The Newsletter of

The Cliometric Society

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Report on Clio Sessions at 1995 ASSA Meetings

by Ann Harper Fender, Gettysburg College, and John James, University of Virginia

(Washington DC) Pouring rain could not dampen the spirits of the enthusiastic participants at the 'Members and Friends' reception and the five Cliometric Society sessions organized for this year's ASSA meetings by the Selection Committee, John James (Virginia) and Barbara Sands (Arizona). Session chairs limited formal presentations at each session, and in true Clio style audiences responded to fill the allotted times with extended discussion.

Friday, 6 January, 8:00 am: The first Clio session, 'Migration and Well-Being in 19th-Century America,' with Joseph Ferrie (Northwestern) in the chair, opened bright and early with Steven Herscovici (Chicago) summarizing his comparisons of wealth accumulation and occupational change for male migrants and non-migrants in Newburyport, Massachusetts, between 1850 and 1860. He reexamines Stephan Thernstrom's conclusion in *Poverty and Progress* (1964) that upward economic mobility was quite limited. Herscovici traced those workers who left Newburyport and found greater upward economic mobility than did Thernstrom. Discussant Jonathan Pritchett (Tulane) focused on the techniques used to locate workers appearing in the 1850, but not the 1860, census, noting that Herscovici had found only 20% of the non-persisters (migrants). He asked both for a model of why non-persisters did significantly better than persisters and for estimates of how much improvement – how much economic progress – had resulted from antebellum migration.

Ferrie then observed that Newburyport's low growth rates could be compared with those of the areas to which migrants moved. Questions arose about the accuracy of the census, who would have been excluded from the census, and records that might permit inclusion of women in this study. Sam Williamson (Miami) asked about Thernstrom's 'floating proletariat.' Herscovici responded that Thernstrom used cross-sectional data, and cited other work indicating that in-migrants were likely to be merchants moving to capture economic gains. Ferrie observed that Thernstrom seems to have presumed that mobility does not improve the worker's lot and structured his research design accordingly, discounting life-cycle effects as well. Further comments addressed business cycles, epidemics, and whether natives or immigrants had fared better by moving.

John Komlos (Munich) and Peter Coclanis (North Carolina) summarized their paper, written with Paul Katzenberger (Munich). Using heights of Georgia convicts, 1817-1885, they inferred regional standards of living and made intra-regional comparisons. In general, their results accord with those from other regions of the United States. Within the state, inmates from the most commercially-developed regions were the shortest, while farmer inmates were the tallest. Komlos showed how rising food prices, accompanied by rising income,

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What's Inside

Executive Director's Notes

Ann Carlos and Ken Snowden have been elected Trustees for 1995-1999. They replace Jeremy Atack and Jeffrey Williamson, whom we thank for their service to the Society for the past four years.

Server News

Last February we told readers about a new server protocol, World Wide Web, a spectacular improvement over gopher. WWW offers many advantages, such as graphics in addition to text files, and easier, more efficient ways to locate archived resources. If you have been using gopher, or just considering installing server-access software on your machine, we highly recommend Web. Since WWW is the 'wave of the future', many universities offer seminars on its use. Ask your computer support staff to provide access to WWW, and have Netscape or Mosaic software installed. If you cannot obtain the software through your institution, write us at our helpline for assistance.

If your equipment does not have WWW capability, you can still obtain information from our server using gopher or even your regular e-mail program. Again, our helpline provides instructions, included in the standard reply.
Addresses: WWW: **http://cs.muohio.edu/**
Gopher: **cs.muohio.edu**
Helpline: **help@cs.muohio.edu**

We are pleased to announce an improved way to browse the List Archives directory, where postings to each list are collected by month. If you are using WWW, you can sort postings by date or author, or by 'thread', an option allowing you to locate all postings with the same subject. For example, in the February 1995 Econhist file, you can find more than 40 postings on 'Bureaucracy'. The original posting – the start of the 'thread' – is at the top of the section, with responses in sequence below. Server users can open and read any or all of the archived postings.

Gopher users will also find an improvement in these archives. We have removed the annoying routing information from the beginning of each message, and the archived postings are thus shorter and easier to read.

Address Alias

Please remember that there is a simple address for every Society member for whom we have an e-mail address. Mail sent to <*first initial last name*>@cs.muohio.edu is forwarded automatically. Postal and e-mail addresses for members are also available in the Directory in the server.

List News

Subscribers to Society lists know that postings now have a subject prefix, such as 'EH' for 'Econhist'. These codes provide an easy method for subscribers to differentiate list mail from other e-mail messages.

We add lists and update existing lists regularly. For the most recent information, send the e-mail message *info lists* to **lists@cs.muohio.edu**

List Reports

In the past few months, **Econhist** has become a high volume discussion list, with lengthy debates on issues such as 'capitalism', 'bureaucracy', and most recently, 'coercion', each with 40 to 50 postings. The number and length of these postings sparked a subscriber debate about whether the list should be monitored to prevent cluttering electronic mailboxes, or remain free-wheeling,

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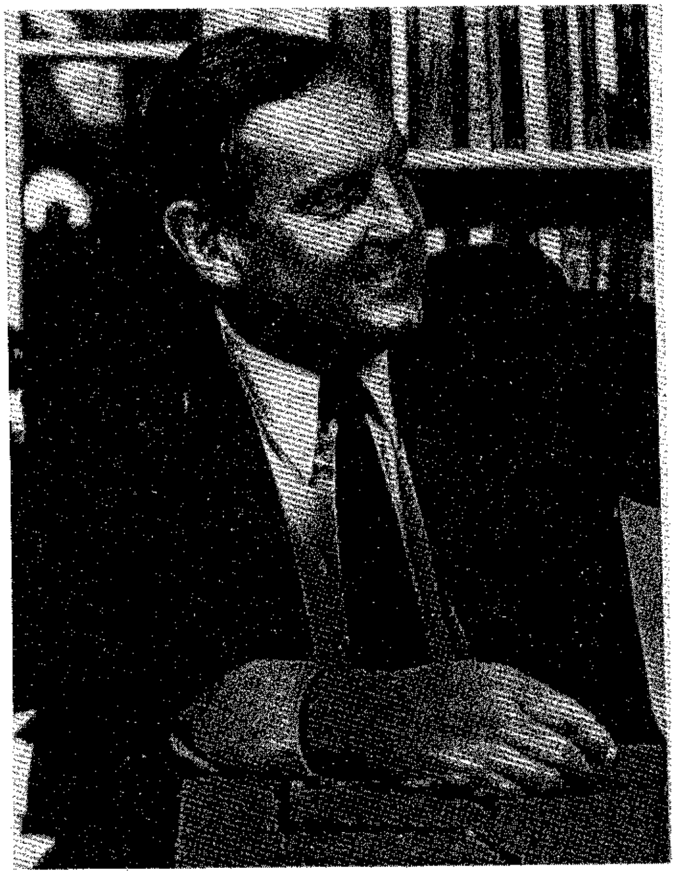
AN INTERVIEW WITH JOHN MEYER

Editors' note: John R. Meyer is the James W. Harpel Professor of Capital Formation and Economic Growth at Harvard University. He received his B.A. from the University of Washington 1950) and his Ph.D. in economics from Harvard (1955). Aside from teaching at Harvard, and at Yale from 1968 to 1973, he has been a Guggenheim Fellow (1958), President of the National Bureau of Economic Research (1967-1977), and vice chairman of the Union Pacific Corporation (1981-1983). Among other advisory positions, he has served on two Presidential task forces on transportation and has chaired a government commission studying railroad productivity.

Our interview took place in early September, 1994, in Professor Meyer's office at the Kennedy School of Government, and was conducted by John Brown (Clark University), who writes:

Together with Alfred Conrad, John Meyer played a key role in the early development of Cliometrics. He has subsequently developed a reputation as a leading expert on government transportation policy. Two papers and the ensuing discussion were particularly influential on the practice of economic history. The best known is 'The Economics of Slavery in the Antebellum South' (1958), which recast the plantation economy in terms amenable to economic analysis. Their finding that the return to an investment in a slave matched the return available elsewhere is the best-known contribution of the paper. It also set out a long research agenda on interregional trade in slaves, the demography of slavery, and other issues within a coherent economic framework. The paper also illustrated the methodological contribution presented in a less well-known piece aimed at economic historians: 'Economic Theory, Statistical Inference, and Economic History' (1957), which argued that economic historians should look for generalizations and test specific hypotheses. It articulated an analytical distinction between first-hand, literary evidence and the indirect quantitative evidence that could be used to test the implications of even qualitative hypotheses. Finally, as acting editor for a year, Professor Meyer opened up ***Explorations in Entrepreneurial History*** *to the new work in quantitative economic history.*



Let me start at the beginning. How did you and Alfred Conrad get involved in the issue of slavery? Did you approach it more from a methodological perspective, or was historical debate your primary motivation?

Alf and I were old friends; we knew each other in graduate school. In the summer of 1955, Alf went off to Northwestern University and I stayed on here as an assistant professor. Both of us, unbeknownst to the other, somehow got conscripted to teach American Economic History. In my case, the course had been orphaned by Jack Sawyer's departure to Williams. I don't know what the situation was at Northwestern. Arthur Smithies, who was the chairman of the Harvard Economics Department, made me an offer I couldn't refuse. If I taught the American economic history course in the spring, I could have half time off during the fall of 1955 for preparation. And Alf had a rather parallel experience; Harold Williamson recruited him to teach the American economic history course at Northwestern and gave him some time off from his other teaching duties to prepare. Alf, continuing some research with the Leontief Project, was in the habit of visiting Cambridge from Evanston about once every month or so. Sometime in January 1956, I believe, we were having lunch and started talking about our mutual experiences preparing to do an American economic history course. We also discovered that both of us had decided that the slavery profitability question seemed particularly suitable for illustrating the role of economics in analyzing historical issues. Both of us had sketched out a paper on the subject. We compared notes and we found that we had almost identical outlines. I had started at the beginning, with a microanalysis of slave profitability and Alf had started on the second part of the paper with an analysis of demographics and macro-market developments.

You were not an economic historian *per se*? Were you already working in transportation economics?

No. My thesis had been on business investment decisions, so my areas of interest were econometrics, industrial organization, and corporate finance. I became interested in transportation economics at almost the same time as I became involved in economic history.

Is it fair to say that you then had a professional interest in seeing that economic theory or econometric methods would actually be applied to historical issues?

I'm not so sure that it really wasn't the other way. Both Alf and I felt, possibly under the influence of Schumpeter, Smithies, and others, that empirical economics was quite obviously historical in nature. While today we have developed some experimental data for measuring economic behavior and phenomena, there were none back then. Even today, it seems safe to say that 98-99% of the relevant data that economists have for doing empirical testing is generated by historical processes.

So you saw this work as an extension of what economics was doing anyway in terms of testing hypotheses?

Yes. The Harvard graduate economics department and program were very much designed around a sort of 'three legged stool' as it was often described: theory, statistics, and history. That perhaps reflected the influence of Schumpeter among others. Schumpeter died about six months before I arrived, so I never really had direct exposure to him. Alf, on the other hand, was probably one of the last, or the last, dissertation student that Schumpeter had. Schumpeter died before Alf finished his work. He therefore completed his thesis under Leontief, who also strongly believed in the empirical foundations of economic analysis.

How would you characterize the intellectual legacy or atmosphere that reflected Schumpeter's influence? Was it, for example, an emphasis on longer-term dynamics of development?

The Harvard department had codified into its general exam requirements the belief that you needed all three of the basic skills: theory, statistics or econometrics, and some historical perspective. Schumpeter apparently liked to stress that economists put the really important things in '*ceteris paribus*': demographics, changes in preference functions, technological developments, *etc.* All the things that often were the most important part of understanding economic phenomena.

And that will come from history?

I think historians have traditionally understood that fairly well. Very often, the role of historians in economics departments is to make people aware of things they might otherwise overlook. To broaden the agenda, so to speak.

I can see this discussion developing two branches I'd like to pursue. One is your assessment of the current role of economic history, but let's leave that aside. Instead, let's turn to the reaction your work on slavery received in 1957-58. As economists with research interests outside economic history, you and Alfred Conrad were not insiders at the EHA meeting where you first presented the paper on the profitability of slavery. Was the response to your work a surprise?

It was a surprise, but I think it's easy to overestimate the hostility of it. The hostility was fairly limited. Most were really quite open-minded and responsive. The thing that really surprised us was how interested they were. Alf and I thought that we were doing something rather dull and pedestrian in many ways: delivering a little sermon, so to speak, on methodology and economic history.

The one on econometrics and history.

The point we were trying to make was that economic analysis is a seamless whole. Almost inevitably if you are going to do empirical work in economics you must become involved with historical data. It might be quite recent history and be more aptly called journalism, but let's put that distinction aside. Basically most of the data available to economists have been generated by historical happenings or circumstances. And so in order to interpret and understand the data they were using, economists inevitably have, or should, become at least somewhat familiar with historical methods and the history of the period from which their data came.

The slavery question was quite interesting because it was such an easy application of economic concepts. We also felt quite confident of the answer. All of economic theory pointed in one direction. You couldn't keep resources committed at that level without some kind of profitability in the activity. Furthermore, the rising prices for Southern farm property in the 1850s strongly suggested that there was somebody making some kind of profit out there. It was also a market with all kinds of potential rents buried in it, the most obvious being for land. Critics (*e.g.*, Doug North) pointed that out and their point was well taken, although it was also a bit more complicated than they allowed. Of course, there were other opportunities available for Southern land than slave plantation agriculture. Nevertheless, you had a market where there are lots of rents around that don't seem to be disappearing, basic factor prices were moving upward, lots of people continued to commit quite a bit of capital, energy, labor and entrepreneurial capabilities. It's difficult to imagine that such an activity didn't return something close to the average level of profits available in the economy.

This insight (in retrospect) would seem clear to someone with an economist's training. Was it simply that your audience had not thought about it in that way?

I don't know. I'm the wrong person to ask.

You said that they were quite receptive to both the methodology and, it seems, the conclusions.

There were some exceptions, of course. For example, Fritz Redlich (I can't remember whether he was at Williamstown or not) was quite antagonistic, again for reasons that weren't always clear to us. His major criticism seemed to be directed toward counterfactuals. In the slavery paper, we were not guilty of that. On the other hand, the paper I had done on British economic development [1955] was counterfactual in character and of course Bob Fogel's work on railroads was, and those ignited very large outbursts from Fritz.

Looking back, then, people were receptive and there was continued interest in the paper. Both Douglas Dowd and John Moes had comments published.

I think his [John Moes'] was rather more balanced. As I recall, more technical in character. I think there was a basic movement underway in the direction that Alf and I were pointing anyway. You had Walt Rostow's work, you had Kuznets's work, you had Gerschenkron's. Fogel, I think had already started on his thesis. You had several people in Gerschenkron's economic history workshop who were doing work of that type. I'm not quite sure that we just didn't catch the trend or the wave, in surfer terminology.

Caught the lip of the wave . . .

Caught the moment! I never could quite understand some of the more emotional objections to the paper, because fundamentally wewere not trying to take a moral stand on any of the larger developmental or political issues. We were simply trying to narrow in on the very specific question of whether slavery was a businesslike operation. Were most of the owners deriving profits from it?

Which with 20/20 hindsight looks like a very reasonable question . . .

Actually many did ask it and many of them had the right answer. In the Lincoln-Douglas debate, if my memory is correct, Lincoln outlined most of the essentials. In particular he put his finger on why continued expansion of the system was crucial to profitability. There were several journalistic and contemporary diary accounts at the time that more or less captured the essence of how the system worked and why it was profitable. Ken Stampp's book, which appeared almost simultaneously with our work, also had the essentials developed from a more historical perspective.

But for some reason, publication of this argument in the *JPE* prompted a much larger response than it would have elsewhere.

I'm still mystified by the response.

How do you think the debate on slavery eventually played out: how did it inform historian's understanding and economists' thinking about the past?

I'm terribly biased, obviously. I think on the whole it's been very constructive. There have been several substantial and fine contributions by Fogel, Engerman, Gallman, Parker, Goldin, Sutch, and many others. I certainly enjoyed most of what I have read and I think we do understand the economics of slavery far better than when we started back in 1957. Some of the data bases that have been developed are really very ingenious and represent very fine scholarship.

You were still pursuing research as econometrician and empirical economist. Did the economic historical research have a bearing on your other efforts?

Looking back on it, I would argue that some of the work that Alf and I did in the late 50s and 60s, separately and together, was from a methodological standpoint historical in character. For example, there was a paper that Ed Kuh and I did ('How extraneous are extraneous estimates?') that built on some very good work done by Richard Stone on estimating consumption functions. Stone used cross-section estimates of income effects so as to conserve on degrees of freedom when doing time series analyses of price effects. What Kuh and I argued was that the processes that generate income effects across the cross section were substantially different from those that one would have observed in a time series. Accordingly, one had to be extremely careful when blending information from these two different sources. We also got into some problems of doing price corrections using Paasche and Laspeyres indices. Almost all of that, by the way, traced back quite directly to ideas that we were exposed to in Gerschenkron's basic graduate course in economic history.

Oh really.

Actually, that particular paper, 'How extraneous are extraneous estimates?', owed more to Gerschenkron than did the slavery paper. Incidentally, both Alf and I first presented the slavery paper as lectures in undergraduate classes. Watching the undergraduate reaction to it was really quite fascinating. We caught their attention to a slightly greater extent than was normal, I think. They took it as an objective exercise with none of the emotional reaction we later encountered with more senior reviewers.

From the perspective of a senior economist, do you have a sense that once the Civil Rights movement moved to the front of the national stage the work on slavery became associated with political concerns? Would the response to your work have been different in 1968 versus 1958?

Who knows? I think so, yes.

And did you encounter any of that reaction later on?

I think that the 1967 confrontation was the last time we were in the eye of the storm. From then on, Fogel and Engerman usurped that position. Alf died a few years after and in 1967 I became involved with the National Bureau and administrative duties absorbed me. Actually, the last paper Alf worked on was one he and I were doing jointly. It was a paper on technological diffusion, a joint project among several economic research institutes around the world. Each institute took responsibility for one technology. These were all production innovations, not product innovations. The National Bureau took responsibility for the basic oxygen process. We did international comparisons of rates of diffusion of BOP technology. Alf and I were doing that collaboratively at the time of his death. Our basic finding was that the U.S. steel industry did not adopt the basic oxygen process quite as quickly as several other countries' steel industries because of U.S. factor prices and the U.S. industry's existing investment in the most advanced open hearth technology. These made it less rewarding at the margin to undertake BOP investments in the U.S. than elsewhere. Thus, it was not so much that American steel industry managements may have been misguided, but that they were confronting a different set of objective facts. Actually, if they did make a mistake, it was in not investing more in minimill or direct reduction technology using scrap metal as the raw material input.

Given the supply of scrap here in the United States?

Yes. Direct reduction was the innovation that really made sense in the U.S. context.

Subsequent discussion of entrepreneurial decline seems to have taken up exactly that point, at least in the case of cotton and steel.

Some of our European collaborators were less than fully convinced, at least in the case of BOP.

Were they more interested in looking at entrepreneurial capabilities?

In some cases, yes.

Cultures?

Cultures and attitudes. Of course, the study was also done at the time (the late 1960s and early 1970s) when finding fault with American management and institutions was somewhat in favor. But again, it is easy to overgeneralize. On the whole, I think we convinced most of our foreign collaborators.

Did you and Conrad have expectations about the impact your work would have on the practice of economic history? Were those expectations borne out?

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Report on the NBER/DAE 1994 Summer Institute

by Joseph P. Ferrie, Northwestern University and NBER

(Cambridge MA) The Development of the American Economy Workshop at the National Bureau of Economic Research's 1994 Summer Institute was held July 12th through 15th at the NBER. The program was organized by Jeremy Atack (Vanderbilt) and Charles Calomiris (Illinois) and featured 16 papers and 37 participants, economic historians affiliated with NBER and invited guests.

The first paper on Tuesday afternoon was presented by Joseph Ferrie. In 'Down and Out or Up and Out? The Occupational and Financial Mobility of Non-Persisters in Nineteenth Century America', Ferrie described the creation of a sample of American males linked from the new 1850 Federal Census PUMS to the 1860 Federal Census manuscripts. He reported some preliminary results. Based on characteristics of individuals who have and have not been linked in the preliminary sample, Ferrie concluded that the linked observations constitute a representative cross-section of American males who survived the 1850s. Only two of five males in the linked sample remained in the same county over the decade, with higher persistence rates in rural places and (surprisingly) on the frontier than in urban places. Farmers were most likely to persist; laborers, less likely; and white-collar and craft workers, least likely. Among non-persisters, the average distance moved over the 1850s was 474 miles. In terms of upward occupational mobility, intercounty migrants fared better than persisters, and migrants who traveled the farthest did the best. This finding contrasts with the conventional wisdom (following Thernstrom's pioneering work) that non-persisters in this period were a 'floating proletariat.' Mobility, however, was not without its risks. Intercounty migrants also experienced more downward occupational mobility than persisters, and long distance movers were the most likely to move down.

Discussion centered on creation of the sample. Several participants requested additional details on the linkage process, particularly the bias introduced by selection of individuals with uncommon names. Richard Sutch (UC-Berkeley) suggested that individuals who were linked, but whose linkage was not definite (differences in age or birthplace), be retained and included in the analysis in order to expand the sample. Tom Weiss (Kansas) suggested using his revised labor force figures for 1860 for calculating structural change in the US economy over the 1850s. Several questions arose regarding definitions of upward and downward mobility and whether these concepts were adequate to explain the experience of intercounty migrants, who may have improved their circumstances by moving, even if they had not changed occupational designations.

Laura Dresser (Michigan) presented the afternoon's second paper, 'Did Federal War Contracts Make A Difference: Labor Market Opportunities of White and African American Women after the Second World War.' Using data from the War Production Board and the 1940 and 1950 censuses, Dresser asked how federal war-time contracts had affected women's labor force participation. She found, despite the lasting effect of such contracts on some communities, little real effect on the participation rate. Like Goldin, Dresser questioned the impact of the war on the post-war increase in female labor force participation.

Questions fell into three categories: the nature of the contract data, inferences which could be drawn from the 'experiment' that the awarding of the contracts may have represented, and the possibility that alternative sources like the PUMS might provide greater detail. Lance Davis (Cal Tech) argued that the year in which the contract was awarded might be a poor measure of when it had its greatest impact, suggesting that data on contract length be included. He also asked about service sector contracts, since the paper examines only manufacturing. Shawn Kantor (Arizona) and Richard Sylla (NYU) both asked whether the contracts had determined labor force participation, or had the participation rate determined allocation of contracts?

Several participants suggested that Dresser's 'experiment' (linking changes in labor force participation in a community between 1940 and 1950 to its receipt of government contracts) may be flawed because of migration to places where contract work was performed. Both Kenneth Sokoloff (UCLA) and Claudia Goldin (Harvard) suggested that migration is a problem, if the individuals observed in the 1940 or 1950 Census were not living in the places where they had worked on wartime contracts. Sutch thought that the War Manpower Board may have encouraged people to relocate to production sites. Sutch, Goldin, and Robert Margo (Vanderbilt) suggested using the 1940 and 1950 PUMS to get better measures of labor force participation and in-migration and to expand the set of control variables. Goldin also suggested using synthetic cohorts to control for the effect on participation rates of variations in age at entry to the labor force. Both Price Fishback (Arizona) and Sokoloff suggested that Dresser use regional dummies to control for the impact of race and for the state of the labor market in 1940.

The papers presented on Wednesday were chapters from the forthcoming *Cambridge Economic History of North America*, edited by Stanley Engerman (Rochester) and Robert Gallman (North Carolina). After a brief introduction to the project by Gallman, Robert Margo discussed his essay on 'The Labor Force in the Nineteenth Century', stressing the major changes in the labor force occurring over the century, in size and composition, in wages, and in hours worked. He relies on Stanley Lebergott's earlier work as well as Weiss's new estimates to document the slow, steady growth of the labor force and the rise in labor force participation. Participation rates varied considerably across demographic groups, with almost universal participation among males aged 15 and older and (before 1865) adult slaves; adult women's rates grew modestly, from 8% to 14% by 1880. Wages grew just over 1% *per annum* over the century, slightly less than the growth in *per capita* income, with the difference accounted for by rising participation rates and more hours worked per year. Important structural changes were the shift in employment from farming to manufacturing, increasing concentration of unemployment, and decreased flexibility of labor markets.

Most questions concerned issues that people thought Margo should either address or address in greater detail. Sutch wanted more information on child labor and changes in formal contractual terms. Richard Steckel (Ohio State) worried that some issues like migration and labor market performance might fall between Margo's essay and Michael Haines's essay on demography (described below). Both Weiss and Atack were interested in how the intensity of work changed over the century, while Goldin suggested the shift from outdoor to indoor work was another dimension along which the 'quality of life' of workers had changed. Calomiris asked whether changes in the nature of output had a large impact on labor relations, and how this change may have affected wage flexibility. Sylla suggested another important structural change, the rise of big business and the depersonalization of the employment relationship. Naomi Lamoreaux (Brown) thought the movement of apprentices and journeymen out of their employers' homes was another fundamental change in the labor force over the 19th century.

The next paper was Claudia Goldin's 'Labor in the Twentieth Century.' Goldin first reviewed the trends in labor market characteristics over the century. Real wages grew rapidly, although less rapidly and less equally during the productivity slowdown of the last 20 years. Labor force participation increased as more women entered the labor force, more than compensating for the disappearance of children and the retirement of older men. Unionization grew and then shrank over the century, while spot labor markets decreased in importance. She then turned to 'downtime', time spent away from work. Downtime resulting from unemployment rose in duration, but decreased in incidence, although it decreased as a result of seasonality and illness. Finally, Goldin described the rising inequality and falling productivity of the past two decades, as well as two long-run trends, the increased importance of human capital and changes in the role of government.

Several questions were raised regarding measures of unemployment, wages and inequality. Sutch pointed out that unemployment at the turn of the century was brief, for just a day or so, while in the post-war period, a person must be out of work for a week or more to be counted. Sutch and Atack argued for inclusion of fringe benefits in wage measures, while Davis urged Goldin to look beyond manufacturing wages, given manufacturing's shrinking share of employment. Michael Haines (Colgate) pointed out that the trend of inequality is very similar to the trend in unionization rates, particularly for large industrial unions.

David Galenson (Chicago) presented his essay, 'The Settlement and Growth of the Colonies: Population, Labor, and Economic Development.' At the outset, Galenson noted how difficult it is for observers today to recognize the novelty of developments in this period, developments surprising to European contemporaries. It is almost impossible to imagine how thoroughly unforeseen these developments were to those who lived through them. For example, why did settlers consistently fail to foresee the colonial labor problem? Galenson suggested that they had no previous experience with an environment of plentiful land and scarce labor. Consequently, settlers took more than a century to make adjustments to their imported European institutions and modes of operation. Even more puzzling is why the same mistakes were made in subsequent attempts to reproduce the English manorial system in America by European investors, *e.g.*, the Calverts in Maryland or Penn in Pennsylvania).

Several participants asked about the uniqueness of the North American experience of colonization. Sokoloff thought that the greater scale of farming and the institutions brought by the Portuguese to South America had produced substantial differences, at least in the short run. Eugene White (Rutgers) pointed to the absence of the equivalent of a viceroy in North America, and Haines remarked on the Spanish Crown's fear of growth of individual power. Lamoreaux observed that South American colonizers ultimately encountered similar obstacles to transplanting European institutions. Europeans did not succeed in placing a manorial system of landholding permanently in the New World.

Attention then turned to the specifics of Galenson's narrative. Calomiris asked whether the change in emphasis from export staples to production for internal markets led to greater economic and political freedom. Galenson pointed to Virginia (a hotbed of revolutionary ideas, yet a major exporter of tobacco throughout the colonial period) as a case where greater freedom occurred despite the absence of a shift in market orientation. Haines asked whether fertility differences across colonies resulted from differences in age at marriage induced by land shortages. White asked for Galenson's response to work by Philip Coelho and Robert McGuire, who suggest that the transition from servitude to slavery in the Chesapeake had resulted from differing effects of disease on servants and slaves. Galenson responded that the transition occurred during the 1680s, when labor market changes reduced the supply of servants to the Chesapeake. Finally, Sylla questioned Galenson's lack of emphasis on the role of colonial governments in shaping the economic environment in this period. Galenson countered that the work of Jonathan Hughes on government intervention in the economy looks at legislation rather than practice; it is difficult to find instances in which prosecutions occurred under colonial economic regulations.

Michael Haines then presented his essay, 'The Population of the United States, 1790-1920.' After summarizing the available sources of statistical evidence, Haines suggested several ways in which the US demographic experience differed from Europe's. Most strikingly, fertility was falling in the US from the start of the 19th century, even before mortality rates began falling, and fell even as mortality rose between 1830 and 1860. In the European demographic transition, fertility rates fell only several decades after mortality rates plunged. Haines suggested two possible explanations for this peculiarity: the land availability model originally proposed by Easterlin, and the more recent model of David and Sanderson which emphasizes the growth of alternative employment opportunities for children and the corresponding decrease in the value of children as a source of old-age security. The US was also characterized by a low age at marriage compared to Europe, a lower percentage never marrying (with both numbers rising through 1900, falling through 1975, and rising again since then), and extraordinarily high rates of immigration.

Several questions were raised regarding the US demographic transition. Gallman asked about the role of the Civil War in disrupting US demographic patterns. Sutch wanted to know how much of the drop in US mortality was accounted for by falling mortality at young and old ages. Calomiris asked whether the 'demographic transition' is invariant to the initial levels of fertility and mortality. Sokoloff noted that resources in the US initially allowed such high fertility that early fertility decline is not surprising, and suggested that Haines examine levels of fertility and mortality in addition to changes. Haines's discussion of nuptiality patterns prompted Goldin to suggest that the rising age at marriage may reflect increasing numbers of women entering the labor force and earning independent incomes. She also asked that Haines consider pre-nuptial pregnancies as a cause of marriage early in the 19th century, offset by a later increase in abortions. Sutch suggested that Haines discuss life-cycle migration. Galenson pointed out that the impact of characteristics such as age and marital status on mobility is swamped in magnitude by the overall mobility of the US population.

Eugene White presented the final paper from the Cambridge volume, 'Banking and Finance in the Twentieth Century: The Domestic Financial Sector.' White focuses on how government intervention (in response to instability) shaped the development of financial institutions. The dominance of commercial banks as sources of funds declined in the three decades after 1916 because of government restrictions, resulting in the rise of investment banking. After New York and other states barred insurance companies from underwriting securities, there was new growth in commercial banking but, more importantly, a mass market for securities developed. Reluctance of commercial banks to finance purchases of household durables led to growth of consumer finance companies. During the Depression, with the rise in the ratio of debt to GNP, the public's attempts to increase liquidity through bank withdrawals resulted in legislation to prevent collapse of the financial system. The Glass-Steagall Act preserved the structure of the financial industry and protected commercial banks. World War II increased firms' liquidity and helped hide the instability in the financial system that the deposit insurance system's perverse incentives had helped to create. The 1960s and 1970s were decades of relative financial stability because there were few macro-economic shocks. In recent years, strains on the system increased, with relative losses from the S&L crisis ten times those of the Depression.

Davis objected to White's initial characterizations; based on his own work, Davis suggested that long-term loans were moving through capital markets rather than through commercial banks even before 1930. Barry Eichengreen (UC-Berkeley) asked if government was a driving force, and whether similar developments had occurred elsewhere at roughly the same time. Several comments were prompted by White's contention that positive consequences of New Deal intervention (fewer bank failures and greater stability in the securities market) were outweighed by higher interest costs (banks' rate of return rose by about 2%). Kantor noted that one must consider consumers' risk aversion to make such an assessment. Lamoreaux added that it is also necessary to consider what consumers had been doing with their money before the Depression, and suggested that the legislation of the 1930s was unique because national attention was focused on the banking sector's problems. About the post-1970s crisis, Sutch asked whether the problem was New Deal policy gone awry or inept deregulation. Calomiris contended that the changes of the 1980s were not really deregulation. Discussion closed with a more general question from Gallman: did government-imposed constraints lead to important financial innovations?

Following a clambake Wednesday night at the Harvard Faculty Club, Thursday's sessions began with Robert Margo presenting two papers based on data he has collected from the manuscript schedules of the Census of Social Statistics (undertaken with the 1850 and 1860 Population Censuses): 'Labor Market Integration Before the Civil War: New Evidence' and 'The Farm-Nonfarm Wage Gap in the Antebellum United States: Evidence from the 1850 and 1860 Censuses of Social Statistics.' In both papers, Margo examined data on average wages for a variety of occupations (farm laborers, common laborers, carpenters, and female domestics) and the value of board, as collected by census marshals for each of the country's minor civil divisions. In the first paper, Margo calculates geographic measures of wage dispersion and its persistence, with the cost of board used as an index of the cost of living in each location. He finds that both real wages and their variance fell between 1850 and 1860, suggesting increasing market integration, but only in the North. At the county level, wage rates converged rapidly from 1850 to 1860, but convergence at the state level was more modest. He explains this finding in terms of increased labor supply and demand in high-wage states, with both shifts probably induced by the settlement process. In the second paper, Margo uses the same data to measure the farm-nonfarm wage gap, finding a gap of 10% within counties, and 10 to 20% across larger geographic units. This pattern appears to have influenced industrialization patterns: the growth of manufacturing between 1850 and 1860 was more rapid in counties with a small gap, with a low price of agricultural labor relative to nonagricultural labor.

Most of the initial questions from the audience dealt with method and measurement. Both Sutch and Gallman worried about boundary changes for counties and townships, while Steckel asked what precise question in the enumerator's instructions had generated the data. Atack asked how the figures for minor civil divisions were used to construct the state-level averages published in 1860, White asked how board was calculated, and Goldin asked if the cost of a room was included. Haines suggested that Margo compare his figures for urban places to those in the Weeks Report. Several people worried about the adequacy of the cost of board as a proxy for the cost of living. Clayne Pope (BYU) suggested that board may have been paid only to certain types of workers. Galenson pointed out that long-distance moves were probably no more costly than short-distance moves in this period, so greater convergence within states than across states is somewhat surprising. He also suggested that differences in the size of states may make measures like cross-state variation misleading. Sutch suggested making all possible pairwise comparisons between locations, then looking at the distances between pairs.

Thursday's second session was devoted to Naomi Lamoreaux's paper on 'The Partnership Form of Organization: Its Popularity in Early Nineteenth Century Boston.' Lamoreaux used a sample of 443 partnerships from the 1845 Boston city directory, linked to the 1850 city directory. She identified several weaknesses of the partnership as a form of business organization, such as its

(continued on page 31)

Conference Report: Divergence and Convergence in the Global History of Economic Development

by James M. Pearson
University of California-Los Angeles

(Pasadena CA) The 1994 Fall Conference of the All-UC Group in Economic History was held at the Ritz-Carlton Huntington Hotel, November 11-13. R. Bin Wong (Chair, UC Irvine), Susan Carter (UC Riverside), Peter Lindert (UC Davis), Ken Pomerantz (UC Irvine), and Jean-Laurent Rosenthal (UCLA) comprised the Program Committee.

The first session, on German fiscal and financial regionalism, with Susan Carter in the chair, was opened by Caroline Fohlin (Caltech), whose paper, 'Bank Relationships and Firm Liquidity Constraints: The Case of Universal Banking in *Kaiserreich* Germany', seeks to explain the role of financial intermediation in industrial development. Using balance sheets collected from Saling's *Borsen-Jahrbuch,* she studies liquidity constraints facing a sample of German firms to test and confirm her hypothesis that firms with close bank involvement experienced lower liquidity sensitivity of investment than did independent firms, and that this effect intensified over time.

Discussant Naomi Lamoreaux (Brown) noted the importance of such work in understanding the role of bank finance in economic development in general, and the effectiveness of universal banking in Germany in particular. However, as firms age and establish themselves they are able to tap new sources of funding. Since all the firms Fohlin studied were found in the *Borsen-Jahrbuch*, they were already mature and well established, and it is thus not clear whether these firms had close ties with banks in their earlier years. If finding interlocking directorates were the main way to ascertain worthiness of firms, then individual enterprises should be studied over time, particularly before they show up in Saling's, better to evaluate the role attributed to universal banks in German development. Fohlin responded that relatively young firms were indeed listed in the equity market, and further that listed firms maintained important links to the banks.

In 'Must Tax Convergence be Downward? Tax Competition in Wilhelmine Germany', Mark Hallerberg (UCLA) studies the extent to which tax competition between individual German states forced their tax systems to converge. He shows that taxes did tend to converge after unification in 1870, but upwards. Furthermore, land, the immobile factor that might reasonably have become the focus of tax policy, was not left with the tax burden. This surprising outcome did not result from cooperation among states, but derived from the dominance of Prussia, which used railroad revenues to cut agricultural taxes and to maintain low income tax rates. Hallerberg concludes by distinguishing between this case and the European Union: the EU lacks a dominant state like Prussia which could lead its members to higher taxes. Thus, the example of Germany 'demonstrates that tax competition is a real danger to the Union if member states do not act more forcefully to coordinate their tax rates on capital and labor.'

Commentator Peter Lindert said there were really two problems: tax convergence and tax increases. He recommended that these issues be decoupled and scrutinized for explanatory political factors such as universal adult male franchise or the French war indemnities after 1871. He observed that land tax reduction implies benefits to the Junker class. Among other discussants, Lamoreaux noted the rough parallel between the German situation and the United States, where individual states attempted to relieve the tax burden on individuals by taxing railroads. The difference was that Germany shifted to an income tax both for individuals and businesses.

Session Two, on Spanish silver and Chinese money supplies, was chaired by Richard Sutch (UC Berkeley). In 'Born with a "Silver Spoon": World Trade's Origin in 1571', Dennis O. Flynn and Arturo Giraldez (both at U. of the Pacific) offer a provocative alternative to the traditional macroeconomic explanation of the origins of world trade. In the standard analysis, China was a relatively passive participant receiving silver for the goods demanded by Europe. Flynn and Giraldez put China at the center by arguing that Chinese demand for silver explains the emergence of world trade. After the Ming empire converted from paper to a silver-based monetary system and required tax payments in silver, Chinese demand drove the silver mining activities of the Spanish empire.

In 'Myth and Reality of China's 17th-Century Monetary Crisis,' Richard Von Glahn (UCLA) evaluates the hypothesis that a sharp decline in the flow of imported silver precipitated an economic crisis in the 1630s, contributing directly to the fall of the Ming dynasty in 1644. The scarcity of silver, further exacerbated by hoarding and tax payments, drove up its relative price, resulting in a commercial crisis. Von Glahn notes that current quantity theory rejects the emphasis on spending and assumes instead that the demand for money and the demand for goods are inversely related – price being determined by demand. He argues that the decline in silver imports has been overstated and that the volume during the 1640s was close to that of the 1620s and early 1630s. Even if earlier projections are accepted, the decline in imports represents only a 4% decline in the total stock of silver, an amount incapable of generating the drastic effects ascribed to it.

Comments by Jack Goldstone (UC Davis) traced the impact of variations in the silver trade on the economies of Europe and other monetized parts of the world. International monetary flows affected governments' policies, particularly the option of debasing their currencies in the face of changing relative prices for precious metals. The second commentator, R. Bin Wong, was dubious of this new explanation of the price revolution. Why did Chinese demand for silver rise in the 16th century, when previously China had relied on brass coins and paper currency? Since the state did not mint silver, the adoption of silver caused it to lose control of the money supply and even violated its notion of a correctly operating political economy. Wong pointed out that Von Glahn himself argues that the importation of silver into China did not correspond to the rise and fall of bullion flows from Spain, and that price fluctuations in China and Europe were related. This suggests that China was a fully coherent market and the price revolution is the basic story of how arbitrage occurred. However, the story ignores the issue of how well money supplies were integrated within China, an area as large as Europe.

Barry Eichengreen (UC Berkeley) presided over Session Three, on economic change in developing countries. In 'Convergence in the Age of Mass Migration', Alan Taylor (Northwestern) and Jeffrey Williamson (Harvard) argue that a principal source of the dramatic economic convergence among present OECD members between 1870 and 1913 was mass migration. Their study asks a counterfactual question: what would have been the measured convergence between 1870 and 1910 had there been no (net) migration. Their analysis suggests that migration could account for large shares of labor productivity and real wage convergence, but only for a small share in GDP *per capita*. Two conclusions follow, that the key axis for convergence was between the Old and New Worlds, and that convergence explanations based on technological or accumulation forces in closed-economy models fail to capture the importance of mass migration.

Commentator Sutch, with an eye on the recent California election, cautioned against an unsophisticated reading which would conclude that immigration simply lowers wages. He suggested that a clearer focus on the life cycle of workers might shed light on the capital that immigrants brought and the capital they created, and on whether capital followed immigrants or came from their labor and savings. Attention to the varied skill mix of immigrants and host country demand for skills would clarify Schumpeter's point that immigrants enrich a nation.

In 'Railroads and the Brazilian Economy before 1914', William Summerhill (UCLA) estimates GNP for a railroad-free counterfactual Brazil in 1913. His results suggest that Brazil experienced significant gains from construction and operation of railroads, with substantially more benefits derived from freight transport than from passenger services. Railroads permitted Brazil to respond to rising demand for exports and had an even more pronounced positive effect on its domestic economy. Discussant Lance Davis (Caltech) noted that Summerhill did not address the amount of growth in Brazil's GDP that was produced by railroads. He considered the greater social savings produced by freight transport in Brazil, in contrast with the results found by Fogel or Fishlow for the US, as due to the comparatively advanced state of the US transport system. Davis suggested that comparing the economic impact of canals on the United States with the effect of railroads on Brazil might be a more appropriate way to put the Brazilian experience into an international context.

The fourth session, on French agricultural productivity, was chaired by Ted Margadant (UC Davis). Phil Hoffman (Caltech) joins a now venerable debate on the nature of French agriculture with 'Growth in a Traditional Society: The French Countryside, 1450-1815.' Traditional comparisons between French and English farming bemoan France's agrarian incompetence, attributed to small farms and absence of enclosure, whereas a revisionist view argues that enclosures were critical neither to French nor British agrarian development. Hoffman approaches the issue by using price and rent series to calculate Total Factor Productivity measures. Using these standards he discovers a wide variation in performance: some parts of France show productivity levels and rates of growth equivalent to England's, while other areas lagged behind. Looking outside agriculture, he ascribes growth and stagnation to urbanization and transportation improvements – the wine trade, especially, was aided by canals. French agricultural progress was inhibited by prolonged warfare rather than by farm size and property rights.

According to Hoffman's revisionist contribution, remarked discussant Jan De Vries (UC Berkeley), peasant behavior was not an impediment to French economic development. Indeed, economic growth *was* market expansion, and the peasantry responded positively to urban growth. However, Hoffman's results show that the important urban effect was due only to the growth of Paris, and not to growth of Rouen, Lyon, or Nantes. De Vries observed in conclusion that the peasants' move from 'market contract' to 'market embrace' needs yet further explanation. Subsequent discussion centered on possible scale effects of urban markets on French agricultural progress, the role of capital markets, and whether or not the French peasantry participated in a 'Smithian' economic revolution rather than a technological transformation *à la* the US or Britain.

In 'What Happened to Feudalism? France, 1780-1820', Donald Sutherland (Maryland) considered the effect of the French Revolution on tax and land laws. Although most people regard 1789 as a crucial transformative moment in European history, France's switch from feudal and aristocratic privilege to contract law was complex and protracted. Although seigneurial imposts were abolished by the Constituent Assembly, landlords were permitted to add the equivalent of their former tithes to their tenants' leases. Sutherland also pointed to the conundrum that the demise of feudalism, with its potentially liberating effects, was accompanied by only modest economic growth.

Jean-Laurent Rosenthal, the discussant, noted that Sutherland's work addresses several possible effects of the abolition of feudalism: its long-run impact on France's tax code, the short-run social impact of abolishing feudal dues, and its short- and long-term impacts on the agrarian economy. Focusing on the second effect, Rosenthal noted that higher rents show that landlords, rather than tenants, benefited from the Revolution. He pointed out that land was broadly dispersed, not simply held by nobles; farmers were often both landlords and tenants. Thus, landlords and owner-occupiers were encouraged to support the Revolution, and tenants, whose condition was not materially worsened, at least remained neutral.

The fifth session, on Chinese enterprise, was chaired by G. William Skinner (UC Davis), and began with "'Traditional" Chinese Business and Capitalist Rationality Revisited.' This case study of a family firm, Yutang Co. of Jining, by Ken Pomerantz (UC Irvine), raised questions about the long-accepted thesis that differences between Chinese and European business practices account for China's failure to develop something resembling modern capitalism. Specifically, the weak separation between Chinese firms and their owners, *i.e.*, the firm's lack of a distinct legal identity, is thought to have hampered capital accumulation and organizational innovation. The Yutang Co., however, consistently attracted investment capital and was successful throughout the period studied (1779-1956). Similarly, the notion that Chinese commerce was carried on by a distinct merchant caste, one which abandoned its commercial role at the first opportunity, was opposite to the experience of the Sun lineage, who were heavily involved both in the Yutang Co. and in politics.

Discussant Zorina Khan (UCLA) began by observing that a sample of one is unlikely to overturn well-established characterizations of Chinese commerce. She suggested that Pomerantz elaborate and distinguish between commercial and status goals. The family-owned firms of China, lacking a cadre of professional managers, may have been adept at short-term decision making that, inadvertently, proved inefficient in the long term. Kenneth Sokoloff (UCLA) commented that the western corporate organizational form was new to China, and was not necessarily superior to the historical structure of the Yutang Co. Thus, more detail on the company's ownership structure would be desirable. Furthermore, Pomerantz's argument that the state was not particularly hostile to commercial development suggested to Sokoloff that additional discussion of private property would be useful. Rosenthal asked if Yutang Co. was successful because the business was profitable or because it was well connected. The discussion concluded with Giraldez's observation that the market in China worked as well as it could; the crucial difference between development in China and Europe (and the United States) was technology. China's growing population meant that labor-using, rather than labor-saving, technological change would be the source of economic development.

In 'The Logic(s) of Capitalism: Investment and Management in the Chinese Silk Industry', Lynda Bell (UC Riverside) focused on the system of 'split ownership/ management' found in the Wuxi filatures, 1904-30. Each filature had one set of investors who built and owned the plant and another set who operated it. This separation tended not only to inhibit innovation but led to an unwillingness to maintain plant, train the work force, and improve product quality. Bell asked, given the apparent inefficiencies of split ownership (a standard form of organization in China) why were the filatures organized in this way? She answered that merchants were part of a multi-faceted elite for whom commerce was often just one of many economic activities. Although Chinese merchants wanted to earn profits, they did not have an equally compelling drive to control production or a predisposition to experiment with new techniques.

Commentator Mary Yeager (UCLA) observed that to attribute the laggard development of Wuxi's silk industry to risk-averse merchants was to neglect the industry's seasonal and agrarian character. Its organizational form may have resulted from the vagaries of silkworm husbandry. Greater emphasis on the details of Chinese silk production might explain why Chinese merchants were willing to leave their industry more fragmented and less elaborated than its Japanese counterpart. Goldstone asked if comparing the organizational style of the Chinese silk industry to an idealized firm based on a Western model was the best way understand it. Bell responded that the Chinese used such models to define their goals during the period of her study.

In 'The Emergence of the Modern Corporation in China', Chi-Kong Lai (U. of New England, Australia) used the China Merchants' Steam Navigation Company for a case study exploring the nature of the government-sponsored joint-stock companies that emerged in China at the end of the 19th century. The China Merchants' Company differed from the Chandler model of a modern corporation in two important ways: it relied on government financial support, and its management was not separated from ownership. Lai said these differences resulted from the perception of its merchant founders that only state support would enable the China Merchants' Company to compete with foreign firms, a perception based on the model of the British East India Company. Chinese government officials saw the joint-stock company as a vehicle by which the large amounts of capital necessary for industrialization could be raised.

In his comments on the actual corporate nature of the China Merchants' Company, Wellington Chan (Occidental) differed with Lai on two points. First, there was no clear public financial market in late 19th-century China. At a time when the average worker made a single tael a month, an individual share in this firm cost 500 taels. The second difference is that the concept of a joint-stock company as a 'legal person', distinct from its shareholders, may not have been understood by the government. The China Merchants' Company was legally different from a western corporation; indeed, it was not until after 1900 that Chinese laws even permitted joint-stock companies with limited liability. While the China Merchants' Company's multi-level organization with distinct operating units resembles Chandler's model, its differences reveal a divergent path to industrial development and, consequently, deserve greater emphasis. Yeager asked how to characterize the relation of the state to the firm over time, and Lai answered that the most pronounced difference was that government policy changed from being supportive to being extractive.

The sixth session, 'US Economic Growth in the Late 19th Century,' was chaired by Alan Olmstead (UC Davis). The first paper on the session, 'The Decline in Southern Agricultural Income 1860-1870', by Garland Brinkley (UC Davis), argued that the dramatic increase in the presence of hookworm was a principal cause of the decline of Southern agricultural output after the Civil War. Although hookworm was not identified as a disease until 1903, symptoms consistent with it were documented by Civil War doctors and suggest its pervasiveness during the war. Furthermore, data from skeletal remains of African-Americans from this period support the thesis that the incidence of hookworm increased after the war. Brinkley also noted that there was a strong correlation between sandy soils and a high incidence of hookworm. This correspondence explains why the greatest income declines were in areas of sandy soil, despite their being the most productive. He concludes that the change in income between 1860 and

1870 was strongly related to the change in hookworm infestation for the same time period.

Discussant Roger Ransom (UC Riverside) felt that Brinkley's thesis helps to explain the large dip in Southern income but not the persistent stagnation in relative income. The contrast Brinkley makes between the health of Confederate and Union soldiers supports the contention that hookworm was spread by the poorly-shod soldiers of the South, but the corollary insight would be that well-fed and well-dressed people resisted the disease. Additional data which more thoroughly correlated the presence of hookworm with soil types would make the argument more persuasive. Was it not more likely that, after enduring the hardships of war, the barefoot poor spread the hookworm, rather than that the hookworm spread poverty? Summerhill asked why Confederate officers, many of whom were former United States officers, were so much less successful than Union officers in maintaining sanitary conditions in their camps. Brinkley explained that the Union forces had Sanitary Commissions to oversee health conditions in camps, while the South had nothing analogous. Sutch wondered if the real change were not a decline in the numbers of workers in the fields rather than a decline in the productivity of individual workers.

[The other paper in this session, 'Location and Technological Change in the American Glass Industry, 1870-1929', by Naomi Lamoreaux and Kenneth Sokoloff, was also presented at the NBER-DAE workshop this past summer. A summary of that paper appears on page 32. Suffice it note that at this conference the discussant, Richard Easterlin (USC), commented that Western Pennsylvania at the end of the 19th century cannot be considered a closely-bound geographical region in the way that the Silicon Valley is today. His substantive criticism concerned the lack of evidence about the supply-side constraints of technological knowledge.*(Eds.)*]

The final session, 'Labor Markets and Property Rights', with Martha Olney (UC Berkeley) in the chair, began with 'Frenchmen into Peasants: Myths and Realities of Agricultural Labor Markets in France, 1862-1929', by Gilles Postel-Vinay (INRA, France) and David Weir (Chicago). The authors describe the economic performance of family farmers in order to evaluate the decision whether to remain on farms or to migrate to urban centers. They challenge the notion that the dramatic economic growth of late 19th-century France was the result of a definitive break with a pre-commercial peasant mentality. Data drawn from both agricultural and general population censuses allow them to measure the wage gap between non-wage family farm labor and urban wage labor, and show that the increasing urban-rural wage gaps observed in other studies of labor market integration were paralleled, from 1862 to 1892, by a decline in the relative earnings of family labor. Where others have assumed that the spread of market capitalism eroded peasant institutions, Weir and Postel-Vinay find a tendency toward increasing segmentation of what initially had been a fairly fluid labor market. They suggested that these gaps – and their persistence – can be attributed to the different life-cycle-earnings profiles available in agriculture and in non-agricultural work.

Commentator Hajime Hadeishi (UCLA) noted the possible use of the Postel-Vinay and Weir data to explore the push-pull factors underlying the gradual, uneven depopulation of French agricultural regions. There appear to be clear regional patterns in the earnings of family labor relative to potential earnings. Regional disparities were pronounced, relatively stable, and followed the traditional demarcation of the line from St. Malo to Geneva, with well-off regions to the north and poorer regions to the south. Hadeishi also commented it would be useful to investigate the compensatory features that might explain who stayed in the poor regions, although departmental aggregates might be too large to capture this information. Hoffman observed there were several ways by which compensating differentials might explain the sustained gaps in wages, the most prominent being the life-cycle. He wondered whether people returned to their farms on a seasonal basis in the late 19th century and whether the life-cycle hypothesis is sensitive to identifying whether workers had children.

In 'Property Rights and Efficiency: The Sad History of the Enclosure Movement', Gregory Clark challenged the well-established historical conviction that English enclosures dramatically increased land rents. He argued that rent increases were modest and that common field agriculture was nearly as efficient as purely private agriculture. The transition from common to enclosed fields was not caused by a struggle over redistribution; it was caused by cultivators being induced to shift systems as a result of enclosure's small gains. He attributed the persistence of the earlier view to the inability of traditional historians to determine so basic a fact as the size of the rent increases attributable to enclosures.

The commentator, Gary Richardson (UC Berkeley), explained that Clark's data, methods, and conclusion that rent increases after enclosure were in fact more modest than historians have previously recognized, are persuasive. He observed, however, that most theoretical models seeking to explain the persistence of open fields implicitly assume that productivity gaps remained small, consistent with Clark's data. He suggested the controversy in Clark's essay was more rhetorical than real, since it depended on overstating the prevailing historical paradigm. Hoffman wondered which institutions are vital to this analysis: local property rights or political institutions? Richardson characterized Clark's argument as minimizing the importance of institutions in driving enclosure. If small differences in transaction costs determine whether a field will be open or enclosed, the legal cost of one form or another might be sufficient to induce change. Such a conclusion suggests that institutions might still have a place in Clark's analysis.

1994 Quantitative Economic History Workshop Meets in Cambridge

The UK Quantitative Economic History Workshop took place as usual in 1994, 9th and 10th September at Peterhouse in Cambridge, but without its accustomed support from the Economic and Social Research Council. The program was organised by Solomos Solomou, and was supported by the Royal Economic Society, the Economic History Society, and Peterhouse. The following papers were presented:

'Bills of Exchange as Money: Money Supply in Catalonia during the Industrialization in the Mid 19th Century': Juan Roses (Pompeu Fabra, Barcelona)

'The Estimation of Yields in Medieval China and India: Revisiting the Problem of Comparability': Shailaja Fennel (Jesus College, Cambridge)

'A Recursive Model of Basic and Applied Research, Innovation and Diffusion: An Application to the Plant Breeding Institute's Wheat Varieties, 1920-1983': Colin Thirtle (Reading)

'The Market for Films in Britain During the Thirties: A Viable Domestic Market for an Indigenous Industry': John Sedgwick (North London)

'Central Bank Behaviour, the Exchange Rate Regime and the Persistence of Inflation in Historical Perspective': Pierre Siklos (Wilfrid Laurier)

'Interests and Ideology in the UK Parliament of 1841-47: An Analysis of Rollcall Voting': Iain MacLean (Nuffield, Oxford)

'Was There a German Transfer Problem in the 1920s? Evidence from a Multi-country Trade Model': Adam Klug (Rutgers)

'Allied Demand for German Exports and European Recovery in the Early 1920s': Theo Balderston (Manchester)

The full Conference Report, by Solomos Solomou (Peterhouse, Cambridge), is published in the January 1995 issue of the European Historical Economics Society *Newsletter* and in the 'Newsletters' Directory of The Cliometric Society fileserver.

Pending success in securing financial support, the next Quantitative Economic History Workshop will be held 8th and 9th September 1995 at St. Catherine's College, Cambridge. Michael Kitson will organise the program.

Please see the Call for Papers on page 33.

On Cliometrics and the 1993 Nobel Award in Economics

by Claudia Goldin, Harvard University

Editors' note: This is the text of a talk given at the Luncheon Honoring Nobel Laureates, Robert Fogel and Douglass North, on January 7 at the 1995 ASSA meetings.

The Royal Swedish Academy, in awarding Robert Fogel and Douglass North the Nobel Prize in Economics, applauded them 'for having renewed research in economic history.' 'They were pioneers', the Academy noted, 'in the branch of economic history that has been called the "new economic history", or cliometrics.'

I remember that October 12th well. I was on leave in D.C. for the year at The Brookings Institution and I was rapidly learning how to craft pithy comments for the press. After the Nobel was announced I was given a chance to use my newly-acquired skill. 'What is this Cliometrics?' was the question of October 12th. By October 13th I had perfected my response: Cliometrics is the application of economic theory and quantitative methods to the study of history. (The less pithy version adds: It is a term that marries the muse of history – Clio – to measurement. It is a neologism coined by Stanley Reiter, a collaborator of two of the first cliometricians, Lance Davis and Jonathan Hughes.)

My pithy answer satisfied the press and other media hounds. But not my economist friends. 'So', they responded, 'cliometrics is just applied economics using old data.' Some suddenly realized they had always been cliometricians. My snappy answer was having the wrong effect. This was not a short-answer question but an essay-length one. The long answer is found in the works and the lives of Bob Fogel and Doug North. For economic history is not simply the use of old data nor the study of old institutions; it is a state of mind in which history serves economics.

Here, then, is the long-answer version. Doug's and Bob's works show how history serves economics:
(1) History is essential because it is risky to base conclusions on transient phenomena.
(2) The histories of the developed world are backdrops, and often provide advice, for current developing countries.
(3) Remnants of the past, which shape the realm of the possible today, are always with us as laws, norms, structures, institutions, and even people. Only the oblivious can ignore history in modern economics and only the unenlightened would choose to do so.

Given the large domain of economic history, it should not be surprising that Doug and Bob are not the first Nobel Prize winners in economics to study and use history. Yet Doug and Bob do not simply join the list of stellar economists who have used history. They are distinctive, because for them economic history is not a handmaiden of economics but a distinct field of scholarship. Economic history was a scholarly discipline long before it became cliometrics. Its practitioners were economists and historians studying the histories of economies. Cliometrics formalized economic history in a manner similar to the injection of mathematical models and statistics into the rest of economics.

The revolution that brought about the new economic history occurred in the early 1960s. As in many such revolutions, the young were pitted against the old. The young turks, who included Bob and Doug at the very outset, formed their own scholarly meetings at Purdue University. These Cliometrics Meetings, as they became known, are still held annually. The New Economic History was able to blossom in the 1960s because its foundations had recently been laid. A stock of quantitative knowledge produced by economists, many at the National Bureau of Economic Research, gave economic historians a clearer sense of when economies grew and what were the sources of growth.

Formalization caused more of an uproar in economic history than elsewhere in economics. The modeling and statistical methods were alien to many practitioners. More importantly the conclusions ran counter to orthodoxy. The most immediate and significant innovation from the formalization of economic history – and one that fueled the controversy – was the 'counterfactual.' The most vivid example comes from the work of Robert Fogel. Bob's now-famous book on the railroads is actually an extended thought-experiment of what the U.S. economy would have looked like had the railroads never been built. Both Doug and Bob focused their early work on transportation advances. That is no coincidence. In the 1950s, a theoretical literature had emerged conjecturing that economic growth could be enhanced by decreased transport costs. Developing economies were advised to increase capital expenditures, particularly in transportation. How decreased transport costs affected the economic growth of the United States – the great success story – was a natural.

Doug and Bob also both examined regional differences in the path to development. Bob's many volumes on the antebellum South revealed how the South grew in the antebellum period, but they were written in large measure to expose a piece of the current American dilemma. And Doug's related and earlier work showed that the South eventually trailed the nation economically because of antebellum institutions. The message, repeated in many of Doug's later works, is that institutions have long lives and that there is no assurance that more efficient institutions will drive out less efficient ones.

Bob and Doug's more recent works have even greater relevance for economic policy and economic theory, ranging from the making of health care policy to the importance of property rights and ideology for Eastern Europe, to the menu of institutions theorists should consider, to the role of childhood nutrition in late-onset chronic disease.

Economic history is not a quaint field, but a highly relevant one. What then is this Cliometrics? Cliometrics is not just the use of historical data or the study of old institutions. It is, instead, a rigorous examination of history as an piece of cloth extending seamlessly from the past to the present.

Robert Fogel and Douglass North continue to be the young turks who gave us this Cliometrics. Bob is the premier empiricist, who establishes a fact and then establishes it over and over again, until he is confident he can persuade the most determined skeptic. Doug is the grand theorist, arbitraging between economic history and organization theory. They were everything Cliometrics needed at the outset – brilliant, dedicated, having the capacity to change, and most of all having the ability to motivate those around them, especially students like myself. I thank them both for enriching my life.

The Cliometric Society Announces

The First Annual Undergraduate Economic History Paper Prize

The prize-winning paper will be published in *The Newsletter of The Cliometric Society.* The author will receive $100 and a complimentary membership.

The goals of this award are to provide undergraduate students with an opportunity to achieve recognition for excellence in research and writing, to enable their work to reach a wider audience, and to encourage economic history teachers to cultivate undergraduate writing and research.

The Selection Committee, Robert Whaples (Wake Forest), Chair, Ann Carlos (Colorado at Boulder), and Paul Hohenberg (RPI) , will judge papers on significance of findings, soundness of method, originality, understanding of existing body of work, clarity of writing, and overall quality. The prize will be announced in mid June.

Rules:

Papers must be nominated by a member of The Cliometric Society.

All types of papers will be accepted, *e.g.*, archival research, statistical analysis, analysis and review of literature. Papers may cover any geographic area and any topic, as long as the primary focus is economic history.

Papers must be written by a student who was an undergraduate during the current academic year. 'Undergraduates' are defined as students in the first degree program of their higher education, *e.g.*, US Bachelor's Degree. There are no age restrictions.

Papers must be submitted by e-mail or on disk, in Microsoft Word, Word Perfect or other commercial word-processing program. They must be *one* document, with graphs, charts, tables, *etc.*, embedded in the text. Maximum length is 5000 words, with an additional maximum of 5 pages of graphics. Papers must be in English.

Author's name and address, nominating instructor, institution, and course title must appear only at the beginning of the document.

Papers must be received by **May 19, 1995.**

The Cliometric Society
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*Submission of a paper is a grant permitting The Cliometric Society to publish the work in the Society's **Newsletter** and in the Society's fileserver. Runners-up may be invited to submit abstracts of their papers for publication.*

Meyer Interview *(continued from page 6)*

Well, we were at least as interested in influencing economists as economic historians. In terms of convincing economists, we felt it behooves them to be aware of what economic historians are doing. I suspect that to the extent that we were preaching to economic historians, it was to suggest they spend a little more time with economics. Everyone had something to learn from each other.

And at the point when you entered the discussion the learning wasn't taking place?

Gerschenkron, Kuznets, and Rostow were certainly major figures and they all emphasized what economists could learn from history. Much of the research program at the National Bureau of Economic Research, at that time and perhaps for two decades previously, had such an emphasis, going back to [Wesley] Mitchell.

Are we talking about different generations here? Rostow was a bridge from an earlier generation . . .

Well, they were all much younger then, especially in 1955 and 1956 when this work was done.

At what point did the interest among economists in the kind of empirical work carried out by Mitchell, collecting and then analyzing time series, really die out?

Well, I suppose that Koopmans's rather devastating review of Burns and Mitchell was one watershed. That certainly pushed back the profession's acceptance of and interest in traditional National Bureau time series analysis. On the other hand, I don't think it killed it entirely.

Has historical economic analysis maintained its position at the NBER after the key role it played during the early years? Or has its role diminished?

Perhaps in relative terms, but I'm not so sure in absolute terms. The whole scale of the NBER program has expanded quite sharply over the years. Of course the Bureau is also organized a bit differently now. Much more of its work is done in liaison or association with outside researchers and less is done exclusively as an in-house activity, so we also have to be very careful what we are measuring. When we look at the whole extended community involved in National Bureau work, my guess is that while the relative portion of historical work may have gone down, the absolute portion probably hasn't fallen that much, if at all.

So there is long-term historical continuity at the NBER?

A lot will depend again on classification. What's quantitative economic history? As I've argued earlier, much of almost anything that's empirical in economics has an historical dimension to it.

What kind of response did you get from colleagues after publishing the *JPE* piece?

Most economists were reasonably receptive. After all, we were engaging in a sort of economic imperialism for the field. Trying to extend its boundaries.

Did the project you participated in at its beginning bear the kind of fruit you would have hoped it would?

Oh, that's for others to judge. My guess would be that economists' interest in economic history bottomed out a few years ago and is beginning to go up again; that is, the interest in the profession in what you might call the more historical side of the field.

What might have prompted the turnaround?

I think that there has been an increased interest, say in the last 10 years, in practical policy problems and that's led in turn to some increased interest in the historical aspects of the field. The re-emergence of interest in the determinants of growth, and the questions of why some societies grow more rapidly than others, also plays a major role.

Although you don't have much professional contact with the economic history group, does the experience at Harvard suggest to you that economic history is carrying out its part of the bargain, stressing the understanding what lies behind *ceteris paribus*?

Yes. Economic history has continued to evolve. I find that some of the work that Doug North and others are doing under the heading of institutional history quite fascinating.

You've suggested that the revival of interest in economic history in the economics profession may arise from a strengthening of interest in policy issues. The young economist does not want to feel completely isolated from policy questions. What about the direct role of economic history? How can economic history inform government policy?

About the only recent experience I've had that might be relevant to answering that question is that I have been involved in some of the reform activities in Eastern Europe and the former Soviet Union. In that context, I have found Doug North's new institutional history fascinating. I understand that this interest was developed (or redeveloped as the case may be) by him in response to acting as an advisor in Eastern Europe on market transition problems. We in the West simply assume the existence of market institutions, financial institutions, legal protections for private property, bankruptcy laws, incorporation, so forth and so on. All these institutional arrangements that we take for granted have to be recreated in Eastern Europe and especially in the former Soviet Union. In the Russian case, these institutions may have been underdeveloped even prior to the revolution, in Czarist days. Of course, they are essential for developing a market economy.

The lesson is that these institutions are critical for economic development and a necessary condition for economic growth.

Yes, and I think it is significant that a good economic historian, Doug North, took the lead in identifying the problem.

In particular, your work has been in Russia itself?

Yes, Russia and the new states of Kazakhstan, Belarus, Ukraine. The work has been done for the European Bank for Reconstruction and Development.

I suppose that it's pretty clear that dusting off the laws from 1916 or 1917 is not enough to create these kinds of institutions . . .

They usually need a bit of updating! Also, people don't have experience with markets and market institutions. One reason why the market transition is somewhat easier in Eastern Europe is that predecessor commercial law is not as antiquated and not quite so many generations have gone by without any experience of modern market institutions.

Then, aside from North's work and the need to recognize the importance of an institutional structure of property rights, are there any other places where history may have something to say? Take, for example, economic history's thinking about the role of transport innovation. Consider the conclusions of Robert Fogel's work. Does that really have much resonance for you in terms of addressing current questions of transport policy or infrastructural concerns?

I really don't know. Very recently, though, Professor Gómez-Ibáñez, one of my colleagues here at Harvard, and I have been working on privatization issues in different parts of the world. One of the areas we have looked at has been highway privatization, and one of the fascinating things we discovered early on was that the U.S. had a quite extensive program of private highway development, or turnpike development, back in the first part of the 19th century. They were mostly privately developed. The supplementary public investment pattern in the U.S. in the early 19th century was much like that experienced with highway development in Spain and France in recent decades. The public role in all three cases became that of filling in where the private sector didn't, thereby completing the system. It worked remarkably well in the early 19th century U.S., and the only reason it came to an end was because, of course, of the emergence of the railroad. The steam engine was apparently better designed for providing locomotion on rivers, waterways, or rails than on roads. Of course, an interest in roads re-emerged at the end of the century when the internal combustion engine appeared, apparently better adapted to highway modes. While we didn't put much of this historical background into our book (it was a matter of space), our understanding of more recent events was enhanced by reading different historical accounts of the turnpike era. Actually, the early development of the public roads movement at the end of the 19th century, and first part of the 20th, is also quite fascinating and helpful in understanding today's highway development problems and prospects for privatization as an alternative.

Are countries that still need to construct a basic road network or develop highway infrastructure open to these kinds of historical examples? Or is there a presumption that the public sector should be providing these services?

No, there has been a diversity of responses in third world and other countries. Mexico has relied very heavily on private investment for development of its high performance highway network. Malaysia and Indonesia have relied less, but still have used the private sector. France and Spain used private investment for highway development in the 1970s and 1980s. The explanation of why privatization occurs some places rather than others is best attacked by taking a broad historical view, and understanding the general economic environment at the particular time highway development takes place. For example, one of the fascinating questions is why the high performance highway development that occurred in California was financed by gasoline taxes, and called 'freeways'. In the Eastern U.S., by contrast, more was done with public toll authorities and the highways were called turnpikes, parkways, or expressways. Some interesting cultural and historical explanations might be advanced to explain these differences.

Another illustration of historical issues in transport research is provided by airline de-regulation, that of simply dating when de-regulation began. The natural instinct is to cite October, 1978, when the legislation was enacted. On the other hand, a good argument could be made that it started three or four years earlier, when John Robson was appointed chairman of the Civil Aeronautics Board by President Ford. Robson was replaced during the Carter administration by Fred Kahn, and Kahn continued, indeed accelerated, the process of deregulation that Robson had initiated. And so, how do you set up your historical comparisons, the before and after?

Is the recent emphasis on the role of infrastructure in productivity growth, by some policy-makers and economists such as David Aschauer, overselling its importance? Or, is the degree to which the U.S. infrastructure has depreciated having an adverse effect on American productivity? A believer in Fogel would be skeptical of those kinds of claims.

The truth is probably somewhere in between. You have an abundance of literature on the subject, including some very good contributions by Dale Jorgenson. Jorgenson is quite critical of the more expansive claims made for the productivity improvements, or enhancements, attributable to infrastructural investments. You really have two camps emerging, one is the macro or heavy investment solution to any infrastructure deficiencies, and the other is the micro 'let us price and manage what we have better' approach. I must admit that I have more sympathy with the latter. Nevertheless, there probably are some cases where a major investment may be needed. But I believe that there are many more cases where the problem can be solved simply, with better management and pricing of facilities. Airports are a striking example of this. We had some experimentation here in Boston at Logan Airport a few years ago with pricing solutions and they worked remarkably well. But for technical reasons, the FAA disapproved, so now the Massport [airport] authority is thinking about alternative pricing solutions, hopefully acceptable to the FAA.

You were acting editor of the *Explorations in Entrepreneurial History* in 1957, and during your editorship papers by scholars such as William Parker and a new Ph.D. named Lance Davis were published. They were highly quantitative when compared with papers that appeared earlier or later in the 1950s. What kind of audience did you have an interest in reaching? Do you feel that you were successful?†

I guess so. Much depends upon how you define 'reach'. I did have an interest in providing an outlet for what I saw as the highly interesting quantitative work that was emerging from Gerschenkron's workshop and from other sources. Probably, though, the reason for taking the chore on had as much to do with personal relationships with Professors [Arthur] Cole and Gerschenkron as it did with any well-conceived notions of what the strategy should have been for the journal.

Do you believe that *Explorations* was helpful in opening up the overall discussion?

Well, I guess I would like to claim at least a little bit of a contribution. Of course, shortly thereafter, the more conventional and established economic history journals began to be more receptive to quantitative papers. So whatever the original motivation might have been, I think it disappeared fairly quickly.

The notion of entrepreneurship occupies a big chunk of the literature on late 19th-century British economic history and has reappeared in discussions of the potential for restructuring Eastern Europe and the former Soviet Union. Can we identify entrepreneurship? How much do we know about it? When does it make a difference in economic development?

I have two comments. First, I tended to be very skeptical of the importance of any intangible, such as entrepreneurship, when I was young. I even had the temerity (and the foolishness) to commit some of that skepticism to paper. As I have aged, I've become more and more convinced that I was probably somewhat misguided if not wrong in

†The first series of *Explorations in Entrepreneurial History* was published by the Research Center in Entrepreneurial History at Harvard from 1949 to 1958. The second series was initiated in 1963 and has since been published continuously, with the revised title, *Explorations in Economic History*, effective since 1969.

my early skepticism. What has brought that home to me recently is spending quite a bit of time in both China and Russia, worrying about development problems. There's not much doubt that the Chinese culture, both in China and overseas, produces disproportionate numbers of quite effective entrepreneurs.

But can we also identify entrepreneurship?

I suppose my reaction is like the old saying (about pornography), that 'I don't know how to define it but I do know it when I see it.' Entrepreneurship may come under that same sort of ephemeral classification. Some of the historical literature certainly helps, at least me, to understand better the dimensions and definitions of entrepreneurship. Historians have made a contribution there. I wish they had made more, but I'm sure they will.

The cliometric school made an attempt at deriving implications of entrepreneurial failure or 'operationalizing' it. Tests of it turned to narrower discussions of whether firms were responding to relative factor prices. The discussion never truly returned to the larger question of what an entrepreneurial culture is.

I don't think that's all bad. I attempted something like this, with my early paper on British economic development in the last part of the 19th century. As I already indicated, I'm not sure that some of that wasn't a bit misguided. On the other hand, I think attempts to quantify entrepreneurship, to identify manifestations of it – basically what the cliometricians have done – are not mutually exclusive with the older, more qualitative approach. Another attractive development (again, I'm viewing this from afar) seems to me to be development of a new social history; that should reinvigorate some of the broader studies and attempts to conceptualize entrepreneurship.

Is entrepreneurship something that can be developed? Or does it depend strictly upon culture?

That's a very good question. I don't know the answer. We're beginning to get some interesting insights from the development literature, which is doing these comparative studies of entrepreneurial successes and failures, and national successes and failures, with economic development. Of course, the Pacific rim countries have been a particular focus for such studies, *e.g.* the World Bank's special survey completed about a year and a half ago. Recently, some of the people at the Harvard Institute for International Development (Mike Roemer and David Lindauer) published a very interesting study comparing the development experiences of several Southeast Asian countries. They get into the contributions of Chinese entrepreneurs, since in all the countries studied (Malaysia, Singapore, Indonesia) these entrepreneurs make major contributions. The obvious next question is trying to identify what it might be in Chinese culture that produces these people. And of course Chinese culture is not the only place where such skills are produced. Many well-informed students of development would argue that Indians, when given an opportunity and government policies that don't interfere too extensively, are also quite capable of entrepreneurial spurts. It's too bad that we don't have Schumpeter to sort it all out for us. It would be interesting to have him explain why he got the relationship between capitalism and the entrepreneurial spirit right the first time, but not the second time. That would be even more fascinating.

That's one area where we have a lot to learn from historians. We are being pushed back to fundamental questions: What prompts economic development? What accounts for successful spurts of development?

Yes, and I think that is helping revitalize the field. The new institutional and social history may eventually give us some good insights, but they're just beginning. We haven't seen their full fruition yet. Perhaps we're a bit impatient!

Let me thank you for generosity with your time. I've enjoyed our discussion.

Glad you got off on transportation, and also that you didn't ask me who was right, Fishlow or Fogel.

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Call For Papers and Dissertations

The Second General Conference of the European Historical Economics Society will be held in Venice, Italy, on 19-20 January 1996. The Conference theme is 'Issues in European Economic History.' It is expected that expenses will be paid to authors presenting papers. Please submit a two-page abstract. If accepted, the full text should be sent by 31 October 1995.

The Gino Luzzatto Prize of $1,000 will be awarded for the Best Dissertation on European Economic History, completed after 1 January 1994 at a university in or outside Europe.

Please send abstracts of papers or abstracts of dissertations before **30 April 1995** to

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ASSA Report *(continued from page 1)*

can lead both to higher utility and lower 'quality' food consumption (and hence physical growth). Commentator Pritchett questioned the validity of generalizing from convicts' heights to the population as a whole, and Ferrie stressed the likelihood of selection bias. Komlos accepted the possibility of bias, but pointed to yet other data showing a decline in heights starting *c.* 1830. Coclanis noted that there are no obvious changes in the penal system over these years before 1865 and, because prisoners served short sentences for such crimes as vagrancy, forgery, horse theft and arson, they likely represent a reasonable cross-section. Questions arose about data on military personnel, weights, regional comparisons, and racial differences. Ferrie wondered whether Georgia price data were used; the authors responded that prices were national, and that occasioned comment. Williamson observed that farm prices reflect the prices of goods sold, not the cost of food to the farmers. Questions then arose about the distinction between height/food consumption and standard of living/utility.

In the session's final paper, Simone Wegge (Northwestern) presented her study of border-crossing Hessians, 1832-1857. The data allow her to determine characteristics of migrants and to compare them with non-migrants. Using wealth data, she considers who could afford to migrate and who benefited from migration, and infers from village-level data that the middle-class had a greater propensity to migrate. The rich had no incentive, and the poor couldn't afford it.

Discussant Pritchett said her research was a valuable contribution to empirical migration studies, especially for its stress on the country of departure rather than the host country. He pointed to the effects on migration of credit market imperfections, noting that with rising incomes and falling transportation costs, indentured servitude would decline. Wegge had organized her study by village, which Pritchett found appropriate, but he also asked that she use data on individuals. Although in the sample the middle class was over-represented, and the lower and upper classes were under-represented among migrants, he was nonetheless persuaded that Wegge's results are robust. He recommended closer study of time trends of occupations of migrants and of the age and occupational structures of migrants and non-migrants.

Ferrie asked if exclusion of 1848-49 would alter the conclusion that the middle class dominated the migrants. Wegge responded that data for 1848-49 are bad, with 1852 and 1857 data more useful. Various persons commented on how Wegge might account for the political turmoil of the late 1840s. Wegge readily responded to questions about definitions of day workers, stated destinations, time spans for the data, and remittances from earlier migrants. Williamson asked about port of exit and about whether information on transportation costs was readily available. The session ended with discussion still active.

Friday, 6 January 1995, 2:30 pm: 'American Monetary and Financial History', co-sponsored with the American Economic Association, was chaired by Ben McCallum (Carnegie Mellon) and played to a standing-room-only audience. The first paper, by Warren Weber and Arthur Rolnick (FRB, Minneapolis) and Bruce Smith (Cornell), presented by Rolnick, views the antebellum United States as a multiple-issuer monetary union, with circulating currencies of gold and silver minted by the Federal government and bank notes issued by state-chartered banks. The system, however, was not very successful in maintaining fixed exchange rates among currencies. They conclude, therefore, that US history implies a uniform currency (*i.e.*, one with fixed exchange rates) does not emerge from a private issue system; some form of government intervention is required. Discussant Peter Ireland (FRB, Richmond) disputed the authors' implication that monetary union required a strong central bank, one more powerful than the First or Second Banks of the United States; he offered the counterexample of the War of 1812 period, during which large volumes of Treasury notes were issued.

Charles Calomiris (Illinois) argued that whether bank notes circulate at a discount depends on whether there is a branch or unit banking system, or on whether there is a regional currency (*e.g.*, the Suffolk system) with multiple redemption points. He disputed the authors' argument that monetary union with privately-issued money is impossible. Weber replied that Calomiris seemed to support their general point that redemption costs must be imposed on the issuer for notes to circulate at par, as in branch banking or the Suffolk system. Although a private market system might emerge to reduce such costs, a central bank appeared to be a more feasible option. Rolnick added that intervention was necessary, but not necessarily government intervention. It was then asked, if fixed exchange rates were so important, why hadn't Bank of the United States notes, which were convertible all over the country, driven state bank notes out of circulation? Calomiris responded that since the Bank's capital was limited, its notes could not have displaced others completely.

The second paper, by Tim Guinnane (Yale), Harvey Rosen (Princeton) and Kristen Willard (Columbia), examined movements in the gold price of greenbacks to determine what contemporaries had considered the crucial episodes of the Civil War. Modeling the greenback price as resulting from an autoregressive process, they identify structural breaks in series of rolling windows by sequential F-tests. Contemporaries and historians alike regarded such major events as the Battles of Gettysburg and Vicksburg to be important, but historically minor events (*e.g.*, Jubal Early's retreat from Washington in July 1864) appear to have been even more important to contemporaries.

The discussant, Richard Grossman (Wesleyan), found the paper's approach quite sensible, but he had reservations about econometric techniques. Is the size of the window important to identifying the break dates? Willard replied that windows from 20 to 200 days were tried and 40 to 60 days gave the most satisfactory results. Grossman was puzzled by the authors' surprise at the importance of the decision to increase the supply of greenbacks and suggested more attention be paid to how the decision was made. Calomiris argued that the structural break of March 1865, coming when nothing particularly noteworthy had occurred, illustrates the pitfalls of the econometric assumptions. If there had been noise in the trading data so that, on some days, there were big price movements, then, as people realized the South was going to lose, a mere blip might be identified as a structural shift. Clark Nardinelli (UMBC) suggested trying the exercise in reverse, choosing dates on which big things happened. If the test fails, why? Had there been countervailing events?

The third paper, by Randy Kroszner and Raghuram Rajan (Chicago), examines the underwriting of securities by commercial banks in the 1920s, before investment and commercial banking activities were legally divorced by the Glass-Steagall Act. What, if any, institutional forms developed in an unregulated market to deal with asymmetric information? They find a strong movement from internal underwriting to external agencies or affiliates. Issues by internal securities departments carried higher yields and lower ratings than those underwritten by separately incorporated and capitalized affiliates, reflecting concerns about conflict of interest. A 'firewall', a formal separation of underwriting from commercial banking, appeared the superior institutional structure.

Grossman was sympathetic to the authors' conclusion that observed yield differences between issues underwritten 'internally' were more consistent with the 'rational discount hypothesis' (internal underwriting would increase uncertainty and lead to higher yields) than with the 'informed certification hypothesis' (internal departments are better informed about the qualities of securities they underwrite). He was dubious about how one could distinguish clearly between 'departments' and 'affiliates.' Calomiris added it was difficult to tell the difference because they were usually wholly owned by the same people, and suggested that the choice of affiliate *versus* department should be modeled explicitly, since banks choosing to establish affiliates might have been different from others. Branch banking, for example, could have made a difference. Kroszner replied that there was a gradation from solely investment banks to affiliates to internal departments. Affiliates and departments were not the same thing; the balance sheet of an affiliate, for example, was reported separately, and larger banks were more likely to set up separate affiliates.

The final paper, by Charles Calomiris and Joseph Mason (Illinois), took up the question of whether banks had failed during the Great Depression because they experienced common exogenous declines in asset values or because contagions swept away even fundamentally sound banks. They analyzed the characteristics of Chicago banks during the panic of June 1932 to determine whether the failures and the survivors looked different *ex ante*. Failures appear to have been the weakest on fundamental grounds; there seems to be no evidence of contagion, no confusion about which banks were weak.

Grossman, the yeoman discussant, observed that, if contagion were a major problem, deposit insurance becomes quite important; if not, then panics pose no more problems than do individual failures. He liked the logit and survivor regressions in the paper, but would have preferred a more formal test for structural change. He also put a somewhat different spin on the conclusion: perhaps there had been contagion, but it was short-lived owing to prompt cooperation. Calomiris agreed there was contagion, but contagion hadn't led to bank failures. The social cost of the panic, therefore, wasn't that great. When asked why failure rates jumped so much in June 1932 if contagion wasn't important, Calomiris responded that there are two separate questions: First, could people distinguish whether banks were risky or not, and second, how much of the decline in asset value reflects the panic? They address only the first question. Banks may have failed because of the panic, but they did not fail because of confusion about the riskiness of individual banks.

Saturday, 7 January, 2:30 pm: The session on 'Institutional and Technological Change', constrained by a five-minute limit for presenters imposed by chair Sam Williamson, began with Leonard Carlson (Emory) quickly presenting his argument that early 20th-century US government irrigation projects on Indian reservations represent a 'Panda's thumb' solution: existing institutions adapted inelegantly, but serviceably, to provide special institutions and laws. After reviewing the relevant federal legislation and the interest groups concerned with irrigation projects, Carlson concluded that a model of a wastage factor, overlaid with interest groups competing for benefits, best fitted the data on Indian irrigation projects revealed in a 1927 survey.

Discussant James Eaton (Bridgewater) thought Carlson's model useful in understanding why special interests operating in a public choice framework don't generate efficient institutions. After a short commentary on the relevance of biological evolutionary theory to institutions, Eaton asked about the homogeneity, alternatives, and words of the Indians. Although the material on tribal government didn't seem to fit well with the rest of the paper, Eaton did find the Panda's thumb explanation of institutions useful. Carlson responded to a question about how white farmers suffered as a result of path dependent institutions by pointing to taxpayer-funded waterless-irrigation systems in Montana. Cynthia Taft Morris (Smith) wondered if Carlson considered only efficiency goals, ignoring redistributional benefits. Carlson replied that efficient projects could have delivered more to the Indians. Richard Sutch (Berkeley) wondered about the value of tax dollars to Indians confined to reservations. Wayne Grove (Syracuse) opined that Indians on reserves might desire an influx of whites to generate jobs. Carlson responded that, if a system had been built from scratch, surely the officials in charge could have devised a better one. He also noted that the more hostile tribes were more likely to get projects. Discussion closed with Sutch arguing that this is a bad test of the Panda's thumb approach to institutions because inefficiency wasn't a concern here. Carlson responded that the Panda's thumb argument applies at the margin; the thumb gets the job done, however awkwardly.

Wayne Grove then reported on his examination of data specifying the number and type of mechanical harvesters owned in each Texas county, 1948-61, to test hypotheses about the technological choices of cotton farmers. He considers factors impeding mechanization as well as those which favored stripping over picking machines. His paper focuses on how changes in the relative prices of inputs drove the adoption of the mechanical harvester in preference to hand picking.

Discussant Alan Dye (Michigan) directed his comments primarily to Grove's work on the choice among alternative mechanical harvesters, rather than the decision to mechanize. This, he argued, was the paper's novel use of the data. Dye neatly summarized the technical details of competing technologies, and clarified how these drive the ultimate choice between the stripper (with its lower fixed and variable costs, but higher downstream cotton cleaning costs) and the mechanical picker. Dye suggested that Grove's regressions might not be fully specified and recommended that the diffusion process be modeled explicitly. He found the paper promising and urged continued work on it. Sutch asked whether land costs matter, especially if mechanical picking limits the number of annual harvests, and Williamson asked whether state data are used. Grove responded that observations on types of harvester were at the county level; those on wages and percentage of cotton mechanically harvested, at the state level. In responding to Sutch's question about cotton plant hybridization, Grove noted the relevant government programs, but replied to Carlson that Texas did not use the one-seed varieties of cotton introduced in California.

Cynthia Taft Morris, protesting what she considered to be the historically-unprecedented time limits for authors, presented her paper, 'The Secret is in the Brew', on why no single quantitative approach adequately explains institutional performance. In the style of the Gettysburg address (which also takes less than five minutes to deliver), she argued that traditional economic models must be supplemented with chance as well as socio-political structures. She circulated a complex diagram illustrating the feedback effects among these variables and pointed to the contrast between Spanish and British settler economies. Her final comments, again Lincolnesque, stressed the importance of egalitarian distribution to economic growth.

The discussant, William Sundstrom (Santa Clara), observed that, in brewing, simpler is better, and he wondered if the same applied to modeling economic growth. A main proposition seems to be that economic growth cannot be evaluated without considering distributional effects; he wondered how to compare low income, egalitarian outcomes with higher income, less egalitarian outcomes. He wondered also if the Latin American settler colonies were less egalitarian or simply poorer than the British. In evaluating Morris's claim that we should use more than one methodology to evaluate economic growth, he thought that her real interest was in examining the role of institutions in a non-neoclassical way. He pointed out that institutional differences also might stem from economic differences, and concluded that, whereas Morris might argue that countries were initially close and institutions and feedbacks generated large differences, he believed it more likely that initial differences among countries were more important than feedback dynamics. Several comments followed, comparing Costa Rica, Argentina, and Canada, with efforts to establish whether differences among these countries stemmed from initial conditions or dynamics. Sutch suggested examining French Canada because it inherited a different set of institutions. Morris found this appealing, and reiterated her conviction that distribution and growth are intertwined.

Moving from the very big picture to a much smaller one, Va Nee Van Vleck (Grinnell) considered why British coal waggons of the early 19th century look so toy-like compared to their US counterparts. Rejecting the hypothesis that the British stuck with obsolete technology, Van Vleck argued instead that coal buyers, who owned the cars in contrast to railroad ownership in the US, took into account the inventory costs of storing coal and pursued a cost minimization approach. To explore her argument, she simulated total inventory costs in Britain and the US and concluded that the British, with multiple kinds of coal, multiple sites, and multiple destinations preferred a rudimentary 'just-in-time' delivery system.

Discussant Dye urged the author to consider that much of British infrastructure was built to accommodate small waggons and could not adapt costlessly to larger ones. While finding Van Vleck's thoughtful approach appealing, he asked for more discussion of the technological possibilities open to coal buyers, the limits to coal storage in London, the exogeneity of the number of delivery sites, and the different final demands for coal. Van Vleck responded to an analogy with the modern UPS delivery system by noting that breaking bulk for coal deliveries was very expensive. Sundstrom noted that what was argued as cause may be effect. Sutch asked what is endogenous and what exogenous, commenting that the analysis struck him as too partial. Van Vleck responded that storage costs change over time as coal buyers move outside cities and car sizes change. Citing the Coase theorem, Carlson asked whether car ownership really matters. Van Vleck agreed that ownership wasn't that important; when users approached the railroad with proposals to build 20-ton cars, the railroads refused. Williamson called time while discussion was still lively; participants continued informally.

Sunday, 8 January 1995, 8:00 am: The first-ever joint Clio-American Finance Association session began at the unbankerly hour of 8:00 AM under the iron hand of Lance Davis (Cal Tech). The first paper, by Larry Neal (Illinois), considers the structure of the London Stock Exchange in the 18th and early 19th centuries, pointing to several peculiar institutional features of the Exchange, all of which had historical roots. First, there was a distinction between proprietors and members; the former set the terms of use of the facility by the latter. In turn, the members were divided between jobbers (who acted as principals in their trades) and brokers (who acted strictly as agents). In the early 19th century the London Stock Exchange emerged as a market for large issues of government debt. While the rise of the canal companies had been quite independent of the Stock Exchange, it later became important for financing the railways.

The second paper, by Eugene White (Rutgers), examines the history of the Paris Bourse from its founding in 1724 to 1815. White views this period as a natural experiment in determining how changes in rules affect securities markets. From 1770 to 1791, there were limited entry and small issues, a fixed number of brokers, and a requirement that stocks and bonds be traded only through brokers. The Revolution, however, broke down barriers to entry, allowing free entry into the Bourse with the payment of a fixed fee, and the number of brokers more than doubled. Under Napoleon (1802-1815), brokers were limited to 60, but issues were large. When there was free entry with small issues, the jumpiness of the price (the change in price between transactions) was small. After 1798, with free entry but large issues, there was great price variation between trades. Later, with limited entry and large issues, price changes were damped. White is puzzled by this pattern. Perhaps the market-making activities of banks had been destroyed in the Revolution; after that, they acted only as agents; later still, the influence of banking houses returned and market-making powers revived.

The third paper, by Richard Sylla (NYU), considers the evolution of the New York Stock Exchange over the 'long' 19th century. He distinguishes three periods: the years after Independence, the Civil War, and the Rise of

Big Business. Changing demands on the Exchange produced structural changes in response. In the first period the increase in public debt led to the Buttonwood Agreement (1792) to form a private club for securities trading. Recognizing that information was valuable, the New York Stock Exchange supplied it fully to members, but to the public (newspapers) only once a week. The Civil War was another shock. The large increase in debt and the increase in trading led to the development of rival boards with lower commissions. In 1868 seats on the Exchange were made salable, and the rival boards were absorbed. The growth of trading volume also led to continuous trading. Finally, with the rise of big business at the end of the 19th century, competition from rival exchanges forced the New York Stock Exchange to start trading industrials, even though their heart wasn't in it.

Davis, who doubled as first discussant, focused on the London Stock Exchange in the late 19th century, in contrast to Neal's focus on an earlier period. The Exchange had been a British market in the first half of the 19th century, but it had become international by the second half. The distinction between owners of the exchange and traders on the exchange, each with different goals, nevertheless persisted. In Britain, those who ran the exchange placed virtually no constraints on membership, while in New York the objective was to maximize the value of a seat on the exchange. Davis liked White's paper, but wondered how much of his 'funny' result was due to the disappearance of banks during the Revolution. How much time did it take for a new broker to learn how to do his job when the number of brokers was rising? Sylla replied that the New York Stock Exchange limited both membership and the range of shares traded. It didn't like new securities from new industries because there wasn't enough volume. Robert Cull (US Department of Commerce and UCLA) offered an alternative explanation, that the Exchange established high standards and strict rules to resolve information asymmetries and to reassure potential investors. It wanted to signal high quality issues rather than resist others as unprofitable.

Comments by Robert Schwartz (NYU), who was unable to attend, were read by Sylla. Schwartz pointed to the universality of the issues raised by these studies, focusing on regulatory, competitive, and cultural influences on the evolution of stock markets. Three forces accounted for change: competition, regulation, and technology/innovation. Among the significant issues were the location of the market within a country, the relation of the secondary market to the primary new-issue market, the concentration of banking and brokerage, the uses of futures and options, and membership provisions. The historical distinction between stocks and bonds was attributed to the deductibility of interest in 20th-century America; otherwise everything was called stocks without distinction. Ann Carlos (Colorado), in reply, noted that joint stock records distinguished clearly between stocks and bonds in account books from an early date.

Sunday, 8 January 1995, 10:15 am: The closing Clio session, 'The Rise and Fall of Bimetallism', was chaired by Ron Michener (Virginia). Stephen Quinn (William and Mary) opened with his examination of the origins of bimetallism in England at the end of the 17th century, focusing on arbitrage among bills of exchange, gold, and silver. Bills proved to be central in accomplishing the export of silver and the transfer of funds to the Continent to finance the Nine Years' War (1689-1697). Quinn uses the records of London goldsmith Stephen Evance to illustrate arbitrage and war finance. Evance swapped silver for bills in the Low Countries; in turn, the bills were exchanged with the British government, which needed them to make payments in return for government debt.

The discussant, Warren Weber, to clarify the arbitrage process, first listed the relevant price relatives: the mint equivalent, the mint price, the mint ratio, the market price, and the exchange rate for bills, and then worked through a representative transaction. He liked the evidence for British goldsmiths, but had some doubts about whether Evance's activities really fit the paper's predictions. Peter Garber (Brown) asked what role the Bank of England played. Quinn replied that it was involved in the remittance program and took a large loss. Michener summarized the argument as follows: the war increased the need for remittances to the Continent, driving the exchange rate up. This was accomplished by gold coins being devalued and silver coins being clipped. However, England had been in lots of wars before without currency derangements. What was different in this one? There seemed to be a chicken and egg problem here: was clipping a response to rises in the bullion price and the exchange rate (Quinn's story), or did clipping cause the exchange rate to increase? Quinn responded that the magnitude of this war's financial demands were unprecedented.

The second paper, by Stefan Oppers (Michigan), is motivated by Milton Friedman's writings on bimetallism, which dispute the view that a bimetallic standard really fluctuates between a monometallic gold and a monometallic silver standard. To what extent did bimetallic arbitrage contribute to the stability of the system? How quickly and effectively did Gresham's Law work? If quickly, a small premium on one of the metals would move the country rapidly to a monometallic system; however, if slowly, bimetallic arbitrage could provide stability to the market ratio for long periods. Oppers uses exchange data to estimate 'arbitrage potential', applying his model to France in the 1840s and 1850s, when large increases in world gold output created bimetallic arbitrage possibilities. He estimated that no arbitrage potential remained in France by the late 1850s. Either no silver coins were left in France, or they circulated at a premium. Gresham's Law thus appeared to work quite quickly and effectively in removing undervalued coins from circulation.

Michener saw the paper as arguing that France should have served as a buffer, since it was so large monetarily, absorbing gold after gold strikes and releasing silver, a role which effectively ended by 1857-59. Import-export statistics, however, show that France was still exporting silver after 1857-59, when, according to Oppers's regression, silver had been depleted. This led Michener to doubt the regression of the pound-franc exchange rate on the price of silver in London. Since the difference was small in absolute terms, small discrepancies could be important. Second, Michener suggested that the price of standard British silver was inappropriate. Using the wrong silver price results in an errors-in-variables problem, so the estimated coefficient may be biased.

Oppers replied that he had distinguished between the stock of coin on the one hand and arbitrage potential on the other. Garber suggested that if the export-import statistics came from customs figures, lots of silver flows may have been missed. Michener responded that, even if there is a margin of error, in France in the mid-1850s there were still large gold imports and silver exports. Oppers interjected that the controversy was not about whether there was any silver in France, but about whether there was any arbitrage potential. Marc Flandreau (CNRS) observed that it was important to keep relative arbitrage costs in mind. The Paris and London markets were more closely linked than many markets within France itself. What information did the Paris-London exchange rate give? At a given time the exchange rate reflected the need to clear balances, but didn't necessarily reflect the structural composition of the money supply.

The third paper, by Marc Flandreau, considers the emergence of the classical gold standard in the 1870s, examining and rejecting four theories of the collapse of bimetallism. First, it was due to increased silver production (*e.g.*, Comstock lode). Second, there was a free rider problem; when Germany moved to gold and started selling silver, other nations followed. Third, as transactions costs fell, since gold was less bulky, it became the better means of international payment. Fourth, gold became identified with the rising bourgeoisie, while silver was the metal of debtors. Flandreau's story focused instead on political rivalry. Before 1870 both France and Germany thought the costs of moving to gold were too high, but after the Franco-Prussian War Germany got an indemnity, felt richer, and switched to gold. The French, however, did not cooperate in the disposal of Germany's silver stock, and central banks were forced to step in and peg the value of circulating silver coins in terms of gold.

The discussant, Peter Garber, saw in the paper the seeds of a 'beautiful' controversy between someone defending the 'fundamental' view (increasing silver supply led to the collapse of bimetallism) against Flandreau's view (the war created an environment in which the system collapsed). Garber remained officially neutral, but expressed a preference for the 'fundamental' story. On the other hand, he was attracted to the view that wars cause fundamental changes in monetary systems. Did France play a role in stabilizing the world monetary system? If so, it could have absorbed a lot of silver. If not, France could not have absorbed much silver, and the fundamental story would hold: the system collapsed when the price of silver began to fall. Oppers's paper suggested France was small, but Flandreau's calculations show that it was big, and that the bimetallic system could have continued, based on normal arbitrage, had it not been for the political factors.

Michener pointed out that there were other countries tied to France in the Latin Monetary Union, so that the proper question was not whether France alone was large enough, but whether the Union as a whole was big enough. Flandreau responded that the rest of the Latin Union was free riding on France, so French specie estimates were really Latin Union estimates. Weber asked whether the proper question is 'What is an optimal national monetary standard?' Michener added that Friedman seemed to imply the whole world was aided by France's stabilizing bimetallic system. Should Flandreau's paper ask whether France's switch to gold was optimal or a mistake? Neal, in conclusion, recalled an earlier literature about India and China acting as buffers – stabilizers with massive capacities to absorb and release silver.

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unlimited liability, short time horizon, and vulnerability to a partner's opportunistic behavior. Despite these shortcomings, partnerships were ubiquitous in 19th-century Boston, and she concluded that the partnerships' benefits (allowing partners to avoid relationships of dependence with employers or benefactors) must have been quite valuable in the early republic.

Most questions dealt with Lamoreaux's sampling strategy. Both Daniel Raff (Columbia) and Sutch suggested that the first part of the alphabet (the part of the directory sampled by Lamoreaux) may have contained a disproportionate share of partnerships. John Komlos (Munich) suggested that a random sample from the 1840 directory would have been preferable, but Sokoloff argued that Lamoreaux's data were consistent with his own data on manufacturing firms. Atack asked whether it was difficult to trace partnerships with very common names, and Pope wondered whether out-of-town partners were listed in the city directory. Sutch pointed out that the Dun and Bradstreet records showed frequent changes in the names of partnerships, as hidden partners were revealed. Kantor and White asked for legal details on partnership formation. Goldin asked if there was a bias in the generation of the data that could have produced the finding that partnerships among people with the same surname were more likely to persist. Raff suggested that Lamoreaux examine births of firms as well as deaths. At the end of the discussion, Fishback questioned Lamoreaux's conclusion that the partnership was popular because of the values of 19th-century businessmen. He suggested she broaden her search for economic advantages of this form of organization.

Edward McDevitt (UCLA) then presented 'The Evolution of Irrigation Institutions in California: The Rise of the Irrigation District, 1910-1930', which examines the transformation of irrigation enterprises from private to public entities. McDevitt offered an explanation for this transformation that does not rely on the usual suspects, market failure and rent-seeking. Instead, he highlighted the role of agricultural, regulatory, and legal changes, and in particular, stressed the role of the subdivision of farm holdings, the imposition of water rate regulation, and legislation that made the sale of public utility bonds easier. His theory was tested and verified using data from a variety of sources, including land prices, bond yields, and voting records of the California legislature.

Several questions were raised about the land and bond price data used by McDevitt. Kantor suggested using the assessed land values for irrigated and non-irrigated land from the Agricultural Census. Sokoloff wanted to know the correlation between the prices of crops on irrigated and non-irrigated land. Pope pointed out that vineyard and orchard land like that examined by McDevitt is priced to reflect a great deal of capital investment, so it should rise with the age of the farm. Davis and Atack were interested in some of the sharp fluctuations in land prices and suggested that external events, like the end of World War I and the end of Prohibition, may have played a role. White was worried by the apparent uniformity of the bond price data over long periods. Attention then turned to some of the paper's political economy issues, with Haines asking whether the new public irrigation districts were a source of patronage jobs. Pope wanted to know if the new public districts could compel farmers to maintain their part of the districts. Atack pointed out that a key issue is ownership of the original water rights.

Claudia Goldin then presented work on education, part of a larger project, 'How America Graduated from High School: 1910 to 1960.' Using a new body of data from the US Bureau of Education, on national enrollment and graduation rates, Goldin made five major points. First, in most regions, the growth in enrollment and graduation rates was fastest from 1920 to the mid-1930s. Second, slower growth in these rates in some states and regions appears to reflect improved opportunities in manufacturing; these places saw rapid advances during the Depression as a result of unemployment and the child labor provisions of the National Industrial Recovery Act. Third, throughout the period, females attended school for more years on average than males. Fourth, there are some big surprises; for example, Idaho had twice the graduation rate of New Jersey in 1925, and Wyoming, Nevada, and Nebraska were also among states with the highest rates. These anomalies were eliminated by controlling for differences in manufacturing employment and the value of agricultural output.

Questions about the impact of World War II on enrollment and graduation trends were raised by Davis, Atack, and Sutch. Weiss pointed out that, although 17 and 18 year olds were not drafted, many enlisted. Margo asked about those receiving GEDs, who were not included in the Bureau of Education data. He was also concerned that the data might miss 'discontinuous schooling.' Sutch suggested that the rise in enrollment and graduation rates might reflect more than a conventional income effect. Since agriculture was a declining sector, sending children off to school may have been a strategy to enhance their earning capacity outside farming.

The day's final paper also dealt with education. Stephanie Schmidt (MIT) presented work on 'Do School Inputs Matter: Historical Evidence from New York State.' Schmidt uses unique historical data – longitudinal information on student performance by school district during a period when the state increased school funding in a number of districts. She concluded that urban districts with larger expenditure increases also saw greater improvement in performance on standardized tests, and that this resulted from lower pupil-teacher ratios and higher teacher salaries. Such effects were not evident in rural districts.

Several questions were raised about the nature of the underlying data. Whether New York is representative of other states was a concern raised by Atack. Lamoreaux was worried that the content of exams may have changed over time, so apparent changes in performance may merely represent changes in standards. Similarly, Kantor wondered whether the pass rate might have been endogenous. Several participants questioned whether the increase in state funding really represented a natural experiment. Sutch suggested that the new law was designed to help rural districts catch up. Fishback recommended testing for exogeneity of the law by regressing funding increments on city and county characteristics. Kantor pointed out that endogeneity might have led to greater disparities in outcomes. Since state funds could substitute for local contributions, tax payers in bad districts may have decided to pay less. Both Margo and Goldin wanted to know if tax relief resulted from the increased state aid.

The first paper on Friday morning was presented by Jeremy Atack and Fred Bateman (Georgia), 'Why Did Larger Firms Earn Lower Rates of Return than Smaller Firms in the Nineteenth Century: Some Hypotheses and Evidence.' In previous work, Atack and Bateman had found large and growing differences in profitability between manufacturing and agriculture over the period 1850-70. For this work, they have added data from the 1880 census of manufactures and looked more closely at profitability in manufacturing, finding there was no difference in profit rates between large and small firms in 1850 and 1860, but, in 1870 and 1880, larger firms had consistently lower profits than smaller firms. They test for a variety of measurement problems before accepting this finding, and offer several hypotheses to account for it. They suggest that capital market imperfections, particularly those created by unlimited liability in early corporations, may account for the lower profitability of larger firms.

Several issues of measurement were raised by the audience. Sokoloff asked how the authors handled firms listed as having zero employees (like many of the firms he had found in the distilling industry), and how they dealt with firms that operated only part-time. White questioned the finding that some firms had profit rates of -100%, suggesting that there must have been some unmeasured capital inputs, like proprietor's capital at call. Sokoloff suggested using comparable data from the 1820 census, and Sutch suggested using the 1875 and 1885 Massachusetts censuses. Steckel asked why there was no apparent big firm/small firm difference for 1850 and 1860. Atack and Bateman replied that this may have resulted from their inability to control for seasonality in those years. Ferrie suggested using the 1870 and 1880 data to adjust the earlier data for seasonality by industry, region, and firm size. Attention then turned to explaining the small-firm effect. Lamoreaux suggested that the inverse relationship between size and profits may have resulted from survivorship: if failure rates were higher among smaller firms, the survivors among small firms will reflect a more selected sample than survivors among large firms. Haines suggested considering why firms might not survive by seeing if the data on the disappearance of small firms are consistent with life tables.

The second paper of the day was by Naomi Lamoreaux and Ken Sokoloff, 'Location and Technical Change in the American Glass Industry, 1870-1929.' Using data from the US Commissioner of Patents, the authors investigate the relationship between the geographic concentration of the glass industry and its level of inventive activity. They found that, although the output of the industry was clustered in Pennsylvania, West Virginia, Ohio, Indiana, and New Jersey, inventive activity did not follow the same pattern. In fact, Indiana (one of the largest producers) was far less inventive than New England (which had relatively little production). This finding runs counter to the notion of local externalities associated with both production and invention. The authors concluded that centers of invention required more than simply raw materials or skilled workers (as both were available in the locations to which production but not invention was moving). Invention needed a system of institutional and cultural support like that found in New England.

Several people wanted more information on whether skilled glass workers had been willing to move as the industry moved. Margo suggested using the 1900 and 1910 Census manuscripts to check the persistence of glass workers; Fishback and Margaret Levenstein (Michigan) suggested looking at union records; and Haines suggested using firm-level payroll records. Margo also wondered whether inventors in some locations could live in urban centers and still be close enough to production sites to reap externalities. Atack noted that many machinery makers were located far from where their machines were used, so invention in the machine tool industry, like that in the glass industry, is not primarily a result of learning-by-doing. Sutch wanted to know why New Jersey and Massachusetts lost their glass industries, while Steckel speculated that firms might have been reluctant to relocate to the South because of the climate, an important consideration in a thermal industry like glass-blowing.

The final paper was presented by Daniel Raff and Manuel Trajtenberg (Stanford), 'Towards a Quantitative History of the American Automobile Industry: Hedonic Analysis, Simple Quality-Adjusted Price Indices, and the Magnitude of Product- and Process-Innovation in the First Four Decades of Automobile Manufacturing.' Using data on the characteristics of specific models offered between 1906 and 1940, the authors traced the evolution of the industry at the level of individual products. They find that, after adjusting for quality, real prices were halved every 14 years, with most of the decline resulting from process innovation, and less than 40% from product innovation.

Lamoreaux asked whether consumers genuinely cared about changes in characteristics, while Atack suggested that some may have resulted from changes in road quality. Margo was concerned about the use of list rather than transaction prices, which would have been more revealing. Geoffrey Carliner (NBER) was dubious about the authors' analogy between automobiles and computers, where real (quality-adjusted) price declines are similarly underestimated because of rapid technological change. He observed that Raff and Trajtenberg needed five times as many variables to capture automobile quality dimensions as have comparable studies of computers, and that there is more continuity across automobile models than there is in moving from PCs to workstations to mainframes.

The DAE Summer Workshop will meet at approximately the same time in 1995. The program is to be arranged by Stanley Engerman and Joseph Ferrie, who will issue a call for papers during the spring.

Call for Papers

The Quantitative Economic History Workshop will take place at St. Catherine's College, Cambridge, England, September 8th and 9th, 1995.

Submit your paper or a detailed proposal, or your request to attend the Workshop, as soon as possible to:

Michael Kitson
St. Catherine's College
Cambridge CB3 0BN
United Kingdom

List Reports *(continued from page 2)*

since e-mail, as a medium with near zero cost, should not be rationed. Siding mildly with the first camp, in mid-February Sam Williamson started moderating to bring some discipline to the list. Problems included some subscribers posting several messages at a time; others copied recent (and often long) postings into their own messages, making them difficult to read. At the beginning of March, Econhist was switched back to an unmoderated list, to see how it would function.

Econhist will shortly be divided into three separate lists. **Econhist.news** will be a moderated list specifically for 'news' postings: announcements, calls for papers, and other material of general interest to economic historians. The editor will review postings and forward messages of interest to subscribers from H-Net and other sources. The Trustees of The Cliometric Society will serve as Editorial Board for Econhist.news. Two other new lists will be established: **Econhist.res**, devoted to research inquiries, and **Econhist.disc**, for discussion and debate. A separate Editorial Board has been formed and will be announced on the lists. They will set policy, in particular whether these lists will be moderated. The new lists will be announced soon by e-mail, with instructions for subscribing.

Econhist.teach was established in August; the number of subscribers continues to increase. Recent traffic has averaged about 45 messages per month. November discussion centered on using fiction and nonfiction literature and films in economic history classes. Dozens of members examined the merits of works ranging from *The Jungle*, *Two Years Before the Mast*, *Women's Diaries of the Westward Journey,* and *Roger and Me*, to the writings of Charles Dickens and Frederick Douglass. If you have considered using literature or films in class, you might read these useful suggestions in the List Archives. Simultaneously, the end of 1994 brought discussions about how to combat boredom in the classroom, methods of 'warming up' students, and the economic history of the postal system. In January, we had a fruitful discussion about teaching the economic histories of Asia, Latin America, Africa, and Native North Americans, summarized in a posting from early February which included a valuable list of citations. Both old friends and newcomers to the list added their contributions. Special thanks go to Pierre van der Eng for sharing his knowledge about Asian economic history.

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Econhist.student was initated in January to link students and faculty interested in economic history, but who are at different institutions. The goal is for students – particularly undergraduates but also graduate students – to engage in dialogue with faculty and other students about research projects. We also hope discussion will address such topics as graduate school options or conferences. We want students to feel comfortable asking questions, and to benefit from the wide range of subscriber knowledge. To date, traffic on the list has been fairly steady, averaging about two postings per day. Traffic will probably increase as the spring term progresses, when students realize that they have papers due soon. We encourage teachers of economic history courses in all areas – American, Canadian, European, Asian, *etc.* – to invite their students to subscribe and to participate in discussions. If anyone has suggestions about the future direction of this list, please feel free to contact us.

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Econhist.macro is a discussion list for macroeconomic history. The list has been active since September, and membership has been increasing every month. We have used the list for three purposes: First, distribution of information. Subscribers post announcements regarding new data sources, papers, research monies, *etc.*; they also request data, papers, and the like. Second, topical conversations. Discussions have addressed the role of fiscal policy in smoothing the business cycle, relevance of the new growth theory to economic history, and differences between international migration in the late 19th and late 20th centuries. Third, discussion of specific papers. The first paper is a new working paper by Charles Calomiris and Christopher Hanes on the relationship between macroeconomics and macroeconomic history. We hope to have such discussions regularly. We encourage anyone interested in macroeconomic history to subscribe – you do *not* have to be doing research! Rather, subscribe if you are thinking of starting a project with macroeconomic aspects. Subscribe if you want to ask questions and learn more about the field or specific topics in the field, or subscribe if you just want to listen to conversations about macroeconomic history. All are welcome!

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H-Business has operated as a moderated list from its inception. Jointly owned by the Business History Conference, The Cliometric Society and H-NET, co-editors are Jeremy Atack (Vanderbilt) and Austin Kerr (Ohio State). Gatekeeping duties have alternated between them (with the bulk of the work being done by Kerr). The editors report to, and discuss issues with, their Editorial Board: Will Hausman (William and Mary) Mark Rose (Florida Atlantic), Mary Rose (Lancaster), Lou Cain (Loyola of Chicago and Northwestern), Richard John (Illinois at Chicago). Relatively few items have been rejected – mostly research-help solicitations from people who haven't done the most basic research themselves – and most have been accepted upon revision and resubmission. The editors feel quite strongly that moderation has restricted accidental or intentional 'flames' but has not interfered with the exchange of ideas; they have also intercepted personal messages mistakenly mailed to the list, preventing some red faces. The list has approximately 275-300 subscribers and is active enough to generate a couple of postings on a typical day. One innovation has been posting book reviews, which the editors plan to have as a regular feature.

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Databases has been on-line since late January, serving users and creators of historical data sets. The forum allows researchers to solicit advice from colleagues on data availability, data collection, electronic organization, and software selection/use. The editors post announcements about location of historical data files and software program sources. Recent discussion of spreadsheets warned about potential memory management problems under Quattro Pro for Windows. Subscribers weighed the merits of database software including ORACLE, Microsoft Access 2, and Paradox, and the alternative of using data managers within statistical programs such as SPSS/SAS/SYSTAT. The editors are considering a subscriber survey (*à la* ***Consumer Reports***) about software preferences in various categories. Unresolved questions posted include: Are there any databases constructed from wills (testaments); if so, how are they structured? Does anyone have a cartography database for Europe or the world before modern times, preferably back to the Middle Ages? Any recommendations for a superior statistical/econometric software package on a Mac or Power PC platform suitable for both research and undergraduate use? Please join our conversation.

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Quanhist.recurrent was established in early December 1994. Its objective is to stimulate and to facilitate comparative analysis of recurrent historical phenomena. The list currently has 70 subscribers, with about 10 so far taking an active role. These 'core members' include researchers from outside the US – from Australia, China, Japan, Poland and Western Europe. The first activity was an inventory of member expertise and reliable documentation resources. Our 'Catalog of libraries' provides Internet addresses and interlibrary loan possibilities for a number of major libraries around the world. This catalog will be available by April 1995 in the server List Archives directory. Next we plan to assess how, and to what extent, 'real' comparative work can be carried out through the list. It is our pleasure to acknowledge collaboration and support from a number of people: Georg Fertig (Konstanz), Raun Kupiec (MIT), Andrew Massey (James Cook), Mikael Olsson (Uppsala), Rafal Prinke (Adam Mickiewicz), Ningwu Qu (Beijing College of Economics), Yasukichi Yasuba (Osaka Gakuin).

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Global.change was started in January 1995 to provide a forum for social scientists interested in discussing long-run economic and social change, primarily but not exclusively in an international and comparative context. The list welcomes perspectives from all the social sciences and beyond. Possible topics for discussion may include whether world systems theorists and cliometricians have anything to learn from each other; comparing perspectives of economists, sociologists and political scientists on causes of rapid growth in some countries, and slow growth in others; and why Western Europe has experienced alternating economic integration and disintegration in the decades since the Cobden-Chevalier treaty. We expect moderate traffic on the list. Since a major potential benefit of the list is to alert researchers in different countries to each others' interests, we welcome short statements from subscribers introducing themselves and their research interests.

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Call for Papers
1996 ASSA Meetings

The Cliometric Society will sponsor sessions at the ASSA meetings in San Francisco, January 5-7, 1996. The Selection Committee will be co-chaired by John Nye (Washington University) and Philip Hoffman (California Institute of Technology). Please share this announcement with colleagues and students who may wish to submit their work.

Authors interested in presenting papers should send a two- to five-page proposal to The Cliometric Society by May 15, 1995. If submitting hard copy, authors should mail three copies of their proposals to the address below. We encourage authors to submit proposals *via* e-mail; send ASCII text format documents to:

CSociety@cs.muohio.edu

Authors must be prepared to send a 3,000-word paper summary to the Society office by the September 1 deadline date, for publication as an insert in the October *Newsletter*. At least one author must be a member of The Cliometric Society.

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Relevant Dates:

Three copies of paper proposals to The Cliometric Society	**May 15**
Authors notified of acceptance of papers	**July 1**
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