The Newsletter of

The Cliometric Society

July 1990 Vol. 5 No. 3

30th Annual Clio Conference Held at Allerton House

MONTICELLO, ILLINOIS - This May, the 30th annual Cliometrics Conference returned to Allerton House, the palatial estate of the heir of the founder of the Chicago Union Stockyards, which since has been given to the University of Illinois as a Conference Center. The facilities were fine and organization was again well handled under the direction of the local arrangement committee of Lee Alston, Jeremy Atack, and Larry Neal. Even though it seems that the entire Midwest has been under a flood watch all Spring, we were blessed with a short spell of relatively dry weather so we could enjoy the gardens of the estate. Some longer walks were truncated, however, since many of the paths were still under water. On both evenings a cocktail hour was hosted by Academic Press, and on Saturday night the "awards ceremony" was held. Unfortunately no one showed up with a guitar, so the singing was limited to the premier performance of a new song "If I Only had a Can." (See page 17)

The annual Clio award went to Larry Neal, who is now the sixth recipient. Larry is now faced with the problem of finding a way to add new names to the small board on which "The Can" is mounted. There was a new twist to the award ritual this year. By contacting the US importer of Clio brand Olive Oil, Tom Weiss was able to get six more cans nearly identical to the one awarded. These have been distributed to all the winners so each can now have a can to put in that spot on his bookcase where the award was sitting when he was the current holder. The picture below shows four of the six can holders. For the historical record, the label on the cans has been changed from "100% Virgin Olive Oil" to "No Cholesterol". On the following pages you will find a write up of the paper discussions, The Mullah's report, and abstracts of the papers presented.

What's Inside

From the right are Joel Mokyr, Tom Weiss, Larry Neal, and Roger Ransom. Previous recipients Don McCloskey and Richard Sutch did not attend the Conference.

CLIOMETS Fileserver DOES Work

The Cliometric Society Database (CSDB) provides registered users with several types of useful information: it serves as a clearinghouse for working papers and other items of interest to economic historians; it also provides an up-to-date list of Cliometric Society members that can be searched by name, geographic region or research interests. Any current member of the Cliometric Society who has given us an E-mail address should be registered to use the database.

We suggest you try the CSDB. Any BITNET user can use E-mail or TELL to communicate with the database. If you are located on a network linked to BITNET, you *must* use E-mail by sending (mail) messages to CLIOMETS@miamiu. For first time users it is probably best to get the detailed intructions on how to use CSDB sent back to you from our database.

An example of E-mail system is CMS' mail utility. The instructions given below are for this mail utility. You may have to modify the instructions below to correspond with procedures on your E-mail system.

The Newsletter of The Cliometric Society
is published three times a year.

Samuel H. Williamson, Editor
John Lyons, Associate Editor
Leslie Smith, Assistant Editor

In order to get the CSDB's detailed instructions using MAIL, do the following: Type "MAIL CLIOMETS@miamiu" and press return. The system then prompts you for a name and subject. These are not necessary, so just press return. In the message area, type "/info" and hit F5, or whatever command is used to SEND your mail. That's it. If all goes well, you will receive the instructions automatically in a few seconds.*

To get the CSDB's detailed instructions using the TELL command, simply type: TELL CLIOMETS@miamiu INFO.

There are two potential problems you may encounter when using the database. The first problem is there may be no response. This occurs when the server is automatically disconnected becausethe Miami University CMS system goes down. We will not know about it until one of us logs on, and then we will turn the database back on. We plan to monitor the database more closely from now on, so you should get a response to your requests at any time of day or night. The second problem is that *after* you get the instructions and are trying to get a file or check a member's address, you get a message that you are not a registered user. This can happen either if you have not paid your dues for this year, or if we do not have your correct E-mail address. If the latter, please follow the instructions you receive on how to register.

The long run success of the system will depend on your contributions of working papers, so that more and more people will be confident that CSDB is a good place to look. We will be the first to admit that it is not worth your effort if our system does not work well—so try it out and give us some feedback. If you have any problems, send us a note to CLIOMETS@miamiu. Thanks for your patience.

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* All lines which begin with a slash (/) before them are recognized as commands when you use the E-Mail method. Any line which does not begin with a slash will be accepted as a mail message and forwarded to the database operator.

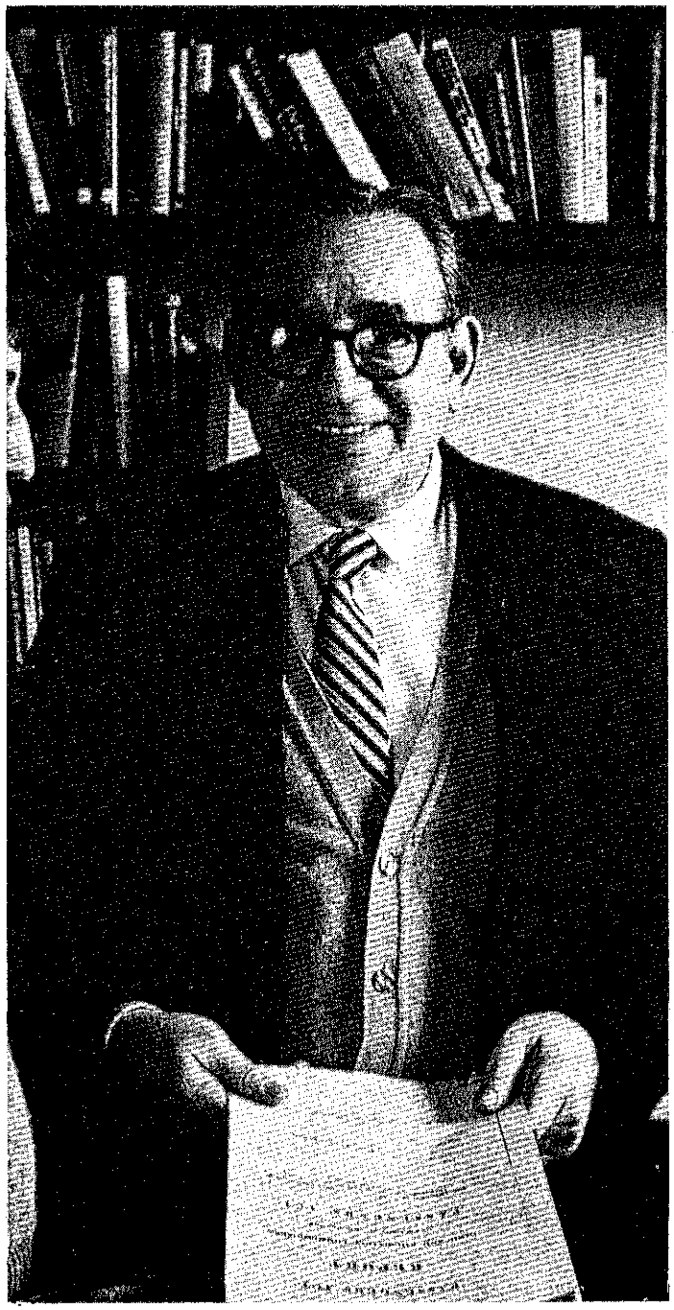
An Interview with Robert W. Fogel

<u>Editor's note:</u> *Robert Fogel is the Walgreen Professor of American Institutions at the University of Chicago. He is also, among his many other appointments, the director of the Center for Population Economics, and the program director for Development of the American Economy section of the National Bureau of Economic Research. He has received many honors including membership in the National Academy of Science, and an honorary degree from the University of Rochester.*

To me, Bob is an exemplar of the old expression "a scholar and a gentleman." I remember stopping in his office one summer in the mid '60's to talk about a tentative dissertation topic I was thinking about. It made no difference to him that I was only a graduate student from Purdue. He spent a couple of hours with me and insisted on taking me to lunch as well.

Of course I was no exception to Bob's desire to nurture those who were finding their way into cliometrics at the time and the best testimony to this is the scores of Bob's former students around the world who today are among the leaders in the field.

The interview was conducted by telephone for almost two hours on June 14th, 1990. The questions were prepared by John Lyons and myself and the transcript has been edited by both Bob and John.



The first paper you presented to the "cliometricians" was on railroads at the inaugural Clio meeting at Purdue.

Right

How long did it go?

I don't remember exactly how long it went. I think it went a full afternoon but, in any case, it went much longer than it was scheduled to go. People found the results of the paper (an early version of chapter 2 of my railroad book) so astounding that they felt they had to lean all over it and they picked away in detail at all of my different estimates. They wanted me to explain in considerable detail how I had estimated this or that factor. The questions focused on the reliability of the data and of the analytical techniques I was using in the various measurements.

When the afternoon was over, were people still skeptical or did they understand what you had attempted to do?

Well, there were 20 or 30 people there. We would have to poll them on their opinions. I certainly felt that although the questions were probing and hard, and some were skeptical, they were not hostile. I had the feeling, as they pressed me, that they felt I had done a lot of work. I remember one issue that was pushed very hard. In order to estimate the social saving I had to estimate the volume of shipments from ten shipping centers in the midwest, to about 40 receiving centers on the east coast and the south. Now, the procedure I used for estimating the volume of shipments involved estimation of the deficits in the trading areas of each of the 40 receiving centers. The first step was to estimate what was produced in each trading area, which was relatively easy, since we had a good census of agriculture. From production you had to subtract what was consumed. I estimated per capita consumption from budget studies. So there were lots of questions about the budget studies. You remember that I computed the social saving on four commodities: wheat, corn, pork, and beef, which represented the overwhelming majority of the interregional shipments in agriculture. The budget studies gave estimates, not of wheat, but of pounds of bread consumed. So there was an issue of how one got from pounds of bread to the wheat requirement. Lance Davis in particular, I remember, pressed me very hard on this issue. I went through different sources that I had used, including a number of formulas that reported the amount of wheat commercial bakeries used in a pound of bread. I had also examined a sizable list of cookbooks of the time, including those that were common in the rural areas. So I was able to present both household and commercial formulas. As it turned out, they weren't too far apart in the estimates of the amount of wheat needed per pound of bread. As I said, you would have to speak to the other people because there may have been a difference between my perception and theirs, but I thought Lance was pleasantly surprised and pleased to discover how much work I had done on cookbooks.

It seems to us that the big issues, once the book was published, were not so much your detailed work, cookbooks and flour and so forth, but the kinds of issues that Don McCloskey has raised. He said that that your global estimate of the social savings reduces to a simple three-line proof. Don analyzes the lengthy discussion in the book and your papers and argues that much of it involves a variety of rhetorical devices aimed at convincing your audience, particularly historians, of the viability of what you were doing. Does his view of your rhetorical approach sit well with you?

I have a considerable amount of sympathy for Don's approach to these issues. I agree with him that there's a lot of rhetoric in economics and the social sciences generally, and that very often points of view are shaped by arguments that lack the rigor we claim to use in settling issues. I don't fully subscribe to Don's point of view, and he was good enough to put some of my demurrals into his footnotes.

I divide Don's position on my railroad book into two parts. Let me begin with his three-line proof. That's an argument you could make only after you have taken the experts through all the details of the findings. If I had gotten up at the first Clio meeting and given Don's three-line proof everyone would have said, "Who's that jerk?" What Don is willing to accept in that three-line proof (for example, that the cost of alternative transportation was twice that of railroads) is after the fact, after a long, intensive debate over the calculation. Prior to that very detailed work the prevailing estimates of the alternative cost were from exceedingly high to infinite. So I think the difficulty with Don's three-line proof is that it presumes as true what could only have been established by an enormous amount of hard work.

The second point is whether there is rhetoric in the book. Well, a lot depends on what you mean by rhetoric. The way Don uses the word, rhetoric includes tightly-knit logical arguments. And there are such arguments in the book. You have to remember that when I started this project, I never expected the result I got. So when I first obtained a low social savings I thought I had done something wrong. After trying to discover where my error was, I gradually convinced myself that the error was not in my computational work but in my original conception of what the social saving ought to have been. I assumed that my own skepticism would be doubled, tripled or quadrupled when I presented my findings to people who had not been struggling with the problem for a couple of years. I thought about the arguments I would have to address in order to prevent readers from dismissing my work out of hand. In the first chapter (and much of Don's analysis is focused on the first chapter) I examined the traditional arguments for the indispensability of railroads, emphasizing the unverified assumptions in that analysis, and I made *prima facia* cases as to what would happen if one modified these assumptions. I also showed that some of the traditional arguments did not go to the heart of the issue of the social savings: the fact that small differences in competitive advantage can lead to very sharp shifts in the locus of transportation has no necessary implication for the size of the social saving. So chapter one was designed to address the assumptions I had originally brought to the research (of course, what I had brought to the research reflected my conventional training on the role of railroads, what I and most other scholars were led to believe were the facts) as well as lots of questions that had come up as I presented papers on my early findings. I tried to explain why the plausible traditional propositions ought to be put aside, or at least held in suspension, long enough to consider the new evidence and analysis.

I never viewed *Railroads and American Economic Growth* as a disputatious book aimed at provoking a controversy for its own sake, but as a very detailed study of the way in which a major innovation increased productivity. That was certainly the way that Kuznets viewed it. Kuznets was the last person who would have been interested in controversy for its own sake, and he would not have allowed me to write a dissertation that was speculative and disputatious, although he was willing to go along with the way I set up the opening chapter. The central objective of the book is estimation of the productivity advantage of the railroad and the allocation of that advantage among the various facets of this form of transportation. In that connection, the book looks at long-haul versus short-haul. It turns out that short-haul is more important than longhaul. And then it breaks down the overall advantage of railroads in both longand short-haul into such components as inventory savings, wagon savings, as well as a comparison of direct payments to waterways and railroads. It turns out that the main advantage of the railroads was not that they were cheaper than waterways in direct service, but that they required much less of a very costly complementary service, namely wagon transportation. So even if one accepts Don's three-line proof, that proof would not answer the question of where the productivity gains attributable to railroads came from.

Bob, we talked before about how you went to Simon Kuznets, your dissertation advisor, and explained what you were going to do. He said that measuring the impact of railroads sounded like a good project. Is that right?

I got the idea from one of his lectures. Kuznets pointed out that although there had been much discussion of the economic impact of railroads no one had yet measured the extent of their impact or analyzed the sources of the productivity gains associated with them.

Okay, we have a further question about that. Who came up with the idea of asking what water transportation would have cost? Who came up with the specific way you set up the counterfactual? Was that your idea or his?

Neither. It was really in the literature because people were comparing railroads to waterways all along. Let me say there is virtually nothing I did in my work on railroads that was not anticipated by some state legislator or other public figure. For example, in my book on *The Union Pacific Railroad,* I used the increase in land values to estimate the social return on the road. Well, there was hardly a session of a state legislature that dealt with a proposal to build a canal or a railroad in which the advocates didn't refer to the predicted increase in land values or use that idea to estimate the social benefit that wouldn't be covered by the income of the road. They used the expected rise in land values as an argument for subsidization. So these arguments were all over the literature. What we did was formalize the analysis; we put it in a form suitable for measurement. If you look at Al Fishlow's book on railroads, by the way, you will see that he made a lot of use of these early estimates.

Economists did not discover cost-benefit analysis. It really comes out of engineering. All the civil engineers who came before state legislatures that were considering internal improvements dealt with the relative costs and advantages of: (a) common roads or turnpikes, (b) waterways and canals, and (c) the "new" (at the time) railroads. And they provided cost estimates and benefit estimates for each of these alternative forms of transportation. So the notion of cost-benefit analysis is very old; it's a very intuitive idea and I think a lot of what we have done in twentieth-century economic analysis is a formalization of these ideas, putting some structure on them, specifying functional relationships that make it easier to estimate both costs and benefits, interpreting various measures within the framework of partial or general equilibrium models, and so on. The fundamental ideas are not due to us. So that's my answer. It didn't come from Simon; it didn't come from me; it was just there.

We're trying to get memories of people who were at that first Purdue meeting in 1960. What was the atmosphere there?

I remember a tremendous excitement and exhilaration on the part of everybody at the meeting. I was brand new and barely a third of the way through my doctoral dissertation. I arrived at Rochester, my first teaching appointment, in August 1960 and the first cliometrics meeting was the following December. I hadn't met any of the people at the meeting except for Henry Rosovsky who had interviewed me for an appointment at Berkeley a few days before the Purdue meeting. I had read the works of many of them and Lance Davis was very highly regarded around Johns Hopkins, as was Duncan McDougall. They were products of the school; their names often came up in the halls. From my point of view it was exciting just to meet other people who were moving in a similar direction, such as Lance [Davis], Jon [Hughes], Doug [North], Bill [Parker] and the others. I had expected to meet [Alf] Conrad and [John] Meyer but they didn't come to the first meeting. There was a general sense that the meeting was an important occasion, that something new was happening, that we were moving in a new direction.

Since you've been to so many Clios since then, including the Second World Congress last year, do you still think there is that air of excitement, particularly among the younger people, or has it turned into just an old and blasé institution?

Well, cliometrics is now the establishment. It's not a movement of Young Turks anymore. But, I'm sure cliometrics is exciting to younger people in the same way that it would have been for me, even if cliometrics had been old. I was making my entry into the area. Moreover, I think we've remained very self-critical. By self-critical I mean we don't take our own work for granted. We're probing. I don't mean to imply that we're free of the problems that come with establishments: of thinking too highly of our own work, of believing that what we do is the only way to do it. I'm sure we suffer from some of that. But I believe we may suffer from it less than other establishments. We remain quite open to innovation, to new approaches and new problems. At the two international Clio conferences there was much of the old excitement and probing criticism. So I think the spirit has held up pretty well, despite the fact that we've moved from Young Turks challenging the establishment to being the establishment.

Right. Let's move on. How did you get interested in delving into the U.S. slavery issue?

Well, I got interested in slavery because of the Conrad and Meyer paper, which was published in 1958 when I was a graduate student and it startled everybody. I think that I mentioned to you, Sam, that I've written a little memoir called "History With Numbers," which describes the long and emotional debate on the use of quantitative methods in history. This essay is not focused on economic history per se but deals with the broader discipline of history. In it I have a paragraph describing my own reaction when the Conrad and Meyer paper was published in 1958.

I didn't believe their main findings. I didn't think that a system that reprehensible could be profitable. I was one of a number of graduate students at Johns Hopkins who got into very long arguments about the paper. Most of the faculty and graduate students in the economics department at Hopkins and some in the history department were drawn into the debate over whether Conrad and Meyer were right or wrong.

So your incentive was to redo it and find out if they were right or wrong?

No, no. Because I was working on railroads my interest in their slavery paper was tangential. It gave me confidence that this was the way to go. Beyond that, I was interested because they had posed a first-rate intellectual problem and I played the game with them that people later played with me: Where did they go wrong? I thought I could find a major mistake that would overturn their results, but I wasn't able to find that mistake. As I say in the memoir, the only major error that was discovered in that particular effort was the error that Yasukichi Yasuba pointed out in a paper that was originally published in Japan, and which Stan and I republished in the collection we did on *The Reinterpretation of American Economic History*. Yasuba pointed out that you needed to compute the rate of return on the cost of reproduction, not on the market price. If you use the market price all you will discover is that the market worked reasonably well and masters were getting the average rate of return. When Yasuba recalculated the rate of return on reproduction costs, it was even higher than Conrad and Meyer had put it. And the return was increasing as the Civil War approached. Yasuba's paper convinced me that Conrad and Meyer were basically right and that I simply had to come to terms with their main finding.

Until the mid 1960s I was interested in the slavery discussions only as a fellow economic historian and as a teacher who was describing what was interesting in the field to graduate students. Then in 1962 or 1963 Stan and I decided to collaborate in writing a textbook in American economic history based on the methods and findings of the new economic history. I have somewhere in my files sketches or outlines of about 20 chapters that represented a proposed approach to the book. We took up one problem after another, set them up formally, pointed out the issues and the key variables or effects that had to be estimated, and then we'd say: unfortunately they have not yet been estimated.

So you felt after a while that you were perhaps being a bit premature?

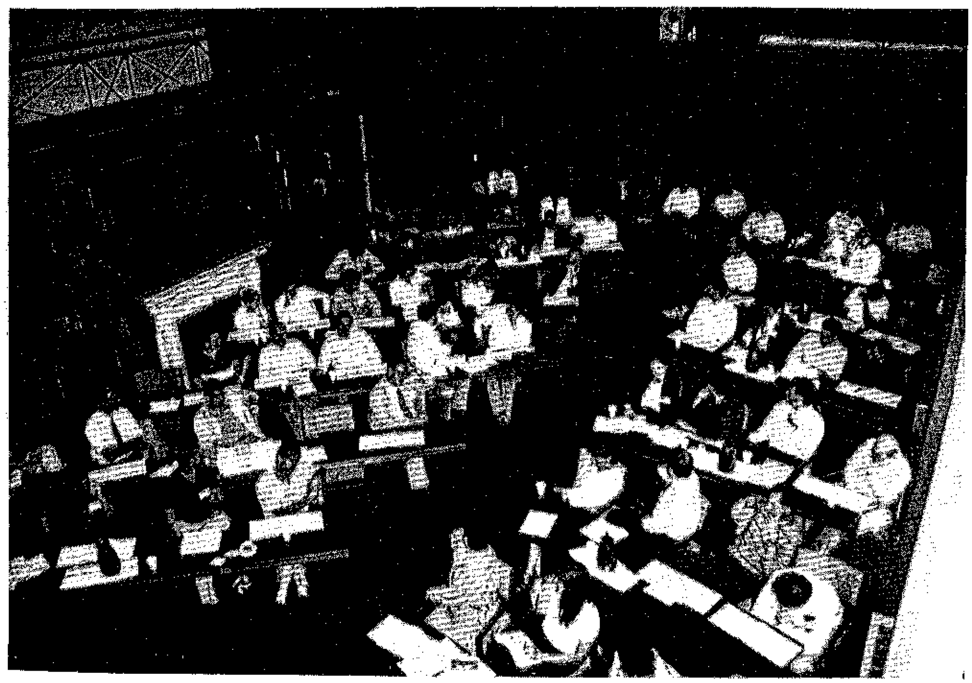
Yes, we were premature by about 20 years. So we talked about what we could do. We decided to edit a book that would bring together the best work in a decade of cliometric research. That led to *The Reinterpretation.* We divided the book into 9 sections, and we were going to write long introductions to each of them. Our first three papers on slavery were actually part of the effort to write such introductions. There were two papers by Stan. In this connection, he did a lot of work on Dick Easterlin's regional income estimates, revising them in ways that are described in those papers, in order to get estimates that were somewhat more appropriate for the issues that we wanted to focus on, as opposed to those that Dick had been focusing on. The revised estimates indicated that the South was growing even more rapidly than Dick's figures indicated. Let me say that when Dick published his paper in 1961 in the Harris volume[1], we were all startled to discover that the South was growing as rapidly as the North. If the first big bombshell was the Conrad and Meyer discovery that slavery was quite profitable, the second was Dick's discovery that the South was growing quite rapidly, and Stan amended Dick's result so that the South was growing even more rapidly than the North between 1840 and 1860. The third piece was a joint paper that became too long for an introduction; we published it as a separate essay. It was an extended review of ten years of cliometric research on slavery. We looked at three issues: Was slavery profitable to the individual investor; secondly, was slavery economically viable, could it have kept going as an economic system; and thirdly, was the South growing economically?

We had originally intended a fourth issue. We raised the question of what cliometricians should look at

Fenoaltea asked about adjustment costs and the existence of annual contracts. Marty Olney, Nye, and Lou Cain commented that, despite Kantor's estimates of actual profitability, it is whether those profits were expected by the participants that mattered. Martin Eisenberg suggested looking at editorials or sermons to get at the specifics of voting behavior [Kantor noted that this is in another paper]. Josh Rosenbloom emphasized that correlation is not causation, here with regard to the connection between an increase in the number of laborers and decreased votes for fencing. Joel Mokyr asked with astonishment if the whole of the argument really does depend on the six to eight foot of "waste" associated with the fences, and pointed out that in this case the shape of the farm would matter to the profit calculations. Kantor replied that he had done much sensitivity analysis (elsewhere) looking at the shapes of farms and that his results had held.

Michael Chwe's paper on pain in a principal-agent model led to several comments from Sokoloff, Wallis, Phil Coelho, and Gary Libecap, raising the question of whether something other than economic rationality explained the use of whipping. Chwe commented, his intention was to show that violence was not economically irrational, not that other causes were absent. Sokoloff asked whether it mattered whether the pain was physiological or psychological, and Whatley said he would argue violence was globally irrational. Betsey Hoffman added that it might be individually rational although globally irrational because of reductions in human capital. Ransom stressed that the *threat* of violence was rational, and Chwe's empirical work excluded threats. Clark suggested slavery be segmented into different classes: were rented industrial slaves beaten less? Rick Steckel noted there was a demonstration effect not included in Chwe's model, and Clark raised the issue of the relevance of a one-period model to what is essentially a multi-period problem. Susan Wolcott noted that everyone was assumed to have the same disutility of beating, or being beaten. David Gabel added that uncertainty should be included. Charlie Calomiris questioned whether, in the absence of obvious externalities, the inclusion of a societal prohibition against violence affected the distribution of net benefits. Chwe responded that with the inclusion of such a constraint, agents were not worse off.

Jeffrey Williamson's paper on compensating wage differentials in Michigan in the 1890s led to much lively discussion, despite the after-dinner hour. Sam (no relation, for those who have always wondered) Williamson led off the discussion by noting that the assumption in the paper is that spells of unemployment are seasonal and expected, but that the cross-sectional samples were drawn from a depression period. John James responded that this high unemployment was actually good for Jeff's case because compensations are thus understated. Sam then asked if the data were daily or monthly pay rates, since this is particularly relevant for analysis of seasonal workers. Robert Whaples asked if there are any data giving the total employment in each industry, or about new-comers versus re-entrants versus leavers. Alston commented that the paper's conclusion that unemployment is not a function of personal characteristics but that unemployment is a function of whether one has a short- or long-term labor contract ignores the possibility that the awarding of short- and long-term contracts was itself a function of personal characteristics. Alan Dye (and later others) noted that the length of a labor contract is not something the worker can choose, contrary to the implications of the paper. Wallis noted that the sample is relatively homogeneous demographically, so it is not surprising that personal characteristics are not statistically significant. Williamson countered that Wallis's assumption of homogeneity of the sample group was wrong. (Ransom later argued that in fact Wallis' assumption was *not* wrong.) Wallis, turning to Table 4, and asked what it meant to say wages depend on the probability of lack of work and the predicted number of months lost. Rosenbloom and Barnet Wagman both asked how to interpret the coefficients of Table 4 (R-squared = 0.31) when the fit in the two tables in which the probability of lack of work and the predicted number of months lost were estimated was so poor (the pseudo R-squared's were about 0.11); had Williamson run the regression of wages on the actual rather than predicted values of these two variables? Williamson replied he had not done so but could. Susan Carter returned to the question of measurement of earnings; how were monthly earnings computed from the survey results? Ransom then took the floor (which later earned him the chattering teeth award) and made several points: contrary to Williamson's earlier assertion, it was possible to know what time of year (spring) and by whom the censuses were conducted; Wallis was right in asserting an absence of heterogeneity of the sample: that there is only a very limited distribution of ages; the savings observed were not precautionary savings but were savings to purchase homes and life insurance; and that age of worker and time lost from work are strongly correlated. All this brings into question the result regarding little role for personal characteristics in determining unemployment. James asserted that Williamson used "implicit contracts" in a different way than most economists would expect and urged him to change the phrase in the paper. Sokoloff asked about seasonal versus cyclical layoffs, and also observed that workers probably could not choose the lengths of their pay periods. Alston followed up, asking whether these were annual contracts with monthly pay periods: one cannot infer length of labor contract from payment frequency. Mokyr asked for clarification of "unemployment" in this context and to what extent it was voluntary. Carstensen noted that one employer could have some people working for short periods and others on year-long contracts, simply because of the diversification in farming. Rosenbloom asked whether seasonality is indeed exogenous; if weather matters, then why (in other work) do we observe similar seasonal patterns in Los Angeles and Montreal? Barbezat asked why the existence of compensating wage differentials would imply few alternative income sources. Sam Williamson wanted to know what these workers were doing in their off hours; were they self employed and working on their own places?



Clio participants in Allerton House Library

Discussion of Ronald Necoechea's paper on effective annuities in medieval England began with Mokyr's question regarding how contract terms had been converted into grain equivalents. Harley noted that the people in the study were far from poor; Necoechea agreed they were "middling" or even higher in status. Clark asked why a log-log form had been specified for the regression and wondered if the results were sensitive to this specification; he also asked if land quality had been taken into account. Necoechea said the required data were not yet published. Libecap requested information on "retirement" in medieval England [with Hoffman responding that the study included all such annuities located in archival sources] and asked whether there were ever transfers of money rather than grain. Kantor asked about the frequency and enforceability of contracts within families, and Calomiris continued by asking about family versus outsider contracts, wondering whether families had more information about expected life length than others did. Sam Williamson countered by noting that in small villages (which were by far the most common, according to Necoechea) the information known about families and outsiders would be similar. Jeff Williamson asked about enforcement of within-family contracts and whether there is evidence of disputes (few); the period of the sample (1200-1450); and whether Necoechea could use his results for studying increases in productivity. Steckel asked how the transfers were made; Necoechea replied, in a single transaction. Richard Grossman wondered what population estimates were used in calculating per capita grain awards (the number of people specified in the contract). Knick Harley asked about the amount of ale consumed, since this would also use grain (Response: an average of a gallon per day per person!!! Talk about fetal alcohol syndrome!). Rotella asked if these contracts had been used to disinherit one's children; Necoechea said it was certainly possible but that he had seen no evidence. Cormac O'Grada noted that the contracts may have existed only for cases of disputes, which would explain their rarity. Ransom wanted to know what the Church did with the land it acquired and where the "retirees" lived; response: they lived at the monastery and the church could opt for liquidating or keeping the land, supposedly for the support of the poor. Jon Moen wondered about the possibility and costs of liquidating such assets with thin markets. Haines (and subsequently Ransom and Clark) wondered about the validity of aggregating the various contracts together in one study; the Church may have had different motives for offering contracts than did families and others. Wallis asked whether the value of grain in the contract could serve as proxy for the value of the land transferred.

Elise Brezis' paper, containing new estimates of net international capital flows for the UK in the eighteenth century, generated heated debate. Cain began by asking how trade with the colonies was counted, and learned that colonial trade was counted as international trade. Calomiris objected to the contention that international capital flows are necessary for industrialization; he argued that it is the marginal and not the average cost of funds that matters and whether international funds are a large or small part of total loanable funds is irrelevant for this point; besides, the argument presumes substitutability between government and industrial lending. Mokyr observed that when data are "shaky," it is essential that sensitivity analysis be performed: that we must be able to say "give or take 50 percent on these numbers, the results still hold." He argued that here the numbers are so specific, that such an exercise cannot be performed. Therefore, we should not put much weight on conclusions regarding whether the UK was a debtor or creditor nation during the 18th century. (This point was later seconded by Harley, Clark, Haupert, Nye, Mokyr!, and a host of head-nodders.) Aside from which, Mokyr wanted to know how it was possible for the UK to go from being a debtor to a creditor nation while it fought the Napoleonic Wars. Brezis replied that the data are the best we now have and that she could not pin-point within twenty years the time when the UK switched from being a debtor to a creditor nation. Viken Tchakerian asked how the paper dealt with the problem of smuggling. O'Grada asserted that Brezis' conclusion that the UK was a debtor nation at the end of the 18th century was sensitive to measured remittances from the East Indies and questioned the accuracy of the data. Michael Bordo wanted to know where to find values of re- exports from London to Europe and how these were measured. James queried the contentions regarding growth; what sort of investment was being financed with these capital imports? Tom Weiss wanted to know which of the assumptions made in developing the data were the most crucial for the results. Wallis injected a cautionary note: Cliometricians should not distance themselves from empirical work just because "it's too shaky," and then asked about possible connections between a capital surplus and industrialization. Wolcott offered a suggestion for getting at the open economy issues by looking also at international interest rates (Brezis countered that Larry Neal had already done this). Sokoloff reminded the critics in the audience that they are themselves prone to data problems (those who live in glass houses...) and noted that even if a capital surplus is not necessary to industrialization, it can surely matter to the process. Mokyr replied to Sokoloff that in those glass houses there are other pieces of evidence offered to back up assumptions made in the development of data. Neal asked the group how they would answer Brezis' question regarding the financing of industrialization and what would they do to understand the links between the industrial and commercial revolutions. Bordo said that *he* would look at interest rate and foreign capital data, anecdotal evidence, and would work with behavioral relationships to understand causality.

Discussion of Chris Hanes' paper on cyclical wage flexibility in the U.S. began with Rosenbloom asking if there had been a gradual transition or a sudden break in employment conditions for particular workers over the period 1870-1907, and also whether there were differences in wage flexibility across occupations. Hanes replied he felt the apparent sudden break was an artifact of the statistical process; similar statistical results were produced when the break was placed anywhere between 1885 and 1892/93. Kahn suggested adding independent cyclical variables. Libecap wondered why Hanes said there was decreased elasticity of demand for products in the late 19th century. When Hanes replied that this followed Chandler, Fenoaltea pointed out that Chandler's point was about sector-specific changes in concentration, not economy wide. Whaples further noted that the sectors studied by Hanes are not Chandlerian growth industries (though Hanes replied there are spillover effects linking his industries with those). Barnet Wagman commented that discussion of firm-specific human capital should not be conducted with these data since the occupations Hanes studied are not ones where firm-specific human capital is a relevant issue. Calomiris suggested that using Jeffrey Sach's equation may not be a good idea because of timing issues (though Hanes later noted he had experimented with various lagged output gaps without changes in the results) and added that there was no clear distinction between real and nominal wage rigidity, nor between anticipated and unanticipated deflation. He also suggested that the money supply regime matters for the degree for of wage flexibility. Carter asked about the 1893 Aldrich data, and whether the wages studied were attached to a worker or to a job. Reply: the data were for "representative" firms and were attached to jobs, not workers. Atack referred the author to Carroll Wright's careful descriptions of the collection of the Aldrich data and also asked about use of the Weeks report. Michael Haupert asked about the possibility of asymmetric responses to changes in prices. Bordo then seconded Calomiris' point regarding the relevance of monetary regimes, and asked for elaboration of the story about wage rigidity, strikes, and so on. Nathan Sussman suggested inclusion of supply variables (like crop harvest) in the equations and Carstensen urged more attention to the role of the Pullman Strike. Cain suggested that the Haymarket Riots were also important at a national level. Ransom stressed that Carroll Wright's labor surveys had been prompted by labor unrest, and asked whether there had been any change in layoff policies. Whaples noted that 1877 is more important for this tale than 1886, since two hundred people were killed in labor violence in 1877, implying that if changes in wage flexibility were reactions to perceived risk of labor unrest, the change should have come much earlier than Hanes asserts. Hanes replied there was a gradual learning curve and that any lessons learned were not invoked until economic downturns; hence the delay from 1877 until the mid-1880s. Wagman contended that unionization was more important than Hanes implied; for example, that by citing overall unionization rates rather than the rates within specific industries, Hanes had missed the crucial role of labor unions in the industries studied. Fenoaltea noted that McCormick cut wages of skilled and semi-skilled workers only, but that Hanes' story seemed to imply we should expect cuts in unskilled wages as well. Wolcott said that the strike threat was not just a threat used in downturns but could also be issued in the absence of wage increases in upturns. Coelho objected to the use of the WPI as the price variable. Merino suggested looking at the accounting literature and discussions there of deskilling of labor, and commented that the absence of attention in the paper to legal action was troubling.

Margaret Levenstein's use of an APS (Abreu *et al.*,

1986) model to study collusion in the pre-World War I bromine industry was questioned by several members of the audience. Libecap began the discussion on a different note, though, by asking first how one defines "successful" with regard to a cartel, and second, how significant was the cost advantage of Dow after they discovered and perfected the electrolytic processing of bromine. Levenstein responded that "success" is defined to be the existence of abnormal profit (not price stability) and that Dow had gained cost advantage by 1902. Tchakerian, Fenoaltea, and others also asked about Dow's technological advantage and its effect on price. Kahn wondered about the costliness of the observed price wars. David Augustin questioned the use of the APS model over time (rather than, as is intended, over a short period). Nye asked for clarification on the German cartels and about differences between world and domestic prices. Kevin O'Rourke wondered if the formation of the cartel had been dependent on international concerns. Olney asked for discussion of the product's use, its buyers, and, given that there was only one direct buyer who then distributed the product to about a dozen second-tier buyers, whether the number of buyers matters to the model. Hoffman noted that the model does not take into account the two-tier structure of the industry. Pablo Spiller argued that the paper presumed, but did not prove, collusion; Levenstein responded that the archival evidence was quite clear on the formal existence of the cartel. Merino commented on the way in which Dow used the cartel to perfect its technological advantage. Barbezat argued (suppported later by Hoffman and Nye) that the APS model is not appropriate to this industry. Whaples observed that the demand-side was given short shrift. Gregson asked about the role of the direct buyer, Mr. Shields. In Don's absence, Harley asked a McCloskey question regarding the rhetorical devices being used.

David Gabel offered the most amazing regression results in his paper analyzing the provision of telephone service in early 20th-century Wisconsin; with cross-section data, he obtained R-squared's of 0.9988 and 0.9993! Wallis asked whether the dependent variable in the regression should have been the total value of capital or a per-customer measure, to avoid picking up the effects of market size. Spiller argued that Gabel's equation was not really a cost equation but more like an investment equation, and noted that output was in fact endogenous and not exogenous. Libecap asked whether Bell had pushed for regulation of the telephone industry and Coelho followed up by asking if small companies wouldn't also want to be regulated. Gabel said that by about 1913 (six years after regulation) the small, independent investors had sold out at a loss. Carstensen asked about the extent of entry into the industry. Whaples asked if Gabel had experimented with the form of the equation. Clark asked why the number of miles of conduit and of poles had been included as independent variables to explain the value of capital stock rather than using, for instance, customer density. Tchakerian, observing that the model assumed no entry, asked about possible use of a model that allowed entry. Rotella (and later Coelho) asked for elaboration on the politics of the story. Fenoaltea, Avner Greif, Alston, and Mokyr asked for some conjectures on the relevant counterfactual: what would demand have been in the absence of regulation? Hoffman suggested looking at experience in other states, and Ransom wondered why any telephone company would have gone after a market initially, since this aspect of the tale is omitted from Gabel's analysis.

Discussion of Susan Wolcott's paper on the 1920s British textile industry and its exports to India began with Mokyr's request for a table showing prices, especially British prices. Nathan Sussman (and later Haines) agreed, and asked for information on the exchange rate and inflation rate differentials between Britain and India. Eric Schubert noted that the war changed supply conditions and asked about the increased tariff in 1930; Wolcott replied that tariffs were raised to increase revenue, not to afford protection for the industry. Gregson asked if there were any changes during the period in the capacity of the Indian textile industry and Clark asked about the role of the Japanese. Alston wanted more of the background for the political economy story about tariffs, labor supply in agriculture (the number of persons gainfully engaged). Sokoloff asked about increases in women's real wages, and Rotella observed that, in the tables, the relative wages of agricultural and non-agricultural workers were not changing, and asked why would there have been a shift of labor out of agriculture. Brandt replied that this was because there was such a rapid adjustment to equilibrium. Wolcott noted that women were employed in textiles only temporarily; Brandt disputed this and the point was left unresolved. Hoffman asked for a little bit of a history lesson for those who had not studied the interwar Japanese economy (n-3 people!). Brant obliged, and then Hanes followed by suggesting that there was apparently no wage gap observed between rural and urban sectors; Brandt conceded that there were, in fact, no price series usable to provide good comparisons of rural with urban real wages. Ransom noted that Brandt's calculations show that real daily and real annual wages track each other until 1916, but that after 1916 the annual wage increases more rapidly than the daily wage. Alston contended that often in agriculture we can see that improved technology decreases labor demand on average, but not at harvest time, and therefore asked about the seasonality of labor requirements. Hoffman returned to Ransom's point about the daily versus annual wage gap, noting that if there had been a harvest-time peak demand for labor, then an opposite effect to what Ransom had noted would have resulted. Sokoloff and later Clark asked about this wage divergence, noting its importance for the story and wondered whether it indicated serious data problems. Clark also noted that rapid inflation could give the observed high correlation of first differences of prices; this might not be evidence of market integration at all but just of inflation. Mokyr asked what had happened in 1930, since there had been decreases in wages thereafter. Ransom commented that looking at prices without data on quantities can be misleading and asked if there were any evidence from labor contracts. O'Rourke observed that the paper seemed to be a "booming sector" story, and wondered why there was a boom in the non-agriculture sector; Brandt noted the boom but did not explain it. Jeff Williamson voiced collective sentiment by claiming he was now confused: he had thought the main story was the change in Japan from a closed to an open economy and that rice imports had led to changes in labor demand; now, perhaps, Clark's point about the causes of the price correlation was correct. Either way, what is important is the gap in prices. Clark restated his point on inflation and price correlations; correlation cannot show integration when prices jump — better, he suggested, to look at price differences. In response, Brandt referred to Japan's agriculture and tariff policies. Carstensen summed up the hour by wondering what had been confused: the story line or the audience. After that, we all went home.

If I Only Had a CAN

by Thomas Weiss

(sung to the tune "If I Only Had a Brain," from the *Wizard of Oz*)

You could while away the time here
conferrin with Joel Mokyr
consultin with the man
and your head you'd be scratchin
while your thoughts were busy hatchin
if you only had a CAN

You'd unravel every riddle
for any individdle
with regressions that you ran
while Dick Sutch thinks he's handsome
you could be another Ransom
if you only had a CAN

Cliometrics tells you things, like three is
less than four
You can measure things that never were
before
then you'll sit and measure more

Your words would be less musky
if you'd listen to McCloskey
your journal would be gran
and the Mullah's yearly prizes
would suffer their demises
if you only had a CAN

Measurement Problems in Absentia

The Mullah knew it was only a matter of time before he would return to the Illini oasis of hospitality and statuary, but he had not dreamed it would be so soon. Only three years had passed since his last visit when by chance he encountered the annual gathering of the philosophers of the counterfactuals. As the proxy for a silver bird approached the gleaming, new infrastructure rising from the rain-soaked plains, he was reminded of the words of Thomas Mann "Wer hätte nicht einen flüchtigen Schauder, eine geheime Scheu und Beklommenheit zu bekämpfen gehabt, wenn es zum ersten Male oder nach langer Entwöhnung galt, eine venezianische Gondel zu besteigen?"[2] As he emerged from the terminal — the pavement shimmering like the Grand Canal, with tall, swarthy plainsmen milling about — he yearned slightly for San Simeon Piccolo. He was, alas, too late to find the city in an even more venetian state, with canals running through every building.

At the Mullah's first episode with the strangely named tribe, he had been so moved by the words of the scholar from the great desert of the Southwest — "Never open a can of worms larger than the Universe" — that he awarded her a prize, and subsequently an annual award was established to be given to that member of the tribe who, in the heat of discussion at the cliometric rites, utters an aphorism of value to society in general.

The Mullah had not realized the implicit contract he had made in establishing the annual award. Fortunately so, for had he, then it would have been an explicit contract, and he would have been ill-prepared to follow the deep and wide-ranging discussion that took place after dinner. It appears that the award has guaranteed him a lifetime contract, albeit with far lesser rewards than he might have received if he submitted his work through the regular editorial channels. For his part he must pay attention throughout the entire proceedings, all the more so when the Oxfordian lion of proverb-listening is absent. Perhaps 100 years from now a future cliom will unearth archival material that will shed light on this oriental notion of lifetime work and identify ways in which the Mullah might have remained occupied otherwise.

The annual experiences had confirmed the Mullah's view that the Cliometrics tribespersons would not produce such enlightening proverbs in abundance. Those uttered in succeeding years had value, but did not seriously challenge the inaugural winner. More dismaying, on occasion the members suffer from silver bird lag, and when allowed to decide between two competitors, they had chosen the wrong one, forgoing "there is more than bubble gum between the U.S. and Canada." Could it be that all the society's wisdom is achieved by chance or irrational choice? If true for this august body of philosophers what hope is there to understand the behavior of ordinary persons?

The Mullah was a bit gloomy about this year's proceedings because absent were that incisive orator from the desert of the Southwest and the garrulous gourmet from the bay area. Also missing was the famous Hawkeye rhetorician who led the long crusade to distinguish small and large. When that campaign was in its infancy brilliant words flew like so many Scrabble blocks, but now the great crusader need only say "you know what I mean." The Mullah feared that with the Hawkeye's absence there would be some backsliding, but instead his many converts kept the discussion on the straight and narrow, albeit without actually discussing the significance of that path.

Any pessimism about dazzling oratory, however, was erased early on. As has become all too common, some members have come prepared to make obvious attempts to get their names in the great book of proverbs. The Jesuit wildcat, who spends much time wondering how do we know we know, and who should know better, claimed "a cow in the hand is worth more than its expected value in the bush."

[1] "Is there anyone but must repress a secret thrill, on arriving in Venice for the first time — or returning thither after long absence."

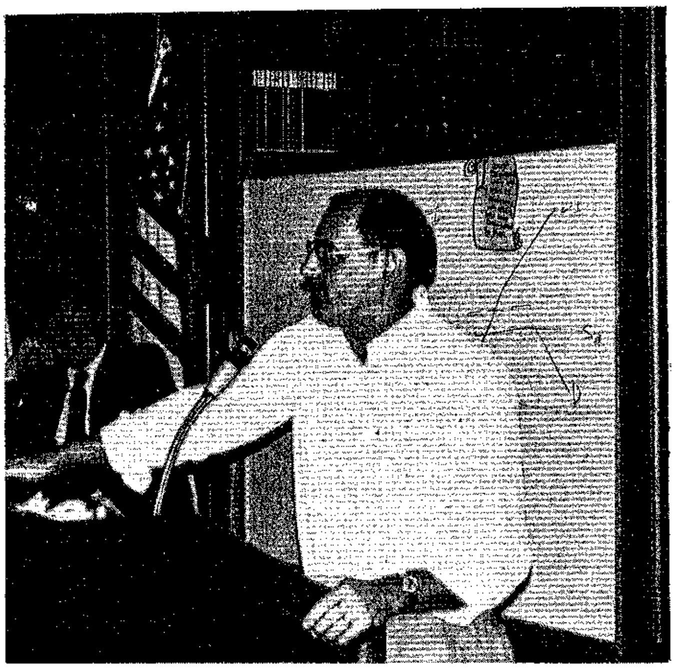
While it reeked of preparedness, it augured well for the true contest — the utterance of pithy sayings in the heat of cliometric battle; that serendipitous turn of phrase containing wisdom for all time and place, and which, as occurs with repeated viewings of the movie "The Cook, the Thief, his Wife, and her Lover," would, upon reflection, yield yet more wisdom, albeit in smaller and smaller doses.

The Mullah felt particularly confident about this year's gathering because he who has studied the potato in great depth was present, and with his many disciples in tow would surely be inspired. Keeping his record intact as a perennial contender, he did offer "X's guess, becomes Y's estimate, which becomes Z's fact." While it had the ring of wisdom, the Mullah was not clear which truth it was conveying. Moreover, it did not specify the role of stylized facts, straw persons, or red herrings.

The scariest bit of wisdom was offered by one of the perennial contender's disciples: "with this group, much violence is rational." That a newcomer could possess such insight is remarkable, and perhaps says much about that citadel on the landfill of the great lake. The aphorism, however, has its shortcomings. Clearly it is not universal, for it would seem to depend upon the group. More importantly, many members made clear that they would not want to test it, nor adopt it as a motto.

A most appropriate saying, spoken by one of those from the big apple area, was "maybe fairy tales work in multiperiod models." This contains much that is pertinent to some cliometric work, and the Illini oasis was filled with those who had spent a great deal of time working with one or both of these concepts. The aphorism, however, seemed to push too far beyond counterfactual propositions to be of value to the world outside the confines of academe.

A very catchy phrase was produced by the scholar from an Atlantic coast institution that owes money to the agency that monitors the amateur sports enterprises. "If you listen to Lance long enough, he will say just about anything." Everyone agreed that was true, but it was far too personal to merit the prize, and of course could be said about many others, so contains little new wisdom. Moreover, it was superseded by some of Lance's own words. A representative of that famous technical school claims Lance said, "masturbation is unlikely to produce children."[2] This has much intuitive appeal, and may be true for some species, but the Mullah's own parable indicating that pots and pans may have given birth, raises doubts about the saying's universality. And, how could the Mullah explain this to his friend Hermaphrodite.



Tom Weiss Awarding; Lou Cain Presiding

The finest quote possessed all the qualities the Mullah looks for: uttered in the heat of battle, true for all time and place, intuitive appeal, the sort of simplicity that makes each of us wonder why we had not thought of that, and for this group, a call for measurement. "It is difficult to count all the manure" was fittingly put forth by he who has wondered why the whole world did not think of this. This has not only the intuition of truth, but the scholar's search for the sources of growth has already provided a test of the proposition. The Mullah can only wonder what appropriate prize he would have devised had this been the inaugural

[2] He did: see the Newsletter V:2 (February 1990) page 8. [Eds.]

winner. And, had the Hawkeye rhetorician been present, the Mullah envisioned endless debate about the unit of measurement, how to determine which pile was significant, and discussion of the appropriate form of the counterfactual to test the proposition "I did not know they piled it that high."

On a more practical matter the Mullah was glad to see the sharp retardation in the number of injuries among clioms. The society may last longer than he had feared. Some foolish members did risk injury by rowing a 2,000 meter race around midnight, taking about 18 minutes to do it, and losing to the computer. In the future, this should be prohibited or else capable rowers should be invited to the gathering.

The Mullah also urges the tribal elders — one of whom was conspicuously absent — to install the cliometric song as a part of the annual rites. [See page _____.] Rather than tax the imagination of the can holder, the society could sponsor another contest, an annual one, urging invitees to compose a cliometric version of an appropriate song, to be sung during the evening's rituals and before *anyone* sings union songs. Next year offers nostalgic possibilities to compose the cliometric version of "Back Home Again in Indiana." Or imagine the North Shore duo giving another joint presentation [a simultaneous translation] of original words sung to the tune of "It Don't Rain in Indianapolis."

Submitted humbly by the faithful and obsequious servant of the Mullah

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continued from page 8

next three decades). The Conrad and Meyer paper was quite consistent with Stampp. The Phillips School was the reigning school in the immediate post-World War II period, and you can find reflections of that view even in the history textbooks written by liberal Northerners such as Morison and Commager. By the way, David Brion Davis published an article in *Daedalus* in 1974 in which he says some of the same things that I am saying to you.[2] So the new approach reflected a generational experience. Anything we did on slavery was bound to be controversial, originally because we were challenging the established scholarly views on these issues. Later on, as the black political movement unfolded (when I say black political movement, I mean to include liberal white allies) everything began to be measured against how it facilitated or hampered the fight for civil rights. Look at the storm of controversy around the work of Conrad and Meyer that erupted at the 1967 meetings of the Economic History Association. The most emotional aspects of that meeting were edited out of the printed version of the debate. Alf began the discussion by reading two letters commenting on their 1958 paper. One was from someone in Athens, Georgia, daring him to come down and make the same statements in Athens; the other was from someone in the North calling him and John Meyer racists. They had simply crossed the ideological wires.

We presume you have enjoyed working on your most recent project on heights, weights, and nutrition. How did you get started on that project?

Well, it arose partly out of the slavery project, but it also goes back to my training under Kuznets. Let me start out with that. Kuznets's main course at Johns Hopkins, a full-year course on economic growth, was divided into four segments: population growth, technological changes, long-term trends in national product and its components, and the use of national income accounts to study comparative economic growth. As a graduate student, I was most excited by the sections on technological change and that's where I did my initial work. But the other parts of the course also had a big impact on me. So in the back of my mind I recognized the importance of demographic work. One of the ablest graduate students at Johns Hopkins, Yasukichi Yasuba, whom I mentioned in connection with the Conrad and Meyer controversy, did his dissertation on trends in fertility before the Civil War, which was published in 1962.[3] He also had a major chapter on mortality in his study. So I had a good introduction to economic demography, although it was not on the front burner of my research prior to the 1970s.

When we began the slavery research I approached demographic issues mainly as they bore on our effort to improve the measure of the labor input. We collected data that would enable us to estimate how much of the available time of a woman was actually used for the production of measured output. As we got into the data, the results we obtained were so contrary to what we had initially believed that we became very interested in a variety of demographic issues we had not previously expected to pursue, and these were reflected in *Time on the Cross*.

In 1974 Stan and I talked about starting work on a new project before the slavery project came to a close. We decided that we should look somewhere in the demographic area. As our talks progressed we decided to focus on mortality. Most of the empirical work in demography at the time focused on fertility, so mortality was relatively neglected. We agreed to investigate the possibility of a project that centered on measuring mortality rates in North America before 1900, because they were basically unmeasured. We hoped to be able to produce a time series on mortality from the earliest European settlement to the time when the death registration system became widespread (about 1930). Despite our plans, we were not able to begin the mortality project in 1974. Stan went off for a year in England in August of 1974 and I was scheduled to go to Cambridge University during 1975-1976. Our plan was to work ourselves into the mortality project gradually over that period, but we expected to concentrate on finishing the slavery project which, at that point, we thought we would do in two or three years.

Then the controversy broke over *Time on the Cross*. We were so deeply involved in the controversy in late 1974 and most of 1975 that we didn't make any progress on the mortality project. Now, some of the controversy turned on demographic issues, particularly on the age of slave mothers at the birth of their first child and on the age at menarche.

This project on height, weight, nutrition, and mortality has been going on since its inception in the early 1970s, yet you're still working on it.

Oh yes, and I expect it to go on after I die. I'll be disappointed if I look down (I'm assuming I'll be in heaven) and discover that the building of life-cycle and intergenerational data sets has been discontinued.

We finally did get going on the mortality project late in 1975. I divided my year in Cambridge mainly between teaching myself technical demography and working in the Public Record Office. Part of the mortality project involved comparing mortality trends in the U.S. with those in the countries from which the American population was drawn. Prior to 1790 the U.S. population was 95 percent British in origin, so the Public Record Office was one of our most promising sources of data.

In addition to getting the mortality project off the ground, we were trying to respond to the critics on the efficiency issue. Stan and I began the work on what became the first AER paper on the question. We were also trying to come to grips with the demographic issues and particularly the issue that was raised first by Herb Gutman, but later also by Ned Shorter and Dick Sutch, that our estimate of the age at first birth was biased upwards by four years.[4] We were aware not only of the bias that Gutman singled out but also of downward biases that tended to cancel the upward one, so we thought our estimate of age of first birth was fairly close to the mark. We drafted a paper dealing with the various biases, which included a technique for estimating them, and sent a copy of that paper to Ansley Coale for his comments. Ansley passed the paper on to James Trussell, who was then a young assistant professor in the Office of Population Research.

James sent me a letter saying the paper was interesting but he thought there were better ways of dealing with the biases. It turned out that he also was going to be in England and he offered to explain his procedure to me. Early in the academic year he came up to Cambridge (maybe it was October) and spent about two hours giving me a lecture on the singlulate mean. It was a powerful technique, much better suited to the problem than the one I had devised. That afternoon provided a chastening lesson for me on the difference between a professional demographer and a novice. The singulate mean produced what is probably a pretty good estimate of mother's age at first birth, but in any case is a downward biased estimate of that age. James collaborated with Rick Steckel who was developing the data needed to implement the procedure. Now the singulate mean answered the question about the average age of mothers at their first birth. We had put that age at 22 in *Time on the Cross*. Herb had said 18. The singulate mean shows it was 21. So our estimate was biased upward by about a year, Herb's was biased downward by about three years.

At that point James said to me, "You know, Bob, there is still the question of the age of menarche." He was referring to our proposition that there was considerable abstention from premarital sexual intercourse among slaves. Stan and I had accepted the opinion of Bancroft and others who reported that slaves were fecund in their mid-teens. So if slaves were fecund at 15 or 16 but did not give birth on average until 21 or 22, there must have been a lot of abstention (given the absence of contraception). Gutman, Shorter, and Sutch argued that no such inference could be made because the slave diet was so bad that slave women were over 18 when they became menarcheal. To support that proposition they cited J.M.Tanner who reported that c.1860 Norwegian girls became menarcheal at age 18. They contended that the age of menarche must have been at least as late for slaves as for Norwegian girls. So no inference could be made about abstention from sexual intercourse because most slave women were, after allowing for post-menarcheal subfecundity, physiologically incapable of bearing children until age 21 or so.

Trussell said, "You know, Bob, if we had data on the height by age or the weight by age of slaves, we could estimate the age at menarche very precisely." I said, "Height by age! Height by age! We have *thousands* of observations on height by age. Stan and I have been going around for years trying to figure out what to do with those data. We also have data on shoe size. What can we do with shoe size?" So that's the way I first came to learn about uses of anthropometric data. Trussell introduced me to Tanner, who looked at our data and said they indicated that menarche was probably around age 15, maybe earlier. After that it was a matter of enlarging the sample and of developing the best way of fitting the growth curve to the data so that we could estimate the peak of the growth spurt as precisely as possible. Most of the work, as you know, has been done by Rick Steckel.

I should say, by the way, that I was working in England, and Stan was in Rochester working on these height data independently, and he did a piece about the same time that has been neglected. It was published in 1976 in the British journal, *Local Population Studies*. In that paper Stan presented, not the velocity profile with which Trussell and Steckel worked, but the age profile of heights. Although his discussion was brief, Stan pointed out that the profile suggested that the physical development of slaves was reasonably good by contemporary standards.

This introduction to anthropometric data changed our approach to the mortality project. One of the key issues in the project is the contribution of improved nutrition to the secular decline in mortality. We had struggled with the question of how to get a suitable measure of nutritional status. Originally we thought we would collect samples from probate records in order to determine the foods that were being inventoried. Of course, such samples would have told us, at best, what foods were available for consumption. They wouldn't have given us a measure of the nutrients that were actually consumed. Once I realized that the anthropometric measures were much more powerful indicators of nutritional status, I began looking at what I could get from the Public Record Office on heights, mainly from military records. The results of that survey are indicated in the long description of the mortality project ("The Economics of Mortality in North America 1650-1910") that the six original collaborators published in *Historical Methods* in 1979. So that is how we came to integrate the work on mortality with the work on height and other anthropometric measures.

What made this new line of research possible for me was my good fortune to have made connection with Trussell and Tanner in 1975 and then with Nevin

Scrimshaw in 1982. They are all exceptional teachers with enthusiasm for their work and with great patience for the bewilderment of novices. From the moment I first met Tanner, who was then the chairman of the Department of Child Health and Growth at the Institute of Child Health in London, he generously spent numerous hours with me (and with others in our project), explaining the fundamentals of the branch of medicine called auxology (the study of human growth), looking at our data and helping us to interpret them, guiding us through basic texts, calling our attention to the latest relevant papers, and reading and criticizing our work. We received a similar education from Scrimshaw, Director of the International Nutrition Program at MIT, in epidemiology (particularly of infectious diseases), in nutrition, and in some aspects of clinical medicine.

This work has been going on now for many years. From the perspective of 1990, what do you think are the most intriguing outcomes of the effort so far.

Well, the original mortality project spawned two other projects. The mortality project was initially going to be based on a sample of genealogies that would eventually contain a million people in about 200,000 families linked together for up to ten generations. The genealogies contain a great deal of information on the vital events of the individuals listed in them. They also contain, less completely, such socioeconomic information as occupations, places of residence at various points in the life cycle (from which one can construct migration and urbanization variables), and military service. We planned to obtain additional socioeconomic information, including wealth, on the individuals in the sample by linking them to information in tax lists, probate records, the manuscript schedules of censuses, and pension records. We also planned to use data on height to measure nutritional status during developmental ages and to develop ecological variables from public health sources that would indicate the exposure to particular diseases in the localities in which the individuals lived over their life cycles.

The height data were so interesting that they became the basis for a separate project called "Secular Trends in Nutrition, Labor Welfare, and Labor Productivity." It is based on samples of height data drawn from 16 populations in the U.S., Europe, and the Caribbean from 1700 to 1980. We have about 500,000 observations in these samples. In 1981 we began a project aimed at tracing 40,000 white Union Army men from the cradle to the grave, looking at the impact of socioeconomic factors in early life, including nutritional status during developmental ages, on waiting time to the development of specific chronic diseases in middle and late ages; and on waiting time to death from specific causes. One of the four subprojects in the aging project will deal with the factors that affected the likelihood of contracting specific diseases while in the army, as well as the determinants of the case-fatality rates of these diseases. In this connection, we treat war wounds as a class of disease. Only about 20 percent of the people who died during the Civil War died as a result of wounds. About 80 percent of all deaths were due to disease.

Now, when you get into the kinds of data sets I've been describing you're involved in very complex problems of file management. In order to describe the whole life-cycle experience of a recruit, it takes 18,000 variables, which means that there are over 700 million pieces of information that have to be managed. So a considerable amount of our time has been devoted to the development of software, both for the laptops used to retrieve the data and for the work stations and mainframes on which the data are analyzed. We have been working with subsets of the overall sample on substantive igsues and have obtained some very interesting preliminary findings. But before I turn to the substantive findings, I want to underscore the importance of the advances in research technology. We are now able to create, at costs within the guidelines of funding agencies, life-cycle and intergenerational data sets that will permit us to get evidence on questions we could not even dream of dealing with a few years ago, that we could, at best, only speculate about.

We have made the most progress in the publication of substantive findings in the nutrition project. Since 1979 project participants have published over 40 papers and three books. The latest is *Height, Health and History: Nutritional Status in the United Kingdom 1750-1980* by Roderick Floud, Annabel Gregory, and Kenneth Wachter which was published in England in June and will be published in the States in September. That book, by the way, is the second to appear in the NBER monograph series called *Long-Term Factors in Economic Development* (I might as well get a plug in for the series). Several papers integrating the preliminary findings of the mortality and nutrition project have been published or are in press. The most comprehensive analysis of the information in our genealogical samples are contained in two working papers by Clayne Pope that should be submitted for publication soon.

We were particularly struck by the paper you gave in Santander last year.[5] Would you talk about the potential policy implications of that paper?

You have to remember that this paper is one of four that will be integrated into a little book I hope to complete in about a year-and-a-half. That book will be like *Time on the Cross*. It will be an early report on preliminary findings. I think we have now gotten to the point in our mortality and nutrition projects where we have a vision of what happened with respect to the secular trend in mortality in both Europe and America. We have a preliminary set of propositions that we think will be useful in guiding further research and that we expect to hold up reasonably well, although we also expect them to be modified in various ways as the research progresses.

Would you say that one of your propositions has to do with the efficiency of government food crisis management?

I've revised the Santander paper to put greater emphasis on policy issues. One of the major policy implications of our work so far calls into question the proposition that adults who are stunted but have a good body mass are as healthy as those whose nutrition during developmental ages permits them to attain full potential in height. Small may be beautiful, but it isn't healthy if it's due to stunted growth. That finding is evident in the iso-mortality map we included in the Santander paper. It shows that even if a stunted individual has the ideal body weight for his height, the probability of his dying is going to be significantly higher than a taller person with ideal body weight, so that malnutrition early in childhood is a major disaster throughout the life cycle. This finding argues for the importance of using whatever levers we have, which probably means some sort of government intervention, to get more nutrients to poor children. Of course, it's one thing to say that if you get nutrients to pregnant women and to children early in life, it's going to make a big difference. It's another thing to have an effective system for delivering the nutrients to them. And I don't have anything to say about delivery systems. I'm just looking at the economic-biomedical interactions. But I can't believe that effective delivery systems are beyond our capacity.

How would you relate your nutrition project to Amartya Sen's twentieth century studies of poverty and famine?

I think he's right in his analysis of why there are famines. I believe that our work is helping to demonstrate that the real issue is not famines, but chronic malnutrition. Famines may be dramatic, but the real loss in life comes from people who are chronically malnourished all of their lives, especially during developmental ages. I hope that our findings will have some bearing on discussions of current policy. Indeed, the book I'm writing is called *The Escape From Hunger and High Mortality: Europe. America and the Third World, 1750-2050.* So the book deals not only with the past and the present but with 60 years that are yet to come. I am structuring the book in that way to emphasize its policy orientation. I also want to emphasize that even the advanced countries have not yet finished their escape from hunger, although we are much better off in 1990 than we were in 1900.

The third book that will appear in the NBER series on *Long-Term Factors in Economic Development* is by Sam Preston and Mike Haines and it deals with the analysis of infant and childhood mortality through a study of the 1900 census. Near the beginning of the book, they make the point that when you go back to earliest times, life expectation was probably about 25 years. In 1900 it was a bit over 40 years. So during all previous history prior to 1900 there was an increase of about 15 years in life expectation. Between 1900 and the present there has been an increase of 35 years. During the last 90 years we increased life expectation twice as much as in all previous history. It's the twentieth century that is really the century of incredible progress, especially for the lower classes. Nor has this enormous advance been confined to the West; it has also taken place in the Third World. Indeed, if you look at how rapidly the death rates have been falling in the Third World, you'll see that they are declining more rapidly than they did in Europe.

Would you say that's more strongly related to the post- 1935 advances in pharmaceutical knowledge or nutrition, or what?

That's the question we're trying to answer. They are both involved. With respect to scientific advances, it is not just pharmaceutical knowledge that is important. Public health measures generally have been powerful factors in reducing mortality and you really can't separate nutrition from public health because nutrition is not just diet. Diet is what you put in your mouth. Nutrition refers to the nutrients available to the body. Diet and nutritional status often diverge. If you have severe diarrhea, no matter how much food you put in your mouth, you're body is going to get very little of the nutrients you ingest. Of course, a good diet is important but a lot of bad nutrition in the past stemmed from the fact that the body couldn't metabolize the nutrients that were consumed or else the nutrients were siphoned off in fighting disease. Diet and public health measures are closely interrelated and we're now trying to separate out, and to estimate, their independent contributions.

In the Santander paper I cited some evidence that suggests that improvements in nutritional status accounted for all of the improvements in mortality between 1750 and 1875 in England, France and Sweden,but only for about 50 percent of the improvement in these countries between 1875 and 1975. Sam Preston, using different data for Third World countries found that increases in income accounted for about 50 percent of the mortality decline in the twentieth century. Those are not bad interim numbers, but I think we can do better than that. In another paper I tried to divide the contribution of nutritional status to the mortality decline before 1900 into two parts: diet and reductions in exposure to disease. I cited data that suggests that a 60/40 split might be the best interim estimate, but that is only a conjecture. Much work remains before we will have something approaching a reliable division.

The last few issues we want to deal with involve a couple of stories, and your long-run perspective on Clio. This asks basically for your reflections on whether cliometrics was really a revolution.

I think we need to distinguish between the view on the spot and the view looking backward. Certainly everybody who was around at the beginning viewed it as a revolution...Well, nearly everybody. I add the qualification because of people like David Landes, who was part of the new wave but who was a little older and a little wiser than the rest of us, who tried to emphasize the points of continuity. The same can be said of many of our teachers, including your father, Sam, and Carter Goodrich, who were very encouraging to the new work but also thought very highly of the old work. They probably saw the lines of continuity better than the rest of us. But most people thought cliometrics was very revolutionary, not only those of us who were doing it, but also the critics. There were a lot of people who felt the techniques we were using—the mathematics, the statistics, the diagrams—were either irrelevant or harmful. Some of those who couldn't read our work told me of their fears that cliometrics would make them technologically obsolete. We were seen as bearers of an alien culture, and an alien language.

The language/culture shock tended to cover over the fact that we were really dealing with the same sets of issues. If you look at the issues that the cliometricians have focused on, they are largely the same issues that our teachers had been concerned with. So there was a considerable degree of continuity in the substance of the work. Some issues came more to the fore, others less, but a lot of that had to do with current public policy. What seemed interesting, what part of the past we gave the most attention to, was to a large extent a function of what society thought was important. Interest in the slavery issue has been enormous mainly because we have spent half a century struggling for the achievement of full civil rights for blacks in America on the political, social and economic levels. Under these circumstances there was bound to be a heavy concentration on black history.

When I first tried to define what was novel about cliometrics, I mentioned the gathering of new evidence, but I made the more explicit use of theory predominant. I now think I would reverse the order. There have, of course, been important advances in theory, yet, when you get down to measurement, the theory used is usually fairly simple and much of it has been around for a long time. If you take the work on the profitability of slavery, Phillips actually refers to the equation used by Conrad and Meyer in *American Negro Slavery*. He cites a man by the name of Gibson who wrote a book called *Human Economics* that was published in 1909. Gibson specifically says we can take the yield equation for bonds and use it to estimate the rate of return on slaves. Conrad and Meyer didn't invent the yield equation. Nor did they choose the yield equation because they read Gibson. Once they decided to treat the economics of slavery as a problem of evaluating the return on long-lived assets, the yield equation was an obvious instrument.

The key difference between Gibson and Conrad and Meyer was that they took the measurement problems seriously. They carefully specified all the different measurements that had to be made in order to implement the yield equation. Of course, they also introduced a very interesting dichotomy between the rate of return on men and women, which plunged them into a whole series of demographic issues, which they and their critics also took very seriously. By carefully specifying all of the variables and parameters that had to be measured, and by showing that much of the information needed for these measurements was already in the secondary literature (although the reliability was open to question) they set up a long-term research program. Gibson, on the other hand, never specified the measurement issues rigorously. In his work the yield equation is largely a rhetorical device that permits him to discuss some theoretical issues. So his discussion did not touch off an empirical train of research, although in another context it might have done so.

Because the ideological implications of the Conrad and Meyer paper were disturbing to many scholars, and because the data that they culled from the secondary sources for their calculations were so questionable, critics began searching for new data that would provide more accurate estimates. Since the secondary sources did not contain the type of data required to estimate key variables, researchers turned to data locked away in the archives.

Although the debate over Conrad and Meyer greatly stimulated the drawing of large samples of economic data from archives, the first historian of slavery to undertake such a task was U.B. Phillips. The large samples of slave prices that he collected before World War I are still widely used and represent an important source of information for current work. The next major effort in data retrieval was undertaken by Robert Evans, Jr. in the late 1950s. He collected new samples, not only of slave prices, but also of hire rates from various archives. Evans worked independently of Conrad and Meyer. Much of his work was undertaken before the publication of their paper. He used a somewhat more sophisticated version of the yield equation and he improved upon their procedures in some other ways. His paper, which was not published until 1962, strongly supported and extended the basic findings of Conrad and Meyer.[6]

It was the Parker-Gallman sample, however, that really ushered in what might be called the mature phase of cliometrics. It was that sample that led many of us to recognize that the new sampling techniques and the new computer technology made it possible for us to exploit vast data collections that hitherto had been far too difficult to utilize.

That brings me to my story about Fred Lane. I was asked to prepare a paper on the difference between scientific and traditional history for the Sixth International Congress of Logic, Methodology, and Philosophy of Science which met in Hanover, Germany, in 1979. That paper was first published in the proceedings of the Congress and then, with revisions, in my book with Geoffrey Elton (*Which Road to the Past?*). At that time Fred Lane was retired and living in New Hampshire. He was working on a new book and he came down to Harvard two or three times a week to work in Widener Library. I ran into him from time to time and we set up two lunches. At one of the lunches I told him about the argument in the Hanover paper. At the other lunch he told me about the way in which medieval historians used circumstantial evidence. The second talk was particularly stimulating; it influenced the approach of my next methodological paper, which I called "Circumstantial Evidence in 'Scientific' and Traditional History."[7]

After I told Fred about the argument of the Hanover paper, he said, "Well, Bob, what I think is really important about the cliometricians is not their use of theory but their discovery of how to utilize bodies of data that have been lying around in archives virtually untouched." I think that judgment is right. And that is another point of continuity with the past. In a sense cliometrics has restored the old emphasis on archival work. Several developments have facilitated the new archival work. We have better theory and the connection between behavioral models and statistical models is more sophisticated. We also have better theories of how to sample. All these things help. The single biggest change, however, is the computer revolution which has reduced the cost of data retrieval incredibly. I read somewhere recently that if there has been as much increase in productivity in the production of Rolls Royce cars as there has been in information processing over the past three decades, you could by a Rolls Royce for $3.50 today.

Oh, if it was only such.

My experience confirms the point. We made our first attempt to draw a sample from the muster rolls of the Union Army in 1978. With five people working full time for about ten weeks (2,000 hours) we were able to collect 13,000 observations with about 50 characters of information per observation. At that rate we retrieved information on just twelve variables. We omitted the names of the recruits because linking the recruits to other sources was prohibitively expensive.

During the summer of 1981 we drew a new and much larger sample. This time in ten weeks we retrieved a sample of 40,000 Union Army men, with more than four times as much information (including names) on each observation. Per character of information, the cost of data retrieval had declined by over 90 percent in just three years. The reduction was brought about by the use of several portable terminals with bubble memories, which could hold 100,000 characters (about as much as a fast typist could enter in 8 hours), and which had built-in modems and built-in printers. The information was entered during the day and then transmitted over the telephone to the mainframe computer at night. Since the information was recorded in strings, it had to be converted into fixed fields. That process, together with cleaning the tape of errors, required another 3,000 hours of work. Taking into account the cost of cleaning and formatting the tape, the new technology reduced the overall cost of retrieving and putting data into machine-readable form by about 80 percent per character of information.

By 1986 laptops had become so powerful and cheap that we switched to them. With the laptop we could input the data into fixed fields. As a result our cost of data cleaning and formatting declined by 90 percent over what it had been in 1981. So what have we accomplished? Are we now doing everything for a song? No, Nonsense! We've now gone from about 200 characters per observation to 18,000 variables with an average of about five characters per variable, or about 90,000 characters per observation. What I'm saying is that our research is still very costly, but now it is costly because we're doing things nobody could have dreamed of doing, certainly not in 1978 or even in 1981. We were not nearly as ambitious then as we are in 1990. If anyone had told me in 1978 that we would be dealing with the issues that we are working on today, I would have said it was a pipedream.

Perhaps the most important thing about the new laptops is the possibilities they open up for graduate students. Projects that would have been out of their reach before, because they required a large NSF grant, can now be undertaken with their own resources or modest support from their departments. The dissertation of Joe Ferrie, a graduate student at Chicago, is a case in point. He has drawn a sample of over 20,000 immigrant heads of household from the passenger ship arrival records on file with the U.S. Customs Office and is linking them to the information in the manuscript schedules of 1850 and 1860 Federal Censuses. His aim is to analyze the circumstances of immigrants at their arrival and how these initial conditions and their subsequent settlement patterns affected occupational mobility and wealth accumulation. Ralph Galantine is another graduate student who is benefitting from laptops. He is drawing a sample from the manuscript schedules of the 1855 state census in New York. He hopes to obtain variables that will permit him to pin down the occupational, wealth, ethnic and religious characteristics of voters in order to estimate the independent impact of economic, ethnic, and religious factors on voting behavior.

Let us close with a final question. Is it true that you once said that you could open any work of history and find a Ph.D. dissertation topic in a paragraph or a few lines? Then refocus that question, directing your comments toward the young researchers in our field and tell them if you see some new or old topics that really need somebody to take a hold of, make careers out of, or just get busy on.

On the first part of the question, I think whoever told you that story conflated two things that I remember saying. I have often told friends that every lecture that Kuznets gave suggested at least one major dissertation and sometimes two, and I still feel that way. I recently wrote an essay called "Some Notes on the Scientific Methods of Simon Kuznets."[8] In this connection I read through my old class lecture notes and there are still a lot of good dissertations there.

You ought to publish those notes.

I probably should. The other thing that I used to do involves an exercise I performed in class to demonstrate the ubiquity of implicit quantification in history books. I challenged students to pick any page at random from whatever history book they had at hand. The odds were, I said, that there'd be either an explicit or implicit quantitative statement that needed to be measured. The challenge was often taken up and I was never shown up, but I haven't tried to play that game in recent years. Anyhow, I think two things that I've said, one about Kuznets and the other about tendencies in historical literature got conflated.

On the second part of your question: There are many more good research projects than we can undertake at any point in time, because we don't have the resources to do them all. Which projects get taken up at a particular time has a lot to do with the priorities of society at that time. Certainly the funding is going to depend on what society thinks is important. I'm using society to mean not only Congress, but also administrators and peer reviewers at NSF, at the National Institutes of Health, at private foundations and, to the extent that they have resources to support faculty research, at universities.

I have never been one for proposing to students what they should do their research on. I feel that if students take my courses they will get a pretty good picture of the current interests of scholars. And that should give them a basis for picking a topic. I usually don't throw a menu before people and I rarely propose specific dissertation topics. What I usually do is to tell the students that they should propose a topic and that I'll tell them whether or not it's a feasible undertaking with the resources at their disposal.

So I don't have any specific topics to single out. I'm obviously most interested in the issues that I'm working on, but they're not the only issues worth working on. I often wish I could live two lives because there are a lot of other issues that I'd love to work on. I recently became deeply involved in the statistical analysis of electoral behavior. There will be a very long paper by myself and Ralph Galantine in volume 2 of *Without Consent or Contract* (subtitled *Evidence and Methods*) which attempts to estimate the effect of socioeconomic factors on the political realignment of the 1850s. I wish I could spend 10 or 15 years working on that problem. It's absolutely first rate. But, I think I like the biomedical issues even more. So that's where I'm going to spend the balance of my career.

But if any students want to work on any of the topics under investigation at the Center for Population Economics, we'd be glad to make our data sets available to them. We're now putting all the data sets that we used in *Without Consent or Contract*, as well as the Union Army sample, on deposit with ICPSR (Inter-University Consortium for Political and Social Research, University of Michigan), including data sets that are still very active in our Center.

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[2] Davis, David Brion, "Slavery and the Post-World War II Historians," Daedalus (Spring 1974), 1-16.
[3] Yasuba, Yasukichi. Birth Rates of the White Population in the United States, 1800-1860, an Economic Study. (Baltimore, 1962.)
[4] Papers initially presented at the MSSB - University of Rochester Conference: "Time on the Cross: A First Appraisal," October 24-26, 1974. Herbert G. Gutman, "The World Two Cliometricians Made: A Review Essay of F + E = T/C," published as Slavery and the Numbers Game: A Critique of Time on the Cross. (Urbana, 1975) Edward Shorter, "Protein, Puberty and Premarital Sexuality: American Blacks v. French Peasants [unpublished]. Richard Sutch, "The Treatment Received by American Negro Slaves: A Critical Review of the Evidence Presented in Time on the Cross," published in Explorations in Economic History 12:4 (October 1975), 335-438.
[5] "Second Thoughts on the European Escape from Hunger: Famines, Price Elasticities, Entitlements, Chronic Malnutrition, and Mortality Rates." For a brief summary of discussion of that paper, see the Clio Newsletter 4:3 (July 1989), 19-20.
[6] Robert Evans, Jr., "The Economics of American Negro Slavery," in Universities-National Bureau Committee for Economic Research, Aspects of Labor Economics, (Princeton, 1962), 185-243.
[7] In David Carr, William Dray, and Theodore Geraets, eds., Philosophy and History and Contemporary Historiography (Ottowa, 1982), 61-112.
[8] Afterword to Simon Kuznets. Economic Development, the Family, and Income Distribution: Selected Essays (Cambridge, 1989), 413-438.

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A data set of European prices (1500-1914) is available for interested users. These data include long term series of market prices for wheat and other cereals (along with quantities for some series). The data are in monthly or annual form and are from Austria, Belgium, France, Germany, and The Netherlands.

We would be particularly interested in the possibility of exchange of data permitting an extension to:

1) other countries such as the U.S., the U.K., Sweden, China.

2) other prices such as long term series of stock prices.

Request should be made to Bertrand ROEHNER
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SOCIETY AND NEWSLETTER ANNOUNCEMENTS

Editorial Changes for Volume VI, 1990-1991: Beginning September 1990 Sam Williamson will be in Australia, on leave from Miami University for a year. For that period, John Lyons, Miami University, will be Acting Editor of the Newsletter, and Lou Cain, Loyola University (Chicago), will be Acting Associate Editor. From January through June, 1991, John Lyons will be visiting at Queen's University, Kingston, Ontario, but all editorial and business matters will continue to be channeled through the Society's office at Miami University.

Newsletter: The deadline for submitting items for the October issue of the Newsletter is September 4, 1990.

Institutional Subscriptions: The Trustees of the Cliometric Society, joined by the Editors, encourage members of the Society to ask the libraries of their institutions to subscribe to the Newsletter, beginning with Volume VI, 1 (October 1990). The institutional rate is $25.00 per year. Pease send a note to your librarian suggessting they subscribe.

Membership List and Directory: A number of errors has already been noticed in the Society's Membership List, Spring 1990. We ask all members to check their individual listings in detail, and to forward corrections to the Society's office in Oxford by 30th September 1990; this applies especially to those members who have moved recently. A supplement to the Directory will be published as an insert to the October Newsletter.

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