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Two Appreciations

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Robert William Fogel: An Appreciation by a Coauthor and Colleague

Sometime in either late 1974 or 1975 I ran across a friend who had just seen a Hollywood musical. It was in the genre of the complications of song-writing partners for whom output required some joint contributions and interactions. This led him to wonder what scholarly work under similar circumstances was like, since both activities were done frequently by individuals but collaborations occurred with sufficient frequency that they were not unusual. In particular, he wondered how Bob and I had begun working together and what was the nature of the input on *Time on the Cross* and our dealing with the related conferences and responses. I doubt if I fully answered his queries—some things are more easily done than described—but, in reflecting on this encounter, certain aspects of our working together did come to mind.

Although Bob and I began collaborating in 1963, we had first begun exchanging ideas at Johns Hopkins soon after I arrived in 1958 to start graduate work in economics. Bob had already been there a year or so, having completed his master's degree at Columbia with Carter Goodrich after leaving his communist past behind in the mid-1950s. When we first met as graduate students, he was completing revisions on his book on the Union Pacific Railroad and starting work on his railroad book, on which I was a research assistant for several weeks, measuring distances of various counties from navigable waterways.

During my first year I had to make a presentation at the Journal Club, a monthly Hopkins graduate-student ritual. The discussion was to be based on a publication in a leading economics journal. After I was talked out of choos-

ing an article on the history of economic thought, I selected the Alfred Conrad and John Meyer article on antebellum slavery in the April 1958 issue of the *Journal of Political Economy.*[1] The ensuing discussions, both at the Journal Club and for weeks after among students and faculty, were lively, with Bob quite heavily involved. Indeed, the arguments led directly to a major publication by a fellow graduate student, Yasukichi Yasuba.[2] Given the impact of the Conrad and Meyer article in history and in economic history, both Bob and I maintained a strong residual interest in the subject, although it did not lead directly to research and publication for a number of years and only after several detours, including Bob's completion of his major work on the railroad.

Bob left Hopkins in 1960 to take a position at the University of Rochester. He soon signed a contract with W. W. Norton to write a text in American economic history. Our next contact came when Bob invited me to work with him on this project. Not soon after, Bob was asked to visit Chicago and, in a related turn of events, I was asked to visit Rochester for a year. Bob stayed at Chicago and I at Rochester. The work on the text moved ahead, but at a rather slow pace. The problem with writing what was meant to be an innovative text was our feeling that, while there was much new in the literature, many issues still seemed open. To get the thing done appropriately (or, rather, as we wanted it) would require much more research and writing, considerably more than we felt we could comfortably accomplish in a reasonable time.

We chose, as an interim step, to publish a collection of articles and papers on the "new economic history," and we called it *The Reinterpretation of American Economic History.* The readings were, for the most part, drawn from the existing literature, but we felt expansions in several areas would be useful. One, in particular, dealt with an analysis of the debates and issues on the economics of slavery that would complement the reprinting of the Conrad and Meyer and Yasuba articles. This, of course, opened up a range of questions and topics that seemed eminently researchable. Thus, when our National Science Foundation grant to study the antebellum iron industry expired, after the collection of primary data from various Pennsylvania archives and the publication of an article in the *Journal of Political Economy,* we decided to shift our research interests to the economics of slavery in the antebellum South, to gather additional data to answer questions posed in the earlier publications and to get at some important historical questions that had not yet received the attention they merited by economic historians.

The actual research and writing on slavery was aided by Bob's appointment to spend each fall semester at the University of Rochester. Rochester at this time was a particularly exciting place to be working on American slavery. Eugene Genovese and Herbert Gutman, in the history department, provided

1. Alfred H. Conrad and John R. Meyer, "The Economics of Slavery in the Ante-Bellum South," *Journal of Political Economy,* 66 (Apr. 1958), pp. 95–122.

2. Yasukichi Yasuba, "The Profitability and Viability of Plantation Slavery in the United States," *The Economic Studies Quarterly,* 12 (Sept. 1961), pp. 60–67.

much in the way of stimulation and interaction, while several members of the economics department, particularly Ronald Jones, Richard Rosett, James Friedman, Sherwin Rosen, and Lionel McKenzie, had interests, for varying reasons, in the application of economics to the study of historical issues. Being together with Bob, even for part of the year, made joint work considerably easier than it had been in our first attempt earlier in the 1960s. We used a then state-of-the-art technology (and one whose use Bob would expand upon)—the tape recorder—and, when not in the same place, exchanged long taped monologues, leaving, at one time, small mountains of tapes in various locales. Being at the same place cut down on the tapes but entailed other costs—including frequent working dinners at the nearest Ponderosa Steak House and a detailed knowledge of various short-order restaurants and diners near the University of Rochester. While we continued to use tapes in future years, we found that the telephone permitted better communicaton, although the bottleneck in exchanging written materials slowed our output in this era before Federal Express, overnight mail, fax, and bitnet. Given Bob's well-known sense of urgency, all this suggests the ability of people to adjust to constraints when necessary but also an eagerness to take advantage of change as it occurs. Bob became one of the earliest users of Federal Express that I am aware of.

Whatever the form of interaction with Bob, the first, and perhaps the most basic, feature was the sense of intellectual excitement. Scholarly work was a cumulative process—there were questions of detail to address, and also the need to discern what the impact would be for broader issues of interpretation and analysis. For example, we had early begun to collect data on slave prices by age and sex from probate inventories at various southern archives. The use of probates was, of course, suggested to us by one of Bob's more remarkable students, Alice Hanson Jones, whose thesis demonstrated their great usefulness in studying many historical problems. Our primary purpose, at first, was to get a set of slave prices by age and sex for use in a refined set of rate of return calculations, as well as to use in making adjustments to the labor force input measure for calculations of southern agricultural productivity. The probate record listings revealed patterns suggesting that slave family units were recognized. These listings often permitted calculations of family size and of differences in age between mothers and children.

Obviously none of this material was as simple to utilize as was initially hoped. But, as some early work indicated, and as the far more extensive collection and analysis of these and related data by Bob's student Richard Steckel have demonstrated, probate listings (and plantation records) have had a larger payoff than originally anticipated. Another example is the use of data on height by age, which were initially found in a source we were made aware of by a graduate student elsewhere. It was suggested that the material on the coastal shipping manifests would be useful in examining the internal slave trade, as they were, but they also opened up to us a line of research with other

broad implications. Such a benefit, alas, never accrued from the data on shoe size, another type of anthropometric data found in a number of the plantation records. James Trussell did locate a relevant article on foot length in *Human Biology* in 1944, but that has been the extent of our research on that subject.[3] The plantation records did have other unanticipated uses however. After a lunch conversation with Herbert Gutman we provided him with copies of slave lists from several plantations we found in various archives. These lists grouped slaves in family units or extended for long periods of time and included information on slave births, having the names of both mother and father. These listings were, as Herb generously acknowledged later, important to him in his examination of slave family and naming patterns.

At times the material we uncovered led in a different direction than we anticipated and from the then more widely held view. The next question was to understand why scholars, including ourselves, had looked at the issue differently. What was the basic evidence presented for these views—what sources and tests were undertaken—and how did this all relate to the broader views of the questions under discussion? There was never a sense that evidence would be found that would end debate. Rather, further examination was seen as essential, both to buttress arguments and also to understand, in as much depth as possible, the arguments of those with whom we seemed to be in disagreement (or, at times, agreement). All this involved Bob's considerable imagination and intellectual energy—a work pace of great intensity in order to deal simultaneously with the many facets of any one question, as well as with the many questions that were to be examined.

Working with Bob one understood that just about all questions were, if not answerable, at least approachable with some empirical data. The data need not be restricted to quantitative information, although many of the questions historians discuss are concerned simply with, How many? Data could also be drawn from various sources, given an interest in different questions. But, for Bob, if a question were asked, there must be some way to get an answer. And an answer that, if not fully convincing, at least moved the question (especially if framed and made in particular ways) one step further along—and it was best not to have merely one answer provided by just one approach or data source. As Bob was often reminded by his teacher Simon Kuznets, there should be several different ways to get at a problem. These ways should encompass different sources and methods, and only a multitude of approaches could make one feel confident with any answer. This belief led to a certain view concerning the nature and possibilities of using evidence. Presumably there was always too much potential evidence, and one could only deal with a limited part of it. Yet even if the magnitude of unutilized (and presently unutilizable) evidence was great, what was available meant that a "tentative" an-

3. Howard V. Meredith, "Human Foot Length from Embryo to Adult," *Human Biology,* 16 (Dec. 1944), pp. 207–82.

swer—tentative in that no one had yet utilized all the data that was possible to use—was possible.

This probing of evidence characterized Bob's efforts in at least two different ways. First, whenever a question was asked, he would start by determining the types of evidence that could be brought to bear on it. What was there in primary sources or in the secondary literature? Whom could we call to find out what types of records existed and what information was available? We were fortunate to work in the computer era, permitting extensive use of records that could previously be used only partially by scholars. Second, the collection of data fed on itself, opening up many new questions. In some cases it was the nature of material collected for one purpose that suggested answers to other questions; in others (as Bob describes in the *Cliometrics Newsletter*) there was a feeling that the data might be useful for other questions that could open up new lines of research. An example, as discussed above, was the case of the height data drawn from the coastal manifests of slave shipments, originally used in the study of the slave trade. In other cases, however, the payoff never materialized, but not because questions were left unasked and the potential unexamined.

There are two other characteristics that Bob revealed in this work. First, as a scholar he is truly interdisciplinary, with interests going well beyond those of an economist or historian but encompassing whatever seems relevant for the issues under study. Clearly an economist by training and inclination, as demonstrated in his approach to shaping questions and collecting evidence, he sought to present findings in their broadest interpretive light. Thus all questions were looked at for their relevance to broader themes and, also, whenever other disciplinary areas needed to be pursued to answer questions, this was to be undertaken. Second, Bob has a keen sense of scholarship as a collective discipline. This goes beyond the involvement with collaborative works and large-scale projects. The data collected for *Time on the Cross,* for example, were early made available to all scholars. And with all the criticisms and debates concerning *Time on the Cross*—debates that started early, continued long, and covered just about all issues (sometimes coming from rather opposite directions)—it remained Bob's belief that more evidence, more analysis, and more detailed specification in argument would help to clarify and resolve the disagreements—or at least some of them. The polemical, not just intellectual, tactics of responding to criticism led to problems (interesting, as well as amusing, in retrospect) because of the quite contrasting nature of some of the questions raised, making caution necessary regarding the expected interpretation of any response. But, at the least, it was believed that the questions raised in debate, even in disagreement, were ones that could (and should) be studied and examined, and that it was only by these steps that scholarly knowledge could be advanced.

There was one characteristic in the songwriter movies that remains in mind: that the collaboration, with all its hard work, ultimately was fun for those

involved. To ask questions, to search for evidence, to piece things together—all of this can, of course, be done by one person; but, sometimes, a joint effort goes beyond the mere intellectual satisfaction, and the act of collaboration provides its own independent stimulation and enjoyment. Perhaps this is why, over recent years, the magnitude of joint works by economists and also by historians has increased dramatically, although curiously, for those who might have predicted this on the basis of a combining of differing specializations and the division of labor, that explanation seems to account for only a small part of the observed increase. But that is a story, and movie genre, for another time and place.

Donald N. McCloskey

Robert William Fogel: An Appreciation by an Adopted Student

Professors must have had teachers who made a difference. After all, they decided to become teachers themselves rather than movie stars or big-game hunters. Counting from when I began studying economics, the teachers who have mattered most to me were Eric Gustafson, John R. Meyer, Alexander Gerschenkron, and Robert William Fogel. Officially I was not Fogel's student but his colleague, from 1968 when I arrived at Chicago as an assistant professor until 1975, when Fogel left for a sojourn at Cambridge and a new job at Harvard. Being Fogel's colleague, though, felt like being his student (a feeling reinforced by the students he had gathered in 1968, who were about my age but knew a lot more economics than I did). One could not be around the best historical economist since Schumpeter for any length of time without learning a lot, even if such a one were not a great teacher. But Fogel was, and is. Gladly would he learn and gladly teach.

Fogel's personal qualities smoothed the way and taught their own lessons. He is for one thing the soul of wit and warmth. Wit is common enough in academic life, and especially so among economists, irrationally proud of their quickness. William James called it the "Harvard indifference"—"the smoking of cigarettes and living on small sarcasms."[1] Warmth, however, is rare. The average academic applies small sarcasms indiscriminantly to his students and junior colleagues and certainly to his rivals. Fogel refrained from "applying" his wit to anyone. Anyone in Fogel's presence, from the cab driver waiting in front of the Quadrangle Club to the president of the university, gets treated

1. Quoted in Gerald E. Myers, *William James: His Life and Thought* (New Haven, 1986), p. 12.

with the same warmth, a warmth which is spiced—not poisoned—by his ready wit.

Fogel, in other words, is more of a democrat than most of us. He therefore does not commit the characteristic sin of academic life, sneering. In the twenty-odd years I have known him I have not seen him sneer; not once, despite his numerous opportunities. Fogel's personal and intellectual tolerance shames us all. I once complained to him about the rank favoritism that another senior economic historian exhibited in his hiring, disregarding merit in favor of his former students. Fogel laughed tolerantly: it is not the worst of sins, he said, to favor one's own. I once tried to persuade him at lunch that certain activities in mathematical economics, hostile to his empirical values, were not good for economics. No, said he: we cannot tell; the investment in today's existence theorem may pay off in the next century. Fogel can teach because he is willing to learn, from the least of us, ready to see merit in the misled, ready to attribute admirable motives to his enemies.

So the first thing he taught students and colleagues was a simple, democratic, even American openness. Openness is hard to learn. Judith Shklar has described snobbery as "the habit of making inequality hurt."[2] Snobbery and sneering are anti-democratic vices, and American democracy has always been uncomfortable with scholarship. But in Robert Fogel's case, being a superior scholar does not entail making the inequality hurt.

His easy relations with students and junior colleagues were something new for me. Fogel and his wife, Enid, took the social responsibilities of academic leadership seriously. My supervisor, Gerschenkron, had been amusing and courteous in a European way but no drinking buddy with junior faculty and graduate students. At Chicago circa 1968, however, drinking with intellectual buddies was the style, the most serious teachers in this line being Al Harberger, Bob Mundell, Harry Johnson, and Bob Fogel. Fogel would meet the students and faculty after the weekly economic history workshop for beer and too many bowls of potato chips at the Quadrangle Club; he would pick up the bill when the last student tottered home; then he would walk up 57th Street with Harry Johnson to the apartment.

The talk at the table was economics. Students learned economics personally, by discussing real economic institutions with a first-rate economist. We never talked about sports, seldom about public or academic politics. The questions were, What do you make of this or that economic argument? Does it fit the historical evidence? What kind of evidence? How would you get it? How do you know?

The evidence Fogel favored, of course, was quantitative. He approved of the remark by Lord Kelvin, slightly misquoted in the stones of the Social Science Building at Chicago: "When you cannot measure it, when you cannot

2. Judith N. Shklar, *Ordinary Vices* (Cambridge, Mass., 1984), p. 87.

express it in numbers, your knowledge is of a meagre and unsatisfactory kind. . . . It may be the beginning of knowledge, but you have scarcely in your thoughts advanced to the stage of *science*."[3]

Even in conversations outside of class Fogel pursued quantitative science. He pursued it with algebra, not geometry. Fogel believed that when one could not express an economic argument in algebra, your knowledge was of a meagre and unsatisfactory kind. Many lunchtime hours in the solarium of the Quad Club were spent in communal attempts to convert someone's geometrical or verbal argument into algebra, and this for two reasons. First, Fogel thinks algebraically. He will not believe a proposition until it has been put through his algebraic tortures, complete with cunning asterisks, subtle subscripts, and mind-stunning tables of variables.[4]

Second, if one is going to do more than speculate on the direction of effects, you need the algebra, because only then can you use actual measurements. Here was the great principle: measure, then measure again, then measure still again. Fogel is like a carpenter of history, spending as much time in measuring and remeasuring as in sawing or hammering: measure twice, cut once. He agrees with John Clapham, the first holder of a chair of economic history at Cambridge: "every economic historian should . . . have acquired what might be called the statistical sense, the habit of asking in relation to any institution, group or movement the questions: how large? how long? how often? how representative?"[5] Substitute "social scientist" for "economic historian" and add to "institution, group or movement" the phrase "alleged explanation," and you have Fogel's procedure exactly.

The procedure meant that as little as possible was left to blackboard speculation. Other economists might be content to note the *likelihood* that social savings of railroads were small, the *possibility* that economies of scale in sectors served by railroads were large, the *existence* of miscegenation among slaves and masters, the *presence* of nutritional effects in death rates. Fogel insisted on measuring them. As he wrote a few years ago about declining mortality since 1700, "the debate . . . revealed that the critical differences were quantitative rather than qualitative."[6] The "debate," a favorite Fogel word, was always "revealing" to him that the issue was quantitative. One sus-

3. William Thomson, Lord Kelvin, "Electrical Units of Measurement" (1883), reprinted in his *Popular Lectures and Addresses*, vol. 1 (London, 1888–89), first page of this lecture.

4. The mathematician Ian Stewart has said that there are two kinds of mathematicians: "Most work in terms of visual images and mental pictures; a minority thinks in formulas." And so in other fields: "Johannes Müller, a famous biologist, said that his mental picture of a dog was like this: DOG" (*Does God Play Dice? The New Mathematics of Chaos* [New York, 1990], p. 95). Fogel's mental picture of slavery seems to be like this: $P^* = H^* - i^* + X^*$.

5. John H. Clapham, "Economic History as a Discipline," *Encylopedia of the Social Sciences* (1930), reprinted in F. C. Lane and J. C. Riemersma, eds., *Enterprise and Secular Change* (Homewood, Ill., 1953), p. 416.

6. Robert W. Fogel, "Nutrition and the Decline in Mortality since 1700: Some Additional Preliminary Findings," National Bureau of Economic Research Working Paper no. 1802 (1986), p. 105.

pects that he didn't really need the debate to know that it was. How large? How representative?

Fogel takes fewer shortcuts in measuring things than any student of society I know this side of the medievalists. He even eschews the shortcuts "implied by theory," as the optimistic phrase among economists has it. I have tried repeatedly to persuade him that Harberger's Theorem suffices to show that the static effect of railroads on growth was small: if you multiply together the share of transport in national income, the share of railroads in all transport, and any rough estimate of the cost saving of railroads over canals (noting that all three numbers are well below unity), you are going to get a small number. Fogel was and is unimpressed.[7] He says that to really know you have to scour the records of the industry and write a 296-page book.

Paul David speculated once that railroads induced economies of scale.[8] In Fogel's astonishing presidential address to the Economic History Association, the longest in its history, delivered one September night in a mock-Tudor college hall in Toronto, he actually measured the alleged economies of scale and showed them to be small. The measurement showed the frailties of the qualitative reasoning that David and I and most other economists rely on.[9] He stands with Newton in saying *hypotheses non fingo*, I do not express mere hypotheses, "For what I tell . . . is not Hypothesis but the most rigid consequence, not conjectured . . . but evinced by the meditation of experiments concluded directly and without any suspicion of doubt."[10] Maybe this is why he is so tolerant of the sterile rigor of mathematical economics, seeing in it a shadow of the rigid consequence of fact. The empiricist and rationalist traditions of the West, British and French, meet on the grounds of certitude.

When I first met Fogel's rigid consequence, reading *Railroads and American Economic Growth* in a graduate seminar with Gerschenkron in 1965, I detected a fellow positivist. Since then I have grown critical of the philosophical position that Fogel believes goes along with being quantitative. As most plainly revealed in his little book of 1983 with Geoffrey Elton, Fogel believes that a quantitative science follows the precepts of philosophy of science circa 1950.[11] He has since 1983 shifted ground some, especially in consequence of his work on religious conviction as a force in British and American abolitionism. Yet he is still loyal in his philosophy of science to the older, received view. He would reply: There is no sin in such loyalty. Surely he is right. Fogel

7. His latest restatement is an interview in the *Newsletter of the Cliometric Society*, 5 (July 1990), pp. 3–8, 20–28.

8. Paul A. David, "Transportation Innovations and Economic Growth: Professor Fogel On and Off the Rails," *Economic History Review*, 2d. ser., 22 (Dec. 1969), pp. 506–25.

9. Robert W. Fogel, "Notes on the Social Saving Controversy," *Journal of Economic History*, 39 (March 1979), pp. 1–54, esp. 39–44.

10. *Correspondence*, p. 96f., quoted in Gale E. Christianson, *In the Presence of the Creator: Isaac Newton and His Times* (New York, 1984), p. 94.

11. Robert W. Fogel and G. R. Elton, *Which Road to the Past? Two Views of History* (New Haven, 1983).

keeps the faith. The positivistic faith that inspired Robert Fogel, Milton Friedman, Paul Samuelson, and the rest may not nowadays be persuasive philosophy, but judging from results it served to motivate a lot of good science.

Other features of his personality taught us, too. His convivial but intellectual socializing was presided over by Enid, between her or his airplane voyages to and from Rochester (my wife and I were charmed to hear that Enid and Bob were accustomed to having dates in O'Hare Airport, as their travels crossed; and this before such jet-setting was common). The Fogels together taught that intellectual life was worthy of ceremony. I think Bob was for this reason exceptionally pleased with his year as Pitt Professor of American Institutions at Cambridge: boy from the Bronx sips hundred-year-old port and smokes Havana cigars after dinner with the fellows of Kings.[12]

Fogel's socialist background made a big impression on me and taught me to outgrow my own socialism. Here was a man who had been a paid organizer for one of the principal youth organizations of the Communist Party.[13] And yet he was reasonable. I had heard the Yogi-and-the-Commissar line, that once a radical always a radical, of the right if not of the left. The line is a sort of McCarthyism of the middle (I pause to note the analogy with the anti-Chicago McCarthyism among coastie economists, from which Fogel has suffered, gracefully). Fogel in the flesh, however, was nothing like either the Yogi or the Commissar. He described himself quite accurately as a Scoop Jackson Democrat and argued genially with us about the good sides of Nixon, Vietnam, and Mayor Daley. One learned that people could change their minds on reasonable grounds, and then go on to argue with civility about things that mattered.

One learned also from Fogel the nitty gritty of being a professor. Only my time as a research assistant for John R. Meyer on his projects in history and transportation economics made as much of an impression. Fogel, for example, sends draft papers out for comment on a massive scale. His students have adopted the practice. Invite criticism and take advantage of it. Mail is cheap. "I'd rather be criticized in private by a friend," says Fogel, "than be savaged in public by an enemy." And unlike most of us he actually believes it. He believes deeply in the conversation of scholarship, often starting a new project by writing long, sweetly reasonable letters to other scholars, whether or not he has been introduced.

Fogel does not spurn the nitty gritty of administration, especially if it too is scholarly. He assembled research teams, larger and larger and larger, with the help of Marilyn Coopersmith, administrative genius of the Fogel band and big

12. Enid will, I hope, not mind if I report that she was not so pleased with Cambridge's chauvinistic ceremonies, once flatly refusing a feminine request to break off a conversation and "come out with the other ladies while the gentlemen have their port and cigars."

13. Enid was scornful at the "paid" part. She told me once that being from working-class parents, unlike Bob, she expected the boss to pay the employees on payday, even if the boss was the Revolution.

sister to us all. He has repeatedly created new institutions and taught his students the desirability of doing the same. His workshop in economic history was one of many in the Chicago department of economics. The institution of workshops is Chicago's main contribution to the culture of the field. But Fogel's was suffused with warmth as well as rigor. Some of the other workshops at Chicago seemed to spring more from the dark side of the Force. Fogel did much to advance economic history on a larger stage, from his active service on the Mathematical Social Science Board of the Social Science Research Council down to his creation of the National Bureau of Economic Research's program on the Development of the American Economy. He broadened historical economics by involving scholars from other countries and other disciplines. Chicago had a stream of foreign visitors coming to study with Fogel because Fogel does not view demographers and historians as engaged in some other enterprise, which we economists can safely ignore. Like most economists he believes in intellectual specialization. But unlike most economists he is consistent in his economics: after the specialization he also believes in trade, rather than the piling up of exports unsold in the backyard.

Fogel embraced with enthusiasm the nitty-gritty task of financing all this work. He taught us that a scholarly life was worth paying for. He got fellowships for his visitors, he argued for appointments, and he paid for much of the resulting intellectual activity out of his own pocket. He spent what seemed like enormous sums on cameras and tape recorders and other equipment, using them to record first drafts of papers in seminars and to photograph participants quarreling with each other at conferences. A tape of the departmental skit ran as background music for the famous annual Indoor Picnic at Bob and Enid's.

All these unifications of Fogel's life with his work were corollaries of the Great Nitty Gritty: put scholarship first. Always, always, scholarship came first. Moses Abramowitz, a student of Simon Kuznets as was Fogel, tells how difficult it was for he himself to encounter Kuznets because the older scholar would invariably ask, as though to a graduate student who was not making very good progress on his dissertation, "Well, Moses, what are you working on?" Fogel acted always as though Kuznets was going to show up in a few minutes and pop the overwhelming question, "Well, Robert, what are you working on?" He worked, and works, incessantly, to a plan that Kuznets would recognize as the most serious of scholarly work.

The work is guided by Fogel's Fifty-year Rule, which he taught us all: Will it matter in fifty years? Because he really does believe the rule, Fogel has been calmer in controversy than you or I would have been under similar provocation. He does not worry about short-run defeats, such as the politically poisoned reaction to his book with Stanley Engerman, *Time on the Cross: The Economics of American Negro Slavery.* What matters is the reaction in fifty years. As the driving instructor advises, Fogel aims high in steering.

The Fifty-year Rule entails thinking big, which Fogel does himself and

encourages in his students and colleagues. He still has the detailed thesis proposal he presented to Kuznets on 14 January 1959. The seminar participants at Johns Hopkins heard a nineteen-page paper entitled "Notes on the Influence of the Railroads on American Economic Growth, 1830–1890." It begins, "The railroads exercised a decisive influence on the course of American economic growth in the 19th century." There follows a Schumpeterian-Rostovian paean to the iron horse, and a two-page outline of the proposed dissertation. Think big. The outline covers most of the railroad subjects written on since then, such as the economies of scale and the population growth attributable to railroads, capital formation in railroads, and comparisons of social savings in other countries. The long book that finally resulted from this exercise in scholarly chutzpa, it turns out, covered only two of the seventeen proposed subjects. Here was someone building a monument for the ages, more durable than bronze.

But he was always willing to change the plans for the monument as the building proceeded. As a graduate student he changed his mind on American railroads, moving from a pro- to an anti-Rostow position in the face of evidence. As a professor he was enthusiastic when students and junior colleagues (such as Jacob Metzer and John Coatsworth) came to contrary conclusions about the role of railroads in other countries. People who have not been close to Fogel cannot believe that he has a flexible mind. They see only the vigorous advocacy in the short run. Dogmatists interpret advocacy by others as dogmatism like their own. Fogel could say more truthfully than could most of his critics, *dogma non fingo,* I do not express mere dogma. Fogel has changed his mind on railroads, on slavery (he started as a doubter of the Conrad and Meyer view), on abolition (he started as a doubter of the force of religion), and on death rates (he started as a doubter of the importance of nutrition). He has changed his mind more than any scholar I know, although one must admit that the competition in this line is not especially fierce.

It was of course his astounding scholarly productions that most kept our attention as students and colleagues. The man could have been a colorless curmudgeon and still have taught us a great deal.

Fogel is the master of historical economics, taking it to the frontiers of economic and historical study. His works on railways, slavery, abolitionism, and now mortality have carried out in unprecedented detail the program of using modern economics to understand history. No economist or historian combines the scholarly values of economics and of history more thoroughly.

Fogel believes that the example of historical economics will make other parts of history and economics broader. A profession that aims at *histoire totale* can be improved by analytic and computational techniques applied to historical numbers. And as the historical evidence improves (indeed, as the present becomes the past), economic history will take an increasing share of the argument in economics itself. The anti-historical frame of mind in eco-

nomics cannot last, no more than can the anti-quantitative frame of mind in history.

It would be a strange aberration in the history of astronomy if astronomers resolved to concentrate exclusively on the solar system or to concentrate exclusively on the red side of the spectrum. The stars in all their radiation nonetheless remain and will at last be studied. The interesting but narrow questions of what caused last year's economic downturn or why women participated more in the economy over the past decade will yield to the broader and longer term questions of what causes the business cycle in capitalist economies or what causes the sexual segregation of the work force.

Fogel believes we should study all the evidence with all the techniques. We cannot achieve all things in historical science by scrutinizing the conventional sources, he says, nor all things in economic science by staring hard at a blackboard. We have to look at the evidence hard, as genuine scientists, and then argue the case hard.

Fogel is above all an economic arguer about the evidence, an attorney for the factual prosecution. He takes an empirical idea—such as that one might measure the social savings of railroads—then asks, What conceivable doubts might someone have that the answer is so-and-so? Before the trial gets under way he imagines every move of the opposing attorney. While others build their cases on a rough-and-ready plausibility, such as might persuade their mothers, using observations "consistent with" the hypothesis (and therefore, statistically speaking, ignoring power), Fogel builds his case on excoriating doubt. It is the scholarly standard that Karl Popper and others have held up as the ideal for science. Recent studies of science have shown that even in the physical and biological sciences the standard is seldom achieved. That makes it all the more remarkable that Fogel does achieve it in historical economics.

Fogel meets or exceeds the standard for factual inquiry of, to pick a few comparable scholars in various fields, Simon Kuznets in economics, Louis Namier in British history, V. O. Key in political science, and Ronald Syme in Roman history. That puts him with the great scholars of our century. No stone is left unturned. Repeatedly the ingenuity of critics swarming around him is made to look foolish when their main point turns out to have been anticipated in an obscure footnote by the master himself. Fogel's address to the British Economic History Society in 1976 provides an instance. An English economic historian, well disposed towards him but unable to resist taking advantage of a rumor that Fogel was worried about a certain calculation concerning the slave trade, rose in criticism. Fogel waited until the fellow had finished his apparently devastating remark; then, smiling broadly, he allowed as he was glad the question had been asked: in the month since the news had gone out that he was having difficulty on the point, he and his co-workers had collected observations bearing on it to the number of forty thousand. Fogel then proceeded to re-establish the calculation beyond cavil.

Fogel's opinion, voiced repeatedly since his earliest work, is that "the major obstacle to the resolution of [most of the issues in history and economics] . . . is the absence of data rather than the absence of analytical ingenuity or credible theories." The opinion is worthy of respect for two reasons. First, to repeat, economics has had a long run with blackboard reasoning; perhaps the time has come to take economic observation seriously. Second, Fogel backs his opinion with analytic ingenuity and credible theories in quantity, but most of all by supplying enormous, Tycho-Brahean masses of data. Not *data*, really, which means "things given," but *capta*, "things seized."

Fogel seizes his from every source. He measured the social saving by mastering the engineering literature on railroads. He measured the efficiency of slavery by making use of dozens of southern archives, tens of thousands of prices of slaves, and detailed knowledge of the manuscript censuses. He measured the sources of mortality by using an array of epidemiological and nutritional studies, the records of military recruits, the Mormon family archives, the experiments of biologists, the records of hospitals. He sets a standard of empirical seriousness that no economist in the history of the discipline has matched.

To put it another way, Fogel combines the best of analytic minds in economics with the highest standards of self-doubt in social inquiry and with what historians call "historical imagination." He is a "scientific historian," not in his own sense, recalled from an acquaintance with the positivist philosophies, but in R. G. Collingwood's sense: "scientific historians study problems: they ask questions, and if they are good historians they ask questions which they see their way to answering".[14] Fogel sees the right historical questions to ask and sees his way to answering them. He brings the highest historical standards of factual veracity to economics. He is both the best of economists and the best of historians.

The difficulty of achieving dual excellence late in the twentieth century is worth noting. Scholarly standards in both economics and history have risen since 1950. What would pass for analytic brilliance in an economics article in the 1940s looks routine circa 1990 (consider Samuelson on the multiplier and accelerator). What would be considered impressive breadth of sources from an historian in the 1930s looks now crude and inexplicit (consider Marc Bloch on French agricultural history). Fogel has done it in both fields.

The dual excellence sets a standard that both economics and history might achieve, if they aim high enough in steering. It is peculiar, to pick one, that economics has allowed itself to ignore certain classes of evidence and argument. Ignoring evidence from opinion surveys or arguments from narrative is not a good idea. Yet the official methodology of economics urges this and other narrowings of the evidence. Fogel does not. You cannot read a piece by

14. R. G. Collingwood, *The Idea of History* (London, 1946; New York, edition of 1956), p. 281.

Fogel without bumping against the startling if obvious standard, borrowed from history for the good of economics: examine all the evidence.

The standard has resulted in large scientific advances. His friend and former colleague Richard Rosett is fond of pointing out that few scientists or scholars have the energy or ability to achieve one great scholarly success in the time allotted to them; in twenty-five years Fogel has achieved three:

(1) He discovered that the iron horse, bestriding the economic historiography of the nineteenth century like a colossus, was important but not colossal. He was here testing the theory of Schumpeter and Rostow that modern economic growth has depended on certain great inventions, the analogue in economic history of great men. He tested it with extraordinary thoroughness and began, as I have noted, by believing it to be true. Yet he found it wanting. Transportation strikes the noneconomist as obviously fundamental in some vague fashion—after all, what would happen if we closed down the highways and railroads tomorrow? Fogel noted that the question was one of long-run dispensibility and brought to bear the latest insights of cost/benefit analysis.

The book (really, two books: his master's thesis on the Union Pacific railway was part of the tale) created much controversy. Fogel's argumentative style rubbed some economists the wrong way, and the less self-confident among the historians, frightened by the quantitative history that Fogel was advocating, were pleased to see the historical economists quarreling among themselves. The same story was to be repeated more bitterly ten years later in the controversy over slavery.

In any event, Fogel was right. He was complimented by imitation, in a dozen replications of his study for other countries and other branches of transportation. His argument was scrutinized in a way that only the most important scientific findings are, by the best critical minds in the discipline, inside and outside economic history. It lasts.

(2) He turned then to American slavery, with his colleague at Rochester, Stanley Engerman. (Each successive project of Fogel's has involved more and more work by teams, as his ambitions for *capta* have grown.) Unlike the railroad book the essential plan of the work on slavery was not original with Fogel. The notion that one might view slavery, however vile its moral basis, as an efficient market arrangement had been adumbrated by Kenneth Stampp, Alfred H. Conrad, and John R. Meyer. But adumbration is not the same as painting in oils. Fogel and Engerman in the two volumes of *Time on the Cross* and in their massive subsequent work painted a picture of capitalism gone wrong, of slavery as an economic success that demanded political intervention to kill, and of a black work force that achieved much in bourgeois terms despite the lash.

The uproar occasioned by *Time on the Cross* is hard to understand. Some of the internal criticism, unhappily, arose from personal jealousies, as anyone attending the various conferences about the book could see. The book was favorably reviewed by the doyen of southern historians in the *New York Re-*

view of Books and reviewed at astonishing length and with great respect in numerous technical journals. Then came a reaction to the publicity. Some scholars seem to have been annoyed by the appearances of Fogel and Engerman in *Time* and of Fogel alone on the Today show on television. One is put in mind of the fury that descended on the chemists who had the temerity to announce fusion in a bottle before clearing it with the physicists. Certainly any book that touches the American dilemma incurs the risk of being badly misunderstood, especially in the overheated days of the early 1970s. Fogel was attacked as a racist in some circles and a running dog of capitalism in others. It is hard to imagine labels less apt.

The sober truth is that he and the group of scholars he led greatly increased our understanding of American slavery. They were the first to take seriously the measurement of efficiency, of slave diets and physical conditions, and of the abuse of slaves. On other issues—such as the demography of the slave population—they permanently and substantially raised the level of debate. Any student of the compulsory labor systems that typified the workplace before the twentieth century must use Fogel and Engerman's work, extended by their students and colleagues, and embodied now in the massive volumes of *Without Consent or Contract.*

To put it more broadly, neither the optimistic correlation of capitalism with freedom nor the pessimistic correlation of capitalism with misery make much sense. Fogel has done much additional work on the abolition movement, tracing its roots in political economy and especially in religious conviction. He has found that abolition was a close call, not inevitable, no automatic result of "modernization." Nor was it a self-interested move of the middle class. A quantitative economist has ended by emphasizing the complexities of politics and the saliency of moral freedom. That is scientific integrity.

(3) As if two home runs in a single game were not enough, Fogel pointed to a spot in center field and produced with a mighty swing an explanation of the fall in mortality, 1700 to 1900. The project is less controversial than his other work. It is international in scope (though emphasizing the American experience), undertaken with a still larger team (running into the dozens), and has moved further away from economics strictly defined. It has pioneered entirely new sources of data, especially military recruitment records. In other ways, though, the work is typical of Fogel's earlier performances, especially in the catholicity of literature brought to bear. Fogel has ransacked the literature of human biology, the history of medicine, demography, social history, economic history, nutritional history, pediatrics, clinical nutrition, embryology, historical sociology, tropical medicine, public health, historical geography, epidemiology, agricultural history, physical anthropology, gynecology, international economics, industrial history, toxology, genealogy, and development economics to discover the link between nutrition and life chances. He has concluded that better food accounted for about 40 percent of the decline in mortality, mainly among infants, leaving a considerable unexplained fall in

mortality well before the coming of modern medicine. The work is in progress—another of Fogel's favorite phrases borrowed from careful science is "preliminary results," and he means it: measure twice, cut once. What is clear is that the project is a major contribution to our understanding of how we grew and how we grew rich.

Robert Fogel, in short, has reunified economics and history. Using the best techniques of modern economics and gathering the widest samples of historical data he has reinterpreted American economic growth in brilliant studies of the railroads, slavery, abolition, and death rates. Rather than conjecturing on the causes of growth he has asked persistently "How large?" and seen the way to answer. He has set a new standard for empirical thoroughness in economics and a new standard of logical cogency in history. The quantitative history he advocates has opened new ways to the past. The historical economics he helped create, an economics made wiser by a knowledge of history, brought economics back to the larger questions. It has already reinterpreted the history of American and now other economic lives.

If Fogel had lived at Athens in the late fifth century B.C., he would have been seen daily in the agora with Alcibiades, Crito, Phaedrus, and the rest, trying to persuade Socrates that the slide rule and sampling theory were just what Greek science needed. The historical Socrates was not a writer, a democrat, or a mathematician, but in other ways Fogel resembles the inventor of serious conversation. Both characters have warmth, humor, intelligence, moral courage, singleness of mind, and a genial tenacity in argument, a tenacity that does not endear them to all their compatriots. If the Greek was known to stand stock still for a day to grasp an argument, the American is known to rise at all hours of the night to perfect one. If the Greek was famed for his courage in the imperial wars of Athens, the American is courageous in less bloody but still danger-filled circumstances, from southern hotels under segregation to universities under student siege.

In short, it could be said of Robert William Fogel, as of Socrates, son of Sophroniscus: "to be sure he never professed to teach this; but, by letting his own light shine, he led his disciples to hope that through imitation of him they might attain to such excellence."[15]

15. Xenophon, *Memorabilia*, Josiah Renick Smith, ed. (Boston, 1903), vol. 1, pp. 2, 3 (lines 12–15).