

The Cliometric Society

February 1991 Vol. 6 No. 1

Clio at ASSA: Washington, D.C., December 1990

by Price Fishback, University of Arizona
and Knick Harley, University of Western Ontario

The Cliometrics sessions at the ASSA meetings were lively and informative. The audiences were relatively small because two of the sessions were scheduled at 8:00 a.m., and later sessions faced direct competition from other economic history sessions. The evening party was hosted by John Wallis in the University of Maryland suite. In general, the discussants emphasized how much they liked the papers throwing out phrases like "ingenious use of evidence," "innovative methods of testing hypotheses," "believable conclusions," and the like. Being cliometricians, however, the discussants and members of the audience did have a few "quibbles here and there." The emphasis here will be more on the quibbles than on the high praise.

The first Clio session, "Economics in Times of Crisis" consisted of three papers on war and its aftermath in the twentieth century. Bob Higgs opened the session with his paper "Wartime Prosperity during World War II?" in which he argued the consensus that the war got the economy out of the depression misreads the history of the period. His argument contained two parts. First, standard models of macroeconomic performance cannot reasonably be applied to the wartime situation in which government interference in the economy had become so pervasive as to warrant the description of a command economy. Second, Higgs reexamined the statistical basis of the income estimates for the wartime years and, following Kuznets, concluded that they must be regarded with great skepticism. Not only are the conceptual foundations of national income accounting inappropriate to assessing the wartime experience, but there is very strong evidence the figures have been inappropriately constructed. In fact, the generally available statistics show sharply divergent trends during the war, and Higgs argued that the case for a decline in consumption, rather than the usually accepted increase, is strongly supported.

John Wallis provided an insightful commentary on the paper. He supported the conclusions and suggested that the major contribution was the renewed attention to the conceptual and evidential basis of the income estimates. Although Higgs' remarks concerning the inappropriateness of uninformed use of standard data in macro-modelling were well taken, they diverted attention from the main contribution of the paper. General discussion followed that stressed the need to reemphasize the concern that the pioneers in national income analysis always expressed about the conceptual and evidential weakness in national income analysis, particularly in unusual circumstances.

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Adam B.J. Klug, currently at Princeton, presented the second paper, "The Theory and Practice of Reparations and American Loans to Germany: 1925-29." The paper investigated German borrowing using a simplified neo-classical model of optimal borrowing, the sort that has been used to assess current third world debt problems. Klug concluded that, on the basis of the best information economic historians now have, the German borrowing was non-sustainable and that American lending was inappropriate. In the paper's final section, Klug attempts to explain American lending. He concludes that it appeared rational when judged using the optimistic assessments of German performance that seemed to have been held by informed contemporaries. The problem was one of faulty "economic intelligence."

Richard Grossman, the formal commentator, raised issues of the appropriate level of abstraction in this type of analysis. There were innumerable issues of politics and economics complicating this period, issues that are difficult to incorporate into the type of analysis Klug presented. The novelty and usefulness of the assessment of economic intelligence was

particularly appreciated. The general discussion tended to follow that same theme. There was widespread appreciation of the insights that this type of modelling provides, but some unease about the abstraction from possible disequilibria and shorter term considerations than can be incorporated in the neoclassical model.

William H. Phillips' "When Two Worlds Collide: War and the Family Economy at Newry Mill Town" concluded the session with very interesting, detailed information about the breakdown of the family labor system in a Southern cotton town under the influence of World War II. The study is primarily based on the personnel records of Newry Mill in the late thirties and early forties. In particular, Phillips has been able to show that women workers' positions underwent a significant change as the wartime economy tightened the labor market. In the prewar labor market, women's earnings functions failed to show any association with age and experience—reflecting their position as secondary workers in a family based labor market. During the war the earnings functions, although they continued to lie below those of men, came to show a positive experience profile.

Elizabeth Field-Hendry led the discussion with an appreciation of Phillips' careful and skillful use of the rich employment records at his disposal. She expressed a disappointment, shared by Phillips and others, that the data source did not allow exploration of the longer term impact of the wartime experience on the post-war evolution of the mill town labor market.

Editor's Notes

Revisions to the Society's directory will be distributed later this spring in a separate mailing.

The next in our series of interviews with the founders of Cliometrics is scheduled for publication in the June 1991 Newsletter.

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The Cliometric Society Newsletter is published three times per year.

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The joint Cliometrics Society/AEA session focused on employment segregation of black workers. Bob Margo presented his work on employment segregation in the south based on Census evidence from 1900, 1910, 1940, and 1950. The paper is from his new book, Race and Schooling in the South, 1880-1950. His presentation emphasized that, while education was an important determinant of occupational segregation, it was not the whole story. Race, not schooling or spatial mismatch, was the principal factor behind employment segregation in the South.

Bob Higgs, the commentator, agreed with the general conclusions but had several minor reservations. First, the earnings ratios in Margo's tables (and in many other studies of black status) are misnamed because there are no race-specific earnings data; the ratios are actually occupational status indexes. Second, there is selection bias in focusing on the South because black outmigrants tended to be more venturesome, depleting the ranks of the able in the South. Third, there are problems in inferring pure race effects from models containing only the "usual suspects" of age, education, and other measurables. Most employers do not restrict themselves to the "usual suspects" in choosing whom they would hire or promote. The race effects are residuals and are only as accurate as the crude measures of productivity. Fourth, the spatial mismatch hypothesis cannot be fully tested with Margo's migration evidence because it is focused on cross-state migration, and much migration was intra-state. Finally, we should not draw a straight trend line between 1910 and 1940; it is likely that black status improved in the teens and possibly in the 20s before declining in the 1930s. In response, Margo agreed that the "usual suspects" were crude proxies, and that the selectivity bias from outmigration might be important. He had tried to examine whether selectivity bias extended to the unobservable aspects of productivity, but the results were uninterpretable.

Bill Sundstrom's presentation focused on racial exclusion by unions. Bill defined a model in which unions faced a cost from excluding blacks because management could then use them as strikebreakers.

His presentation emphasized empirical findings. Unions were more likely to exclude blacks in industries where there were few black workers at the time of unionization. Although Whatley found there were relatively few instances of blacks acting as strikebreakers, Bill argued that the threat of strikebreaking was important. Finally, in summarizing his model, he found unions faced a higher cost of excluding laborers when labor supply and labor demand were more elastic.

Joe Reid commented that Sundstrom's work added a dynamic element to the analysis of unions. In the past, blacks were less represented in unions than in blue collar employments, and less represented in craft than in industrial unions. Sundstrom does not ask why blacks more than others might be excluded from unions. He repeats common explanations that racism has declined and that industrial unionists were less racist than craft unionists. Reid suggested more plausible explanations are that the past harshness of law and custom toward blacks made them easier to exclude, while blacks' color made it easy to police their exclusion from union benefits. Therefore, it was more valuable to exclude them from craft than from industrial unions. What Sundstrom does ask is why blacks' degree of exclusion from unions varied over regions, occupations, and time. Sundstrom answers, in part, it is that the net economic benefit of exclusion first increases with the size of the excluded group (for increased exclusion makes substitutes more scarce), but then decreases (as the excluded become so numerous as to constitute a substitute work force free of union discipline, available to break strikes or otherwise drive down wages). Further, national unions will be more inclusive than locals because nationals internalize the cost of a locally-excluded worker who could compete wages down elsewhere. Sundstrom, Reid believed, presumed too rashly that unions perfectly serve their members and that their only service is monopolization of labor.

In the discussion, Bob Higgs and Bob Margo suggested ways for Sundstrom to make allowances for the concentration of black workers in the South.

Some industries in his table contained no blacks because there were no plants in the South. In particular, Margo suggested that he include a measure of the percent of Southern employment in his regressions of black union membership on black percentages in the industry. Bernie Elbaum noted that the percentages of blacks in the trades often varied greatly. Robert Whaples suggested that in cases where there were very few blacks, the costs and benefits of exclusion were both close to zero.

Gavin Wright presented the final paper of the session describing his joint work with Warren Whatley on the experience of black workers at Ford. Ford is an interesting case because it employed more than 10,000 black workers, far more than any other car manufacturer. Entry-level wages for blacks and whites were roughly the same, but black workers were older and more often married. Regression analysis shows that after accounting for differences in several variables, blacks were paid 2 to 3 percent less. The results appear to imply stereotyping, as whites received premia for marriage, education, and the like, but blacks did not.

Robert Whaples questioned several aspects of the measure of discrimination and the use of the term stereotyping. If blacks were rewarded for such things as education, but whites were not, this also could be described as stereotyping. Who is to say which is the "true" way of paying people? The white coefficient may have been higher because educational quality in white schools was higher. The age coefficient reflects more industrial experience for whites than for blacks. There was a great deal of segregation at Ford. At whom do you point the finger for causing the segregation: the employer or the other workers at Ford?

Bob Margo pointed out that the results seem consistent with the model proposed by Lundberg and Startz in the early 1980s. He also asked whether they could control for Southern birth to get at some of the issues raised by Whaples. Higgs suggested that something special was happening in the foundry and that it might be useful to separate foundry workers from other workers. Wright suggested that they were trying to do that. He defended their use of the term stereotyping and said that the

stereotype might reflect real differences in characteristics. Carmela Chiswick suggested that lumping all whites together missed the differential experience of native and immigrant workers, but Gavin noted that the period examined was one in which there were few new immigrants. Higgs asked about the fact that the education variable was not filled out on many forms, suggesting that the schooling coefficients might be spurious. This led to speculation that Ford required some minimum level of education, but did not care beyond that level. In response to David Mitch's question on the recruitment process, Gavin said that many black workers were recommended by their ministers. When Knick Harley asked about how many workers left the plant in less than 30 days, Gavin said the dropout rate was roughly 1/3 within the first month.

The session on Financial Institutions: Failures, Insolvency, and Moral Hazard drew the largest attendance of any Clio session. All the authors tied their papers to current problems with savings and loans and deposit insurance, but the discussants and audience often challenged the linkage between the historical example and modern day problems. Lawrence Kryzanowski and Gordon Roberts led off with a paper challenging the stylized fact that no Canadian bank became insolvent during the Great Depression because their national branching structure allowed them to diversify against regional downturns. Using market-value accounting they found that 9 of the 10 banks were technically insolvent in 1933. The Canadian system avoided failure through regulatory forbearance, guaranteed deposits, the forced merger of troubled banks, and because national branching reduced competition.

Mike Bordo suggested that the banks were technically insolvent because they were reacting to the accounting rules established by Canadian regulators and were taking more risks because they knew that the Government would act as a lender of last resort. George Benston asked a series of questions about

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Cliometrics in the International Economic History Congresses

by Rondo Cameron, Emory University

Editor's note: Rondo Cameron is William Rand Kenan, Jr., University Professor at Emory University. He attended the First International Congress of Economic History in Stockholm in August 1960 and the first Conference on the Application of Economic Theory and Quantitative Methods to Problems in Economic History at Purdue University (the forerunner of the Clio meetings) in December of that year. He has regularly attended the meetings of both groups since then. John Lyons asked him to provide "a perspective on the changing degree and impact of diffusion of quantitative/theoretical approaches among the world-wide community of economic historians" (Lyons to Cameron, 30 May 1990).

Cameron holds two degrees from Yale (BA, economics and mathematics, 1948; MA, economics and history, 1949) and the PhD from the University of Chicago (economics and sociology, 1952). He taught at Yale, Chicago, and Wisconsin (where, with Eric Lampard, he founded the Graduate Program in Economic History in 1960) before going to Emory in 1969.

.....

When John asked me to provide an account of the influence of cliometric methods in the international congresses of economic history I hesitated only because I knew that I would be in Brazil during the fall of 1990, and thus would not have access to my records or those of the meetings in question. What follows, therefore, is strictly a personal memoir, subject to all of the limitations of the genre, but I did have an opportunity to fill in a few dates and correct some obvious errors when I returned to Atlanta at the end of December. Both I and the editors will appreciate any corrections or emendations by those who have different recollections or, especially, evidence. Another disclaimer: although I have attended most of the Cliometrics meetings, including the two world congresses (Northwestern and Santander) I have frequently admitted—and others have undoubtedly

said of me, though not to my face—that I am not really a cliometrician, but only a camp follower. I am a sheep in wolf's clothing.

The First International Congress of Economic History took place in Stockholm, Sweden, in August 1960, just prior to the Eleventh International Congress of Historical Sciences (ICHS), before there was an International Economic History Association. I am not sure who organized it, but E.F. Soderland of Stockholm certainly had a hand in it, and I think that Fernand Braudel of the College de France and M.M. Postan of Cambridge were also involved.

Attendance was not large, perhaps 100 or 150. All sessions were plenary sessions. The congress lasted for two or three days, although most participants stayed to attend sessions of the ICHS. The program was rather general, without any explicit focus on quantitative methods. (In contrast, the ICHS featured a session on price history in which my mentor, Earl Hamilton, summarized his work.) There were two major themes, "Industrialization as a Factor in Economic Growth since 1700," and "The Comparative Study of Large Estates since the End of the Middle Ages," plus a number of sundry "communications." Tom Cochran led off in the first session with "An Historical Approach to Economic Development", and was followed by Walt Rostow on "Industrialization and Economic Growth." (Walt had published The Stages of Economic Growth earlier in the year and was en route to the famous Konstanz conference of the International Economic Association.) Other Americans who gave papers in the first session were David Landes on industrialization in Germany, Hal Williamson on mass production and mass consumption in American development, and Daniel Thorner on "de-industrialization" in India. Bill Parker, with "The Slave Plantation in American Agriculture," was the only American to present a paper in the second session.

The Purdue meeting in December was quite different. It was very small. Bob Fogel, in the interview reported in this Newsletter (Vol. 5, No. 3, July 1990) said 20 or 30 people were there, but as I recall the number was more like 15 or 20. (Lance Davis should have the last word—maybe even records.) And, as Bob indicated, the interaction, the questioning and discussion, was very intense. As I recall, the session devoted to his paper ran even longer than he indicated, but that was the longest—and most memorable—session of all. As to whether or not he persuaded us, my own attitude when it was over was one of sympathetic skepticism. On the basis of his performance I invited him to speak at Wisconsin the following spring.

Meanwhile, the IEHA was organized with Braudel as president and Peter Mathias as secretary. The Second International Congress took place in Aix-en-Provence in 1962. Again, the program did not contain any explicit cliometric themes (Stanley Reiter had not yet coined the phrase, or, if he had, it had not become common currency), but I gave a paper on the theoretical basis for my comparative study of banking in the early stages of industrialization (a project I had just entered upon), and Francois Crouzet gave a heavily quantitative paper on capital formation in Britain during the misnamed industrial revolution. Doug North gave a paper in the same session, but it was not in the cliometric mode; instead, it was a plea for broadening the concept of capital to include human capital, which recently had been introduced by Ted Schultz. Lance Davis made an extensive critical comment on Doug's paper, but it too, uncharacteristically, was not in the cliometric mode.

The Aix congress featured a session on the history of prices and economic fluctuations in which Gerhard Bry and Charlotte Boschan analyzed secular trends in real wages in the United States, Great Britain, and Germany. Other American participants in this congress, without cliometric topics or methods, included Daniel Thorner, Bert Hoselitz, Alexander Gerschenkron, Evsey Domar, David Landes, Phil Curtin, Morris D. Morris, and Sidney Ratner.

The Third International Congress of Economic History was held in Munich in 1965. It is the only one I did not attend, as I had just entered on a new position with the Rockefeller Foundation in South America. Simon Kuznets delivered a notable keynote address at the inaugural plenary session on "Capital Formation in Modern Economic Growth," which was published in the proceedings. A cursory perusal of the latter reveals other American participants, some with quantitative papers, others without: Sam Bass Warner, Jr., Bob Gallman, Paul Gates, Martin Wolfe, Domenico Sella, Nate Rosenberg, Lance Davis, Al Fishlow, Paul Trescott, and Shep Clough.

The Fourth International Congress came to Bloomington, Indiana, in 1968. Fred Lane was president and program chairman; Ross Robertson chaired local arrangements. No special concern with cliometrics was evident, although we were only a short distance from West Lafayette, the birthplace. I may be wrong about that, however, as the full proceedings were never published, so far as I know. Instead, the March 1969 issue of the *JEH* (the "Tasks") carried a selection of the papers from only one session of the congress, "The Formation and Development of Capitalism," an archetype of the "old" economic history. In refreshing contrast, the summaries of doctoral dissertations (because the regular annual meeting of the EHA was merged with that of the IEHA) introduced the work of a number of bright young scholars who were destined to become leading figures in the ranks of the cliometricians: Gabriel Tortella (then at the University of Pittsburgh), Lew Solmon, Don McCloskey, Stefano Fenoaltea, and Richard Roehl.

The Fifth Congress, held in Leningrad in 1970, was notable for two developments, one political, the other intellectual. As a result of the Soviet invasion of Czechoslovakia, just before the Bloomington Congress, many persons opposed holding the next congress in Leningrad. In the end a compromise was reached: go to Leningrad, but elect a Pole, Witold Kula, as president, instead of the chairman of the soviet delegation. On the intellectual side, the Leningrad Congress was the first explicitly to in-

clude cliometric themes. The initiative came from the Soviets. Iu. Kakhk and Ivan Koval'chenko, the pioneers of cliometric methods in the Soviet Union (see the article by L.I. Borodkin, "Cliometrics in the USSR," in the February 1990 issue of this Newsletter, Vol.5, No.2, pp. 11-15) who wanted to meet Bob Fogel and other notable practitioners of the art/science of cliometrics. The proceedings of the Leningrad Congress (not all devoted to cliometrics, to be sure), were published in no fewer than eight volumes!

With the Sixth International Congress in Copenhagen in 1974, the IEHA settled down to a regular 4-year cycle for its congresses. Although I did not become a member of the executive committee until the end of that congress, the president, Kristof Glamann, had invited me personally to participate in the planning. I was therefore able to introduce the concept of workshops, now known as "C sessions," the initiative for which comes from individual participants rather than from the executive committee as such. As a result, participation in the congresses has since increased substantially, and there is a wider range of topics, especially those concerned with cliometrics.

The Seventh Congress (Edinburgh, 1978) witnessed the innovation of a professional organizer under the supervision of Michael Flinn, the local arrangements chairman, and Peter Mathias, the president. That is one indication of the growth in the size and complexity of the congresses. Although that growth was not directly a result of the inclusion of cliometrics topics in the congresses, it facilitated their inclusion. By 1978, cliometrics was no longer an American monopoly, but had numerous practitioners and enthusiasts in other countries.

Prior to the Edinburgh Congress it was assumed that the following congress would take place in Switzerland, with J.-F. Bergier, the former secretary-general of the IEHA, as president. But in December 1977 a troika of elder statesmen in the profession decided that the 1982 congress should be held in a socialist country. The three were Fernand Braudel, a former president, whose decision was for personal reasons;

Vinogradov of the Soviet Union, who had been deprived of the presidency in 1970, for both personal and political reasons; and M.M. Postan, also a former president and in this case apparently an innocent dupe. There was much contention in the meetings of the executive committee in Edinburgh, but in the end a compromise was reached; the 1982 congress would take place in Budapest, Hungary, with Z.P. Pach, a well-respected long-time member of the executive committee as president and with the clear understanding that the 1986 congress would be held in Switzerland. Just prior to and at the first meeting of the executive committee in Budapest, Braudel again tried to derail the Swiss congress and the installation of his former heir-apparent, J.-F. Bergier, as president. He was unsuccessful and returned to Paris the next day.

Despite the political complications, the Budapest Congress went smoothly, and cliometric themes and methods were much in evidence. Session B1 was devoted explicitly to "Economic Theory and History," and Session B2 to "New Applications of Quantitative Methods in Economic and Social History." In other sessions, as well, cliometry found its place.

The 1986 congress in Switzerland was notable for, among other things, the prominent place it accorded Bob Fogel and his horde of disciples from many countries studying long-term changes in nutrition and the standard of living. By this time, however, cliometrics was no longer a novelty but an integral part of the methodology of many economic historians around the world.

The international congresses of the IEHA have played a vital role, not only in integrating cliometrics into the purview of economic history more generally, but also in internationalizing the discipline as a whole. Solid bonds of friendship have been forged between individuals of different nationalities who otherwise would have remained isolated in their national groups. On the whole, this process of internationalization has proceeded gradually and peacefully in the spirit of

scholarly, scientific cooperation and collaboration but occasionally, as in 1968-70, and again in 1982, political and personal motives have interfered.

Meanwhile, the Cliometric Society officially went international with its first world congress at North-western in 1985, under the stewardship of Joel Mokyr, and with its spectacularly successful Second World Congress in Santander, Spain, in 1989, due to the organizing genius of Leandro Prados de la Escosura.

The most recent Tenth International Congress of Economic History, in 1990 in Leuven, Belgium, showed how far we have come, and come together. Herman Van der Wee, the president of the congress, is well-qualified as a cliometrician even if he does not wear the label. The program featured many sessions with cliometric topics and participants, especially the innovative sessions of doctoral dissertations (on the American model, but largely at Herman's initiative)—an encouraging portent for the future. Finally, Dick Sutch, a genuine cliometrician, succeeded me as a member of the executive committee.

Child Labor and the Industrial Revolution

By Clark Nardinelli

"Industrialization, far from being the source of the enslavement of children, was the source of their liberation." —From the book



A piece of anti-child labor "10 Hours" propaganda. From Frances Trollope Michael Armstrong, *The Factory Boy*, 1839.

This is the first full-length study by a modern economic historian of one of the most controversial aspects of the industrial revolution in Britain and is certain to spark heated debate. The employment of children in British textile factories has long been a symbol of the harmful effects of the industrial revolution. Unlike most other critical studies, *Child Labor and the Industrial Revolution* considers child labor from the standpoint of the family economy, in particular the often miserable alternatives open to poor families that made factory work preferable to other possible uses of children's time. Clark Nardinelli's economic approach leads to new interpretations of such topics as economic exploitation and the effects of child labor laws. \$25.00

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Report on the 1990 Canadian Clio Conference

by Marvin McInnis, Queen's University

The 17th Conference on the Use of Quantitative Methods in Canadian Economic History assembled November 9-10 at Queen's University, Kingston, Ontario. A full and interesting program was organized by Mary MacKinnon. Local arrangements were in the capable hands of Alan and Anne Green. A bracing dash of early winter reminded participants that intellectual exchange in a cold climate is decidedly an indoor sport.

Two features distinguished this meeting. First, many of the papers made use of the new annual series of historical Canadian national income produced by Malcolm Urquhart which first appeared in Studies in Income and Wealth, Volume 51. A large volume providing full details of the new series will be published shortly by McGill-Queen's University Press. A second feature of the meeting was thus greatly increased attention to macroeconomic issues.

Joe Haubrich led off the macro binge with an examination of the Canadian evidence relating to Bernanke's argument that the U.S. financial crisis, by raising the real cost of credit, contributed importantly to the depth of the Great Depression. Canada experienced as prolonged and almost as severe a depression as the U.S. The problem is that the Canadian banking system of a few nation-wide branch banks did not witness outright bank failures, the key variable in Bernanke's analysis, although the number of bank branches was reduced considerably. Haubrich represented financial distress in Canada by three variables; bank branch closings, the real prices of bank shares, and the spread between commercial and federal government bonds. Econometric analysis of those variables failed to reveal a separate, non-monetary effect of the financial crisis in Canada. Angela Redish worried that Haubrich was allowing the historical experience of the U.S. to define the stylized facts. The standard account of the depression in Canada continues to be an essentially Keynesian story (e.g., Safarian). Redish is unconvinced that the monetary model has been effectively rejected, a view Haubrich seemed to welcome. Hank Gemery questioned whether Haubrich

had provided an adequate test of the Bernanke model since he was unable to get Canadian proxy variables that mirrored Bernanke's. Jim Irwin suggested Haubrich try an analysis of counterparts of the available Canadian variables on U.S. data.

Jean-Louis Arcand, in a paper written with Elise Brezis, presented their analysis of the disequilibrium dynamics of the Great Depression in Canada. Their motivation was the notion from Tobin, Hahn and Solow that wage and price rigidities may not have been at the heart of the depression, but that the converse, wage and price flexibility, may have been the root problem. The argument is that the dynamics of markets in the economy may have been such that prices and wages were highly flexible in a setting of dynamic neutrality. A shock to the system could have sent prices and wages on an unrestrained downward slide, so it is price rigidities that underscore the possibility of macroeconomic equilibrium. Arcand and Brezis, following Quandt, set up simple aggregated models of the Canadian economy and test the dynamic neutrality hypothesis by estimating the slope of excess demand in the goods and labor markets. They estimate models premised on both Walrasian and Marshallian dynamics, but their discussion emphasizes the Walrasian models. They do not report the results of the Marshallian assumptions. Similar to what others have found for the U.S., they are unable to reject the hypothesis that the Canadian goods and labor markets were dynamically neutral over the period 1926-39. The Canadian economy, in Walrasian terms, was not anchored by wage and price rigidity. In his comments, Gemery requested more information about the microeconomic roots of dynamic neutrality. Redish doubted that annual data were adequate for estimating a disequilibrium model. It emerged from the discussion that just what flexibility means is rather subtle in the context of this model. Knick Harley wondered if the authors were looking carefully at the data to assess whether peculiar data points were dominating their results. Richard Pomfret reminded them, in that context, of the possible role of some discrete, explicitly protectionist moves.

Trevor Dick's paper, on price flexibility and economic instability before World War I, in essence asked whether Canada in the gold standard era was in a Robert Lucas world (flexible prices and quick adjustment) or a Robert Gordon world (sticky prices with impacts on real output). To pursue this, Dick estimates some basic macro-models that closely reflect the work of Gordon and of John James. He estimates these models with three different sets of time series: the old Firestone series, that series as modified by Morris Altman, and the new Urquhart series. Overall the results lean toward supporting Gordon rather than Lucas. Late 19th century Canada does not appear to look much different from the U.S. as described by James. Dick posed the question of whether there was an important structural break in 1896 by asking whether the underlying monetary/macroeconomic regime was somehow different after 1896 than before. The answer was no. That, it was noted, is not the same thing as finding that the performance of the economy had no break around 1896. In commenting on Dick's paper, Michael Edelstein elaborated on the work of Gordon and James, then agreed Dick's results were generally encouraging to the Gordon approach. Edelstein noted this approach raises questions about how trends should be established, about how sophisticated one ought to try to be. Questions were also raised about the appropriate designation of the exogenous shocks that are supposed to disequilibrate the economy. A further suggestion was to run the model through the highly disrupted period that followed World War I.

Kieran Furlong presented a paper proposing a revised dating of Canadian business cycle turning points in the late 19th century. This paper, in the N.B.E.R. tradition, offered a reconsideration of the much used dates originally proposed by E.J. Chambers in 1964. Furlong pointed out that Chambers used only a few monthly series and that financial series were overrepresented. Further, some of the series were aggregations of others, so that Chambers double-counted in a scheme where series were weighted equally. Furlong removed the double counting and added two new series (railway traffic receipts and Montreal call loan rates). The outcome is a business cycle chronology that differs in some important respects from Chambers', with turning points differing by as much as twelve months in some

cases. Furlong puts the onset of the 1874 depression rather later than Chambers. He also finds evidence supporting an upturn in mid-1876 with another downturn in mid-1877, an interpretation supported by the new Urquhart series. Marvin McInnis also pointed out that such a view gained support in the trade evidence for forest products, Canada's leading export. It appears the depression of the 1870s, which has figured so largely in earlier writing on Canadian economic history, is very much due for reexamination. The same is true of the 1890s. Furlong finds that the downturn in 1890 was so slight as hardly to qualify as a cycle, but that the cycle of 1893-96 was more severe than has usually been recognized.

Alan Green and Gordon Sparks presented some preliminary results from their on-going development of a macro-model for Canada in the years from Confederation to World War II. They applied the cointegration error correction approach of Engle and Granger to ask, as had Dick, was there a fundamental break in the growth trend around 1896? Such was the contention of the earliest writers on Canadian economic history, but it is one that has been called into question by "revisionist" writers. The Green-Sparks analysis, using an extremely simple model that emphasized exports, investment and the terms of trade, generates the observed pattern of change without an underlying change in the structure of the economy. Regardless of the sophistication of the econometric technology, or perhaps even because of it, Mac Urquhart remained skeptical of results that did not show such a break in 1896. He expressed his strong conviction that the post-1896 period unquestionably represented a different standard of performance of the Canadian economy. What changed was investment, which increased significantly after the beginning of the 20th century. While that may not have involved a change in the relational structure, the exogenous determinants of investment certainly changed. Edelstein commented that the really interesting change in the Urquhart series is a sharp rise in domestic savings. The question was raised whether this increase might simply be a redirection from unmeasured real investment (farm capital) and childbearing to conventionally measured saving.

William Gibson presented a paper written jointly with Steve Easton and Clyde Reed on "Testing the

Chambers and Gordon Model." This too is an old chestnut; the first meeting of Canadian Clio was the occasion where Chambers and Gordon first displayed their model. Gibson *et al.*, are concerned with the model's testable implications. They start by showing that Canada did not follow a unique growth path in the pre-1913 world. Indeed the growth of the Canadian and U.S. GNPs was quite similar, as were the Canadian and U.S. patterns of immigration. No one talks of the U.S. having a "wheat boom" in this period. The implications of the Chambers and Gordon model they chose to test are that agricultural prices should have no effect on wages but that wage changes should reflect changes in the price of "gadgets." The empirical evidence, once again the new Urquhart series, seems to bear out the hypothesized wage/price relationships. Pomfret wondered just what was to be made of the international comparisons and asked if we should think of the Canadian boom as export-led. The results appear plausible and to offer support to Chambers and Gordon, but one can ask whether this is the best test. Pomfret suggested they might test the proportionality of prices. Tony Ward questioned what it was about the wheat boom that should get the most emphasis.

At this juncture Mac Urquhart expressed a strong desire to pronounce the obituary of the dispute over the Chambers and Gordon analysis. He believes there is a real need to move on to a line of endeavor more driven by actual issues. The period during which the Canadian prairies were settled was one of outstandingly high investment expenditures. As has long been appreciated, the investment boom came well in advance of any surge in wheat exports. What is most needed is to understand the reasons for the surge in investment.

Kris Inwood and Jim Irwin also made use of the Urquhart series and offered estimates of the regional distribution of national output for 1870. This was a preliminary effort, but one that did not encounter any notable resistance.

Livio Di Matteo reported on the distribution of wealth in Wentworth County, Ontario, in the late 19th century. His data cover both the city of Hamilton and some of the surrounding countryside. Di Matteo sampled probated wills and linked them to assess-

ment rolls and tax assessments. These data point to rising wealth in the late 19th century, especially for younger descendants, and to some decline in the extent of inequality. In discussion, Tanis Day worried about the bottom end of the distribution. If there were no probate, was no record of wealth given? Did this bias the ultimate results? Hugh Grant thought the time pattern of change in wealth was considerably more complex than claimed.

Ruth Dupre presented an account of the operation of the Quebec Dairy Commission in the early 1930s, arguing that it was the first case of price support in Quebec agriculture and one of the first in any province. This new direction emerged rather quickly and surprisingly from an atmosphere where interference with market pricing had been strenuously opposed. It also appears that Quebec dairy farmers were, in relative terms, not especially hard hit by the depression. The usual conception is that Quebec was a latecomer in interventionist government action, yet here was Quebec taking a lead in support pricing. In commenting on Dupre, Herb Emery thought it was left unclear why price fixing should have been the regulation of choice.

Ron Shearer, in a paper written jointly with Don Paterson, asked "how did the industrious classes of Her Majesty's subjects learn to save?" This dealt with a little known subject, the very early experimentation with savings banks in Montreal and Quebec City. These were operated virtually as charitable institutions, for the instruction and benefit of the working poor. These curious institutions, which eventually were absorbed into the chartered banks, have left a valuable archive of records. They pioneered in the payment of interest on deposits, lasted a fairly long time, and stand out as institutional curiosities. Doug McCalla suggested that it would be helpful to see what foundation these institutions may have laid for the trustee savings banks that came upon the scene later.

Greg Marchildon, using a large body of new data he assembled, provided an international perspective on the first Canadian merger wave, which peaked in 1909-12. Marchildon found it had similar characteristics to merger waves in the U.S. and U.K., but came almost a decade later. Mergers were predominantly

in growing industries, but otherwise there are few indications of real causal factors. The important influences seem mainly to have been financial; the lag in timing may largely have been the result of a later emergence of a developed market for Canadian industrial securities. Peter Wylie did not think that the alternative real explanations involving technological change and increased competition from new foreign investment in Canada had been thoroughly examined. There is also the matter of the relationship to Canada's initial experimentation with anti-trust policy (the Combines Act).

The final paper of the regular sessions reported on beaver population estimates by Ann Carlos and Frank Lewis. What would economic history be in the land of Innis if someone did not bring up *castor gras*? Carlos and Lewis reopened the question of the often claimed depletion of the beaver population by the Indians who traded with "The Bay." They pointed out that the depletion of fur supplies has been taken as an established fact by previous writers, but no one has actually demonstrated it. They simulated, using standard natural resource models, the beaver stocks for three important trading areas which involved varying degrees of competition between the Hudson's Bay Company and French traders. They estimated actual populations of beaver and the maximum sustainable yield for each area. These estimates show an initial pronounced decline in the beaver population from numbers well above those consistent with harvesting at the maximum sustainable rate to a period of stabilization. Around the middle of the 18th century through to 1821, however, there was a further pronounced decline in the beaver population. Was this the depletion to which the literature has referred? Spatial and temporal patterns point to an important role of competition between French and British traders. Where and when the Hudson's Bay Company had a virtual monopoly, the beaver population was approximately sustained. When French competition heated up, as it did through 1821, the beaver population fell markedly. Company traders were aware of the possibilities of depletion, but with fluctuating harvests it would have been hard for them to get a clear reading on when the population was falling to dangerously low levels. Peter George thought Carlos and Lewis generally understated the beaver populations. Since their estimates depended upon informa-

tion on the numbers of beaver harvested, a backward bending supply curve of effort by native harvesters, a not entirely improbable suggestion, could undermine the estimating procedure. George raised another interesting possibility, that it may not have been the beaver population that was falling but the population of Indian trappers.

In addition to regular sessions there was the traditional session discussing new research in progress. Morris Altman presented some early results from his investigation into the hours of labour in Canada, 1880-1920. Louis Cain talked about the evolution of the modern zoo. Gillian Hamilton reported on 19th century apprenticeship contracts in Lower Canada. Wayne Lewchuk described his study of census data on occupational mortality differentials in 1871 Hamilton. Benoit Papillon talked about the structure of transport costs in Quebec and Ontario in the latter 19th century and how the differences between the two provinces explain differences in the form of non-metropolitan urban development.

Friday night we banqueted at the Queen's Faculty Club. The after dinner address, complete with magic lantern show, was given by Brian Osborne, retiring chair of Queen's Geography department and co-author of a recently published history of Kingston. He gave a well-illustrated review of change and continuity in the evolution of what both is and looks like one of the oldest urban centres in Canada.

The 18th Conference will be held in the Spring of 1992 at the University of British Columbia. Ruth Dupre will organize the program.

Report on the Standing Committee on Archives of the Economic History Association

by Michael R. Haines, Colgate University

On the occasion of the fiftieth anniversary of the Economic History Association, President Richard Sutch proposed the creation of a standing committee of the Association to advise on matters dealing with archives. An organizational session was held during the annual meetings in Montreal on September 15, 1990. The following have agreed to serve on the committee: Michael R. Haines, Chair (Economics, Colgate University); Ruth Ann Becker (Economic History Association); Carol Leonard (History, S.U.N.Y. at Plattsburgh); Joan U. Hannon (Economics, St. Mary's College); Daniel Raff (Harvard Business School); Michael D. Bordo (Economics, Rutgers University); and Robert McMurray (Economics, Bloomsburg University of Pennsylvania (retired)).

Presentations were made by Roger Ransom (History, University of California, Riverside) and Michael Haines. Many topics and issues were presented and discussed in an effort to define the role and function of this new committee. A significant number of those topics will need to be addressed. First, there is the issue of what sorts of materials the EHA and this committee ought to consider. The discussion focussed primarily on quantitative (or potentially quantitative) materials, but qualitative sources could be included. Second, for quantitative data, there is the archival function as conventionally defined and understood: information, documentation, retrieval and archiving of old data sets. Third, attention is to be paid to coordinating data collection, including documentation standards, sampling methods, and encouragement of the widest possible collection coverage. Fourth, it is important to give information and guidance about use of new technology, including data storage (e.g., high density diskettes, CD ROM, Bernoulli technology, mainframe and micro tapes); computing alternatives, both in terms of hardware and software (e.g., micros versus mainframes, memory requirements, data entry software, database software), data entry (e.g., optical scanning), and data transfer and transport capabilities. Fifth, the

Committee on Archives could conceivably respond to requests for information from members, outside researchers, and archivists. Sixth, the committee could also function as a liaison with various data collection and archiving centers such as the Interuniversity Consortium for Political and Social Research (ICPSR) at the University of Michigan, the Laboratory for Historical Research at the University of California at Riverside, the Newberry Library in Chicago, the Center for Population Economics at the University of Chicago, the Agricultural History Center at the University of California at Davis, and the Center for Monetary History at Rutgers University. One recent initiative has been by Roger Ransom who, as editor of Research in Economic History, has proposed that papers submitted to that forum for publication should, if accepted, be accompanied by copies of data sets with appropriate documentation to be made available to interested readers (see the "Cliometrics Newsletter", February, 1990).

Comments from the floor were lively and extensive. Fred Carstensen (Economics, University of Connecticut) commented that we need to speak with archivists regarding the preservation of records. For example, many current records of great historical value are being destroyed by firms on an ongoing basis. Advice to data collectors on appropriateness and availability as well as advice on technology were strongly urged. Michael Bordo noted that macro data, particularly financial data, need to be considered. Serious problems exist with some of our macro data, such as the NBER-ICPSR financial data tape. Richard Steckel (Economics, Ohio State University) suggested that we communicate with other professional associations (such as natural and biological scientists) on these issues, establish regular relationships with other social science groups, such as the Social Science History Association, possibly take up an affiliation of the Economic History Association with ICPSR, and talk with university presses about the possibility of published data sets.

John Lyons, (Economics, Miami University, Ohio) expressed concern with the deterioration and destruction of underlying raw data archives. He recommended a clearinghouse for information of the location, contents, etc. of various data archives. Marvin McInnis (Economics, Queen's University, Ontario) noted that Canada has been working on this problem through the Social Science Federation. The Canadian national archives already are preserving private as well as public records and has a division dealing with machine-readable archives. Robert McMurray recommended an evaluation of data quality, Martha Olney (Economics, University of Massachusetts) suggested a regular section of the *Journal of Economic History* describing data sets. Also, our profession needs archives funded on a permanent basis (i.e., "hard money" archives).

Charles Calomiris (Economics, Northwestern University) commented that some people may not want to archive data if it is going to be released immediately. Provision for some types of restriction should be considered. Also, many government agencies are capricious in their policies about retaining data. We should consult with the National Bureau of Economic Research or the American Economic Association to help us lobby for data preservation. Helen Hunter (Economics, Bryn Mawr College) suggested that we coordinate the construction and maintenance of data series with the federal government and with relevant committees of the American Economic Association and Population Association of America. Carol Heim (Economics, University of Massachusetts) recommended that we create a guide to documentation practices.

It is quite apparent that there are many issues to be addressed and a considerable need for policies and coordination. It is anticipated that this work can only be partially addressed by the EHA standing committee. The committee anticipates consulting and meeting about this to develop policies and an agenda for activity. Suggestions, comments, recommendations, etc. are invited. Please respond to Michael R. Haines, Department of Economics, Colgate University, 13 Oak Drive, Hamilton, NY 13346; Tel.: 315-824-7536; BITNET: MHAINES@COLGATEU; fax: 315-824-7726.

Clio at ASSA (continued from page 4)

their use of market-value accounting, particularly the evaluation of intangible assets and bank stock. Kyrzanowski and Roberts said they had looked at hidden reserves and faced problems in evaluating bank stock because most of it was owned by employees. Further, the stock market was closed for much of the period. An extended discussion followed about the importance of branch banking as a method of avoiding bank failures. Bordo, Benston, Charles Calomiris, and Ken Snowden challenged Kryzanowski and Roberts' statements that the technical insolvency of the Canadian banks casts doubt on the effectiveness of branching as a means of preventing bank failures. The authors agreed that branch banking was an important means of preventing bank failures.

Turning to the United States, David Wheelock, Wayne Grove, and Lee Alston studied the impact of a variety of factors determining bank suspensions during the 1920s. Pointing to a number of parallels between the 1920s and the 1980s, their study of the 1920s showed that agricultural distress was the dominant cause of bank suspensions, the presence of new Federal lenders to farmers contributed to the distress, more branch banking lowered failure rates, and deposit insurance had a very limited role.

Ken Snowden's discussion asked the authors to sharpen the parallels to the 1980s. He pointed to several areas where the data could not fully test the authors' hypotheses. First, they could not test the impact of deposit insurance because it was confined to only 5 to 8 states in the agricultural midwest. Second, there are measurement problems in the tests of branch banking, because the branching data include urban branches, while the dependent variable is the suspension rate of rural banks only. Third, rather than focus on Federal lending, he encouraged the authors to emphasize the increased competition from insurance companies and larger banks. Ken felt that the banking system experienced a technological

shock with the development of the auto, which gave rural people access to banks in larger towns. He suggested two additional parallels with the 1980s. Just as automobiles shocked the intermediation system in the 1910s and 20s, computers offered a technological shock to the system in the 1970s and 80s. Government intervened in the early period to support the mortgage sector, and the development of Fannie Mae, Ginnie Mae, and the like altered the structure of mortgage markets in the later one. Gordon Roberts suggested there might have been differences in regulatory forbearance across the states, and Wheelock noted they had tried to control for differences in supervision of banks. Charlie Calomiris noted that because this was a study of suspensions, depositors, not regulators, controlled the process.

Richard Grossman presented the final paper on deposit insurance, regulation and moral hazard in the savings and loan industry in the 1930s. Richard saw the 1930s as a good test of the effect of deposit insurance because S&Ls were much slower to insure; only 50 percent were insured by the 1950s. In the 1980s the S&L problems seemed to start with a slackening in regulation. Grossman compared the experiences of S&Ls in Milwaukee and Chicago; Chicago S&Ls faced a much looser regulatory environment. Using several tests, he found that a tighter regulatory environment had more impact on limiting risk-taking than the absence of deposit insurance.

Discussant Mike Bordo was skeptical about the applicability of Grossman's results to today's situation. Comparing the 30s to the 80s reveals more dissimilarities: The 80s experienced price level and interest rate shocks from the late 70s, and deregulation and deposit insurance induced moral hazard, while the 30s experienced a retrenchment after a shock, more regulation, and the depression of 1937-38. Thus, in the 30s, the surviving S&Ls probably acted very conservatively, making it harder to detect moral hazard. He also felt there might have been sharper distinctions between types of regulation.

David Wheelock and Grossman agreed that there were potential problems with two-way causality in

his two-stage regressions. George Benston pointed out that the insurance funds were different for S&Ls and banks. Since S&L depositors were really shareholders, the insurance payoff took much longer than FDIC payoffs to bank depositors. He also noted that regulation might have stopped banks from taking acceptable risks; less risk is not always better. The building and loans may have failed in part due to lack of diversification because they were required to make local loans. Michael Bordo suggested that regulation might even be destabilizing if it prevented diversification of portfolios. Larry Neal pointed out that the Chicago problems that Grossman ascribed to economic conditions might also reflect differences in the regulatory environment.

The final Clio session dealt with industrial organization issues in economic history. Tony O'Brien asked whether Ford was the first modern manufacturing corporation. Tony's presentation emphasized that, in the 1910s, Ford had developed a decentralized multidivision corporation. There were regional divisions with their own assembly plants and a sophisticated sales accounting system that allowed them better to time production. Although many of these techniques are credited to General Motors, O'Brien found that Ford had developed them at least a decade earlier.

Fred Carstensen was bothered by Tony's characterization of Ford as the first modern corporation. Singer and McCormick already had multiple divisions and assembly plants as early as the 1870s and had daily sales reports by the 1880s. O'Brien claimed Ford was more efficient than GM in the 1930s because its inventory to sales ratio was lower, but large inventories might be useful to the firm. Singer had felt that lost sales through lack of inventory offset extra inventory costs. Further, Ford's inventory data were from dealers, and GM dealers may have held more inventories because GM had more model types. Does Ford deserve more credit for its conservative approach than GM does for a different approach? Tony responded that we know Ford lost its position as the sales leader, and he wanted to discover how Ford became the leader in the first

place. Michael Haines suggested that Tony was focusing more on the day-to-day aspects of management and less on the leadership at the top. A series of questions followed about the extent to which Alfred Sloan followed his own dicta about keeping the corporate staff from meddling with decisions at the divisional level. Peter Temin suggested we look more carefully at the communications between firms. Do we know from the record whether GM learned from Ford's mistakes? Carstensen noted that the move to sales accounting by GM was a response to a severe problem.

Susan Carter presented her joint work with Richard Sutch on plant turnover and its impact on unemployment rates. Their view of the 1890s is that unemployment rates were high, the incidence of unemployment was widely dispersed, and that unemployment duration was brief. Previous explanations of this phenomenon emphasized that workers retained preindustrial work cultures, and firms did not provide workers with specific human capital. Susan's recent work found that most employees were in lengthy jobs and that there was not much difference between individual and firm behavior today and in the past. In modern times a surprising amount of unemployment came from jobs ending when companies closed or downsized. Further, turnover was higher in small firms. The goal of their paper was to assess how much unemployment came about because firms came to an end in the 1890s. They used data from factory inspection surveys in Wisconsin to do the calculations and found that very little of the difference in unemployment rates between the 1890s and 1980s could be explained by differences in the extent of firm failures.

Mike Haines commented that while the assumptions about missing data on firms seemed all right for the unemployment study, they were more questionable for answering questions about firm dynamics. The data came from two incomplete censuses of firms seven years apart, and little is known about the nature of the missing firms. There was a large number of firms in the 1898 survey (but not in the 1891 survey) who reported they were in business prior to 1891. More about those firms could be found by examining

city directories and the like. The remainder of Haines' comments focused on how Carter and Sutch might use demographic techniques to create firm life tables and proportional hazard models.

The final paper was Dan Raff and Tim Bresnahan's study of the census manuscripts for manufacturing from 1929 through the early 1930s. Raff presented this as a facts paper, examining whether the patterns they had discovered previously for motor vehicles were present in other industries. This version of the paper proved quite different from the abstract. Although they expected to find substantial heterogeneity, Raff and Bresnahan found substantial homogeneity. Except for the cotton textile industry, most of their findings for motor vehicles carried over to other industries. For example, the firms that failed did not look much like the ongoing firms, exitors contributed a great deal to the decline, the composition effects are all in the exit, and employment of labor was procyclical, as firms hoarded labor.

Peter Temin asked how interesting is diversity *per se*. Can we examine diversity in a systematic fashion? He was concerned the authors may be misusing the term labor hoarding because industry still lost workers, primarily through plant closings. They offered little discussion of the distinction between plants and firms. Possible differences in closing behavior could be explored more fully. Although Raff and Bresnahan say increasing returns to scale is the reason for the exit of small plants in motor vehicles, continuing firms contracted uniformly which suggests that returns to scale were not present. Finally, Temin asked if there was a single theory covering exit and continuation of firms. Following Mike Haines's lead, he stated that this paper focuses on mortality, but probably should deal with morbidity as well. Dan Raff noted that the paper was primarily descriptive and suggested that the real title of the paper was "First Glimpses." That ended all discussion, with Dan winning the award for the phrase that best describes nearly all of our attempts to understand the historical process.

Announcements

Association of Business Historians seeks Interested Individuals

The Association of Business Historians was formed in September 1990 in the United Kingdom and is seeking members. The Association will publish a newsletter, *Business History News*, twice yearly beginning in April 1991, and will hold its inaugural biennial conference on Business in Crisis, Glasgow, 27th-28th September 1991. For information on the conference, write Dr. G. Jones, Department of Economics, University of Reading, Whiteknights, READING RG6 2AA, U. K. Membership applications with name, address, and affiliation should be submitted with annual subscription of £6.00 (£9.00 overseas), payable to the Association, to Dr. M.B. Rose, Membership Secretary, The Association of Business Historians, Department of Economics, The Management School, Lancaster University, LANCASTER LA1 4YX, U. K. (Memberships are renewable on 1st September.)

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British Quantitative Economic History Study Group

The next meeting will take place on September 13/14, 1991 at the University of Edinburgh. Those wishing to attend or to present a paper should contact Dr. David Greasley, Department of Economic and Social History, University of Edinburgh, 50 George Square, Edinburgh, Scotland.

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Clio Elects New Trustees

Jeremy Atack, University of Illinois and Jeffrey Williamson, Harvard University have been elected to serve as trustees of the Cliometric Society for four year terms. They fill the position vacated by Don McCloskey, whose term expired, and a newly created trustee position.

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Have You Renewed for 1991?

Members of the Cliometric Society are asked to promptly remit renewal forms with dues payments. The 1991 Membership Directory is in the works, thus the speedy return of updated address information is essential.

1991 Membership Dues are as follows:

Student\$8
Regular\$15
Sustaining\$20
Non-North American members may add \$5 to receive the *Newsletter* by air mail.

Add \$33 for your subscription to *Explorations in Economic History*. Outside North America, add \$44 for your subscription to *Explorations in Economic History*.

Call for Papers for ASSA Meetings 1992

Anyone interested in presenting a paper at The Cliometric Society Sessions at the ASSA meetings in New Orleans, January 3-5, please note the following deadlines. Members are urged to pass this announcement on to colleagues and students who may want to submit their work.

Deadlines that must be met:

May 20- Two copies of a two-five page proposal of your paper is received by:

Lee Alston
225H Kinley Hall, University of Illinois
1407 West Gregory Drive
Urbana, IL 61801

Lee and co-chair Donald McCloskey, University of Iowa, will notify presenters of acceptance by June 21.

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). Summaries will be published in the October Newsletter. Please do not submit a paper if this deadline cannot be met.

December 6- The final version of your paper is received by the discussants and other presenters in your assigned session.

Session presiders and discussants are needed. If you will be attending the ASSA meetings and would like to be involved in the Clio sessions, we would appreciate hearing from you by June 21.