The Newsletter of

The Cliometric Society

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Cliometric Society at the 1989 ASSA Meetings

by Samuel Williamson (Miami University)

This year's sessions of the Cliometric Society at the ASSA meetings were quite successful, with 40 or more in attendance at each session. I attribute much of the success to the topics chosen and congratulate Dan Raff and Susan Carter for putting together a program that had such broad interest. Summaries of the papers were sent to you with the October *Newsletter*; they are now also available on our file server.

The sessions had a nice balance of cliometricians and economists in both the paper givers and the audience. This produced an interesting mix and allowed for useful cross discussion of the interrelationships between the historical evidence and current problems.

The first session started out with Brad de Long's question, "did J. P. Morgan's men *add value*"? He said yes, and (as is the trend in many discussions today) compared the investment banks of the 1900's with the "money trusts" of present day Japan. His data showed that a company with a Morgan man on the Board outperformed comparable companies. A possible reason for this is that the Morgan firms were selective and spotted winners before the market did. Also, once on the board, Morgan men could help to replace poor managers, increase the ability of the company to exercise oligopoly power, and increase the firm's market value because of the Morgan name.

The next two papers came to almost opposite conclusions about the benefit of market monitoring of corporate performance. Bill Lazonick argued that in the heyday of U. S. industrial capitalism, it was the upward mobility of career managers within the firm that contributed to firms taking actions promoting long term growth. Decline set in once top executives started receiving stock bonuses. They became interested only in the bottom line, and became excessively mobile. Michael Jensen, on the other hand, argued that the restrictive laws of the 1930's, including parts of the SEC Act, the Chandler Act, the 1940 Investment Company Act, and the Glass-Steagall Act, all resulted in corporate managers becoming increasingly autonomous. During the 1960s, corporations generated excess cash and used it to form unproductive conglomerates, but the LBOs of the 1980s have both gotten rid of much unproductive management and imposed new market discipline.

The discussion of these papers was lively. Two issues dominated the discussion: first, can the German and Japanese banks now operate as

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Morgan's bank once did, to the disadvantage of the United States, with its restrictions on bank participation in corporate activities. Has this helped their economies relative to ours? Second, does the stock market do its job, or is the market's rate of discount higher than what is "right" for the economy?

The second session began with a paper by Claudia Goldin and Robert Margo, reporting on some of their findings from analysis of a nominal wage series they have extracted from the payroll records of civilian employees of the United States Army from 1820 to 1856. By looking at changes in both prices and nominal wages, they conclude that there were persistent shocks to real wages during this period. While the impact varied among industries, skills and regions, the shock persisted even five years after an innovation, though eventually the impact vanished.

While the discussion asked about some of the statistical tests and the appropriateness of using particular price series to extrapolate for the whole country, the participants agreed that Goldin and Margo had produced a useful new series to study the past.

The next paper by Simon Johnson, discussed bank credit during the German hyperinflation. He explains that during the 1920-23 period, it was usually only large firms who had access to credit. As nominal bank deposit rates were fixed, bank deposits fell and fewer loans were available to smaller firms. When even large firms were rationed by the commercial banks, the central bank stepped in to provide these firms with large amounts of credit, but the small firms were able to survive by using various methods of disintermediation. The real effect was modest since both output and real wages did not fall until the non-monetary shock of the occupation of the Ruhr in 1923. The discussion complimented the author for providing a more careful look at the data, but suggested that a look at the impact of flight capital and the political situation would add to the analysis.

The last paper, by Jean-Louis Arcand and Elise Brezis, proposed a dynamic disequilibrium explanation of the Depression . They conclude that there was little price or nominal wage stickiness and that the Aggregate Demand curve was upward sloping. The discussion focused on the usual issues of model specification and the appropriateness of particular statistical tests.

The session closed with a spirited discussion sparked by discussant Ian McLean's allusion to the "Solow Syndrome" as it might apply to some of these papers: Solow lamented five years ago at the ASSA meetings that it was increasingly difficult to distinguish economic history from the rest of economics in its excessive reliance on econometric technique. Quoting from Solow: "[T]his sort of economic history gives back to the theorists the same routine gruel that the economic theorist gives to the historian." [Solow's remarks were reprinted in full in William N. Parker, ed., *Economic History and the Modern Economist,* Blackwell, 1986.].

Friday evening the Society sponsored a cocktail party hosted by the University of Georgia and organized by Price Fishback. While there was no beer in the bathtub, and we may have bought too much wine, a good time was had by all with about 30 people in attendance during the evening.

The third session revolved around the issue of Bank failures, then and now. Paul Trescott started off

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An Interview with Lance E. Davis

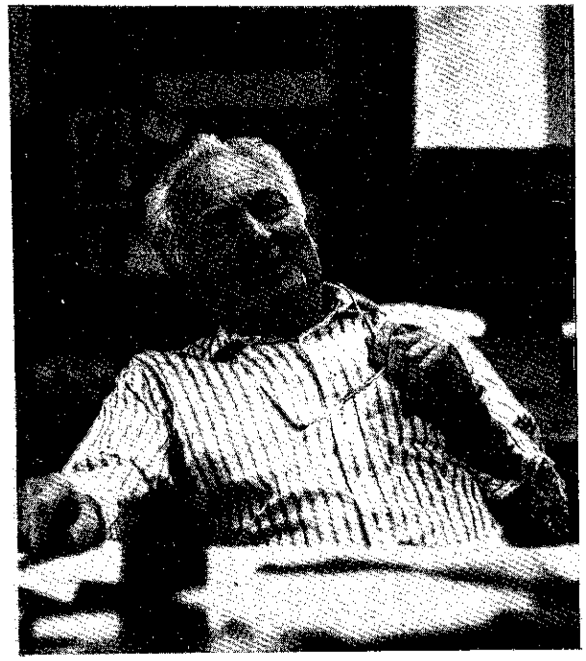
Editor's Notes: With this issue we bring you the first of a series of interviews with distinguished members of our oddly-named tribe.

Lance E. Davis is the Mary Stillman Professor of Social Science at the California Institute of Technology, an appointment he has held since 1980. He started his teaching career at Purdue in 1956 after receiving his PhD at The Johns Hopkins University, and moved to Cal Tech in 1968. He is the author or co-author of numerous works including two important texts in the field, is past president of the Economic History Association, and, most significantly to us, he was co-originator of the Cliometrics Conferences.

I first met Lance while I was a student at Purdue. My first class with him was the second course of the two semester required sequence in economic history, part of Purdue's economics graduate program at that time. I don't think any of us attending then thought that method of study was "revolutionary" or "new"; after all it was history. As a seminar project for Lance, I was running a linear programming model of the 1820 U.S. economy which tested Doug North's model. It seemed like a reasonable approach to me. Little did I know how foreign this was to many in the field.

One of my early impressions about Lance was his frankness. If you wanted advice, he would give his honest opinion. If he thought something was lousy, he would tell you. And he had this technique of saying "why not" when you were not sure you could do something, but he was. And he was usually right.

The questions were put together by John Lyons and me and FAXed to Lance. The first five refer to views expressed by Lance in his article, "And it will never be literature," reprinted from EEH *1968 in Ralph Andreano, ed.,* The New Economic History, *1970, and in his EHA Presidential address of 1979 published in* JEH *March 1980. Later queries relate to Lance's own research and to the recent and early history of the Cliometrics Conferences. Lance responded to us by FAX and the interview was completed by phone. We are sure you will find his replies as interesting as we have.*



Has Fred Lane's piece on "The Economic Consequences of Organized Violence" [*JEH* 1960] had the influence you expected (and wished for)? Or is the relevant long run still far in the future?

I am not certain what I expected, but certainly there has been no massive rush to the barricades. However, there has been a substantial amount of excellent work in the area that could loosely be termed HISTORICAL POLITICAL ECONOMY. Let me suggest at least three different strands to that work:

Work Largely in the Original Spirit of Economic History: One cannot fail to mention Gary Libecap's work. Of particular interest are his two recent pieces, one on the regulation of oil prices (and the Texas Commission) and the other on food and drug regulation and the growth of the meat packing industry [See *JEH* December 1989 and his contribution to the papers of the Second World Congress of CLIO.]. In a similar vein David Feeny's work on Thai development is very much in the spirit of the '79 talk [His 1982 book and *JEH* June 1989].

Historical Work by Political Scientists: In the same way that it was the economists who led the so called "Cliometric Revolution", I expected the political scientists to lead the "Polymetric" revolution. While there has been less work than I would have predicted, David Brady, whose work is precisely the kind I called for, is a full professor at Stanford, and even Michigan (the flagship of traditional political science) has turned out a brilliant young scholar, Doug Dion. Dion, even more than Brady, makes specific use of formal models in his historical research.

A Really New Field: Here I would like to cite two quite different efforts. On the one hand, the cooperative venture by two of the most modern historical quantifiers (Al Bogue and Joel Silbey), with the maverick Brady, and the voice of the political science establishment, Nelson Polsby has proved quite fruitful. On the other hand, there are young scholars who have in fact begun to create a new field. I cite only two - Jeff Friedan's student, Lawrence Broz, who works on the politics of the Federal Reserve Act and Morgan Kousser's student, Shawn Kantor, who has produced a really pathbreaking methodological study of the economics and politics of the Georgia fence laws [Kantor and Kousser, Caltech Social Science Working Paper # 703, July 1989; Kantor, Draft ms. January 1990.].

Have we economic historians become more sophisticated, or not, in the following areas:

Understanding Government Behavior?

Some progress has been made, but mostly not by economic historians, and what progress there is has been spotty (i.e. some valuable, some, at best, of negative value).

Politicians certainly want to get elected, and some work (e.g. by Alesina, *Economic Policy*, April 1989 and others on political business cycles) has produced very useful results. Closer to home, Bob Fogel's work on the 1850's, in *Without Consent or Contract*, has been hailed by both Al Bogue and Morgan Kousser as the most successful attempt to explain that political realignment.

On the question of Bureaucratic behavior there has been lots of smoke but little fire.

Inclusion of income in the Bureaucrat's utility function [Niskanen, *Journal of Law and Economics*, 1975.] may have been a step forward, but certainly the attempts to test that *assumption* fly in the face of all we know about the scientific method.

Analysis based on the assumption that the source of a Bureaucrat's power rests in the asymmetry of information (he is a monopolist) in games with the politicians has not proved fruitful (it really assumes a corner solution). Work in that vein but extended to include an analysis of constitutional constraints (that is, initial conditions) by Romer and Rosenthal on Oregon school boards [*QJE*, 1979; *Journal of Public Economics*, 1979; *Economic Inquiry*, 1982..], however, suggests that there may be some hope in sorting out the cases marked by asymmetry from those without. The work on bureaucrats' discretionary income (Migue and Belonger) is probably more suggestive, but the concept has proved very difficult to operationalize successfully [Migue and Belonger, *Public Choice*, Spring 1974.]. On a more hopeful note, Kiewiet's work on committees and their relations with the bureaucracy have provided on some clues about the relationship between electoral events and bureaucratic behavior [Kiewiet and McCubbins, *Journal of Politics*, 1985.].

Although much criticized, work by Vern Ruttan on the Department of Agriculture seems to me to provide a potentially very useful methodology [*Public Choice*, 1980 and 1982.].

Understanding Human Motivation ("beyond greed")?

There has been a decided unwillingness by economists to look for other motives. The bitterness of Paul Schultz's attack on Easterlin gives you some feeling of the opposition [*Population and Development Review*, March 1986.] - I admit I find it difficult to believe that they really view such research as a mortal threat to the discipline. There are, however, some notable exceptions. First, of course there is Dick's work on international welfare comparison and, with Eileen Crimmins, on fertility, and, more recently, on American youth [Crimmins and Easterlin, *The Fertility Revolution.*]. These are all clearly attempts to explore both economic and non economic motivations. Second, Bob Fogel's *Without Consent or Contract* makes a specific attempt to relate cultural and religious values to politics (and thus, of course, to economics). Third, on a slightly different tack, Naomi Lamoreaux's studies of kinship networks in New England banking is an obvious attempt to extend the traditional list of motives; and she is very effective in linking economic with more anthropological motives. [See *JEH* September 1986 and her forthcoming book.]

Applying useful and relevant theory to empirical historical issues; i.e. using a better mix of theory and fact?

As I have said before, I think the discipline lost a decade to the "I'd rather be clever than right" boys, and we still haven't entirely escaped the application of clever but hopelessly misspecified models. We are, however, gradually emerging from that ethos (only one item on my misspecification reading list is less than five years old), and there is a real hope that we will produce work with a better mix of fact and theory. Although I could cite a number of examples of "better mix", I can think of none that captures the spirit more than Eugene White's piece on the causes of the depression of the 1930's - it is a particularly neat attempt to specify alternative theories and then confront them with the facts. In a similar, although less formal, vein, I also call your attention to Price Fishback's study of company towns and company stores in the early 20th century coal mining industry and to Ken Snowden's work on 19th century capital markets. [See *JEH* September 1986; *JEH* December 1986; *JEH* September 1987 and *EEH* October 1987, respectively.]

Are we still, as you earlier feared, economizing too often by choosing subjects to match up with readily accessible sources?

I am afraid in this regard I remain quite pessimistic; and I think there are two related problems, not just one:

The first is the one to which you allude directly - i.e. the bias in the choice of projects. For example, developments in finance coupled with the availability of some stock market tapes have produced a spate of studies on the behavior of security prices that, as far as I can tell, contribute little to our understanding of anything. In a more general sense, as important as monetary developments are, I think it is the availability of data that at least partly explains the relative fascination for financial as opposed to "real" studies.

The second problem deals with the relative rewards for data collection - particularly collection from archival sources. The Legler-Sylla-Wallis enterprise is a notable exception (and can you guess how many economic historians will make their reputations off those efforts), and, perhaps, it proves the rule. [They are collecting a consistent data set on U.S. "state" and local revenues and expenditures for the late colonial period through 1906.]. Economic historians have generally displayed a marked unwillingness to devote the requisite resources to such activity both because the rewards tend to have gone to the chap with a model and a couple of stylized facts and because it is very difficult to maintain a proprietary interest in a data set for the time needed really to capture a reasonable return from the investment expended in collecting and cleaning. In my thirty-five years in the profession I have noticed few publications that granted the scholar who developed the data equal billing with the person who analyzed it.

This second point brings up another issue that ought to be touched on. Funding for economic history has gotten much more difficult to come by, and funding for archival research almost impossible to obtain. By my last count (no accuracy guaranteed), only one new economic history project (a study of Russian development) has been funded by the NSF over the past three years. The rejected submissions included an excellent proposal by Galenson and Pope that would, as a byproduct, have yielded an important data set on interregional mobility and income. The extent of the problem becomes apparent when one examines a book like Baumol, Blackman, and Wolff's *Productivity and American Leadership.*

The authors are interested in problems of long term growth, but the quality of much of the data on which their argument depends is, at best suspect - and it is not that the authors did not attempt to utilize the best numbers available - as a profession we have failed to provide those basic series. As an aside, I might add that it is not really surprising that the authors did not feel obligated to fill the gap either.

Has any progress been made in our understanding of the "big questions", of long term economic change? Is there anything to learn from recent "big think" works of, say, Eric Jones, Immanuel Wallerstein, or Tony Wrigley?

Thus far, I have not achieved much in a way of an understanding of the Big Picture, but of course I am still young - in spirit if not in the flesh. In the case of both the Jones and the Wrigley books, what I have found useful is not the big but the little pictures - the evidence the authors have adduced to support their more general arguments. In the case of Wallerstein, I admit I haven't found anything very useful, but that is probably only the result of my visceral reaction to the methodology.

Is there a new, brightly illuminated, focus on fundamental work in economic history, emerging from the 1980's? Or is the illumination dim and diffuse?

I guess I believe we are still using the light bulbs of the 70's rather than the halogen lights of the 80's, but that there is substantially more illumination than in the days when we had to depend on whale oil lamps.

In the U.S., a substantial amount of work has been underwritten by the NBER's program in the Development of the American Economy. I make no attempt to provide a complete enumeration, but important work (including the assembly of number of data sets based on primary sources) has been done by Goldin and by Weiss in labor history, on nutrition, welfare and productivity by Fogel and by Floud and Wachter, on savings and the capital stock by Gallman and by Pope and Kearl, on productivity by Sokoloff and by Davis, Gallman, and Hutchins, and on the government by Rockoff and, most importantly, by Legler, Sylla, and Wallis.

Within the next two years the DAE and assorted hangers on will sponsor conferences on Living Standards and Industrialization 1790 to 1860, the Internal Operations of Business Enterprise in Historical Perspective, The Evolution of Labor Markets, and on Historical Studies of Political Economy.

While it is Robert Fogel who has taken the lead in this enterprise (as an aside, I note that Claudia Goldin has agreed to pick up the mantle in a couple of years), and while the steering committee (Davis, Engerman, Gallman, Goldin, and Pope) has devoted time and resources to the activity, special mention should be made of the efforts of Clayne Pope. Without his dedication much less would have been accomplished.

I am less familiar with the work abroad, but let me mention a couple of indications of recent wattage. Up north, Mac Urquardt's massive compilation of Canadian development is all but complete; and that empirical exercise should be grist for the mills of unborn generations of historians who don't want to get their hands dirty with original sources. I have just finished reading Sydney Pollard's, *Britain's Prime and Britain's Decline*, and while not commenting on the work, I can say that the bibliography indicates the extent to which the Cliometric Revolution (and I mean that in the best sense of the word) has affected British historiography. Finally, it does not take a genius to recognize that George Grantham, Phil Hoffman, and David Weir are in the process of rewriting the history of French development. [See, e.g., *JEH* March 1989; *JEH* March 1986.].

Striking the appropriate balance between modesty and conceit, can you comment on how well your work on the British Raj has met the goals and heeded the cautions you set out in 1979 for us all? [L. E. Davis, R. A. Huttenback, S. G. Davis, *Mammon and the Pursuit of Empire*, 1986.].

This question is really impossible to answer. Personally, I feel that the economic chapters of the book are as good as Bob and I could make them. Perhaps with unlimited funding we could have added more firms to the sample (the period 1860 to 1880 is not covered as well as it might be), but I don't really think we would have changed the conclusions. I am not so sanguine about the political analysis. The results are inconclusive and they scream for micro studies of the relationship between the business sectors (finance, agriculture, and industry), the social elites, political parties, parliament, and the bureaucracy. Unfortunately the project had already taken from 1971 to 1985, and how much time does anyone have? Also, of course, there were financial constraints - even the cost of pulling the wills of the 3900+ MP's in order to get a handle on wealth and asset holdings proved prohibitive given the financial constraints. These are, of course, not valid excuses, and the political section of the work could, and probably should, have been better.

In terms of the reception the book has received, it leaves you with a feeling that all you have done is preach to the converted. If you do a 2 x 2 matrix with US and UK the horizontal designators and economists and historians the vertical ones, the upper left hand box is full of enthusiasts, the upper right and the lower left representatives appear to like and probably accept the findings, and the lower right hand box is full of academics who both hate and reject everything about the enterprise. I might add that some members of each set have probably never read the book.

What are your feelings about having been a pioneer in Cliometrics in the 1950s/60s; was there a clearly-defined "enemy"; what happened to them; were they converted, or don't they care?

Looking back at the period (and remembering that the mind tends to black out unpleasant experiences - I sometimes catch myself thinking that my years in the Navy were not really that bad), I don't remember ever feeling that there ever were any real enemies, although there were certainly lots of skeptics. There was direct lineal descent and much affection between the older growth scholars (Simon Kuznets, Mo Abramovitz, and Irv Kravis) and the representatives of the younger generation like Easterlin, Gallman, and Parker. As for the more historiographically inclined, perhaps it was just the extreme arrogance of the young economists, but how could you really believe that anyone who argued that "it's immoral therefore it must have been unprofitable" could be treated as a serious intellectual threat let alone an enemy? Moreover, even the older generation had its share of excellent scholars. For example, Kenneth Stampp, using strictly traditional historical methods, had come to the same conclusion as Conrad and Meyer before they did.

It certainly was difficult to get quantitative work published; and I had my share of losing bouts with George Rogers Taylor. Looking back on it now, however, I can't help feeling that some of that early research may not have been as good as we were convinced it was, and it is just possible (although of course not likely) that it might well not have been published, even if the editing had been unbiased. Moreover, we should not forget that Henry Rosovsky was editing the old *Explorations* in 1955 and John Meyer took over in 1957; moreover, North and Parker became editors of *The Journal* in 1961.

There were, of course, both skeptics and critics. Since the debate was at its peak some 30 to 35 years ago, some have died and all have retired. Some were converted, some still refuse to acknowledge that there ever was a revolution, and some moved into different fields. Economic history has been largely taken over by economists, and they don't much care what anybody else says, sometimes, it seems, even when their critics are correct. Moreover, since most of the practitioners have appointments in economics departments, they have never been forced to confront more traditional historians.

Outside of economic history, and despite heroic efforts by the likes of Al Bogue and Jerry Clubb, QUASSH (QUAntitative Social Science History) has not fared so well - there aren't many jobs and the work has had substantially less impact in social and political history than the work of Fogel, Gallman and the boys had on the economic side. Recently I spoke to a now not-so-junior scholar who had been a post doc at Caltech. In explaining why he had been such a success as "the quantitative historian" in a well known history department he said, "I've got just the mix of skills they want - some quantitative interest but not too much quantitative interest."

Similarly, some of the new labor historians clearly don't care. They have a message to peddle and neither facts nor analysis are allowed to get in the way.

However, the entire intellectual landscape is not so bleak. Let me suggest you read Gene Genovese's review of *Without Consent or Contract.* Gene, a very distinguished left wing historian of the Old South, was among the most strident critics of the entire Conrad-Meyer to Fogel-Engerman line of thought. Today (*Los Angeles Times*, Sunday February 18, 1990) he writes that *Consent* should be added to the list of the five most important books about the South and slavery. Clearly, there has been some give, and maybe some take, on both sides.

Please comment on the direction that studies of financial markets have taken since your early work on the US. In retrospect do you think that the different path you took has been a good guide to fruitful later work by others?

First, let me say, and only partly in jest, that, given the fact that my most recent work on financial markets is more than two decades old (and some of it goes back fifteen years before that), I sometimes find myself both amazed and appalled that anybody has ever read it, let alone is still writing about it. On a more serious level, I think we now know considerably more about the evolution of financial markets now than we did in 1955. Whether you want to enter the dispute about transactions costs versus market segmentation or not (and I think its a silly dispute), most everyone (with the possible exception of Barry Eichengreen) is more or less convinced that efficient well functioning markets are not always there - even the Soviets have begun to realize that, while economists know a great deal about how mature markets work, they know very little about how markets grow and develop. I think that most of the research that has followed my initial forays has been productive, and even some of the conjectures that have later been partly disproved, have provided fruitful insights. I still believe that economic growth and development involve both capital accumulation and capital mobilization, and mobilization can be effective in a decentralized economy only after financial institutions are invented and innovated.

Moreover, I had always viewed my work on capital markets as research that, while focused on an important substantive area, also raised questions of institutional change and analysis in general. I am probably an old fogy, but it is still an article of my faith that for questions of long term change, at least, economists who take institutions as fixed are engaged in a practice that is about as likely to produce useful insights as masturbation is to produce children. Old economic historians had long been aware of the effects of institutions, but cliometricians, with training rooted in comparative statics, were often more than willing to ignore that aspect of the economic environment - it was, after all, difficult to model. Thus, like my belief that my contributions to the capital market literature had no net negative effect on human welfare, I believe that my attempts to call attention to questions of institutional change and analysis, have not measurably reduced the intellectual output of the profession and may even have increased it somewhat.

Continuing, has our historical insight anything to tell current policy-makers? Would they listen if we told them, or have we been addressing too narrow an audience, so that the policy makers haven't gotten the word? Specifically, could historians have helped avoid the S&L disaster of the 1980's if we had thought about the deregulation business and/or could we have provided better policy advice than others might have done (or did)?

On the one hand, as far as I know, only Peter McClelland has ever attempted to use economic history explicitly as a vehicle for policy analysis, but he was certainly successful enough to make further exploration productive. [P. McClelland and A. L. Magdowitz, *Crisis in the Making: The Political Economy of New York State Since 1945*, 1981.]. On the other hand I think it not unlikely that, although thoughtful economic historians might well have raised some flags about the deregulation of the S&L's, those flags would not have been raised very high, and it seems highly unlikely anyone would have responded to the danger signals.

It seems to me, however, that economic historians do have a potential contribution to make to policy analysis. As a group they are particularly well equipped to make three points: (1) the study of institutional technology may be at least as important to any understanding of economic growth and development as the study of technical change more traditionally defined; (2) when considering alterations of existing, or the innovation of new institutions, a policymaker should always attempt to assess the potential long run implications, even if the change is thought to be nothing more than a short term adjustment; and (3) that institutions, unlike traditional machines or processes, have an amazing ability to resist the scrap heap [McClelland makes this point very well in his story of Robert Moses and the Triborough Bridge Authority].

Moreover, the present appears to be a particularly propitious time to turn some fraction of the discipline's attention to these policy related questions. Recent work by economic theorists in mechanism design, by experimentalists in the study of alternative institutional structures, and by political scientists on the political basis for alternative market structures (e.g. is it possible to have a market economy in the absence of property rights) have opened up a new field of research. Moreover, economic historians are particularly well placed to play a major role in this endeavor, since, Charlie Plott aside, history provides the only effective laboratory for the study of long term institutional change.

Where exactly did you expect it to end when you turned down the hall in Stanley Coulter Annex [in 1961 at Purdue] to present Ron Stucky with the Hughes-Davis (or Davis-Hughes or McDougall-Hughes-Davis) manifesto?

Memory fails me, but here goes. Jon Hughes and I had observed at first hand the first of Stan Reiter's Midwest math econ symposia, and we concluded that the seminar had achieved at least four worthy goals: (1) it had provided a format that permitted the few high-tech economists who then graced the halls of the Midwest campuses to carry on a productive intellectual interchange (if memory serves me, Minnesota and Purdue aside, no university was represented by more than one of this strange new breed of scholar, if scholar is the right word); (2) it gave Purdue, its Dean, and its new program both intellectual stature and fine PR - at least within math-econ circles; (3) it made it possible for these few scholars to spend a substantial amount of free time together to discuss mutual problems in a very informal way (Oz Brownlee always organized a "high" stakes poker game); (4) and it gave hangers on like Jon and me the chance to drink a great deal of "free" beer while observing the practitioners of this strange new discipline.

I really don't remember whose idea it was, but it appeared to one or both of us that some of these same goals (particularly #4) would aid our efforts to establish the New Economic History at Purdue (if Stan and his buddies got all the recognition and press, there were going to be few dollars left for the likes of us) and maybe elsewhere as well. On a more serious note, it was the chance to break out of the relative isolation of West Lafayette and talk with other like minded economic historians about what we saw as the most important potential contribution of the enterprise. I had been much impressed by the group of scholars who had collected at the Chapel Hill Conference on Income and Wealth and by those who had attended a seminar I had given in Alex Gerschenkron's Harvard workshop. Jon still retained pleasant memories of his work with colleagues at the New York Fed and the work then going on at Columbia (my memory is weak, but I think maybe he had recently been on leave teaching David Landes' classes at that University or maybe he had just been there to give a paper).

Anyway, partly in defense of the local economic history establishment and partly to see what the rest of the world had to offer in terms of the new history, we approached Emanual T. Weiler and then, with his blessing, Associate Dean Ron Stuckey. Our pitch was straightforward. Ours was a new "hot" field. With three scholars in residence we had a leg up, but we were not alone and the opposition was gaining: Penn had established a joint history-economics PhD program, Gerschenkron had produced Al Conrad, John Meyer, and the two Henrys (Rosovsky and

Broude), and rumor had it that the seminar was now full of young potential hotshots (since that group included Al Fishlow, Paul David, and Peter Temin, the rumor was apparently correct). Ron bought the story (with Em's blessing there was probably no way he could refuse), and we got the go ahead.

There was only one more problem: Who should be invited? Clearly we could not let ourselves be limited to the Midwest (it would have become obvious that there were far fewer economic historians than mathematical economists and there weren't many of the latter); and we opted for a "national" meeting. It's not clear how any group of twelve or thirteen could be truly national, but we did entice Doug from Seattle, Bill from Yale, Bob from either Ohio State or North Carolina (I can't remember which) and a few others. Although that first meeting was exciting (and a lot of beer was drunk), it was not until we saw the potential list of invitees for the second and third years (we had picked up Fishlow, Fogel, David, Temin, Easterlin, to name only a few) that I became convinced that we were really onto something. Jon probably recognized it earlier.

Finally, do you still think, after another 20 years or so of reading cliometric work, that "it will never be literature"?

[Chuckle] With the possible exception of Bob Fogel's latest book, which I have not yet read, the answer is yes.

(ASSA: continued from page 2)

reporting on the failure of the (private New York commercial) Bank of United States in 1930. He explained that though this failure did have some local effect on consumption expenditures, it was not an atypical failure for any downturn, and its downfall was because of poor management of its real estate portfolio.

Ed Kane followed with a model that explains the failure of various deposit insurance funds as resulting from a conflict of goals. His hypothesis is that beside safety for the depositors, they were either set up to assist small banks to compete or, in the case of the FSLIC, to "help the saving and loan associations attract funds." Many of the failures were because weaknesses were concealed from the public to prevent loss of confidence.

The next two papers were by Charlie Calomiris and Michael Bordo. Calomiris argues that the prohibition of branch banking in the U.S. has increased the number of bank failures and that, in his view, branch banking with a private insurance program would have "been adequate to protect the payment system from exogenous disturbances that might produce banking panics."

Bordo's paper explained that the classical doctrine of how central banks should operate as a lender of last resort has been long understood, but that in the last 20 years it has not been adhered to. The policy established in the 19th century was to loan money at a penalty to illiquid but solvent banks, but not to bail out those who were insolvent. Recent behavior of central banks has created a serious moral hazard problem.

The discussion was lively with much of it dealing with ex post explanations of what had caused the saving and loan crisis of the 1980's. At one point, Lawrence White, who had been with the Federal Home Loan Bank Board, complained that no one had told them that after deregulation the S&L's would "go wild." He acknowledged that Ed Kane and others had said the system had problems, but no one had predicted how much worse it would get. This brought home to the Society members in the audience an important point discussed at last year's meeting, that is, how do we get policy makers to listen to the lessons of history?

Next year's ASSA meetings will be in Washington, D.C., with Knick Harley, Paul David, and Price Fishback as the Clio program chairs (see page 20). We hope that many of you will attend.

CLIOMETRICS IN THE USSR*

by L. I. Borodkin (Moscow University)
* translated by Carol S. Leonard

Cliometrics has existed in the Soviet Union since the 1960s.[1] From the founding by Academician Ivan Koval'chenko of the first cliometrics organization, the Commission on the application of mathematical methods and IBM to historical research at the Academy of Sciences of the USSR, the field has expanded to include about seventy active scholars. The Commission has accomplished a number of goals: publications (seven collections of articles have been published under the aegis of the Commission and the Academy of Sciences press)[2]; annual conferences (organized every two years by Moscow University and in alternate years by the Academy of Sciences of the USSR—averaging 100 participants); the creation of a working seminar ("Quantitative Methods in Historical Research"); and generation of data bases (there are now about 50 data collections covering seventeenth to twentieth-century history).

Although there are research groups at Academy of Sciences institutes in Moscow, Estonia, Belorussia, Kirgizia, and at university centers in Leningrad, Dnepropetrovsk and Azerbaidzhan, the center for cliometrics is the history faculty at Moscow University, where there is a laboratory for quantitative methods. In the 1970s, when universal knowledge of computers and statistics became a goal of the educational reform movement in the USSR, Moscow University began to offer a year-long course in quantitative methods for historical research. Beginning in 1986, this course—and the textbook written for it[3]—have become a part of the instructional program at 68 universities. The large majority of participants at cliometrics conferences are always students and younger scholars.

The distinguishing feature of cliometrics in the USSR is that economic history has not been drawn away from social, political and cultural history. Most quantifiers begin as historians rather than economists, and this affects both the subject matter and methods used in their research. Soviet cliometricians use statistical methods, especially multivariate analysis, game theory, linear programming, differential equations, Markov chains, and the theory of fuzzy sets. However, they rarely use macroeconomic modeling, equilibrium models, efficiency functions, or concepts of demand and supply. They rarely apply counterfactual models.

Another distinguishing feature is more substantive, reflecting both historical concerns and available data. Comparing cliometrics in the United States and the Soviet Union, I. D. Koval'chenko and V. A. Tishkov wrote in the introduction to *Quantitative Methods in Soviet and American Historiography*,

> one may acknowledge that in the number of volumes using quantitative methods, the number of specialists, the resources, and the amount of machine-readable data, the field of cliometrics is more backward in the Soviet Union than it is in the United States. However, in its more gradual pace of penetration, the influence of quantitative methods in Soviet historiography is more organic, and it has established sway over the analysis of fundamental scholarly problems.[4]

The main concerns of Soviet quantitative historians can be summarized by periods:

Feudalism:

Agrarian history under feudalism has been explored by analysis of land surveys and topographical materials, lists of fugitive peasants, price data, and surveys of feudal dues.[5] The most interesting work has been done on the first half of the seventeenth century by the Moscow historians in a team led by L. V. Milov.[6] In general, the seventeenth century was characterized by social conflict: the time of troubles between regimes, the revolt of Bolotnikov, the Polish-Swedish interventions—which occurred against the background of a deepening depression. Milov has examined landholding categories for this period. He has used regression analysis, principal components and cluster analysis. He has shown that estates of feudal lords, for example, were more efficient than those of lesser nobles. This research showed need for reevaluation of the idea that the decline of feudalism dates to the end of the sixteenth century.

Russia in the 19th and 20th Centuries:

For pre-revolutionary Russia, there has been extensive study of the development of capitalism. In the 1960s and 1970s, agrarian cliometricians looked at the internal structure of peasant farming and nobles' estates[7] and yields.[8] They have developed typologies for the study of agriculture.[9] The capitalist agrarian market was studied in a fundamental work by I. D. Koval'chenko and L. V. Milov.[10] With N. B. Selunskaia, Koval'chenko also published two statistical studies of peasant farming and nobles' estates at the end of the 19th century, developing criteria for measuring capitalist agrarian development before the revolution.[11] There is a cycle of works by B N. Mironov tracing the dynamics of grain prices in Russia over two centuries.[12] Leningrad historians have done computer analysis of "Ustavnye gramoty" [regulatory charters], which defined the relationship between landlords and peasants after emancipation in 1861.[13] I. D. Koval'chenko and L. I. Borodkin have developed a social typology of agrarian development and examined regional aspects of the bourgeois evolution of European Russia on the eve of the 19th-20th centuries. Using data for fifty provinces, the authors determined that of the two paths of evolution, the American way (small farming), where it was practiced in Russia, led to a higher level of economic and social development than the Prussian way.[14] P. G. Ryndziunskii used quantitative methods in his study of the peasant and the town in the nineteenth century.[15]

Estonian cliometricians (Iu. Kakhk and his colleagues) have had enormous influence on historical writing. They have used quantitative methods to analyze social and economic conditions and to correlate them with peasant revolts, and they have studied influence of class interests in the preparation of agrarian reform.[16]

Soviet cliometricians have used statistical methods to examine the history of industry, trade and finance for the period of capitalism. They have developed a regional typology of Russian industrial development on the eve of the 20th century; they have constructed an index of industrial development for each of eight clusters, derived from 50 provinces.[17] A Gini index was used to compute the concentration of production and workers and the market outlet for branches of industry in Russia at the turn of the 20th century.[18] Industrial statistics have also been used to examine living conditions of workers and their correlation with strikes.[19]

T. F. Izmest'eva used conditional ranking to examine factors in the movement of prices of coal, kerosene and other exports from Russia.[20] I. M. Promakhina has done spectral analysis of regular fluctuations in state revenues and expenditures for 1812-1914."[21] Identifying the fluctuating component yielded eight-year periods, which she interpreted as cycles in the economic development of Russia.

Soviet period:

The most decisive impact of cliometrics for the Soviet period has been in the area of the agrarian history of the 20s and 30s. The use of mathematical methods and computers has produced a data base drawn from materials of budget surveys of peasant households for the period before collectivization (the first half of the 20s). The work of Iu. P. Bokarev, V. A. Obozhda, V. P. Pushkov, N. G. Minialo and others produced structural models of social types of households for several regions of the country.[22] Mention should be made of work by research teams, led by V. P. Danilov and V. M. Selunskaia, who studied the 20s,[23] and E. M. Skvortsova's work on collectivization (1934-1937) of the non-blackearth center.[24] There have been quantitative studies of kolkhozy, agricultural production (for the entire period of Soviet history), industry, and the working class.[25]

Foreign area studies:

There are fewer works by cliometricians on the history of foreign countries. K. V. Kvostova has written works on Byzantium in the 13th-14th century; Iu. Kakhk, the Estonian specialist, has written about Europe from the 14th-19th century; V. N. Malov has written about the formation of the internal market for grain in France in the 18th and 19th centuries.[26]

Apart from source analysis, in applying mathematical methods to history, Soviet scholars have modeled historical processes by simulation of alternative outcomes. V. A. Ustinov first experimented with models. The goal of his research group was to simulate the social and economic development of ancient Greece during the period of the Pelopponesian wars (431-404 BC).[27] Using a system of differential equations, they reconstructed an array of yearly indicators of the condition of the economies of the warring city-states. This work has evoked a lively debate among cliometricians.[28]

In the 1980s, the history of NEP in the 1920s and Russian-German relations in the late nineteenth-early twentieth century have been the subject of modeling. B. I. Grekov looked at foreign (primarily German) investment in Russian publicly-held banks from 1900-1914. He examined two questions that have been important in Soviet historiography, 1) the extent of development of monopoly capitalism in Russia before the war, and 2) the process of "denationalization" of Russian finance capital in that period. His findings do not uphold the traditional Soviet view.[29]

V. M. Sergeev and V. P. Akimov developed a model based on game theory ("prisoner's dilemma" and "chicken") to analyze Russian-German relations in the 1870s.[30] They described the development of conflict situations between the two countries by game theoretic matrices.

Iu. P. Bokarev has studied the period of NEP. Analyzing the disproportion between agriculture and industry, the so-called "scissors" of agricultural and industrial prices, Bokarev asked if the lowering of industrial prices would have eliminated the scissors. He chose a game theoretic model with two agents: the state and the peasant. He demonstrated that the crisis of the 1920s was inescapable (the price of a unit of industrial goods was more than two times the price of agricultural goods). Bokarev used another optimizing model to study the structure of the peasant economy in the 1920s. In this situation, optimality meant the maximum income under certain constraints (cost of the means of production, cultivated land, etc.). The optimization model, drawn from linear programming, showed that the typical semi-proletarian household during the early 1920s had parameters close to the optimal.

L. I. Borodkin and M. A. Svishchev have modelled social mobility in the NEP period. They analyzed tax data on private enterprise in the Ukraine in 1925. All firms (about 100,000) were placed into five groups by scale of enterprise, where the fifth and largest extensively relied on hired labor. Movement between categories was extensive: within one year, more than 1/3 of all firms changed in status. The question was, what would have been the structure of the economy if NEP had run its course instead of ending in the mid-twenties? They were especially interested in the fate of the fifth group, in 1925, roughly 3%, since this was the element whose rapid growth was feared. Borodkin and Svishchev used Markov chains and five different possible scenarios for all firms. For all scenarios, it was discovered, for the first half of the 1930s, that the fifth group could not have exceeded 4-5%. The model thus showed that assumptions about the ineffectiveness of state regulation of the private economy during NEP were unfounded.[31]

In conclusion, the position of cliometricians in economic history is strengthening. Most historians accept the necessity of methodological precision and wider use of statistical data. This understanding has grown in part because Soviet cliometricians attempt to discuss their results in language that is accessible to all. There have been special books on methods, where examples are provided. These books have been addressed to the historian[32] and to the general reader.[33] I. D. Koval'chenko's monograph in 1987 on contemporary methods in historical research was particularly important for the development of cliometrics in the USSR.[34]

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(Moscow, 1975), hereafter *MMISEI* 1975; Ed. I. D. Koval'chenko, *Matematicheskie metody v istoriko-ekonomicheskikh i istoriko-kul' turnykh issledovaniiakh* (Moscow, 1977), hereafter *MMEKI* 1977; Ed. I. D. Koval'chenko, *Matematicheskie metody v sotsial-no-ekonomicheskikh i arkheologicheskikh issledovaniiakh* (Moscow, 1981), hereafter *MMSEAI* 1981; Ed. I. D. Koval'chenko, *Matematicheskie metody i EVM v istoricheskikh issledovaniakh* (Moscow, 1985), hereafter *MMII* 1985; Ed. I. D. Koval'chenko and V. A. Tishkov, *Kolichestvennye metody v sovetskoi i amerikanskoi istoriografii* (Moscow, 1983); Ed. I. D. Koval'chenko, *Matematicheskie metody i EVM v istoriko-tipologicheskikh issledovaniiakh* (Moscow, 1988), hereafter *MMITI* 1988.

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[11] I.. D. Koval'chenko, N. B. Selunskaia, and B. M. Litvakov, *Sotsialno-ekonomicheskii stroi pomeshch' ego khoziaistva Evropeiskoi Rossii v epokhu kapitalizma* (Moscow, 1982); I. D. Koval'chenko, T. L. Moiseenko, and N. B. Selunskaia, *Sotsial' no-ekonomicheskii stroi krest' ianskogo khoziastva Evropeiskoi Rossii v epokhu kapitalizma* (Moscow, 1988).

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[34] I. D. Koval'chenko, *Metody istoricheskogo issledovaniia* (Moscow, 1987).

Clio, Glasnost', and Perestroika

In January 1990 a delegation of Americans traveled to Moscow to continue the growing scholarly interchange among U. S. and Soviet economic historians. Carol Leonard and Alan Olmstead , delegation leaders, were accompanied by Roger Ransom, Richard Sutch, and Theodore Huller, Chancellor of UC Davis. The visitors were made most welcome, and were treated to good company, good food, a trip to the old monastery at Suzdal', and a great deal of scholarly interchange with many of those whose work is surveyed in the preceding article.

A major purpose of the delegation's visit was to continue preparations for a proposed joint Soviet/North American conference, to be held in the San Francisco Bay area in March or October of 1991. The general topic is "Rural to Urban Transfers of Resources in the Process of Development of the USSR and the USA, 1840-1940." Final plans are not yet complete; interested persons should write to Alan Olmstead, Institute of Governmental Affairs, UC, Davis, California 95616. The Conference will be the third in a series begun in 1984 in Montreal, followed in 1987 by a conference in Tallinn. [Papers from these conferences have been published in *Research in Economic History*, and in *Agricultural History* and *Russian Review*.]

In addition, Sutch lectured on US cliometric methodology and on results from the Ransom-Sutch History of Saving project, Ransom spoke on the US Civil War period, and they both exchanged data with Soviet cliometricians. Leonard and Sutch met with Soviet archival authorities to discuss forming a consortium to continue improving access to Soviet archives, and to begin building a data bank on the Russian/Soviet rural economy before 1940.

Academic glasnost' is a reality and Soviet quantitative historians are enthusiastic about expanding scholarly discourse with counterparts in the West. Further reports will appear in these pages.

The CLIOMETS Fileserver is Working Better (We Hope)

(Oxford, OH) We know some of you have had problems using our BITNET file server and that there has not be much on there for you to use. This is changing. We have put a great deal of work into getting the system to where you can update info from our membership files and acquire papers that are on file. We have had one working paper sent to us and we have put up all the papers that were presented at the December 1989 ASSA meetings. We should soon have the ASSA papers presented in 1988 and the abstracts of the 1990 Cliometrics Conference as well.

For those who are directly on BITNET the easiest way to interact is to use the TELL command as explained in the February 1989 Newsletter. For those on other networks we have devised a way to use the MAIL system to interact. The same commands work; however, you must put a / before each command.

If you want new directions and you are logged onto BITNET, the procedure is to type "Tell CLIOMETS @MIAMIU Help" or "Tell CLIOMETS @MIAMIU Get Info" The second will give you a more detailed set of instructions than the first.

If you are on another system (this will work on BITNET as well) you can use the mail system. While logged on you should type MAIL CLIOMETS @MIAMIU and hit return. When it comes to the place to send the message (you may have to hit return again if it asks you for ####) type /help, then hit return and type /get info and return. You can have as many lines requesting things from the fileserver as you please. When you are finished typing, send the mail and CLIOMETS will mail back what you request.

You can still send normal mail to CLIOMETS and it will be checked regularly. I realize that the last few months things may not have been getting through. If you have any problems or questions you can send them to me at SHWILLIA @MIAMIU. Again I apologize for any problems you may have had using the system.

Proposal to Establish a Data Archive at UC Riverside

Updates in Future *Newsletters*

by Roger Ransom
(University of California, Riverside)

(Riverside, CA) The availability of micro-computers capable of performing complex operations with very large data bases has facilitated the collection and analysis of sizable bodies of data by those engaged in quantitative historical research. Research tasks that only a few years ago seemed Herculean are now regarded as routine. This is all to the good; the benefits to researchers from the enormous fall in the cost of data collection and compilation are obvious to even the most casual observer. Still, the proliferation of individual data sets in machine-readable format has created some potential pitfalls for researchers — particularly if one takes a macro perspective in viewing the proliferation of samples being collected by researchers working on specific research projects. I think it is time we paused to reflect upon some problems posed by the explosion of data collection that has accompanied the spread of the micro-computer.

To illustrate how the micro-computer has transformed the "industry" of data collection, we need only look at the ways in which researchers have used one of the major sources of quantitative data in the United States: the manuscript returns of the decennial censuses. Since the late 1960s these documents have been the focus several major research efforts. Without taking time to construct a careful bibliography I can think of six major computerized data samples drawn from these records (the Parker-Gallman, Atack-Bateman and Ransom-Sutch samples of farms in 1860 and 1880, the Atack sample of firms in 1880, and the two public use samples from the 1900 and 1910 population schedules). Each of these projects required major financial support. But with a micro-computer, anyone can take the microfilms and drawn a random sample to suit individual needs. I would hazard a guess that in the past five years at least a dozen smaller samples have been collected from the same sources. One obvious problem from this situation is that research effort may be duplicated. A more subtle problem is that the greatly lowered costs of data collection have made it impossible to keep track of all the small samples collected for specific purposes — a task that has not been adequately addressed by either the researchers or the funding agencies. NSF now requires that data sets generated from projects funded through their largesse must be "archived". But the definition and choice of an "archive" remains vague. The Inter-University Consortium for Political and Social Research [ICPSR] at the University of Michigan is the favorite for large data sets. Over the years, the ICPSR has amassed a large collection of data sets pertinent to quantitative history, but their collection is hardly complete, and costs of belonging to the Consortium leave many researchers outside the circle of those able to tap this valuable resource. For many, the only viable option for "archiving" a rather modest data set is to put it on a floppy disk and then mail a copy to those who request it.

While this system works, it suffers from two serious drawbacks. First is the absence of any "clearing house" to provide information on data sets being developed by cliometricians. A second drawback is the preparation of careful documentation for easy use of data by other researchers. Two years ago, when I assumed the editorship of *Research in Economic History*, one of my objectives was someday to provide an "efficient" means of disseminating information on data sets to interested researchers. The project was back-burnered during the various delays in getting my first volume of *REH* in print. Volume 12 of *REH* is finally out, and I have resurrected my goal of having *REH* act as an information "clearing house" and repository for data sets of interest to researchers in quantitative economic history. The idea of such a data archive was suggested by members of the All-UC Group in Economic History at the University of California (which already has helped fund a pilot program to collect and archive data on nineteenth-century workers). The archive would be in the Laboratory for Historical Research at the University of California, Riverside.

Let me offer my thoughts on how such a project might be set up. As editor of *REH* , I have emphasized the journal's interest in publishing manuscripts that present and examine new quantitative evidence at some length. Beginning with the next volume of *REH*, the editors will ask that any data generated in connection with the research relevant to an article must be submitted to the journal in a machine-readable format <u>with documentation</u> ready for distribution. The journal will then make the data available to anyone interested in pursuing research in that area. This, of course, is only an initial step in providing for the collection and dissemination of information on data sets.

Since there are many data sets that will not appear in *REH*, and there is a considerable publication lag, the journal is not a suitable vehicle for disseminating information on what is current in the world of computerized data collection. Fortunately, there is an obvious outlet for such information: *The Newsletter of the Cliometric Society*. *REH* can act as the center that collects information, and there would be a regular column in the *Newsletter* announcing the latest sets deposited, how to ask for copies, etc. This information would be kept up to date and members could check BITNET for complete information between *Newsletters*.

The creation of a forum to deal with issues surrounding computerized data sets would enable researchers to discover the existence of data sets; getting the data to those interested in using it is the next challenge. For those who work with high-powered machines and ingenious assistants to unravel the complexities of machine languages and software interfaces, exchanging computerized data is reasonably simple. For the rest of us, who work with less-sophisticated equipment and levels of technical assistance, getting someone else's data up and running can be a most vexing task. My own experience both in distributing data and deciphering data sets sent by others has underscored the need for some degree of standardization in the handling of data for micro-computers. For the past several years, Susan Carter, Richard Sutch, and I have been developing a project that will eventually encompass the assembly of more than

100,000 responses contained in more than 150 surveys taken by state bureaus of labor between 1874 and 1920 into a computerized format. To date we have completed collection of 12 of these surveys containing just over 16,000 responses. (Those interested in knowing more about this project can write me for a copy of a paper describing the data and the plans for collection.)

The experience gained in that effort has convinced us that the organization of data into common formats such as ASCII (or "text"), DBASE, or LOTUS is relatively simple. What is not simple is the compilation of careful documentation for each data set. As anyone who has struggled with the collection of data can attest, the preparation of documentation is a painstaking and not very exciting task. (Which may explain why researchers interested in quickly getting to the analysis of specific variables in the data set often overlook the need to document <u>all</u> of the variables.) A major motivation behind my proposal is a concern that the documentation of data sets must be improved to the point where any researcher can easily use the data provided. The Carter-Ransom-Sutch data collection project has devoted a major effort to the development of a format for documentation to accompany the distribution of each data set. Each of our "codebooks" contains a brief description of the data, a full list of variables and codes used in the processing of data, and some tabular presentation of the means and/or distribution of key variables. We would expect that data sets submitted to *REH* for distribution would be accompanied by a codebook that follows a similar pattern. (I will gladly send one of these codebooks to anyone interested in examining it as a prototype for their own work.)

Let me close by noting that the ideas outlined above are still tentative. Two things should be apparent. First is that I am not seeking to replace the ICPSR as an archive for major data collections. My focus is on the smaller samples that are derived from focused research projects. Second, it should be clear that, if my proposal is to work effectively, there must be a high degree of co-operation among those interested in exchanging information and data. Neither *REH* or the Lab for Historical Research has sufficient resources to "clean" and document data sets submitted for dissemination. Nor can we check every report of data collection for accuracy. In short, it will be up to the researchers themselves to see that reasonable standards are maintained. Despite the well-advertised shortcomings of "self-policing", I believe the time has come for the creation of a "clearing house" for cliometric data.

I welcome your suggestions and reactions.

Classifieds

The deadline for submitting items to the July Newsletter is May 30, 1990.

Seventeenth Conference on the Application of Quantitative Methods in Canadian Economic History

The 17th Conference on the Application of Quantitative Methods in Canadian Economic History will be held on 9 and 10 November, 1990, at Queen's University in Kingston.

This is a working conference: the objective is to encourage the use of a wide range of quantitative and theoretical methods in Canadian economic history. Participants will present and discuss papers or work-in-progress. Interested scholars who wish to contribute a paper or report on their current research should submit a brief abstract of the proposed paper, and a copy of their curriculum vitae (which is required for conference grant applications). Graduate students are encouraged to participate. The deadline for submission of abstracts is 1 June, 1990.

Please send abstracts to:

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CALL FOR PAPERS FOR ASSA MEETINGS 1990
INCLUDING JOINT SESSION WITH AEA

Anyone interested in presenting a paper at The Cliometric Society sesions at the ASSA meeting in Washington, DC, December 28-30, please note the following deadlines. Members are urged to pass this announcement on to their colleagues and students who might want to submit their work.

Each year we have had a joint session with the American Economics Association. This year Paul David has been asked to organize one or two sessions for the American Economics Association. He will select papers for the joint AEA/Cliometric Society session from those submitted in response to this call. Papers selected for the joint session might be published in the *AER*.

Deadlines that must be met:

May 21 - Two copies of a two-five page proposal of your paper is received by:
Price Fishback
Department of Economics
Brooks Hall
University of Georgia
Athens, GA 30602

Price, Paul David, and Knick Harley are serving as the selection committee. Notification of which papers have been accepted will be mailed from the Society office by June 15.

August 30 - An eight-page summary of your paper is received at The Cliometric Society office: Department of Economics, Miami University, Oxford, OH 45056 (Tel. 513-529-2850). This will be published in the October Newsletter. Please do not submit a proposal if you cannot meet this deadline.

December 4 - The final version of your paper is in the hands of the discussants and other members of the session to which you have been assigned.

Chairs and discussants needed. Since it is difficult to know who is going to be attending the ASSA meetings, if you are interested in being involved in the Clio sessions, we would appreciate hearing from you by June 15.

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