

The Cliometric Society

Spring 2000 Vol. 15 No.1

Report of Clio Sessions at 2000 ASSA Meetings

(Boston) The ASSA met in a mild Boston, a pleasant contrast to its last visit when two feet of snow challenged anyone who ventured out of the Copley Place cocoon. The record-setting weather and absence of Y2K problems meant that all were free to enjoy the three Cliometric Society sessions organized by Kyle Kauffman (Wellesley) and Lee Craig (North Carolina State), as well as the attractions of Boston and Cambridge. Because summaries are available as an insert to the October *Newsletter* and via **eh.net** (with some complete texts also on the website), I shall give only brief accounts of the papers and concentrate on comments and discussion.

Friday, January 7, 10:15 am: Chairing the first session, "Slavery and Race," Kauffman introduced Joshua Rosenbloom (Kansas) to present the findings of his paper, "Slave Values," written with Peter Mancall and Thomas Weiss (both of Kansas). The authors find no trend in slave prices for the 1720s to the 1760s. However, this was followed by a period of rising prices into the early 19th century. A chief determinant of the price of slaves was the price of rice. Slaves were a large part of the region's wealth, which increased because of asset price inflation rather than asset accumulation. The authors conclude that there was no large increase in total factor productivity.

Jonathan Pritchett (Tulane), discussant, found two papers hiding in the one presented. The first estimates slave prices from imperfect data (or data collected imperfectly). If, as the authors assume, prices were determined outside the state, why were South Carolina prices different from those in other states? Pritchett was also concerned by the limited number of observations used to determine price elasticities. The second paper relates the price of

slaves to the economy of South Carolina. Pritchett wasn't sure of the first argument, that a decline in rice relative to slave prices would indicate an increase in total factor productivity, or the second, that because such a decline is not observed, TFP did not rise. It is possible that an adjustment would have taken time.

Discussion revolved around determination of slave prices and elasticities of different factors of production. Robert Margo (Vanderbilt) asked about prices of slaves in other states. Siddharth Chandra (Pittsburgh) suggested that if slave prices were deflated by rice prices, the real price of slaves was constant, not rising. Rosenbloom agreed. Sam Williamson (Miami, Ohio) wondered about other prices, particularly for land. Rosenbloom concluded
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Executive Director's Notes

Dear Colleagues:

Please join me in welcoming the new editorial team of the Cliometric Society Newsletter. Mike Hauptert (University of Wisconsin-La Crosse) has kindly offered his services as the executive editor, and Jean Bonde of La Crosse is the managing editor. Pam Nickless (UNC-Asheville) and Mary Beth Combs (Iowa) have taken over as associate editors. When the trustees surveyed the membership in 1998-99, the conference summaries and interviews in the newsletter were frequently mentioned as important services of the Society. I am sure the new editors will continue to provide excellent coverage of Clio and related events.

I would like to once more thank Debbie Morner and Sam Williamson for their years of work on the newsletter, and I also thank Lou Cain and John Lyons for their dedicated service as associate editors. Lou and John were particularly helpful in volunteering to stay on until the new operation was up and running.

Elsewhere in the newsletter, you will find an announcement concerning the Society's sessions at the 2001 annual meetings of the Allied Social Science Associations. Kyle Kauffman (Wellesley) continues to serve as the organizer of the Society's sessions and as our agent at ASSA. Please contact him with ideas for papers and sessions.

We are still in the process of moving the Society's operations from Oxford to Raleigh. I will spare you the details of those transactions, because someday I will try to persuade one of you to replace me; and let's just say the less you know the better. Debbie, as always, has been quite helpful, and if membership renewals, subscriptions, or address changes have lagged, fallen through the cracks or been otherwise mishandled, then I take full responsibility. Please let me know of any problems.

Finally, don't forget about the Fourth World Congress of Cliometrics in Montreal, July 6-9. If you have not already registered, please do so as soon as possible, and make your hotel reservations by May 1. I look forward to seeing you in Montreal.

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Executive Director

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An Interview with Stanley Engerman

Stanley Engerman is John H. Munro Professor of Economics and Professor of History at the University of Rochester, and Research Associate, National Bureau of Economic Research. He joined the faculty at the University of Rochester in 1963. He earned his Ph.D. from Johns Hopkins in 1962, working under Richard Musgrave in public finance. He also holds an M.B.A. from NYU. Professor Engerman's current research interests are in the area of factor endowments, culture, institutions and economic growth, and the economic history of naval blockades. This interview was conducted by Tony O'Brien (Lehigh University) during March of 2000 by e-mail, fax and telephone.

Why did you decide to become an economist? How did you first become interested in economic history?

My undergraduate major was in accounting, and, while getting an M.B.A. at night, I worked for two years for a C.P.A. in New York. I had always been interested in economics and in history, so when I found that I did not want to work as an accountant for the rest of my life, I applied to graduate schools to study economics. For my M.B.A. thesis I wrote on Henry Carey and his times, and in my first year at Johns Hopkins I gave a "journal club" presentation on the Conrad and Meyer article, one product of which was an important article on slavery by Yasukichi Yasuba, a fellow graduate student who attended this meeting. Thus, there was no single event that led me to economic history. My Ph.D. thesis in public finance, written for Richard Musgrave, was also somewhat empirical.

How did you first come to meet and work with Robert Fogel?



At Hopkins I shared an office with Bob Fogel and several other graduate students, and we had frequent discussions on topics such as the Union Pacific, the railroads, and slavery. I spent most of one summer working as a research assistant on his railroad book, and, after we both graduated, he asked me if I would be willing to work as a co-author of an economic history text, which he had been planning to write.

Would you comment on the *Reinterpretation of American Economic History* book? What was the inspiration for bringing those articles together? Did the book have the impact on economic historians that you had expected?

Reinterpretation was intended to bring to the attention of historians the contributions made by recent work in economic history and to indicate that the work dealt with problems of interest to historians. However, the impact of this book, in bringing together many significant essays, seemed greater on economic historians. And while some essays were used by other historians, the impact overall was probably less than originally hoped for.

Do you have other reflections about the 1960s-early 1970s period?

The early cliometrics meetings were generally quite exciting and had many interesting intellectual exchanges. There

was a sense of dealing with major issues in economic development and history, and the caliber of scholarship was extremely high.

Do you see the New Economic History or Cliometrics as having been a significant break with what had come before in economic history? Did you see yourself as participating in a revolutionary reorienting of the field?

Most of us saw cliometrics as either a significant break with or, at least, a significant extension of past work in history, including economic history. The questions came both from economics and history, but the central point was in method – the use of explicit economic analysis and the attempts to get a strong empirical and quantitative base for the measures used. Some thought of themselves as revolutionaries while others regarded the work as the only reasonable way to reach important answers to long-standing historical questions. The reactions to this work often led to a hardening of positions on all sides, so the break with the past sharpened.

Would you discuss the origins of the slavery project that eventually became *Time on the Cross*? When you and Robert Fogel began the research for the paper that was published in *Reinterpretation* did you already intend to embark on the larger project?

The original discussions on slavery emerged in our graduate school days based on understanding Conrad and Meyer. This interest continued since slavery was an important issue in American history, and when we were putting together readings for *Reinterpretation*, we felt that it would be useful to supplement the essays chosen with one bringing the debate up-to-date. It was while writing that essay that we thought in detail about a more extensive project to try to answer some of the questions we felt were

still open and could be answered with new data. This seemed a useful project because of the centrality of slavery to American history and the fact that many important questions were basically posed in economic terms, no matter how previously answered. It was this combination of applicability of economic and quantitative methods and the importance of the specific questions that made the study of slavery so central to discussions about the new economic history.

You have mentioned Conrad and Meyer's article on the profitability of slavery a couple of times. Could you expand on why that article had the impact it did on you and other young economists at the time? If it had been published in the *Journal of Economic History* rather than in the *Journal of Political Economy*, would you and your colleagues have been as likely to take notice of it?

The Conrad and Meyer article was a very clearly presented piece dealing with the economic analysis of a major historical problem. It described the basic investment approach, pointed to the data needed to answer the questions raised, and examined the sensitivity of conclusions to possible alternative measures. It demonstrated how economics – both theory and data – could be used to advance the debate. I think it would have attracted about the same attention if published in any other journal, but it was interesting that the *JPE* was willing to publish it.

Obviously *Time on the Cross* was met with an extraordinary reaction. Did you expect the book would receive such a reaction?

Certainly the extent of the reaction and its duration was somewhat unexpected, although in some ways it was quite flattering. It indicated that economic and quantitative approaches could deal with

questions that many people seemed to regard as important, as well as questions that did require the use of empirical data to obtain answers.

Would you say the key results of *Time on the Cross* have held up over time? Are there still important unanswered questions with respect to the economic history of slavery in the United States?

In general, I would regard the key results of *Time on the Cross* as having passed the test of time and, with the new results of studies by others, having had some influence on the study of slavery in other times and places than the United States. The issues of profitability, viability, and increasing income, as argued by numerous others as well, seemed to have some agreement, while the flexibility and dynamics of a slave economy are also noted. The economic issues with the most uncertainty are probably those of possible increased industrialization over time and a better relating of antebellum and postbellum labor behavior.

At the time, many critics of *Time on the Cross* seemed to focus on what they felt was the overly benign picture of slavery as painted in the book, at least with respect to the material standard of living of slaves. The result was an extended discussion of slave diets, housing, chances of promotion within a hierarchy of occupations, and so forth. What is your take now on the outcome of that aspect of the debate?

My reaction to the current literature is that most of the work about the material standard of living provides a view somewhat similar to that presented in *Time on the Cross* and that recent arguments of slave autonomy, which in many ways go beyond our arguments regarding slaves and masters, do

provide some consistency with earlier writings.

Some critics, particularly conventional historians, have argued that cliometric literature on American slavery focuses too much on large cotton plantations in the late antebellum period to the neglect of either earlier periods or of slavery off of large plantations. Do you think that is an accurate observation?

I think that the point about focusing on a particular set of units has been useful, but I also think that any examination of the literature on southern antebellum slavery has the same concentration. When you remember the great importance of cotton plantations and the share of slaves on them, it is not obvious that for certain questions such a focus is unreasonable. For different questions, other types of agriculture and industry and other sizes of units would be necessary, but how different the answers will be from the cotton plantation case is not always clear.

When Robert Fogel eventually published his further thoughts on slavery in *Without Consent or Contract*, he acknowledges his collaboration with you on slavery research following the publication of *Time on the Cross*. Why didn't that collaboration carry through to joint authorship of *Without Consent or Contract*? Were you more content than Fogel to let *Time on the Cross* stand as your major statement on these issues?

By the time Bob was working on *Without Consent or Contract*, I had gotten interested in other related questions such as the postbellum South, slavery elsewhere in the Americas, and the reemergence of contract labor throughout the world. I also wrote a number of essays on U.S. slavery dealing with a variety of issues which arose. In general, there was no disagreement between

Bob and me on most issues of interpretation of the U.S. case, and there were no marked changes that I would have made.

You seem to have devoted a great deal of time in recent years to research on comparative slavery. What are the major lessons to be learned from the comparative approach to the study of history?

I think that there is so much to be gained by comparative studies, since studies of similarities and differences can highlight important aspects of each specific case. The studies of settlement patterns and of the adjustments to the ending of slavery have been quite fruitful as they provide a context for interpreting the U.S. experience. They also remind us that comparative information was often examined by contemporaries to forecast the future and to formulate policies.

In what ways would you say our overall understanding of the economic history of slaves in the New World is distorted by a focus on American slavery?

I think that the distortions of individual studies can go two ways – misunderstanding other New World slavery by extrapolating on the basis of the U.S. case and also misunderstanding the U.S. case by not observing patterns of commonality with other slave societies. Recent as well as past comparisons have, for example, highlighted the demographic differences between the US and elsewhere in the Americas, including the social, cultural, and physical differences between sugar and cotton production and the similar withdrawal from plantation labor whenever possible after emancipation. Also of interest is the debate over free vs. slave labor in all places and the variations in the process by which emancipation was accomplished.

You have been interested in the slave trade and, more generally, the African background to the settlement of the New World. Not much of the work done in these areas is by cliometricians (for instance, the excellent essay by John Thornton in the first volume of the *Cambridge Economic History of the United States* is a more or less conventional historical account). Do you have any thoughts as to why these areas do not seem to have caught the attention of cliometricians?

Probably the main reason is, thus far, the limited data that can be used to study those questions important to economic historians. Little systematic material on inputs, outputs, prices, and populations is currently available, and the range of uncertainties in any estimates remain large. Some shifts have recently taken place regarding the usefulness of the postulate of some rational economic behavior, so some questions can now be examined in more detail than before. Limited data makes economic history difficult, but it has the same effect on other types of history as well.

Do you believe a consensus has been reached from the debate touched off many years ago by Eric Williams as to the role of profits from the slave trade in financing the growth of manufacturing in England and the United States?

The Eric Williams [thesis] has its moral as well as analytical aspects, and a general consensus is difficult to achieve. Nevertheless, looking at how the question is approached, there have been some refinements in certain areas. The questions asked have included (1) the size of the shock, (2) the impacts on other parts of the economy, and (3) the nature of the economy in which the shock has been introduced. The concept of dynamic effects on other sectors has been used to argue for a large

impact, presumably in lieu of large direct effects from unusually high profits. I still find the question of how Britain was able to achieve a well-functioning economy at the time to be an interesting one.

Like most other fields in economics, economic history has become more technical and quantitative. On the whole, has this been a good thing?

One big change in cliometric work was, for obvious reasons, that great attention was now being given to 20th-century topics. Earlier, the 19th century and colonial period were more frequently studied. With the great abundance of qualitative as well as quantitative data prepared by the government for the 20th century, more sophisticated analysis can be undertaken using more technical means of analysis. As long as the studies are asking useful questions and are grounded in some empirical base, they can make contributions to the field. Given the trade-offs made necessary by time limitations, technique can sometimes come at a cost. Nevertheless, we should remember what Adam Smith said about the advantages of a division of labor and not expect any one work by itself to answer all the questions.

Has anything important been lost as a result of the more technical approach? Are there any new areas or approaches to economic history, anthropometrics perhaps, that you consider particularly interesting and important?

What new approaches will prove to be interesting in the future is hard to determine now, since what seems possible to do often has surprises — as with expanded anthropometrical studies which initially emerged from the various questions related to slavery.

On the one hand, economic history has had some trouble holding its own in the economics profession, with departments dropping economic history requirements in their Ph.D. programs (and sometimes dropping economic historians as well). On the other hand, many people feel that a large gap has opened up between economists doing economic history and conventional historians. Do you have thoughts on either of these issues?

The role of economic history in both economics and history has become puzzling. In many ways, the quantitative and analytical approach has become a standard part of the historian's method, and many more studies utilize these approaches than before, even if they are not at the technological frontier. Similarly, more of the work in applied economics fits into the range of economic history, based on quantitative analysis of past data. But while applications of economic history have increased in both disciplines, there is less call for those trained specifically as economic historians. The demand among historians had dropped a few decades ago and does not seem to have made a recovery. The more recent decline in the economic history requirements in many departments does not always represent a loss of interest in economic history, since other courses may pick up that approach. Meetings of the Cliometric Society and the Economic History Association do seem to attract younger (and older) scholars and to present new research. One just wishes for a better set of job market prospects for new people in the field.

Does the economics profession at large see economic history as a legitimate subfield or simply as a tool to occasionally be made use of, as, for example, Bernanke uses the Great Depression to gain insight into macroeconomic theory?

I remember that in *Reinterpretation*, there were articles by economists and by historians, as well as by economic historians. The selections were made on the basis of questions and methods, and I still think that provides a useful approach.

When Fogel and North won the Nobel Prize, many economic historians seemed to feel that this would raise the prestige of economic history within the profession. Do you believe things have worked out that way?

The Nobel Prize did indicate that a jury of peers regarded the field of economic history as being central to the discipline. What is less clear is whether this enhanced prestige was translated into gains throughout the age distribution of economic historians.

A great deal of your work has dealt with the economic history of African Americans. It seems as if many scholars dealing with the history of African Americans have the impact of their research on current political questions firmly in mind. And certainly the reactions to such research often seem to reflect current political concerns. Has this been an issue for you?

It is difficult to do any work in African-American history – or, indeed, other fields of history – without being sensitive to possible applications to the current scene. The major difficulty is in trying to determine what we think the best or most appropriate way to see the past. During some times, the politics of seeing slaves as victims was deemed most important; more recently, it is slave autonomy that appears most frequently argued. Now, both contentions are important to understand, but emphasis on one or the other does leave any scholar open to criticism.

You have produced a great quantity of original and important research. So I wanted to take a page from Brian Lamb's interviews on C-SPAN's *Book Notes* program and ask you to describe your routine.

I usually divide my workday so that I am in my office from about 8 to 5, meeting students, talking on the phone, writing e-mail etc., as well as reading when there is time. In the evening, since my children are now on their own, I usually read and write for several hours at home unless there is a good basketball game or good old movie on TV. Most first drafts I write at home, usually on weekends, since there are usually fewer interruptions. As long as I still enjoy research and writing this schedule works out well.

Do you have any observations about the age-old quandary of how to juggle teaching responsibilities with research?

The teaching quandary has never been resolved, since although I have no problem preparing lectures, it is the delivery that still poses problems. The two things I try to do, if possible, is teach courses for which there is a possible research interest and to teach early in the day so I have a block of time without worrying about class performance.

When will the second and third volumes of the *Cambridge Economic History of the United States* be out, and do you know if Cambridge will ever bring these volumes out in paperback?

The second and third volumes have more contributors than did the first volume. With the first volume, everything worked well. Every contributor completed his or her assignment on time. With the second and third volumes, some people dropped out along the way. Some were replaced; some were not. In the preface to the *Cambridge*

Economic History of Europe, published during the 1950s, the editors mention that the volumes were late in appearing because this contributor died in a concentration camp, or that contributor was killed fighting on the Finnish front. Nothing so melodramatic happened here. Volume II and Volume III should both be out this year. Volume II will be about 1000 pages long, and Volume III will be about 1300 pages. Cambridge did not have much success with paperback editions of a previous series, so it looks as if they will not be bringing this series out in paperback.

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that the price of slaves was high because demand was shifting outward quickly.

Because Leonard Carlson and Robert Chirinko (both of Emory) were unable to present their paper on the profitability of slavery, Robert Margo and William Collins (Vanderbilt) followed with theirs on "Race and Homeownership, 1900-1990." Margo opened with the general comment that they are focusing on issues of wealth, which hasn't been explored as much as income, and that home equity is a large part of wealth. Collins then presented their results. Both blacks and whites experienced large increases in home ownership after 1940, but the gap between ownership shares of white and black owners increased until 1960 and then diminished to 1980. The increasing disparity from 1940 to 1960 resulted from the black migration from the South to northern cities. Reasons for the ensuing reduction are less clear, since the property value gap declines less than would have been expected.

Jens Ludwig (Georgetown), discussant, wondered about the nature of errors in self-reported data and asked for greater emphasis

on the effects of housing and social policy. He wanted the authors to separate stock and flow issues, suggesting that convergence in the latter might come before the former. Williamson suggested that values did not converge, because demand for housing in segregated areas may have declined. In response to a question from Karen Clay (Carnegie Mellon), Margo stated that blacks had a higher proportion of their wealth than whites in housing and a lower proportion in financial assets.

For the final paper, Siddharth Chandra moved the audience halfway around the globe to discuss the origins of nationalism in the Netherlands East Indies – a topic of concern in light of recent events in Indonesia. Very simply, he finds the roots of nationalism in the decline of the wages of indigenous workers relative to those of Chinese workers in the first two decades of the 20th century.

The discussant, Sumner LaCroix (Barnard College and Hawaii), noted the revival of interest in the colony about which much information is available. He found that the paper documented rising wage inequality and demonstrated a correlation between this rise and racial conflict, questioning the origins of the inequality trend. Because, after 1910, Chinese were able to move anywhere in the colony, perhaps the local inhabitants feared they would be displaced as middlemen. (Other examples of this pattern are common.) Chandra suggested that the World War I boom might have led to a greater demand for what was perceived to be more skilled labor, as, for example, were the Chinese.

Friday, January 7, 2:30 pm: The session on "Monetary and Financial History" was particularly well attended, with many scholars of contemporary finance in the audience, illustrating the vitality of banking and financial history. Douglass North

(Nobel laureate) entered after the session began and, in the words of one participant, "remained uncharacteristically quiet."

Chair Richard Sylla (NYU) introduced Eugene White (Rutgers) to present the paper he and Cormac Ó Gráda (University College, Dublin) had written. They try to distinguish between two types of banking panic: non-informational, such as the panic of 1854, that affect the entire industry, and informational, such as the panic of 1857, which affects only banks like one that has failed. Their study is based on the remarkably complete deposit records of the Emigrant Savings Industrial Bank.

In his discussion, Peter Temin (MIT) observed that our interest in panics is not purely historical. We want to know about the vulnerability of banks. Does bank structure play a role? What can substitute for banks if they are in trouble? What is the role of a lender of last resort? This paper begins to answer these questions, particularly about structure. For example: Can we assume the same patterns for a commercial bank as for this savings bank? What were the bank's assets? What was the length of assets and liabilities? What were deposits used for? Temin wondered whether, in addition to characterizing people by demographic variables, they could be classified by their pattern of deposits. White replied that in 1854 the ESIB held 40% of its assets in mortgages, 40% in bonds, and the rest in cash. In 1857, the bond market fell so far that the bank's capital could have been wiped out.

In the lively discussion that followed, participants were interested in the behavior of both the bank and its depositors. White indicated that the bank's directors did pay attention to developments in the bond and call markets that might have affected the value of their assets. Joseph Mason (Drexel) noted that some depositors withdrew money

but did not close their accounts. Did these people consider that the bank might write down the value of deposits and behave accordingly? White discounted the importance of such behavior, since 80% of the money leaving the bank was accounted for by closures. Some 8% of deposits were lost in 1854 and between 12 and 15% in 1857. The bank made up for the outflow by drawing on the cash it had available, calling loans, and selling bonds. Among those depositors less likely to panic were professionals and – at least in Manhattan – priests.

The second financial history paper was "The Stock of Closed Bank Deposits, Duration of Credit Channel Effects, and the Persistence of the U.S. Great Depression," which Joseph Mason presented for himself and his co-authors, Ali Anari and James Kolari (both of Texas A&M). Mason began with the conceptual background, that bankruptcy transforms creditor claims into investments with uncertain returns and unknown maturity. If assets decline in value, then trustees may decide to hold them. This situation could lead to the persistence of business cycles with no investment or lending. Using vector auto regression (VAR) models to establish their case, this is what the authors believe happened during the Great Depression.

Sylla, as discussant, contrasted the Depression with conditions in the US in the late 1980s and early 1990s, when the Resolution Trust Corporation worked well, and in Japan during the 1990s, when bad debts hung on and the economy stagnated. He had some questions about the choice of predictor variables – Why use industrial production and the M1 money supply rather than GDP and M2? – but otherwise found the argument convincing, if not that original.

In the discussion that followed, all participants agreed that liquidating assets as

soon as possible would shorten depressions. Marshall Reinsdorf (FDIC) compared the US depression to contemporary Japan, where insolvent banks can't make loans. Mason agreed that many crises were similar. Differences between assets, for example real estate vs. corporate loans, affect the success of liquidation. Temin pointed to the difference between failed and moribund ("zombie") banks. Because the latter were more problematic, the Bank Holiday of 1933 was beneficial in the way it changed expectations.

For the final presentation, Arthur Sweetman (Victoria) and Aidan Hollis (Calgary) examined the relationship between depositors and employees of Irish Loan Funds, quasi-bank charitable organizations that made loans to the poor. Sweetman, who presented the findings, argues that monitoring by depositors was an important method of insuring Fund employees did not abscond with the cash.

Discussant, Robert Whaples (Wake Forest), speaking as a labor economist, wanted to examine all the reasons for pay differences among workers but found only a few possibilities actually discussed in the paper. He wondered whether other forms of monitoring, aside from that by depositors, might have been likely. These may have included advance screening, clerks watching each other, or monitoring by the charity that received the fund's "profits." Sweetman responded that, even if one controls for the number of people working in the institution, the effect persists. To a question from Sylla, the author replied that monitoring was facilitated by the small regional compass of these banks, whose depositors had access to the books, met with the managers (some of whom acted as monitors), and knew the borrowers.

Saturday, January 8, 8 am: One is always dubious about a session titled "Frontiers in

Economic History," knowing that the title is another way of saying, "Papers that we could not put anywhere else." This time, however, the sizable early-rising audience found that the translation was, "Papers that were too interesting to omit." Brad DeLong (Berkeley) was unable to be present, so his duties as chair were taken on by Eugene White.

Karen Clay and Werner Troesken (Pittsburgh) use the Whiskey Trust of the late 19th century to test for the market power of monopolies. Clay reported their finding that markets for distilled spirits (which were what the trust sold) remained for the most part competitive, although there were a few instances of collusion.

The discussant, Stephen Margolis (North Carolina State) noted that markets with falling prices, as was the situation here, were often the objects of government investigation. According to the paper, the Whiskey Trust sometimes raised prices and sometimes lowered them, with the average effect being nil. He wondered about the meaning in the regression of the trust dummy - based on the comments of observers that at certain times the trust was effective. Did similar products such as whiskey and moonshine affect the nature of competition in the market? Further discussion revolved around the degree of competition in the market. Collins and William Hutchinson (Miami, Ohio) questioned the possibility that either Canadian whiskey or moonshine might be competitors to the Whiskey Trust. Clay doubted that Canadian whiskey was a competitor since the markets were not connected. As for moonshine, it probably did compete because it wasn't subject to the high alcohol taxes. Of course, information on moonshine is hard to come by.

Maristella Botticini (Boston University) continues her work on Renaissance Tuscany

in her paper on dowries, written with Aloysius Siow (Toronto). If parents want to transfer their wealth to their children, why do they choose dowries as the mechanism for giving part of it to daughters? Botticini's answer is that in a "virilocal" society (where brides live with their in-laws), dowries permit such transfers while preserving sons' incentives to work hard at home. In neolocal societies and those with more active labor markets or human capital investment, dowries disappear. Margo, acting as a substitute discussant, was satisfied that the model stacked up pretty well against the data. The brief discussion that followed clarified a few specific points.

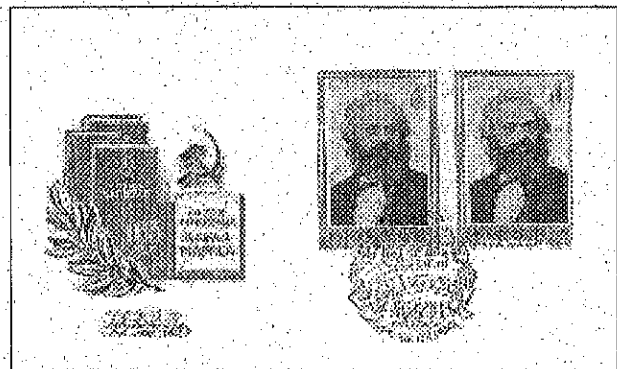
Next, William Hutchinson returned the focus to the United States, presenting his paper on trade around the turn of the 20th century. He finds that models of international trade which stress factor proportions are successful in predicting trade volumes, but those models which suggest that trade will occur between countries with similar tastes and incomes (gravity models) have little predictive power. The net exports of the US in the 1870-1910 period were thus positively correlated with the capital content of goods and negatively correlated with their labor content.

Collins, doing yeoman service as discussant for both this and the final paper, was puzzled by the poor performance of the gravity model (although the factor proportions theory didn't do very well either). He was troubled by the specification of the model, which seemed to allow for little variation. Were the country dummies proper? Collins suggested that since the results contradict Gavin Wright's conclusion that the US exported goods with high natural resource intensity, Hutchinson ought to examine Wright's model more closely. Hutchinson responded that he was uncertain about the worth of the gravity model.

The final paper of the session was a study of market integration in Egypt by Tarik Yousef (Georgetown), who wants to change the focus of discussion from the failure of nations on the periphery to industrialize to the success (at least of Egypt) in creating an integrated local market. Yousef finds that during the approximately 100 years of laissez-faire ending with World War II, Egyptian markets for a wide range of commodities did become integrated.

Comments by Collins began with the proposition that integration is a matter of degree. Since the data all come from the 1920s, for which frequent price reports are available, it is difficult to tell how or whether integration changed over time. Thus the importance of different factors – railroads, institutions – cannot be determined. Even if it means using less regular data, there might be a gain from covering a longer period.

That concluded the formal Clio sessions, although Kauffman, following tradition, had also arranged a Members and Friends Cocktail Party at an Irish Pub in Boston's trendy South End. Those who attended found it a pleasant occasion to chat, meet each other's significant others, and (for the more staid and over-the-hill among us) to gain some insight into the Gen-X culture.



Karl Marx holds the distinction of being the most postally-used economist in history.

A Letter from the Editor

Dear Colleagues:

The Clio newsletter has finally returned. For those of you who have renewed your subscriptions, welcome back. For those of you who are new members, welcome aboard. This edition of the newsletter comes to you about two months later than normal. This is due to the recent transfer of the executive offices from the University of Miami to North Carolina State and the University of Wisconsin - La Crosse. The transition has gone about as smooth as one could expect - several unexpected glitches, a hold up here, a lost file there, a misaddressed letter . . . they all add up, but we have pulled everything together and are up and running once again.

You will notice a few slight changes in the newsletter, in both form and content, over the next year. This is a result of the transfer of the newsletter from the capable, veteran hands of Sam Williamson, Lou Cain, John Lyons and Debbie Morner, to what I believe will be the equally capable, but currently novice, hands of myself, Jean Bonde, Mary Beth Combs and Pam Nickless. The look of the newsletter will evolve over the year as we transfer the production to the software packages supported by UWL and the production management of Jean Bonde. We do not intend to make any radical changes, but some changes will occur never the less.

This will not be the only issue that will be late this year. Because the World Congress of Cliometrics is meeting this July in Montreal instead of the traditional meeting of the Cliometrics Society in May, the summer issue of the newsletter will be postponed until August so that we can include a report of the World Congress meetings. The fall issue is expected to be published at its usual time and will include abstracts of the papers to be delivered at the ASSA Clio sessions in New Orleans in January (see page 16 for the announcement regarding the call for papers).

As the new staff makes the transition to full time responsibility for the newsletter, I would like to take this opportunity to publicly thank Sam Williamson, John Lyons, Lou Cain and Debbie Morner for their extraordinary efforts in aiding the change of venue for the newsletter. This issue would not have been possible without their help. I cannot count the number of e-mails and phone hours I have already logged with seemingly endless questions and problems. Each one has been deftly answered and without complaint, though I know that some of them have come at an extremely high cost in terms of time. My undying gratitude goes out to these folks, and I encourage you to extend your thanks to them for the efforts they have logged over the years in support of the newsletter. There is a lot more to producing the newsletter than meets the eye. Special thanks must be given to the student staff of our Information Technology Services, who logged in more hours answering our questions than they did for all of our other projects combined in the last year. This issue would not have been possible without their efforts. I sincerely hope that Sam was correct when he indicated that it gets easier as it goes along and that the learning curve is indeed shorter than my life expectancy.

As the year proceeds, and the newsletter begins to take shape, I would encourage you to provide your feedback. Your comments and suggestions will be valuable as we venture forth with this undertaking. Let me know what you think of the changes that we make, what things we should not change, what you would like to see or not see in the future. The newsletter is for the membership of the Society, and should reflect the Society's interests, so let me know what those

are. If you have any comments, please send them to me at haupt.mich@uwlax.edu or call me at 608-785-6863.

The backbone of the newsletter is the reports on meetings and conferences. To that end, your help can make the newsletter even better. While I keep a sharp eye on the eh.net calendar, not all conferences of potential interest to cliometricians are posted there. If you know of a conference or workshop that should be reported in the newsletter, please let me know about it. In order to get a report written, I need to know the name, date and location of the event, and a contact person with whom I can communicate. In addition, I need interested individuals to report on the event. A set of style guidelines is available to help authors prepare their report, and we will provide the editorial assistance necessary to get it into print. In return, you get a byline, a front row seat at the event, and I'll buy you a cup of coffee the next time we meet at a conference. If this sounds too good to pass up, please contact me and let me know of your interest in reporting a future event. If you are going to be at a conference and would be willing to report on it for the newsletter, please drop me an e-mail and we can go from there. Having said that, the World Congress of Cliometrics is coming up in July . . .

Michael J. Hauptert
Editor

Call for Essays

Economic History Society

The Economic History Society invites 1,500-2,500 word essays from members on the topic 'What Economic History Means to Me' for publication in an EHS 75th Anniversary collection. Submit essays by **September 30, 2000** to:

Pat Hudson
HISAR
Cardiff University
PO Box 909
Cardiff CF1 3XU
United Kingdom

Authors may submit their essays via e-mail. Please send documents as attachments to the following e-mail account: hudsonp@cardiff.ac.uk

Call for Papers

2001 ASSA Meetings January 5-7, 2001

The Cliometrics Society will sponsor three sessions at the ASSA meetings in New Orleans, January 5-7, 2001. The Selection Committee will be chaired by the ASSA Coordinator for the Cliometrics Society, Kyle Kauffman (Wellesley College).

Authors interested in presenting papers should send a one to two page proposal, including a description of the paper, and all contact information by **May 22, 2000**. Authors are encouraged to submit their proposal via the on-line submission form at www.eh.net/assa.html, or authors may submit their proposals via e-mail. Please send documents as attachments, in either Word or WordPerfect format, to the following e-mail account set up solely to receive proposals: Econhist@wellesley.edu, or mail hard copies to:

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Authors must be prepared to send a 3,000-word summary to the Society office by September 1, 2000 for publication as an insert in the Fall Newsletter, as well as on the Cliometrics Society webpage. At least one author must be a member of The Cliometrics Society.

Relevant Dates

Proposals due	May 22, 2000
Authors notified of acceptance	July 1, 2000
Summaries due	September 1, 2000
ASSA Meetings in New Orleans	January 5-7, 2001
