example, only 1.6 percent identified themselves as artisans, as contrasted with Fogel and Engerman's estimated average of 15.4 percent.^[10]

Fogel and Engerman published an imposing bar graph purporting to show the "prudishness" of slave morality by the high age of slave mothers when they first gave birth. The graph proves nothing of the kind. Actually it is based on the mother's age at the birth of her first *surviving* child, a very different figure in an age of high infant mortality. Their calculation also disregards the possibility that some children were separated from their mothers,

or simply not listed with their mothers in plantation records once they reached a certain age. Under pressure from Sutch, Gutman, Peter Laslett, and Edward Shorter, Fogel seemed to concede on the final day of the Rochester conference that the average age-at-first-birth of American slaves is one of the lowest of known populations—yet he still professed to see in the data evidence of "prudishness." But of course it could be argued that the authors commit an even more elementary error by equating high morality with the observance of a taboo on adolescent intercourse.^[11]

Sometimes Fogel and Engerman cast statistical data in misleading form. Readers cannot help but be impressed with the infrequency of slave sales when told that only 1.92 percent of all slaves were sold each year. But the authors, being mathematically adept, must have known—and should have told their readers—that the same data mean that in the course of a thirty-five-year lifetime the average slave had a fifty-fifty chance of being sold at least once himself and was likely to witness the sale of at least eleven members of his immediate family (parents, siblings, spouse, children).^[12] Expressed in this form, the figures take on a different meaning.

Similarly, as Gutman points out, readers of *Time on the Cross* are inclined toward a benign view of slavery when they read that the average slave on the Barrow plantation received only 0.7 whippings per year. In the first place the figure is too low because it is based on an erroneous count both of the number of slaves Barrow owned and the number of times he whipped them. But more important, the figure is not the most relevant measure of the importance of whippings. A whipping, like a lynching, is an instrument of social discipline intended to impress not only the immediate victim but all who see or hear about the event. The relevant question is "How often did Barrow's slaves see one of their number whipped?"—to which the answer is every four and a half days. Again, the form in which the figures are expressed controls their meaning. If one expressed the rate of lynchings in the same form Fogel and Engerman chose for whippings, it would turn out that in 1893 there were only about 0.00002 lynchings per black per year. But obviously this way of expressing the data would cause the reader utterly to misunderstand the historical significance of the 155 Negro lynchings that occurred in 1893.^[13]

Anyone who cares to see how outrageous Fogel and Engerman's pyramiding of assumptions can become ought to read Sutch and Gutman's exposé of their treatment of the slave trade and its impact on the slave family. The chain of aberrant reasoning in *Time on the Cross* is too long and convoluted to reproduce entirely, but some of its weakest links may be noted. Fogel and Engerman claim that of the multitudes of slaves who were carried from the Old South to the fertile cotton lands of the New South in the period 1820-1860, only 16 percent were conveyed by means of the slave trade—i.e., sold on the open market—the vast majority instead moving with their owners in whole plantations.

Narrowing their attention to the small (16 percent) proportion of all migrating slaves who were sold on the open market, Fogel and Engerman further claim that the vast majority of these were unmarried adults—so their sale imposed no great strain on the integrity of slave families. Indeed, the authors calculate, on the basis of what appears in volume one to be direct evidence from New Orleans slave market records, that no more than 13 percent of interregional slave sales broke marriages. Combining these two "small" proportions, they conclude that "it is probable that [only] about 2 percent of the marriages of slaves involved in the westward trek were destroyed by the process of migration."^[14] Clearly then, even during one of the world's largest forced migrations, masters managed to protect the integrity of the slave family.

The basis for this conclusion is a web of assumptions that are individually implausible and cumulatively preposterous. In the first place slave "marriages" had no legal standing. The bills of sale in the New Orleans slave market do not record the marital status of the slave sold. How, then, could Fogel and Engerman determine that only 13 percent of sales broke marriages? By assuming that all men sold alone were unmarried. By assuming that all women sold alone were unmarried. And by assuming that only those women who were sold with children but without a man represent marriages broken up by sales. These assumptions, unwarranted in themselves, rest on the further assumption that masters were highly averse to separating mothers from children. No empirical evidence is offered for this assumption which, in fact, begs the question since the master's attitude toward the slave family is one of the questions at issue.

We have not yet reached the muddy bottom of this pyramid. Underlying their calculation of marriages broken

by sale is Fogel and Engerman's earlier calculation of the proportion of migrating slaves who entered the slave trade in the first place—16 percent. Neither volume of *Time on the Cross* tries to explain how this figure was calculated. By writing to Engerman, Sutch discovered its origins: Fogel and Engerman do not really know what proportion of migrating slaves entered the slave trade throughout the South—their figure of 16 percent represents only one state, Maryland. Nor do they even have hard evidence for Maryland. They arrived at 16 percent by combining two estimates made by other scholars. These estimates do not jibe with each other, and Sutch could find no way to combine them to yield 16 percent, and had to conclude that this whole sorry business may rest on an error in long division.^[15]

Taken singly most of these blunders and distortions and unwarranted assumptions fall within the ordinary range of fallibility. Every scholar has made some mistakes. But it was the cumulative impact of these and numerous similar gaffes that left many Rochester conferees wondering if *Time on the Cross* deserved to be taken seriously at all.

The flaws of *Time on the Cross* are not confined to its parts but extend to its conceptual heart: the efficiency calculation. No finding raised more eyebrows than the dramatic claim that slaves, through their personal diligence and enthusiastic commitment to the work ethic, made southern agriculture 35 percent more efficient than the family farms of the North. My own nonspecialist's doubts about this contention, published in these pages last September, have been amply confirmed (and superseded in expertise and weight of evidence) by the work of a half-dozen economic historians.^[16]

Fogel and Engerman should have known from the beginning that any comparison of regional efficiency in the antebellum period was fraught with breathtaking difficulties. The basis for their comparison, a rather controversial economist's tool known as the "geometric index of total factor productivity," gives results whose interpretation is debatable in even the most conventional applications. The index is essentially nothing more than a ratio of output to input: it ranks as most efficient that region, or other economic entity, which achieves the highest output with the lowest inputs of capital, labor, and land. The fatal limitation of the index, given the uses to which Fogel and Engerman wished to put it, is that it measures output in market value, rather than physical units (contrary to the impression given in volume one of *Time on the Cross*). There is no escaping this limitation, for one cannot aggregate a total output composed of bales of cotton, bushels of peas, pounds of pork, etc., without reducing everything to dollar value.

Since the index is based on market value it reflects not only the performance of producers (which is what we have in mind when we talk about productive efficiency) but also the behavior of consumers, whose eagerness for the product helps to determine its market value. Consumer behavior is clearly irrelevant to productive efficiency and the index is misleading to the extent that it is influenced by this factor.^[17] In short, the index is sensitive to demand: if two producers organize their work in equally rational ways, work equally hard, and even produce equal amounts of physical output, the so-called "efficiency" index may nonetheless rank one producer more "efficient" than the other because his product is in greater demand. As David and Temin observe, this is not the accepted meaning of "efficiency."

Given the sensitivity of the index to demand and the heavy demand for the South's principal crop, cotton, the index by itself is utterly incapable of justifying the chief inference that Fogel and Engerman drew from it—that slaves must have been hard-working Horatio Alger types and their masters skilled scientific managers. Gavin Wright confirms that the efficiency gap has more to do with voracious consumer demand for cotton than with any Herculean feats of productivity by southern producers. He is at particular pains to show that the differing efficiency ratings Fogel and Engerman observed *within* the South, between slave and free farms, are attributable to the differing abilities to specialize in cotton rather than to the economies of scale or other virtues of slave labor cited by Fogel and Engerman.

The bias introduced by cotton demand is only the most obvious of the flaws in the efficiency calculation. Even apart from the inherent frailties of the index in this especially difficult application, Fogel and Engerman's use of it rests on some extremely dubious assumptions. The choice of 1860 as a typical year for measurement has been sharply questioned. So has the authors' proposition that an acre of northern farmland was on average 2.5 times better in

quality than southern farmland. This extraordinary assumption alone is enough to guarantee a finding of southern superiority in productivity.

The originator of the geometric version of the efficiency index, Evsey Domar of MIT, has in the past gone out of his way to condemn simplistic interpretations of its meaning. Indeed, he once declared that "however tempting, it would be just as well not to treat the Index as a measure of efficiency, or even as an approximation to it." [\[18\]](#) The authors of *Time on the Cross* heaved a visible sigh of relief at Rochester when Domar, in his remarks there, confined himself to a brief, cautious, and rather enigmatic statement. Instead of "flunking" his onetime student, Fogel, for misusing the index, Domar gave him "about a 'C'," as Peter Laslett wryly observed.

Domar did not defend the efficiency calculation as presented in *Time on the Cross*; on the contrary, he accepted the main criticism of it. But granting the frailty of the index, he insisted that rival arguments ought to be couched in its terms as long as economists have no better tool for the job. Many economists at Rochester preferred simply to concede that the index is not up to the task of comparing the antebellum North and South.

Lance Davis of the California Institute of Technology, a prominent cliometrician, singled out the efficiency calculation as the least plausible argument of a generally unpersuasive book. He estimated that Fogel and Engerman's chances of successfully defending the efficiency finding were about one in ten. This is a telling judgment from the man who introduced the term "New Economic History," who once called Fogel's railroad study a "great book," and who even crowned Fogel himself as "the best" of the cliometricians nine years ago. The efficiency calculation has been closely scrutinized not only by Davis, Wright, Temin, and Paul David, but also by Stanley Lebergott of Wesleyan, Harold Woodman of Purdue, Jay Mandle of Temple, and Frank B. Tipton, Jr. and Clarence E. Walker, both of Wesleyan. No one has a kind word to say for it. [\[19\]](#)

Friends of *Time on the Cross* have suggested that the authors might do well to try to ride out the storm by jettisoning the efficiency calculation altogether. But this is plainly impossible. Fogel explained to a stunned audience on the last day of the Rochester conference that the efficiency calculation is the propelling idea behind the whole book. The efficiency calculation was the initial discovery that led the authors to conceive of a book on slavery in the first place. The surprising superiority of slave to free labor is by their own account the puzzle they set out to solve, and all the other research presented in *Time on the Cross*—on slave treatment, diet, living conditions, etc.—was developed in the course of their effort to show how and why slave labor was so efficient. [\[20\]](#)

Moreover, the generous grants from the National Science Foundation which have supported Fogel and Engerman's vast research establishment for the past five years—grants totalling \$362,300—were allocated specifically for research on "Factor Productivity in American Agriculture"—i.e., efficiency. [\[21\]](#) The entire book and the ongoing research effort behind it, probably the largest and most costly enterprise in historical scholarship ever undertaken, rest squarely on what now appears to be the book's most vulnerable argument. Of course the raw data assembled by Fogel and Engerman with this grant money constitute a permanent acquisition for all scholars. Nonetheless it is staggering to think how much effort has already been expended—vainly—in hope of finding support for this exceedingly ephemeral calculation.

The foregoing discussion of criticism of *Time on the Cross*, it must be added, is incomplete. I have not touched on the extensive work that Sutch and others have devoted to overturning Fogel and Engerman's findings on slave nutrition and health. I have not discussed the withering attack that Vedder, David, and Temin have launched against Fogel and Engerman's findings on the rate of expropriation, or Gutman's use of the notorious Barrow diary to show that slaves were whipped precisely because they lacked an internalized "work ethic," or David and Temin's criticism of the moral obtuseness built into the questions that Fogel and Engerman asked. If Fogel and Engerman cleared themselves of all the charges I have discussed, they would raise our estimate of the quality of scholarship that went into *Time on the Cross*, but the book would still be shrouded in controversy.

Can *Time on the Cross* survive? So far the book and its authors are prospering, notwithstanding Fogel and Engerman's failure to respond convincingly to their critics. The book has already appeared in English, Spanish, and Italian editions. The trustees of Columbia University bestowed upon it the prestigious Bancroft Prize, albeit by a hesitant, divided vote. [\[22\]](#) Fogel will be visiting Pitt Professor at Cambridge during the academic year

1975-1976; upon his return to this country he will take up a new joint appointment in both economics and history at Harvard.

Even when the criticism discussed here reaches print and is fully disseminated one cannot predict that *Time on the Cross* will promptly disappear from view. There are at least two reasons. First, the authors are resourceful and talented scholars. They encouraged a vigorous debate by trying to make their assumptions explicit—they did try, I believe—and, as C. Vann Woodward said at Rochester, they deserve respect for exposing themselves to a degree of scrutiny from which no scholar could emerge unscathed. Moreover they repeatedly described *Time on the Cross* as an "interim report" on a continuing research project of which volumes three and four are already in preparation ("I wish I'd waited for volume four," was one response to this news at Rochester). In the long run Fogel and Engerman may salvage arguments that now appear lost. Even if they do not, these "interim reports" may serve the unintended function of decoys, exhausting the critics before the "final" report is made public.

The book may have a longer life than it deserves because of the awesome financial and institutional apparatus that stands behind it. *Time on the Cross* is a product of factory scholarship and we know what happens to artisans who compete with factories. The production line for *Time on the Cross* was subsidized by government grant money and manned by dozens of graduate research assistants who are now fiercely loyal to their company and its products. Without so much as a PhD to hide behind, they threw themselves into battle at Rochester, fearlessly controverting scholars twice their age and eminence. Can we be confident that the life or death of this book's ideas depend, as scholarly ideas should, solely upon their intellectual merit?

Eugene Genovese closed the Rochester conference with the careful opinion that *Time on the Cross* was a "creative failure." Given his Sisyphean view of scholarship it was, as he intended, a sincere compliment, though a fragile one. Earlier, when the book first came out, Genovese generously conceded that the authors "accomplished all that they should have dared hope: they have thrown the burden of proof back upon their opponents...." Now, though only a year and a half has passed, it is not too soon to suggest that the burden of proof has passed back again to Fogel and Engerman. And it is a far heavier burden, for now they must prove, before any of their particular contentions, that their book merits further scholarly attention.

Notes

[1] R. W. Fogel, "The Specification Problem in Economic History," *Journal of Economic History*, Vol. 27 (Sept. 1967), pp. 283-308.

[2] Fogel's railroad book stirred up a valuable discussion of the nature and role of causal attribution in historical scholarship, but his substantive conclusion that the railroad was dispensable is regarded with great suspicion by cliometricians. For a sampling of reservations and objections, see Paul A. David, "Transport Innovation and Economic Growth: Professor Fogel On and Off the Rails," *Economic History Review*, Vol. 22 (Dec. 1969), pp. 506-525; Meghnad Desai, "Some Issues in Econometric History," *Economic History Review*, Vol. 21 (April 1968), pp. 1-16; Stanley Lebergott, "United States Transport Advances and Externalities," *Journal of Economic History*, Vol. 26 (Dec. 1966), pp. 437-461; Harry N. Scheiber, "On the New Economic History and its Limitations: A Review Essay," *Agricultural History*, Vol. 41 (Oct. 1967), pp. 383-395; Peter D. McClelland, "Railroads, American Growth, and the New Economic History: A Critique," *Journal of Economic History*, Vol. 28 (March 1968), pp. 102-123; Alexander Gerschenkron, "The Discipline and I," *Journal of Economic History*, Vol. 27 (Dec. 1967), pp. 454-458.

[3] Fogel, "From the Marxists to the Mormons," *Times Literary Supplement* (June 13, 1975), p. 670.

[4] *New York Times* (May 2, 1974).

[5] Gutman, "The World Two Cliometricians Made: A Review-Essay of $F + E = T/C$," *Journal of Negro History*, Vol. 60 (Jan. 1975), pp. 53-227. The many typographical errors in this printing have been corrected in the book.

[6] The chapters by David and Temin are revisions of "Slavery: The Progressive Institution?" *Journal of Economic History*, Vol. 34 (Sept. 1974), pp. 739-783, and "Capitalist Masters, Bourgeois Slaves," *Journal of*

Interdisciplinary History, Vol. 5 (Winter 1975), pp. 445-457.

[7] A shorter version of Wright's paper appears in the *Explorations in Economic History* collection, as does a criticism by Richard Vedder of Fogel and Engerman's calculation of the rate of expropriation.

[8] *Time on the Cross*, Vol. I, p. 116; Sutch, "The Treatment Received by American Slaves," *Explorations in Economic History*, Vol. 12 (Fall 1975), pp. 25-30 in manuscript; *Reckoning With Slavery*, Chap. VI. Emphasis added.

[9] William K. Scarborough, "New Direction or False Direction?—A Critique of the 'New Economic History,'" Rochester conference paper; Gutman, *Slavery and the Numbers Game*, pp. 66-69; Sutch, "The Treatment Received by American Slaves," pp. 20-24 in manuscript; *Reckoning With Slavery*, Chap. II.

[10] Sutch, "The Treatment Received by American Slaves," pp. 17-20 in manuscript; Gutman, *Slavery and the Numbers Game*, pp. 69-77; *Reckoning With Slavery*, Chap. II.

[11] Gutman, *Slavery and the Numbers Game*, pp. 140-152; Sutch, "The Treatment Received by American Slaves," pp. 113-117 in manuscript; *Reckoning With Slavery*, chapter IV; Edward Shorter, "Protein, Puberty, and Premarital Sexuality: American Blacks vs. French Peasants," Rochester conference paper; Peter Laslett, "The Slave Family Household in the Old South," Rochester conference paper. Although the text adjacent to the bar graph says that the graph is based on first surviving children, the graph itself is mislabeled and Gutman shows (*ibid.*) that Fogel, in a widely delivered pre-publication paper, lost track of the distinction. Even if the graph was what it purports to be, Fogel and Engerman's interpretation of it would be odd: they claim it shows a "relative shortage of births in the late-teen ages," but actually it shows more births in this age category (15-19 years old) than any other, and no basis is given for comparison with other populations. (*Time on the Cross*, Vol. I, pp. 137-138.)

[12] Sutch, "The Treatment Received by American Slaves," p. 87; *Reckoning With Slavery*, Chap. III. Sutch also shows that Fogel and Engerman miscalculated the rate of slave sales, so the probabilities given here are too low.

[13] Gutman, *Slavery and the Numbers Game*, pp. 18-19. (Gutman miscalculates the rate of lynching but the point of his argument is entirely sound.) *Reckoning With Slavery*, Chap. II.

[14] *Time on the Cross*, Vol. I, p. 49.

[15] Sutch, "The Treatment Received by American Slaves," pp. 85-104 in manuscript; Gutman, *Slavery and the Numbers Game*, pp. 102-140; *Reckoning With Slavery*, Chap. III.

[16] "Were Slaves More Efficient? Some Doubts About 'Time on the Cross,'" *NYR* (Sept. 19, 1974), pp. 38-42.

[17] There is one narrowly circumscribed sense in which consumer behavior is relevant to producer efficiency: namely, the producer, in order to be efficient, must choose to produce those goods for which demand is greatest. We would not call "efficient" the entrepreneur who tries to manufacture and sell ice at the North Pole, no matter how much ice he produces, because there is no demand for his product. But responsiveness to demand is a proper criterion of producer efficiency only on the presupposition that competing producers are equally able to respond. We judge the antebellum Massachusetts farmer *unlucky* because his soil and climate were unsuitable for cotton—not *inefficient*.

[18] Domar, "On Total Productivity and All That," *Journal of Political Economy*, Vol. 70 (Dec. 1962), p. 559, cited by Harold Woodman, "The Old South and the New History," Rochester conference paper.

[19] David, "One Potato, Two Potato, Sweet Potato Pie: Clio Looks at Slavery and the South," Rochester conference paper; Stanley Lebergott, review of *Time on the Cross*, in *American Political Science Review*, Vol. 69 (June 1975), pp. 697-700; Harold Woodman, "The Old South and the New History," Rochester conference paper; Jay Mandle, "Strength and Growth in a Plantation Economy: An Appraisal of *Time on the Cross*," Rochester conference paper; Frank Tipton, Jr. and Clarence Walker, review of *Time on the Cross*, in *History and Theory*, Vol. 14 (Feb. 1975), pp.

91-121.

[20] Fogel, "From the Marxists to the Mormons," *Times Literary Supplement* (June 13, 1975), pp. 667-670.

[21] I am obliged to James H. Blackman, Program Director for Economics, National Science Foundation, for this information.

[22] The trustees said they were "not disposed to put aside" the recommendations of their anonymous award jury, but some of them felt compelled to announce that "...had the choice of prize winners been theirs alone, the decision with respect to...[*Time on the Cross*] would have been different or postponed pending further supporting evidence." A spokesman explained that the trustees believed that the book represented "thorough and honest scholarship," but thought the authors' methods "may require further study before full value can be placed on the book's findings." This misses the critics' point altogether, for it is not so much the method as the authors' abuse of it that is at issue. *The Washington Post* (May 2, 1975).

Letters

December 11, 1975: William N. Parker, [Funds for Clio](#)

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