

# The Cliometric Society

October 1994 Vol. 9 No. 3

## 'PIONEERS OF SOCIAL SCIENCE HISTORY' HONORED AT 1994 SSHA MEETINGS

(Atlanta) A year ago the Editors of *The Newsletter of The Cliometric Society* 'stopped the presses' on October 12 after hearing this announcement: 'The Royal Swedish Academy of Sciences has decided to award the Bank of Sweden Prize in Economic Sciences in Memory of Alfred Nobel for 1993 jointly to: Professor Robert W. Fogel, University of Chicago, USA, and Professor Douglass C. North, Washington University, St. Louis, USA, for having renewed research in economic history by applying economic theory and quantitative methods in order to explain economic and institutional change.' It continued, 'They were pioneers in the branch of economic history that has been called the "new economic history", or Cliometrics.'

During the ensuing 12 months, media coverage and a variety of events have honored Fogel and North and publicized the little-known field of cliometrics. A Special Event at the 19th annual Social Science History Association meetings capped the year for the two laureates and illustrated this progress. Almost 34 years ago, Fogel and North met while standing in line at O'Hare for the DC3 flight to West Lafayette, where they joined 10 others for the first Cliometrics Conference. On October 13, 1994, 10 times that number assembled to hear session Chair Samuel H. Williamson and speakers Eugene Genovese, Richard Easterlin, Lance Davis, and Robert Gallman reminisce about these 'two pioneers', and reflect on their work and its significance for cliometrics. The speeches are reproduced below in order of presentation.

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*Robert W. Fogel: Historian*

Eugene D. Genovese, Emory University

The day that Bob Fogel and Doug North won their richly deserved Nobel Prizes was a joyous day for their old friends. Since I go back some forty-five years with Bob and have always counted him among the few people I can honestly say I love, it was an especially joyous day for me. But perhaps I did not quite always love him. For I do not think I loved him back in 1950 when he presided over my expulsion from the Communist Party, announcing to the assembled comrades that the Party was significantly stronger for getting rid of undesirables like that undisciplined, trouble-making revisionist Genovese. In retrospect, however, I recognize that I should have loved him even then, for, obviously, he did it all for my own good.

It would be in poor taste to talk out of school, boring you with nostalgic recollections of Bob's youthful follies. But perhaps I may be permitted two observations. First, you will be pleased to learn that Bob has always been first-rate at everything he does. From personal experience I can assure you that, even back then, in brilliance, loyalty, toughness, and skill, he had no peers as a party bureaucrat.

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## Executive Director's Notes

### Cliometric Society Trustees

The annual meeting was held October 8 in Cincinnati, Ohio. Trustees discussed the following matters:

**Membership:** In 1994 the Society has 407 members from 23 countries, a 5% increase from last year, with 37 student, 333 regular, and 37 sustaining members. 250 members subscribed to *Explorations in Economic History*, and 152 members have in the past two years opted for dual membership with the European Historical Economics Society.

**Finances:** The financial report for Fiscal Year 1994 was presented, showing a deficit of \$416.24 (see page 31). Because the endowment has been declining and expenses rising, the Trustees approved a small dues increase, the first since 1989. Unfortunately, Academic Press will raise their subscription rates for *EEH* for the fifth consecutive year.

**Newsletter:** The Trustees approved a revision to paragraph H.4 of The Cliometric Society By-Laws: "The

Editor will have the right to add other editors with the approval of the Board."

**Conferences:** The 1995 Cliometrics Conference will be held at the University of Kansas, May 19-21. Tom Weiss and Joshua Rosenbloom will serve as local hosts and Selection Committee. Trustees approved exploring the possibility of holding a third World Congress of Cliometrics in the summer of 1996, as a supplement to the annual Cliometrics Conference.

### Report on IEHA Meetings

The quadrennial Congress of the International Economic History Association was held in Milan, Italy, September 12-16.

As the official representative of The Cliometric Society, I attended the Assembly of the Congress. A responsibility of the Assembly is electing the new Executive Committee, who serve for the next four years. Seven of the 16 members of that committee are also members of The Cliometric Society: Roberto Cortes Conde, Scott Eddie, Akira Hayami, Stephen Nicholas, Jaime Reis, Richard Sutch, and the new President, Gabriel Tortella. The 12th International Congress is scheduled for September 14-18, 1998 in Seville, Spain.

I also attended the informal meeting of the European Historical Economics Society, convened by James Foreman-Peck and Gunnar Persson. Much of the meeting focused on a proposal that EHES should encourage founding a new European journal. Questions were raised about the likely number and quality of submissions and the language of publication, and the proposal was referred to a committee. Discussion then turned to future EHES activities. Four specialized workshops and a summer school aimed at young scholars have been arranged for June, 1995 in the Netherlands, with the 1996 session to be held in or near Pisa. A second general conference, organized by Gianni Toniolo, is scheduled for January, 1996 in Venice. I proposed that EHES and The Cliometric Society consider co-sponsoring a World Congress in Europe in the summer of 1996. The two groups will explore the possibility.

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## AN INTERVIEW WITH NATHAN ROSENBERG

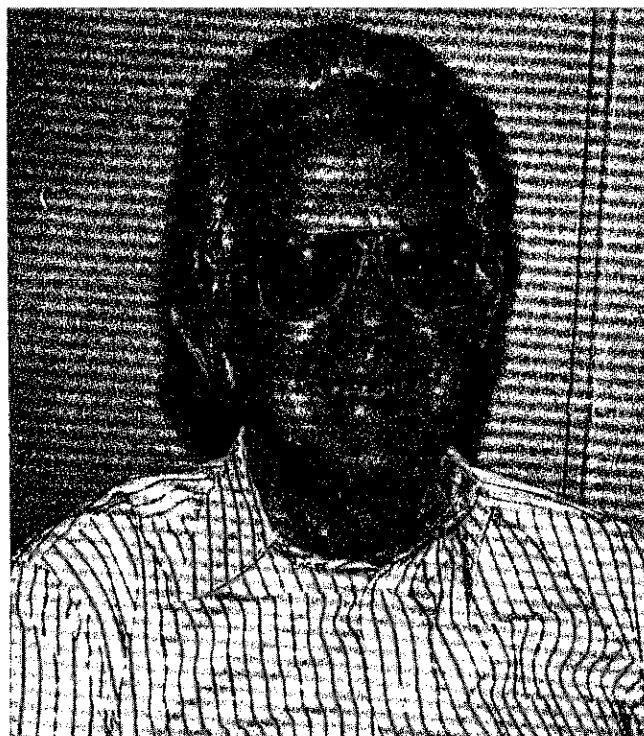
*Editors' note: Nathan Rosenberg has taught at Stanford University since 1974; in 1980 he was appointed to the Fairleigh S. Dickinson, Jr. Professorship of Economics. Our interviewer was William C. Sundstrom of Santa Clara University, who writes:*

*I spoke with Nate in his office on February 10, 1994. Being there brought back memories of conversations I had with him during the early 1980s, when I was a graduate student studying economic history and history of thought at Stanford. As usual, Nate was open, hospitable, and eager to talk about his many research interests, both past and – especially – present and future. I began by asking Nate for a brief intellectual autobiography, including his schooling, influential teachers, and how he became interested in issues of economic history and technology.*

I come from a very poor working-class background in northern New Jersey, and I was the first and really the only member of my family to go to college. This is partly relevant because my first serious intellectual immersion was in Marxism: my father was a Marxist and a Communist. The early intellectual discussions around the kitchen table during my childhood were all observations on conditions of the working class not in 1844, but in, say, 1944. This shaped a lot of my thinking. I think I unloaded a lot of my Marxist freight by forcing myself to read through all three volumes of *Capital* when I was 18 or 19 years old. That really did it. Volume II was a killer.

Then I went off in the Army and spent a year in Korea. This exposure to an extremely poor country was also a formative influence, leading me to ask questions like why are some countries rich and some countries poor. When I went to school – I did my undergraduate work at Rutgers and my graduate work at Wisconsin and Oxford – my interests were primarily on the question of long-term economic growth.

One thing that I carried from my early Marxian exposure, which I still carry, is the notion that technical change is a central part of that long-term growth phenomenon, and that's an element of my intellectual orientation I guess I will have with me life long. I found in graduate school, and in reading the economics literature in the mid to late '50s, that there was really very little if anything that the literature had to say about the origins and the causes of technological change, as opposed to its consequences. And that, I think, is still broadly true even today. There is a great deal of interest in the economic impact of technical



change, but there has been precious little done trying to unpack what it is that accounts for technological dynamism – for the rate and direction of inventive activity.

In my younger days I found myself teaching courses on economic development and found very little, still, in the economics literature that shed any light on how technological change came to be the kind of phenomenon it was. That was what really brought me into economic history. As a graduate student I didn't even take a course in economic history, although I did take a wonderful seminar with Abbott Payson Usher one year when he was visiting at the University of Wisconsin.

So, in many ways it was my persisting interest in economic growth and what generates it that really sent me back into economic history, and to the degree that I became an economic historian I became one because I was frustrated with not finding anything useful on the subject of technical change in economic theory. I decided instead to look at it historically. Thirty-odd years later I think that was a very good decision – that a good way to find out about technological change was not to sit down or go to a blackboard and theorize about it, but to look at how it occurred.

## How did you begin that 'historical look'?

Along the way I was befriended, as a young non-tenured assistant professor, by two people who came to play a very important role in my own thinking, Bill Parker and Dick Easterlin. And it was really through their encouragement that I did the first kind of systematic study of some important technological change. They invited me to give a paper at an Income and Wealth Conference and we agreed that I would write a paper on machine tools. In the end, the paper was not published in the conference volume, because there were no numbers in it. So, failing that, the article appeared in the *Journal of Economic History*, I guess in 1963. Most of my work, not all, but most of my work since then has involved looking at technological change in particular industries over time.

One thing that has impressed me very strongly is the obvious observation that technological change is really a great many very different kinds of things. It was something very different in the machine tool industry from what it has been, let's say, in agriculture, the aircraft industry, chemicals, forest products, or any number of other industries that I have looked at in particular at one time or another. And so, I guess it would be fair to say, most of my work has been a kind of an inductive search, looking for observations about technological change as an economic phenomenon, but again my primary interest was in looking at the causation more than the consequences, and that is what I am still doing today.

In 1961 I went to Purdue and there I found Jon Hughes, one of my dearest friends. Jon and I had actually been students together and lived together at Oxford. I was not only Best Man at his wedding, I was his only man. That's true: Jon was a Rhodes scholar, and back in those benighted Neanderthal days, Rhodes scholars were not allowed to marry. So, Jon was married secretly and I was his only man. I found an extremely lively intellectual climate at Purdue: Jon Hughes, Lance Davis, Ed Ames (with whom I did quite a bit of work), and a number of other people; and of course what happened every year was that the cliometrics people would meet, for some strange reason in late January, in West Lafayette.

Often a good part of the meetings were spent at my house. Sometimes the weather got bad; on one occasion, Henry Rosovsky ended up spending the night at O'Hare and Bob Fogel ended up spending an extra three nights at the Rosenbergs', from which we have a large collection of wonderful pictures of our kids that he took. And so, we

would get the crowd of Paul David, Al Fishlow, Peter Temin, Bob Fogel, Doug North, Bill Parker, Stan Engerman – which is to say, a group of people that includes some of my oldest and dearest friends. So, I have been very close to the cliometrics movement. Later on, when I ended up the University of Wisconsin for some years, the Cliometrics meetings gravitated there. And so I learned a great deal of my economic history sitting at the feet of the most innovative members of the cliometrics profession.

Since I came to Stanford, very nearly 20 years ago, my focus has changed in certain ways, but the common theme is still trying to determine the wellspring of technological innovation. I focus now much more than I did on the role of science – on the role of science, but also on the role of research generally. One of the interesting questions that I'm working on right now is, really, when you look at R&D, what is it that you are looking at? In fact, economists tend to make use of the R&D numbers and then frequently talk about it as if what they're talking about is research in basic science. Whereas the fact of the matter is that in the United States, not only today but for the last 30 years, if you look at total R&D spending, only about one-twelfth of it is what the NSF would call basic science; roughly two-thirds is development work. It's not basic science at all. Much of it is product development and testing and revision and further development and testing and so on.

For some time I have been particularly interested in – I use a word I think is appropriate in this context; it's an overused word in some circles – the dialectical relationship between science and technology. And a lot of that involves looking at how technology has shaped and influenced the scientific enterprise as much as science has shaped technology. To a very considerable extent, I regard the causation as being technology generating new science.

**How old would you say that dialectic is? I think of science-based technology as something that isn't of much importance in technological progress until the turn of this century, or the late 19th century. Is this a dialectic that goes much farther back than that?**

The question you ask is precisely what I am particularly interested in. I think it is increasingly a 20th-century phenomenon, even though I also believe that even today we still very much exaggerate the extent to which new technology is based upon scientific research. We cer-

tainly wildly exaggerate it when we suggest, as it is often suggested by the spokesmen for science in Washington, that technological change depends upon recent developments in science. I mean, it is one thing to say that technology is shaped by science, and I would certainly not challenge that proposition, but an observation that I make over and over again is that the science that turns out to be influential is frequently very old science. If you look, for example, at the revolution in metallurgy – I think that is a fair term – after Bessemer, say, at post 1856, and if you look at the development of the basic Bessemer process, you are looking at technological developments that indeed draw upon science, but, for the most part, that science was there before the French Revolution.

### **But has metallurgy ever really been a science?**

That's a good question, because if you look in college catalogs, as you can here, you will not find 'metallurgy'. You will find entries under 'materials science'. So, if science is what universities teach and say are sciences, then the answer would have to be 'yes', but, see, I have no difficulty saying metallurgy is a science. I have no difficulty saying that many intellectual pursuits incorporate large elements of basic science, even when the pursuit would today be thought of as an engineering activity.

What about 'computer science'? Is that a science? If so, what do you mean by Science? Solid state physics by definition has certainly underlain the development of the hardware that is made for computers, but if you look at what people who call themselves computer scientists do, they are not scientists in the sense of solid state physicists, I tell you, not by a long shot. They are working on questions like the nature of human intelligence, artificial intelligence; in some cases they are looking at developing algorithms for how human beings as well as machines solve certain kinds of problems. To use a useful term of Herbert Simon, computer science is one of the 'sciences of the artificial'. It is a science built upon, and in turn contributing to, the building of a discipline which involves the study of human artifact; but can't the study of the behavior of the human artifact be science?

The dialectic, I think, is [very] pervasive. I mean, consider the fact that much of the progress of science, after all, has depended very heavily upon the development of new techniques of experimentation going back to Galileo's observations, which changed our view of the world. That couldn't have happened without a telescope. Pasteur's work would have been, I think, inconceivable without a

microscope, without a pretty powerful one. Much of what has gone on in 20th-century molecular biology would have been impossible without x-ray crystallography. So, clearly technology influences science in a very profound way by providing its instrumentation. But it does it, I think, in a great many additional ways.

The fact is that technology also leads to anomalous observations which then require explanation. In a certain sense, Thomas Edison discovered the electron but didn't know it. It was of no particular use to him that he could see, so he hardly did anything more than write it up. But if you look at the history of 20th-century technology you will find time and time again that pushing the technological frontier in one way leads to all sorts of observations, whether it is the collapse of a bridge or the crashing of an airplane or the corrosion of a cable underneath the Atlantic ocean, which in turn lead to very serious research, as in the last case it did at Bell Labs, which ultimately resulted in some important breakthroughs in polymer chemistry. So I think that dialectic is really a very complicated one.

**Now let me drag you back toward economic history. I was at the 1993 Economic History Association meetings in Tucson, where Don McCloskey gave a paper asserting we really have not much understanding at all of what brought about the Industrial Revolution and what has been the major set of driving forces in the extraordinary growth of *per capita* production since then. I wonder if you can give an overall assessment, going back to this early interest of yours in the sources of growth – and putting it in the perspective of Denison's growth accounting: how far along are we now in understanding the process of growth, in particular as it's driven by technological change? That's a big question.**

Yes, that's a big question, but a very fair one of course. I guess I'd be inclined to say that we know a great deal more today than we did, say, 40 or 50 years ago. One thing we know is an increasing awareness of how much we don't know. I think of the work of people like – well, in my own firmament of stars here – Jack Schmookler; I think, of course, of Griliches; I think of Kuznets; I think of my esteemed colleague Mo Abramovitz; it seems to me that we have a much better appreciation for the contribution of technology than we once had.

The economic historians have played a very important role here; our two Nobelists in economic history have

each contributed in their own very different ways to an important understanding of the role of technological change. You know, Fogel's book on railroads in American economic growth – it's not very often thought of this way – but when I read that book the most impressive thing to me about it, aside from the extraordinary energy with which he built his counterfactual world, was his successful criticism of what he called in the book the 'axiom of indispensability'. His basic point was a technological one. He picked what he regarded, and what most economic historians at the time would have regarded, as the most important innovation of the 19th century in America – the railroads – and with his huge energetic undertaking he established, to his own satisfaction at least, and to that of a great many others, that by 1890 railroads had made less than a 5% difference to GNP.

His general conclusion is that no single innovation was indispensable. I think that was a profoundly important observation. I'm not sure I would have defined the conclusion in exactly the same way that he did, but what he was saying, from my point of view, was that no single innovation is indispensable because a society that has managed, one way or another, to become technologically dynamic therefore has the capability of developing a wide range of substitute technologies for any given technology. So, I've always read Fogel's book as pointing to the impressive ability of advanced industrial societies to develop technologies over a wide range and to provide substitutes when one technology or one particular natural resource base, for example, becomes increasingly scarce.

Going back to the question that you posed from the McCloskey paper, it seems to me that through the works of people like Schmookler, Abramovitz, Kuznets, and Schumpeter, we have in fact learned a great deal. But if you say what of the growth accounting exercises of, say, someone like Ed Denison, I would have to answer that first of all I think it was an enormously important intellectual achievement; it was an attempt to break down the specific components of growth – and in that sense if nothing else it gave us a kind of a target to shoot at. It gave us some numbers and that, of course, is in the best spirit of cliometrics. It gave us some numbers and therefore something to chew over and to challenge and to disagree with. My own sense is that the growth-accounting exercises were largely doomed to failure because there was an underlying methodology which strikes me as being somewhat simplistic – that is to say, most basically, the notion that you can take the separate contributions of

capital, labor, human capital, make separate estimates for reallocation of resources, separate estimation of economies of scale, attach a value to each and then add it up, until you get to 100%, although even Denison still ended up with a rather significant residual.

What strikes me is that you can't simply attach values to each of these separate variables and then add them up. It seems to me that the decisive fact was in the interaction of things. Denison, I don't think, really got very far, nor do I think he ever really tried, to deal with these interaction effects. You know, how can you talk about the contribution of capital to growth without saying something very explicit about technological change?

On the other hand, I guess I have a lot of reservations about some of the other ways of simply playing with or massaging the time-series numbers on capital and labor. If you are allowed to play sufficiently with them, you can exhaust the residual by some appropriate weighting procedure, which has been a kind of a popular intellectual exercise of other people in the profession. So, I think we have learned a lot, but I'm still very much impressed with how much we still do not know.

**I take it then, in a sense, one of the reasons that you are not a cliometrician – wouldn't consider yourself one – is that the things that seem most interesting about the process of economic growth may be qualitative processes, often *sui generis*; your emphasis on industry studies and historical specificity suggest that it may be a mistaken presumption to think that you could find something measurable in the same sense you can measure labor force or capital expenditures.**

Well, I certainly agree that it's subtle and qualitative and that technological change is very difficult to deal with by the traditional tools of cliometrics. Let me get into a question which isn't readily dealt with in a straightforward cliometric way. A lot of Doug North's work, for example, has been very much in this, if you wish, qualitative, or at least non-quantitative, tradition. Doug has been enormously concerned with questions of institutions and how institutions affect motivation and incentives, and very concerned with legal institutions and political institutions.

Going back to McCloskey's question 'Why did the Industrial Revolution as we know it first occur in Great Britain?', I guess one of the first things that I would like to say is that British society was characterized by a high

(continued on page 27)

## Cliometric Society Sponsors Session at 11th IEHA Congress

(Milan) The first Cliometric Society session at an International Economic History Congress took place at the recent meetings in Milan. The session was held on a sunny Thursday morning following the rainy excursions of the previous afternoon. Given the diverse nature of the audience, the usual Clio format was altered to give authors more time to present their ideas, and formal discussants raised points to precipitate conversation. Withal, the usual vigorous audience participation took place. Newcomers to Clio sessions as well as old-timers commented it was one of the better sessions of the Congress. Clear empirical support for that hypothesis was that the session was, perhaps, better attended after the Congress's extremely long coffee break than before. This report is based on the paper summaries published with the July 1994 *Newsletter* and the comments supplied by the discussants. Remarks from the floor reinforced substantive points raised by the discussants.

In 'The Economic Progress of Immigrants in the Mid-Nineteenth Century United States: Results of Recent Research,' David W. Galenson (Chicago) presents micro-level evidence drawn from the manuscript censuses on the economic progress of 19th-century European immigrants. Systematic measurement of these immigrants' progress is important, both in order to judge the extent to which the United States was a genuine land of opportunity and to assess the contribution immigration made to American economic growth. Where immigrants chose to settle in the United States may have had a substantial impact on their accumulation of wealth. While there was considerable variation in levels, the mean wealth for English and Irish men was higher in 1860 in both Chicago and Davenport IA, than in either Boston, Lowell, or Newburyport MA. The economic success of immigrants in the Midwest may have been due to the presence of higher quality settlers, or to particularly favorable local economic conditions, or both.

The first discussant, Paul Katzenberger (Munich), observed that the self-selection hypothesis in Galenson's argument is a plausible supply-side explanation for westward migration, but it does not address demand factors in the sorting process. For instance, did more literate immigrants migrate west because they were more ambitious (a supply-side factor), or did regional variations in the demand for literacy play a role? Neither does the paper

reach a firm conclusion about which of the two hypotheses, 'self-selection' or 'capital gains', is preferred. Even if, as Galenson reports, the empirical evidence does not suffice to distinguish between them, he could have presented a research strategy aimed at making a distinction.

Galenson also does not investigate spatial differences in prices. Since Eastern cities were closer to manufacturing centers, consumer durables may have been cheaper in the East than in the West. Nominal data may therefore overstate differences in real wealth. Finally, Galenson discusses wealth accumulation in the first part of his paper and human capital accumulation in the second without dealing with the connection between the two. The terms income and wealth are used interchangeably. This is reasonable only if wealth and income are highly correlated, but, in the absence of income data, it is difficult to judge whether this is a reasonable assumption.

Robert Whaples (Wake Forest), the second discussant, focused his remarks on how well the paper accounts for the basic finding, that immigrants 'prospered' more in the late antebellum Midwest than in the East. Galenson first asks why this prosperity differential could occur in equilibrium by looking for selectivity in migration and by considering compensation for possible Midwestern disamenities. These factors do not seem to explain all the differences, so perhaps there was a disequilibrium. Demand for labor may have been shifting outward in the Midwest so rapidly that it outstripped increasing supply, driving wage and income levels above those in the East and culminating in greater levels of wealth.

Galenson's basic argument seems very plausible; so Whaples suggested an additional reason that a more prosperous Midwestern economy would fail to attract some migrants. Perhaps Midwestern wages included a differential to compensate for the lack of women. In his recent *Journal of Economic History* article, Galenson finds that married men were much more likely than single men to stay on the 'urban frontier'. Some single men may have been attracted by the extraordinarily prosperous economy, reached a target wealth, then left for greener marital pastures.

The 1850 census figures for the white populations of the counties containing Chicago and Davenport reveal the



'maleness' of these frontier communities.

Age group	Cook County IL			Scott County IA		
	M	F	Ratio	M	F	Ratio
20 - 30	5492	4170	1.317	565	462	1.223
30 - 40	4208	2876	1.463	479	350	1.369
40 - 50	2191	1393	1.573	278	211	1.318

Moreover, in the year ending June 1, 1850, Cook County registered one marriage per 222.5 residents, while the state of Illinois registered one marriage per 92.7 residents. The prospects of finding a wife were dim in a city whose marriage market was so glutted with potential husbands. Finally,

in his own work, from regressions using 1919 city-level data on wages in manufacturing, Whaples has discovered that wages were higher in cities with higher unmarried proportions of the adult male population. In 1919, a one percentage point increase in the

share of unmarried men was associated with a wage 0.82% higher. Thus, the city with the lowest marriage rate would pay wages 29.5% higher than the city with the highest. This phenomenon may help explain part of the remaining 1860 wealth gap.

In the next paper, 'Profitability of Japanese Imperialism in Korea, 1910-39', Mitsuhiro Kimura (Tezukayama) observes that many Japanese historians contend that in Korea, the largest formal colony in the Japanese Empire, colonization from 1910 to 1945 was quite profitable to Japan and contributed substantially to its economic development. Focusing on the 1910-39 period, Professor Kimura examines imperial economic gains and losses from the colonization of Korea, finding that, with the exception of the rice trade, gains and losses were quite small. Contrary to the theory of economic imperialism

adopted by previous Japanese scholars, Professor Kimura argues that imperial policy served to redistribute income from farmers to workers, especially lower income earners who spent much of their incomes on rice. By contrast, the economic interests of investors and industrialists were largely unaffected.

Loren Brandt (Toronto), the first discussant, praised Professor Kimura for his contribution to a long historiographical tradition looking at causes and effects of Japan's early 20th-century expansion into Asia. What distinguishes this paper from much of the earlier Japanese research is its strong empirical bent and Kimura's formulation and testing of alternative

hypotheses regarding the *raison d'être* for this behavior. Kimura fails to find empirical support for 'orthodox' views of the distributive implications of Japan's expansion, concluding that the motives must have been non-economic in nature. While non-economic motives surely cannot be dismissed, data limitations,



*Paper presenters, discussants, and C-Session participants.*

problems of econometric interpretation, difficulties of generalizing from the Korean case alone, and dynamic general equilibrium considerations all suggest that it may be premature to rule out an economic dimension to Japanese imperialism.

The second discussant, Tom Weiss (Kansas), interpreted Professor Kimura to say that imperialism was uneconomic because those with political power did not seem to benefit. The economic interests of those who had the most power, investors and industrialists, were unaffected as a whole; farmers, who had some power, seemed to be the biggest losers; and workers gained even though they had no power. The seemingly perverse results in Kimura's paper spring largely from his analysis of the impact of increased rice imports from Korea. He argues that these imports must have had a significant effect on



the price of rice, but that price changes did not affect wages. He estimates a reduced-form equation in which the explanatory variables are factors which should have influenced wages, finding no relationship, or even a negative one, between wages and the price of rice, so that workers appear to have reaped the gains.

Weiss then asked whether Kimura's specification gets at the crux of the matter. Did imports of Korean rice actually influence the price of rice in Japan? What might have happened in the absence of those imports? Perhaps alternative external sources would have arisen with the same consequences for domestic prices, wages and farm incomes. Or, had there been no alternative sources, how much would the price of rice have risen? That is, did rice imports hold down nominal wages and permit expansion of manufacturing without a reduction in the industrialists' profitability?

Although expressing reservations about some of Kimura's broad conclusions, Weiss said this was a provocative paper – all the more so for dealing with issues that are still touchy ones in Japan – and it does make some very interesting and worthwhile points. For example, the Korean venture was small potatoes in the broad scheme of Japanese economic behavior. Exports to Korea were small; imports from there were small; total expenditures on the Korean venture were small, and the basic conclusion seems secure: imperialism in Korea could not and did not have a substantial impact on Japan. Also, does this experience relate to the performance of MITI or whatever agent receives the credit for Japan's economic success? In the Korean rice case, the workers seem to have benefited from certain government policies. Was the Japanese state at that time not controlled by business interests? Did the state not realize the redistributive effects of the rice policy? Is this the same state, the same bureaucracy, that has been lauded for its miraculous economic performance since World War II – at least until recently? If not, when and how did that bureaucracy acquire its expertise?

Following the coffee break, Samuel Williamson (Miami), who chaired the presentations, showed a videotape indicating the various electronic services supplied through The Cliometric Society. The tape elicited a great deal of discussion as well as many compliments.

Elena V. Lobanova (Russian Academy of Sciences) then presented her paper on 'Gini-Index Modification: In the Problem of Irregularity of Distribution of Work-Force and Production in Russia: Leading Regions at the Beginning of the 20th Century'. She discusses the considerable



*International audience learning the 'Clio format'.*

concentration of Russian industry accompanying the rapid growth of industrial production in the 1890s. The 1900-1908 period was not marked by such a rise in concentration, but there was strengthening of the largest firms. Lobanova's research examines the regional and branch peculiarities of industrial concentration during the industrial boom before World War I, using statistical information collected by the all-Russian inspections of industry of 1909 and 1914.

Modified Gini indices of industry concentration and work force are calculated for all branches of industry, and for both the Petersburg province and the provinces of the Central Industrial region. Her results show that industrial concentration increased during the economic upsurge preceding World War I. The dynamic of concentration was steady and positive both in periods of crisis and in periods of growth. In general, the growth of work-force concentration outstripped that of industry concentration. This tendency was seen for all branches of industry, even for those where there was not large manufacturing, and it was a result of qualitative changes in the organization of production.

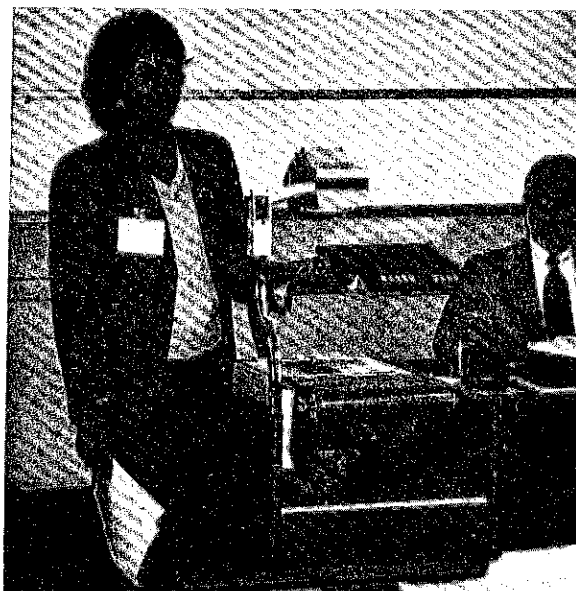
Discussant Mark Spoerer (Munich) remarked that the data available to Lobanova are of outstanding quantity and quality, with comprehensive information on all

industrial enterprises in all Russian provinces, including work force, horse power and even sales, which is probably unique for this period. In order to show increasing concentration, Lobanova compares concentration in both work force and sales, and concludes that the widening gap between the two measures indicates an increase in the capital intensity of production. Although she is correct to use indices both for relative and absolute concentration, Spoerer expressed reservations about the modified Gini index, and recommended use of the Hirschman-Herfindahl index for measuring absolute concentration. Spoerer was also not persuaded that increasing concentration was a consequence of new, more capital-intensive technology driving up the minimum efficient scale, and suggested that another possible explanation is imperfect competition. He urged Lobanova to include a detailed analysis of then-strategic industries (such as mechanical engineering or petrochemicals), which could enhance our understanding of the economic dynamics which made Russia the fastest growing large economy before World War I.

Leonid Borodkin (Moscow State), the second discussant, argued that Lobanova's modified Gini index was not a substantial improvement over the 'classical' form of the index. An important result of the paper is her showing that the process of labor force concentration varied widely across different branches of St. Petersburg industry at the beginning of the 20th century.

The final paper of the session, 'Gold Points and Real Exchange Rates', was presented by Maria Cristina Marcuzzo (Modena) and Annalisa Rosselli (Firenze). The authors observe that, in the late 18th and early 19th centuries, the international monetary system relied on the responses of merchants to changes in the profitability of gold import and export to bring about the required adjustments. Price arbitrage in the precious metals trade gave rise to a unified bullion market and paved the way for the Gold Standard in the late 19th century. From the mer-

chants' point of view, the profitability conditions of gold export and gold import involved knowledge of the price of gold, at home and abroad, and of the rate of exchange. Proper analysis of gold flows, the authors argue, depends on identification of the appropriate measure of the exchange rate. They observe that the 'market' exchange rate, as determined in the international market for bills, usually differed from the 'real' exchange rate, as determined by the relative value of gold (or silver) in each of the two currencies. Thus, unlike official rates under the Gold Standard, real exchange rates were not constant.



*Presenter Annalisa Rosselli and session chair Samuel Williamson.*

In their paper, Professors Marcuzzo and Rosselli distinguish three components that explain variations in the market rate of exchange: one due to the deviation of the market ratio between gold and silver from the official ratio, one due to conditions prevailing in the market for bills of exchange, and one due to variations in the market price of the standards prevailing in the two countries. They calculate each of these three components with reference to the London market for bills on Paris between 1790 and 1870, at which time French convertibility was suspended.

Those who participated in and those who attended the session responded in a decidedly positive way. Accordingly, we hope there will be at least two Clio sessions at the 1998 IEHA meetings in Seville, and that the standard Clio format can be adapted to the special circumstances presented by the International Congress.

## The Cliometric Society Server

The Cliometric Society server has been in operation more than a year and now averages about 300 connections per week. Several 'directories' of interest have been installed or upgraded on the server, accessible through World Wide Web (WWW), gopher, or by 'telneting' directly to our machine. The gopher and telnet address is **cs.muohio.edu**. If you use Mosaic, open the URL and type in our address **http://cs.muohio.edu/**. Please refer to the February 1994 *Newsletter* for more information. If you want further instructions, send a blank e-mail message (no subject is needed) to

**server.help@cs.muohio.edu**

### Directories:

*Syllabi* is one of the most popular directories, helping users to find fresh ideas for existing courses and to prepare new courses. The collection includes more than 50 syllabi, which cover both graduate and undergraduate classes. A few are for courses which examine issues in world economic history, about a third focus on European economic history, and the remainder deal with the U.S. All syllabi contain course outlines and reading lists, and some append written assignments and examinations.

Another directory offers membership data for the Economic History Association and The Cliometric Society. The Society's directory now allows users to search for members by research interest codes as well as by name, affiliation, country, and other basic information, and is updated monthly. We plan to add directories for the Business History Conference and other groups as available.

*Conferences* contains abstracts of papers presented at Cliometric Society-sponsored sessions at meetings and conferences, including the upcoming 1995 ASSA. With the Mosaic interface, users can view tables, charts, graphs and maps in addition to the texts of these papers, and can move quickly from text to footnotes and references.

*Data Series* contains the 36 Bureau of Labor Statistics datasets collected by the University of California Historical Labor Statistics Project. Recent technical improvements have made these files easier to download. We are adding other datasets to this directory.

*List Archives* contains all postings to our lists, organized by list name and month. You can view postings by subject and read or download complete postings.

*Books* is our most recent directory, containing both announcements and reviews.

## The Cliometric Society Lists

We now operate seven electronic lists. Three are quite active, and the other four are in various stages of coming 'on line'. Below is a series of reports provided by individual list editors.

### ECONHIST (Topics in Economic History)

Econhist, the first list created by the Society, was started in 1993. Subscribers now number more than 300. In the last five months, list activity has increased significantly, averaging 70 postings per month. An analysis of use finds that 11 people have posted seven or more times; 25 people, three to five times; and 85 people once or twice in this time period, and about 180 subscribers are 'listening' to conversations.

The list has been used for a variety of purposes, from announcing the death of Brinley Thomas to asking for a phone number. On occasion, people have mistakenly sent a message to the list they intended for an individual but, for the most part, postings have been of interest to a majority of subscribers. The following are some of the questions which prompted several comments: 'What is a

1926 cash award of \$1,000 worth in today's dollars?' 'What happened to family incomes and the standard of living after 1850?' 'Has anyone studied how financial intermediaries would evolve in a dynamic framework?' 'Are there any *bona fide* cases where we can show that the institutions of an economy made any difference to economic outcomes?' (This last question generated 46 responses.) Views differ about the appropriateness of certain postings. Some people regard the list mainly as a source of general announcements, while others see it as a place for a lively and informal discussion similar to one you might have over a drink or in the coffee lounge. I ask that everyone have patience. The Internet is a new medium of communication, and we are still learning the best ways to use it.

If you have hesitated to subscribe because you are worried about receiving too much mail, I invite you to join us using the 'digest' option (see page 31). If you have any questions or comments please contact me.

Editor: Samuel H. Williamson  
swilliamson@cs.muohio.edu

### **ECONHIST.TEACH (Issues in Teaching Economic History)**

This list was established in early August and currently has more than 100 subscribers, including many from outside the U.S. and many graduate student teaching assistants. Recently traffic has averaged about three messages per working day.

Early questions and responses centered around new readings to put on the Fall syllabus, the structure and content of the syllabus, the syllabi collection, and course objectives in general. A common response to the issue of course objectives was to assert that the comparative advantage of an economic history class is in developing analytical and writing skills. The editors actively participate by posting items to prompt discussion: in September Robert Whaples posted a paper in several installments on 'Where is There Consensus among Economic Historians: A Survey of Forty Propositions,' and David Mitch posted several messages about the teaching styles of noted economists. Resulting 'conversations' included the value of field trips in economic history classes and ideas for creating new courses.

Sample postings from a few subscribers illustrate the flavor of the list discussion. In October, Linda Barrington provided the list with her minutes on the EHA Teaching Breakfast in Cincinnati. David Mitch asked if it would be useful to 'vet' exams on the list, and Richard Sutch responded by posting his midterm exam along with the rationale behind each of the questions, the strengths and weaknesses they are meant to discover, and his instructions to TAs. Several textbook authors joined in a discussion of what topical and chronological coverage should be included in American Economic History texts. Other discussions have included the use of review sheets and teaching students how to read a book. Participants have raised issues about a wide range of teaching situations. Martha Olney explained in some detail how she has approached teaching economic history classes of several hundred at Berkeley. Akira Motomura raised issues about advising independent study projects at Barnard and Albion. Tom Maloney and others discussed teaching an economic history course for students with no prerequisites in economics. The editors look forward to the continued active participation of subscribers and encourage others to join Econhist.teach.

Editors: David Mitch  
dmitch@cs.muohio.edu

Robert Whaples  
rwhaples@cs.muohio.edu

### **ECONHIST.MACRO (Macroeconomic History)**

Started this fall, Econhist.macro is devoted to macroeconomic history, broadly defined. It is designed to provide a forum in which issues in macroeconomic history, data sources, new research results, and old controversies may be discussed, disseminated, distributed, and debated.

Macroeconomic history has many dimensions, and we want the list to reflect all and any of them. Thus, subjects for discussion may include business cycles; long-run growth; monetary studies; financial markets and their effects on the aggregate economy; effects and determinants of fiscal policy; labor markets and their interaction with other markets; sectoral shifts (*i.e.* the movement of resources across sectors of the economy); comparative work, examining topics across countries and regions; evidence supporting or disputing the notion of economic decline, *e.g.* late 19th century Britain or late 20th century America; and any subject of interest which you would include under the rubric of macroeconomic history.

As editors, we view our job as similar to that of a baseball umpire: we call the plays as we see them, and administer justice if necessary, but we otherwise let the players play the game. One difference is that we will often bring new issues and ideas to the attention of list members, and sometimes initiate discussions ourselves.

We have approximately 100 subscribers to the list. We have just begun our discussions, and look forward to contributions from anyone interested in macroeconomic history.

Editors:

J. Bradford DeLong  
bdelong@cs.muohio.edu

Louis Johnston  
ljohnston@cs.muohio.edu

### **H-BUSINESS (Teaching and Research in Business History)**

Since H-Business began in September, it has added 200 subscribers. This list operates as a 'moderated' list, with postings to the list submitted to the editors for approval. Only they may actually post an item to the list. The purpose of this 'gate-keeping' is to minimize clutter and to prevent posting of inappropriate items, such as commercial solicitations.

So far, the list has functioned primarily as a bulletin board and newsletter for the Business History Conference, informing subscribers about fellowship opportunities,

calls for papers for upcoming programs, and opportunities for using the Internet to learn about resources for the study of business history. Subscribers have asked for assistance regarding specialized needs in business history projects, and have obtained quick responses from experts in the field. The request eliciting the greatest response to date involved an inquiry about the use of scrip during depressions when currency is in short supply.

The most important contribution is probably the private correspondence which the list has facilitated over the Internet. Through H-Business, subscribers have learned of other scholars with common interests, and have discussed those interests with one another.

Beginning in December 1994, H-Business expects to post its first book review; another review has been commissioned. We have also asked publishers to include H-Business as a recipient of review copies of business history books. All reviews will be available to editors for posting on other lists. One subscriber has volunteered to review some popular books about contemporary business from the perspective of business history, commencing in 1995. In addition, the editors plan to start a Business History course syllabus collection, and invite contributions from all subscribers.

Editors: Jeremy Atack  
jattack@cs.muohio.edu

Austin Kerr  
akerr@cs.muohio.edu

#### **QUANHIST.RECURRENT (Comparative Analysis of Recurrent Phenomena)**

Comparative analysis hinges upon the observation of events occurring in various countries at different times. To date, social and political scientists have played a prominent role in using comparative approaches; accordingly, this list welcomes all contributions in quantitative history from subscribers in both economic and social history fields. The primary objective of the list is to build bibliographical guides which may constitute useful starting points for further investigation. Marc Bloch suggested that comparative history would benefit more from practical improvement in interlibrary-loan operations than from theoretical epistemological debates. The creation of this list is in line with Bloch's observation.

We are convinced that the collaboration of small teams of researchers from different countries can carry out such tasks most satisfactorily. Electronic mail clearly opens a new era by making international collaborations much easier and more effective. List members may contribute in several ways: by providing new references, by facilitating access to sources for other list members, and more generally by sharing their respective expertise. This list

will be composed of several 'Chapters', each devoted to one recurrent phenomenon. Planned Chapters include 'General Strikes', 'Hyperinflations', 'Price Bubbles', and 'Expulsions of Religious Congregations'. List members may propose additional Chapters. Sample postings and a model bibliography (entitled respectively 'Details for Subscribers' and 'Appendix') are available in the The Cliometric Society fileserver's Archive of Lists 'Quanhist.recurrent' folder.

Editors: Francesco Galassi    Bertrand Roehner  
fgalassi@cs.muohio.edu    broehner@cs.muohio.edu

#### **GLOBAL.CHANGE (Economic History Dimensions of Global Change)**

This list provides a forum for discussion among economic historians and others engaged in the study of major long-run structural changes in economic and social organization. The list welcomes interdisciplinary perspectives from subscribers beyond economic history and the social sciences, including, for example, studies comparing performance of different societies over long periods of time, as in recent studies of comparative economic growth, or the investigation of long-term change in a single economy. Suitable topics for discussion might include environmental consequences of industrialization; demographic transitions; long-term economic and social impact of new technology; causes and consequences of migration; market integration and economic transformation; institutional change and economic performance; and climatic change and agricultural development.

Editor: Alan Taylor    Co-editor: To be announced  
ataylor@cs.muohio.edu

#### **DATABASES (Design and Management of Historical Databases)**

This list is for those interested in discussing problems particular to creating or working with economic and historical databases. Such topics as interpreting and coding techniques, coding management and quality control, how to compress large sets, and the best types of software might be discussed. Subscribers can ask about existence of data sets, announce data set compilation, or seek collaborators.

Editor: Don Schaefer    Co-editor: To be announced  
dschaefer@cs.muohio.edu

**Note:** Essential list commands are shown on Page 31. Members are encouraged to remove the summary from the *Newsletter* for easy reference, and to share the information with potential list subscribers.

## Inaugural Hughes Teaching Prize Awarded

by David Mitch, University of Maryland-Baltimore County

(Cincinnati, October 8) At the 1994 EHA meetings, Douglass C. North was awarded the first Jonathan Hughes Prize for Excellence in Teaching Economic History. Winners are chosen by the Economic History Association's Committee on Education and Teaching, which for 1993-94 consisted of David Mitch, Chair, Mary Schweitzer (Villanova), and Samuel Williamson (Miami). The Committee was impressed by the very thoughtful and enthusiastic letters it received nominating candidates for the Prize. Nominees came from a wide range of teaching situations, from prestigious research universities to small liberal arts colleges. In the final phase of its evaluation, the committee received enthusiastic cooperation from department chairs, administrators, colleagues, and current and former students of those nominated.

The Committee on Education and Teaching was most impressed that many of North's former students were inspired to continue their studies in economic history. Some were his graduate students and others were undergraduates who then went on to work in economic history at other institutions. Many of them became leaders in the field. One such student was Jonathan Hughes himself. In his essay on 'Douglass North as a Teacher' (in Ransom, Sutch, and Walton, eds., *Explorations in the*

*New Economic History*, 1982), Hughes offered the following appraisal of what was so distinctive about North's approach to teaching economic history:

'To have been in his seminar was a once-and-for-all experience, but also the beginning of a life long dialogue that disturbed the slower ones as well as the gifted. Even the least distinguished of his students can talk for hours, will if given the chance, about the seminar and how it influenced them. It was not a slick well-planned "course of study." North's interests changed, the subject matter changed, the arguments changed. The students who faced North-the-Marxist and those who faced North-the-reactionary worked with different intellectual materials. But the critical attitude was a constant, like the drive for focus and creativity.'

'He also handed his students a sense of fun and life. The last they do not forget. He always believed he was good, his ideas important, economic history an essential discipline. You were his student, so you were something in the world by right. The nice thing about his criticism of you was that he also was always willing to think that his adverse judgment of you might be wrong. Try it out, he dared you. So we did.'

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## Call for Session Proposals

### Twelfth International Economic History Congress

#### Seville 1998

The International Economic History Association will hold its Twelfth World Congress in August or September 1998 and hereby invites proposals from scholars of all nations wishing to organize sessions.

Proposals for sessions must be submitted by the following dates:

**A- and B-Sessions: 1 February 1995    C-Sessions: 1 February 1996    D-Sessions: 1 February 1997 (tentative)**

North American applicants are advised to send copies of their applications to the General Secretary and to the two North American members of the IEHA Executive Committee:

Prof. Joseph Goy, General Secretary  
International Economic History Association  
Centre de Recherches Historiques  
E.H.E.S.S.  
54, Boulevard Raspail  
75270 Paris FRANCE  
fax: [011.33.1] 44.16.64.01

Richard Sutch  
Director, IBER  
University of California  
Berkeley, CA 94720-1922 USA  
fax: [510] 642-5018  
rsutch@econ.berkeley.edu

Scott M. Eddie  
Department of Economics  
University of Toronto  
Toronto M5S 1A1  
Ontario CANADA  
fax: [416] 978-5519  
eddie@epas.utoronto.ca

The complete announcement is available in The Cliometric Society server *Conferences Directory*.

## Conference Note: Health and Welfare during Industrialization

by Richard H. Steckel, Ohio State University

This conference was organized by Richard Steckel and Roderick Floud (London Guildhall) and was sponsored by the National Bureau of Economic Research. It was inspired by long-standing interest in the fate of the working classes during industrialization and the emergence of new techniques and data for assessing living standards. Since its inception in the 19th century, the Standard of Living Debate has focused on England's Industrial Revolution. Despite the rise of quantitative methods and exploitation of new sources of evidence on incomes, real wages and health, optimists and pessimists are divided over the direction, extent, sources, and meaning of change.

Our approach gives international perspective to the issue by considering the experience of nine countries that industrialized in the 19th or early 20th centuries. We enhance the comparative perspective by using common indicators of welfare, including *per capita* income, stature, life expectancy, and literacy. The papers consider divergent trends in these indicators and attempt to assess their implications for overall trends in welfare.

The authors and a small group of informal discussants met for a pre-conference at the NBER in Cambridge on July

11-12, 1994 to present preliminary papers and to discuss details of the project. Dora Costa (MIT) and Richard Steckel discussed the United States, the first country for which contrasts in the time trends of stature and *per capita* income were noticed. Roderick Floud and Bernard Harris (Southampton) followed with results for England. Lars Sandberg (Ohio State) and Richard Steckel reported on Sweden, and David Weir (NORC and Chicago) took up the case of France. The experience of women during industrialization in the UK was presented by Stephen Nicholas (Melbourne) and Paul Johnson (LSE). Gail Honda (Chicago) discussed patterns of health and industrialization in Japan. Sophia Twarog (United Nations, Geneva) spoke of Germany, with a focus on Württemberg. John Komlos (Munich) considered Austria, and the Netherlands were covered by J.W. Drukker (Groningen). Stephen Nicholas and Greg Whitwell (Melbourne), in joint work with Christine de Souza (Monash), concluded with the case of Australia.

The final conference, including paper presentations and comments by discussants, is scheduled for April 21-22, 1994, in Cambridge. At this gathering, Stanley Engerman (Rochester) will present research on the standard of living debate in international perspective.

## Conference Note: Economic Mobilization for World War II

by Hugh Rockoff, Rutgers University

'Economic Mobilization for World War II: The War Economy and Postwar Perspectives of Six Great Powers in International Comparison', a conference organized by Mark Harrison, was held at the University of Warwick, September 3-7, 1994. Seven papers were delivered. (1) 'Economic Mobilization for World War II: the United Kingdom', by Stephen Broadberry (Warwick) and Peter Howlett, discussed by Avner Offer. (2) 'From Ploughshares to Swords: The American Economy in World War II', by Hugh Rockoff (Rutgers), discussed by Nick Crafts. (3) 'How to Lose the War and Win the Peace: the Italian Economy in the Second World War', by Vera Zamagni, discussed by Rolf Petri. (4) 'The Soviet

Union', by Mark Harrison, discussed by Grigorii Khanin. (5) 'Germany', by Werner Abelshauser (Bielefeld), discussed by Richard Overy. (6) 'The Wartime Economy of Japan', by Akira Hara (Tokyo), discussed by Bryan Sadler. (7) 'Six Great Powers: an Overview', by Mark Harrison (Warwick), discussed by Vladimir Busygin.

Each paper provides a general overview of the mobilization and its consequences. After considerable discussion the group concluded that it is still too early to determine who won the War; further research is therefore warranted. Those interested should write the authors for copies of the papers.



**'Pioneers'** (continued from page 1)

And second, rarely have I met a man who can boast such a high level of consistency in his life. When I read *Time on the Cross*, I was pleased to note how little Bob had changed. In the Party, those who enforced the policies of the Central Committee were described as people who 'fought for the party line.' Recalling that, I said to him, 'Bob, you haven't changed one bit. In the old days you always fought for the party line; today, you are still fighting for the party line. The only difference is that for one party of revolutionaries – the Communists – you have substituted another – the cliometricians. But you're still fighting for the line.' He replied, 'But, Gene, you have to have a line.'

I shall not belabor what is a matter of record: Despite deep disagreements with *Time on the Cross* and the work of other econometric historians of slavery, I was not among the mean-spirited, green-with-envy, and just plain silly historians who failed to grasp the powerful methodological contributions that Bob and Stan and no few others were making, and who wished that cliometrics would somehow just go away. Social scientists might wonder about that preposterous reaction, looking for deep ideological, political, or disciplinary explanations. I am afraid, however, that there is much less there than meets the eye. The truth is depressingly simple. Most historians are terrified of economic theory and incompetent in mathematics. Invoking high principles of personal interest, they therefore declare it all to be nonsense. For it is hard to reply to arguments that are written in a language you cannot understand. Personally, I have not had that problem. It is true that I am weak in economic theory and never did get much past intermediate algebra, which I passed with a C-. But, being a creative chap, I solved the problem in a manner I warmly recommend to young historians who suffer from similar defects. I married a woman who is at home with economic theory and mathematics and is especially talented at rendering sight translations from foreign languages to a dim-witted husband.

To be sure, the claims of our early revolutionaries taxed the patience of historians who understood that history is at bottom an art and could never be transformed into an exact science. And no, cliometrics has not replaced all previous history and given us an infallible mode of interpretation. But, in the hands of its ablest practitioners, it has devised methods that compel all honest historians to quantify the quantifiable and to bring unprecedented rigor to the study of that range of human experience

amenable to measurement. The revolution has been real and irreversible, even if it has disappointed the youthful hopes of those who thought it could introduce the Kingdom of Heaven here on earth. Accordingly, the award of the Nobel Prize in Economics to Bob and Doug honored the economics profession for its contributions to history and the humanities. And it honored the discipline of history itself for having the wit to accept that gift and thereby render itself as scientifically accurate as humanly possible.

And the best of the cliometricians have transformed themselves in the process – none with greater success than Bob Fogel. *Time on the Cross*, notwithstanding its extraordinary contributions, was a product of economists who were working at history, whereas *Without Consent or Contract* revealed a full-fledged historian of extraordinary talent.

Cliometricians have long suffered from a debilitating tendency that few seem to be aware of. In general, if you show me an economist who writes history, I shall show you an economic determinist. Few economic historians embrace economic determinism as a philosophy of history. To the contrary, they vigorously deny the charge, protesting that only Marxists do that. But no matter how important the insights they bring to their work, they are more than likely to find an economic explanation for every important political, social and cultural change they discuss. Since history is an art, in reconstructing it, we all fall back on what we have going for us. Economists may, therefore, be pardoned for falling back on the economic theory they have going for them, especially since it often does explain much.

Yet the Good Book teaches that man does not live by bread alone. Historians have the special task of writing an integrated history of society. Yet social historians, for example, often ruin their enterprise by proceeding as if social history could be written without careful attention to economics, intellectual currents, and much else. They thus become neo-antiquarians who forget that social history emerged in the first place as an effort to deepen our understanding of the wellsprings and ramifications of political power. Without that, history becomes little more than romance and ideological projection.

Economic historians confront a similar danger. An analysis of the market may have something to offer the study of any society and much to offer the study of market societies. But the strength and role of the market cannot be taken

for granted and must be investigated empirically. That the market has dominated modern history cannot be gainsaid, and, accordingly, the application of economic theory is likely to explain much. But earlier societies present greater difficulties, and so do those modern societies which, while enmeshed in a world market, have resisted transformation into market societies.

Even in market societies – that is, societies in which the market mentality has come to dominate social and cultural life – stiff resistance may be expected. We proceed at high risk if we ignore the extent to which sensibilities rooted in religion, family, and tradition influence economic development or if we assume that such sensibilities will automatically follow the dictates of the market. Early cliometric work not only assumed that just about everything of importance could be measured with scientific precision, it assumed that economics itself could be understood as a historical prime-mover. Bob Fogel long ago warned historians that they were fooling themselves if they thought they could do without rigorous economic theory. He was certainly right. For every historian must do his work, whether in political, social, or intellectual history, with an eye on economic conditions, and, to cast that eye properly, he must be conscious of the assumptions he is bringing to his implicit account of the economy. Those historians who think they are doing without an economic theory are merely applying theories, probably long discredited, they do not understand.

Economists gave Bob a standing ovation for saying that. Unfortunately, they themselves have often relied on implicit psychological, anthropological, and other questionable theories quite as mindlessly as others have relied on questionable economic theories. I do not think that *Time on the Cross* was entirely free of that weakness. But Bob Fogel and Stan Engerman did not retreat into a bunker in the face of the severe criticism they got from many sides. They refined their calculations and defended the essentials of their scientific work, but they also took to heart a broad spectrum of criticism that transcended the technical problems.

At bottom, the issue came down, not to whether they had made their case for cliometric methods or for a variety of specific theses, but whether they had properly defined the limits within which cliometric methods could prove efficacious. In *Without Consent or Contract*, Bob gave us a lesson in how a serious intellectual works. Restating and elaborating the case for cliometrics, he wrote a splendid work of integrated history that gave economics its full due

without succumbing to economic determinism.

The personal accomplishment was formidable. I shall again risk poor taste by talking out of school. I had the sneaking suspicion that the Bob Fogel who did so much seminal work in his early days and then co-authored *Time on the Cross* had never given much thought to, say, the impact of religion, not to mention theology, on economic development and the broader course of history. To be frank, I doubted if he knew the difference between Calvinism and Arminianism or Protestantism and Catholicism, or thought for a moment that any of it mattered. Yet, in an astonishingly short time, he tooled himself up, as economists are wont to say, and gave us a book in which he demonstrated a sure hand in matters about which he had previously either been ignorant or insufficiently attentive. In the event, he analyzed the relation of economic thought and interests to religion with a sophistication any historian might be proud of. I may be dumb about economic theory and hopeless in mathematics, but I flatter myself that I know something about theology and the history of religion and can only say that Bob performed at a level any specialist should be proud to display.

Permit me to return to his determination to fight for the party line, for he has indeed been consistent in that fight. But what comes through, above all, is that the only party he has fought for has been the party of those committed to a disinterested search for truth. In any historical era, that would be admirable. But today it has a special significance. For we are living through what ideologues tendentiously call a postmodernist era, in which everyone is called upon to eschew a sense of objective reality for the promulgation of self-serving personal preferences. As Bob doubtless remembers, in the good old days we were fed on insidious claptrap called 'class truth.' Today, the idea of class truth has been broadened into something called 'positional' or 'situational' truth. Alas, the quest for 'class truth' went hand-in-hand with mass murder on an unprecedented scale and ended with the collapse of the societies and movements that preached it. And I, for one, do not doubt that the quest for 'positional truth' will end no differently unless we rally against it.

Bob Fogel has made some grave errors of judgment along the way, but he has refused to cling to them in the face of hard evidence. He does not claim to have an absolute truth. But, being a genuinely great scholar, he insists that on the day we abandon a commitment to an objectivity we know we can never fully obtain – on the day we

proclaim our opinions and preferences to be a truth as valid as any other – on that day we open the flood-gates to nihilism. And he pursues his course not by sanctimonious preaching but by doing the scientific work that alone can render the art of history morally and politically responsible. He insists that we have the courage to face the truth, no matter how deeply it offends our sensibilities, ruffles our precious feelings, or makes us uncomfortable by contributing to what whimpering children call 'a hostile environment.' In short, he calls upon us to recognize the tragic dimension of all human endeavor. It is that commitment to truth which remains Bob Fogel's party line. In congratulating our distinguished colleague for the great honor bestowed upon him, we would do well to honor ourselves in the one way worthy of his achievement – by following his example.

*Doug North: Quo Vadis?*

Richard A. Easterlin, University of Southern California

This occasion seems like the good old days of Clio, with Doug, Bob, Gene, and Lance at the table. It is an honor to be here and participate in this celebration of intellectual accomplishment.

My friendship with Doug goes back to our early days as research associates at the NBER in New York, and I have since followed his career with growing admiration. In what follows I shall try to note some of the reasons why. But winning a Nobel prize endows one's work with an aura of authority, and Doug is much too human to bear this burden with ease. It would not be true to Doug or to the traditions of cliometrics if all I did here was praise Caesar. Indeed, I suspect that Doug might even be getting a little bored by the outpouring of tributes (rare though they are in academia) and reminiscences. I hope I may be forgiven, therefore, if, along with hailing achievements, I enter a couple of demurrals. Doug would no doubt agree that this is in keeping with our historical relationship.

Doug's early work, on United States economic growth, was classic cliometrics. It established him as one of the pioneers in the field and a leading spokesperson for the cause. This work used traditional neoclassical economic theory to provide new insights into the causes of historical change. It also involved pioneering quantitative work in the measurement of the American balance of payments. Yet, despite the favorable response to this line of work, and Doug's rise to eminence as an economic historian, it is clear that Doug himself became increasingly dissatisfied with the cliometric approach. The basic reason for

this was its failure to solve many of the puzzles of economic growth and decline. In Doug's view this failure arose from the neglect of institutions, and this neglect stemmed, in turn, from the exclusion of institutions from traditional economic theory. One would be hard put to find much in mainstream economic theory about codes of conduct, conventions, norms of behavior, statute law, common law, or contracts, subjects that became of growing concern to Doug.

Over the past twenty-five years, Doug has turned, therefore, to the study of such institutions, and sought to integrate institutions with both economic theory and economic history. Initially, drawing on his theoretical heritage, he tried to construct an economic theory of institutions, in which institutional change is the result of relative price changes. This process would lead ever onward to the creation of more efficient institutions.

The real world persistence of inefficient institutions led him, however, gradually to abandon this view, and in 1981 in *Structure and Change in Economic History*, Doug devised a framework to account for the existence of inefficient institutions. Still left unanswered was the question of institutional *change*, the *persistence* of inefficient institutions. In subsequent work Doug has gone on to address this question, devising an analytical framework aimed at explaining institutional change and its interaction with economic performance. Doug sees this framework as a general one, applicable to all times and places.

All of Doug's work has been immensely influential. Doug is a gifted teacher, speaker, and writer, and, over the last two decades, has recruited a large number of students and colleagues to the study of institutions. Moreover, as his thinking has evolved, the disciplinary scope has widened. The present framework that Doug has devised goes well beyond the bounds of traditional economic theory, assimilating work on transactions costs, organization theory, preferences, and material from anthropology and political science. As the disciplinary scope has expanded, so too has Doug's influence in other disciplines. Indeed, my guess is that citations to Doug's work are as frequent – perhaps more frequent – among noneconomists than economists.

This is an impressive record of intellectual accomplishment. The systematic study of institutional change and its interplay with economic performance is, as Doug claims, an important and badly neglected subject. Doug's theoretical framework is ingenious and eclectic. In develop-

ing it he has been ever attentive to interpreting the real world and integrating into his framework the explanation of seemingly aberrant conditions.

As a general framework, devised to explain all situations, Doug's schema does not posit specific quantitative relationships – to revert to the jargon, it is not fully specified. To make it relevant to the real world, Doug offers certain judgments on what is important and what is not. It is with regard to some of these judgments, essentially *obiter dicta*, that one may have some doubts. For example, one will find in Doug's work repeated assertions that institutions and the associated incentive structure are the *most important* determinant of economic performance. To quote:

[C]ontrasting the institutional framework in countries such as the United States, England, France, Germany, and Japan with Third World countries or those in the historical past in advanced industrial countries makes clear that this institutional framework is the *critical key* to the relative success of economies, both cross-sectionally as well as through time. (North 1990, p. 69, emphasis added.)

Sometimes Doug is even more concrete:

[T]he inability of societies to develop effective, low cost enforcement of contracts is the *most important* source of both historical stagnation and contemporary underdevelopment in the Third World. (*ibid.*, p. 54, emphasis added.)

These are strong statements. One may agree that at a point in time the institutional framework and associated incentive structure is one important determinant of economic performance. But if economic *change* is the subject of study, isn't it possible that there may be other determinants at work within a given, *fixed*, incentive structure that significantly affect economic performance . . . forces such as ideological movements (nationalism, humanism, rationalism) or the evolution of scientific knowledge?

To take the growth of knowledge as an example, I am sure that Doug himself would no doubt agree that the evolution of knowledge has a certain internal dynamic of its own. In his own quest for knowledge of the sources of economic growth, Doug has been led from a resource-based staple theory of growth, rooted in traditional economic theory, to a multidisciplinary analysis of institutions and the incentive structure to which they give rise. One wonders whether the evolution of Doug's own thinking can be explained by incentive structures? (Well, the Nobel prize

in economics was established about mid-way in Doug's career, but I doubt that even he thought that the way to get this prize was by attacks on neoclassical growth theory.)

To make the point more specifically, consider the Mortality Revolution *vis-a-vis* the First Industrial Revolution, the latter marking the onset of what we now know as the epoch of modern economic growth. Contrary to what many economic historians assert, the Mortality Revolution is not simply a result of modern economic growth, but is a largely independent phenomenon. The Mortality Revolution starts about a century later than modern economic growth, spreads much more rapidly throughout the world, and occurs, in some cases, in countries that are stagnating economically. Moreover, unlike differences in economic development, which have widened since World War II, those in mortality have narrowed sharply as life expectancy in the Third World has converged toward that in the leader countries.

It seems possible that the later occurrence of the Mortality Revolution than modern economic growth lies in the peculiar evolution of scientific knowledge – that breakthroughs in public health and biomedical knowledge came later than those in understanding the physical world. Similarly, it has been argued that the technological breakthroughs associated with the Second Industrial Revolution as compared with the First, reflect the evolution of knowledge of the physical world from mechanics to chemistry and electricity (Parker 1984). Now it is possible that the evolution of scientific knowledge can be explained by the changing nature of the incentive structure. But another possibility is that the way scientific knowledge evolved was due to differences in the complexity of the problems posed in different disciplines, the ease with which the human mind might grasp the underlying phenomena, and the internal logic of scientific inquiry itself. If one is willing at least to tolerate these possibilities, then it seems less certain that if we are to understand economic performance, the incentive structure deserves quite as much priority as Doug would give to it.

Or, to turn to ideological influences, consider the growth of universal public schooling. Doug views formal schooling as important for economic growth, but he sees such schooling as due to the presence or absence of 'institutional incentives to invest in productive knowledge' (North 1990, p. 80). In the first part of the 19th century, the leading geographic areas in the establishment of universal schooling comprised a band extending from

Scandinavia across northwest Europe to the northern United States. It is possible that this group of areas shared in common the same institutional incentives to invest in productive knowledge. But one cannot help but be struck by the fact that these areas certainly shared in common distinctive ideological influences, such as Protestantism, which advocated formal schooling in the vernacular language in order to provide personal access to the Bible, and humanism, which preached the ultimate perfectability of human-kind, and thus fostered a view favorable to the spread of mass education. These areas also had central governments interested in political integration, and to these governments formal schooling was not only a productive economic investment, but an instrument of nationalism and political socialization.

So while I think Doug's emphasis on the systematic study of institutions and incentive structures in the study of history is salutary, and his work is a major contribution to such study, I wonder if there isn't equal need for the systematic study of ideology, and of the growth of knowledge itself? In response, Doug would probably say that he agrees, but what we need to study is how incentive structures and institutions shaped and were shaped by ideology and scientific knowledge. My point, however, is that understanding the growth of science and ideology may involve more than institutions and incentives. Human thought – scholarly and ideological – evolves within given institutional frameworks *via* mechanisms only dimly understood, and has important long run effects on economic performance.

It has been said that Doug has never stopped to answer his critics. This may be true, but it is also true that Doug has never stopped trying to understand reality, and to reassess his thinking in the light of reality. If I know Doug, even now at the peak of success, he will not rest on his laurels. In noting some possibilities that to my mind do not fit easily into his current thinking, my hope is to spur him to even greater accomplishment.

To Doug and Bob

Lance Davis, California Institute of Technology

I want to thank the chairman of the session and the program committee of the Social Science History Association for giving me an opportunity to take part in this session; more importantly, however, I really want to thank the Economics Nobel Prize Committee for making this session possible at all. Bob and Doug are old friends, and I am pleased for them, but I am even more pleased that

the discipline – economic history, that is – has finally received some, although some might say, belated, recognition. Enough of these nonsense formalities, we are here to honor Professors Fogel and North, so let's get on with the show!

There is no doubt that the two deserve the Prize, but I am enough of a historian to note what appears, at least on the surface, to be a certain anomaly in the award. Joint Nobel prizes are normally given for joint work, work on the same problem, or work on similar problems. Granted, both of today's honored guests are economic historians, but while one might imagine a joint prize to Hicks and Samuelson (both did, after all show that the standard micro model could be formalized), it would be difficult to imagine a joint prize to Samuelson and Solow, although both certainly qualify as laureates and as economic theorists. Why then did the committee choose to honor **jointly** one scholar who had worked on *business history, staples and regionalism, and transactions costs* and another who had worked on *railroads, slavery, and nutrition and health*? Both life-time efforts certainly deserve the prize, but why a joint prize? That is the puzzle that I lay before you tonight.

1. Both Bob and Doug were certainly early recruits to the so-called cliometric revolution – a revolution that can be dated from the publication of Al Conrad's and our missing friend John Meyer's 'The Economics of Slavery in the Ante-bellum South'. Doug arrived at the University of Washington from Berkeley in the early 1950s; and, although Bob did not take up a position at Rochester for almost another decade, his master's thesis – written before he had even started the graduate program at Hopkins – was already at the press. But length of service alone can hardly be the explanation.

2. Although they did not work on similar problems, perhaps there is something similar in their style.

- a. Even if the subjects chosen are different, do they focus on the same underlying issues? Perhaps, but certainly the similarity is not obvious. Bob's work has been largely focused on exploring historiographic problems – were railroads important to American growth, was slavery efficient and profitable, and how badly were slaves exploited – while Doug's central focus has been on issues of growth and development.

- b. Do they approach problems in the same manner? Hardly. As Claudia Goldin has recently written, Doug,

despite his lack of formal technical training is, at heart, a theorist, while Bob is the consummate empiricist. Moreover, differences in their style are readily apparent in the difference in the way they respond to critics. Bob has never let a critic go unanswered – even if the critic has died. Doug, on the other hand, in the words of Bill Parker, ‘has never stopped to answer a critic because he [Doug, that is] is in too much of a hurry to get on to his next mistake’.

c. Do they organize their research in a similar fashion? Again, hardly. Doug has had frequent co-authors, but he is actually the *doyen* of cottage industry research. Small scale, hands on, and the entire enterprise would probably not qualify as one of Ken Sokoloff’s artisanal shops, let alone a modern factory. In fact, I think the only research assistant that Doug has ever had, in a career that has spanned almost half a century, was Matt Simon. Bob, on the other hand, works in the Henry Ford mold – mass production, economies of scale, and marching legions of faceless research assistants – I met my wife, in fact, because she had worked on the slavery project.

No. Clearly we must look elsewhere for the thread that binds these two laureates together. Careful research suggests that the link (aside from their friendship, of course) lies in their late childhood training. The Secretary of the Economics Nobel Committee has been criticized for his bias in favor of conservative economists; and, at first glance, both Bob and Doug appear to be ‘politically correct’ choices from his point of view. First glances, however, do not capture the entire story; and the Secretary might have been shocked had he examined their backgrounds more carefully.

Although both Doug and Bob were the sons of relatively well-to-do parents, both early became aware of the fact that most Americans were not as privileged as they were. In Doug’s case it was probably his experience in the merchant marine during World War II, tempered by his association with the denizens of Room 3 in South Hall at UC Berkeley that produced the young leftish professor who joined the economics Department at the U. of W. in 1951. I am less certain of the facts, but I would guess that it was the association with the union movement in New York as well as his association with his wife Enid and our colleague on this panel, Gene Genovese, that led Bob into the CYO and to a place on *Collier’s* list of the ten most dangerous men in the U.S.

In both cases, their commitment to a Marxist interpreta-

tion of history did not survive a swift and almost religious neo-classical conversion. For Doug, it was not a meeting with the angel Moroni in upstate New York, but a year of living next door to Don Gordon in the basement of Savory Hall supplemented, probably needlessly I might add, by a year at the Bureau with Ken Buckley. For Bob, I am not certain; but it may well have been associated with his entry into graduate school at Columbia. It certainly occurred before he got to Hopkins and fell under the sway of Simon Kuznets. His book on the Union Pacific, for example, reflects the intellectual arguments of Carter Goodrich but owes nothing to the likes of Hobsbawm or Hobson.

Each, however, carried something away from that period of their lives. For Doug, it was a mindset – a willingness to go beyond, to break out of the existing economic paradigm. For him economic determinism had briefly replaced the economists’ neo-classical paradigm; but, far from narrowing his perspective, that flirtation suggested the possibility of a much broader intellectual framework. Ever since I have known him – and that time also goes back to the years in the basement of Savory Hall – Doug has always recognized that ideology, beliefs and culture matter, that markets do not always exist, and that, when they do, they are not always perfect, that institutions constrain market behavior, and that we will never be able to understand growth and development unless we understand how ideology, beliefs and culture are determined. Equally importantly, he has been willing to make these positions known and has been prepared to let the doubters take their shots. In so doing, for four decades he has set a large part of the agenda for the discipline; and two generations of scholars have made their reputations by attempting to disprove his work. His willingness to breach the paradigm has made him a catalyst for the profession.

For Bob it was something different, something rooted, perhaps, in his rejection of communist philosophy. Since he entered the lists of economic history, Bob has always refused to accept any piece of stylized ‘truth’ no matter how prestigious the descriptors of the man peddling the particular jar of snake oil. The *Credit Mobilier* was a public counterpart of the Great Train Robbery; railroads played a crucial role in American development; slavery was immoral therefore it must have been unprofitable. ‘Ha!’ and ‘Bah, Humbug!’ to them all!

In his first year at graduate school Bob learned that, if you give an economist free choice of assumptions, he can get

any conclusions that he likes; and he (Bob, that is) had long known that historians frequently did not even need to choose their assumptions before arriving at any conclusion that pleased them. Bob too has been willing to stand up and take the shots – see *Reckoning with Slavery*, for example – and a substantial portion of the profession have attempted to make their reputations by attacking his work. Thus far, they have not proved particularly successful.

Given the early common link in their careers, it may, perhaps, not be surprising that, despite the fact that their research began on very different paths, over time those paths have tended to converge, although that convergence may be asymptotic. Looking at each career we see:

#### 1. For Doug:

a. His has been a career of theoretical studies in a historical background – studies largely directed toward policy issues. We should not, however, forget that the 1799-1860 U.S. Balance of Payments data that we all use – and the data that are reproduced in *Historical Statistics*, admittedly with some *caveats* – are his.

b. Doug's first published work – the product of his thesis research – dealt with life insurance companies before the Armstrong investigation of 1905. Although it reflects some of his Room 3 anti-business biases, it is really much more. Tontine and industrial insurance come under attack as does the aggressive sales tactics of Henry B. Hyde. There was certainly corporate dissembling and, North argues, outright fraud and corruption as well; but Doug saw a positive side to the industry as well. North applauds the role of the industry in helping to solve the nation's capital accumulation problem, and he even admits that there are potential benefits from well-designed and administered tontine and industrial policies.

c. In *The Economic Growth of the United States 1790 to 1860*, North turned to Harold Innes's thesis – a staple of Canadian historiography – to provide an explanation of regional growth in the United States. When published in 1961, the book was on the cutting edge of modern Cliometrics (although at that time, Stan Reiter had still not coined the word, and we labored under the awkward title of the New Economic History). North combined a model with data to provide an explanation of the nation's growth. Although subsequent work has indicated that he was only partly correct, that result is not really important. What is important is that he had attempted to provide a model of the process of economic growth; certainly not

a perfect model; but a model that was substantially better than those offered by the rest of the profession. Moreover, the work triggered a substantial body of new studies that have themselves contributed immeasurably to our understanding of the path of American development.

d. Over the past two and a half decades North has taken on an even larger problem – or as one only partially sympathetic critic has argued, 'he has been jousting with larger windmills'. The question: what has been the role institutional invention and innovation (or, as Doug prefers, changes in transactions costs) in the process of economic growth? Initially his focus was the United States (Davis and North, 1971), but the background was enlarged to include the entire western world in North and Thomas's *The Rise of the Western World*. The argument has been largely theoretical with examples drawn from history, but he has made serious efforts to provide quantitative empirical support; and, as we all know, the North and Wallis paper is enshrined in volume 51. In this case again, although the work itself is important, its total impact has been far greater than the direct contributions of North and his co-authors. Doug has been responsible for the birth and growth of the 'New Institutional History', and, this time again, a score of young economic historians have attempted to become famous by, on the one hand, sniping at and, on the other, by attempting to substantiate and expand his findings. Even as we speak, the great debate over whether institutions matter rages on – to say nothing of cluttering up – The Cliometric Society's electronic bulletin board.

e. Finally, in the recent past Doug has gotten his hands really dirty in public policy issues. Since the collapse of the Wall, he has provided advice to Eastern European countries on appropriate institutional structures; and there is every indication that Doug's advice to the future business and government leaders of the Czech Republic (on the need for well-defined property rights, for example) will be more effective than the advice given to the leaders of than Poland and Russia by much more famous members of the international economic jet set.

#### 2. For Bob:

a. For Bob, although economic theory has always guided his research, until recently there have been few deliberate attempts to push the boundaries of that theory. It has been the historiographic issues themselves that have been the central focus of his research.

b. As everyone recognizes, Bob's first target was the



then-current historiography about the nation's railroads. His Master's thesis on the *Credit Moblier* and the Union Pacific scandal was a masterpiece in the use of theory to focus the argument in order that the correct quantitative evidence could be adduced to answer the question: were the profits too large, given the fact that the UP was a risky enterprise. Less noted, perhaps, but probably of near equal importance, were his conclusions about the nature of the need for government subsidies.

From one railroad to all railroads – this was, after all, Fogel 'on and off the rails'. Here again we find the nexus of a neat use of a formal model (in this case a linear programming formulation) to focus the argument, very clever construction of upper and lower bounds on his parameter estimates, and the construction of a substantial data set (he read 19th-century cookbooks to find how much flour was used in a loaf of bread). Taken together they produced a set of compelling conclusions – even if Bob never did solve the linear programming problem. First, given the geography of the US, railroads were probably not crucial to American development; and, second, if you want to have a theory of American growth that focuses on backward and forward linkages, you would be better served to build one based on iron stoves rather than railroads.

Not only have Bob's findings rewritten a chapter in American history, but they also triggered a number of comparative studies of the social savings from railroads in other countries; and, as one might expect, it appears that the levels of social savings depend on the availability of water transport. In Mexico, for example, the railroads' contribution was substantial; and in Brazil social savings fell somewhere between the U.S. and Mexican cases.

c. Having disposed of one myth, Bob turned to an even more visible pillar of American historiography – slavery. The earlier work of Conrad and Meyer, Yasuba and others had fairly well convinced the economics half of the economic history profession that slavery was profitable and the Civil War was not an unnecessary police action, if the goal of policy was to remove that moral blight from America's collective conscience. Bob and Stan, however, went much farther. They argued that the real income of slaves was relatively high if the standard was free white workers in the North, that slave families were relatively stable, that slaves were not too badly treated, and that the rate of exploitation was less than the level imposed on most Americans today by the federal income tax.

If the intellectual reaction to *Railroads* can be termed violent, the reaction to (Happy) *Time on the Cross* looked like World War III. Bob found himself arrayed against economists, historians, sociologists, psychologists, and political scientists – the full array of social scientists. It is, of course, a comment on Bob's something-less-than-svelte figure that he could alone be arrayed against that phalanx of academics. That comment aside, however, now, some 20 years after the opening guns of the campaign, Bob's views have come to be generally accepted – of course the fact that many of his critics have retired or died may have contributed to that result.

d. Finally, for the past decade, or more, Bob and his minions – minions drawn not only from economics and economic history, but from demography, medicine, and public health as well – have launched an even more massive project: a study of nutrition and health and its relation to economic development – development not only in the U.S. but in other industrialized countries as well. This time, however, the main focus has shifted. While the work will undoubtedly dispel some myths about the growth of welfare in the American past, it is really a study with immense policy implications – implications that may, in the long run, be more important for the underdeveloped than for the developed world. Only time will tell whether the promise will be borne out, but the recent \$5 million grant from the NIH and the NSF suggests that there are people out there willing to bet someone's (if not their own) money that it will.

At this point in their careers, Doug's research continues to focus on policy issues, but the work has become more applied; Bob too has begun to direct his interests to more policy relevant matters. Thus their two research trajectories are moving closer together. As an aside, it is also interesting to note that Bob, all evidence to the contrary, has, deep in his heart, shared with Doug a belief in the importance of beliefs and culture – see, for example his work on the great awakening.

One thing is certain. One of my colleague's comments on one recent Nobel laureate ['He's over the hill and it wasn't much of a hill to begin with'] obviously was not directed toward this dynamic duo. The intellectual mountains that both have been climbing are at least as high as Mount McKinley, neither has yet gotten to the top, but there is no evidence that the pace of their ascent has slowed down – quite the opposite, in fact. I am honored to have had a chance to make these remarks.

*The Nobel Laureates*

Robert Gallman, Univ. of North Carolina, Chapel Hill

Since I have had my say about these two on more than one occasion, there are those in the audience – not to mention the honorees themselves – who will have previously heard parts of what I plan to say. I give you my apologies. But short of inventing some new stories about them – which seems inappropriate – there is not much else I can do about it, except to suggest that you think of something else until I am through, which will be soon.

Sam asked Lance and me how we first met Doug and Bob. I take this curiosity on his part as a license to us to reminisce a bit – at least to the extent of telling you in a few words what this distinguished pair was like in their salad days.

Bob and I were undergraduates at Cornell together and I seem to recall seeing him across the room in Fred Kahn's industrial organization class. Otherwise, we traveled in different circles. Bob was the president of American Youth for Democracy and a founder of the Marxist discussion group, whereas I was the editor and proprietor of a very bad campus magazine which couldn't make up its mind as to whether it was the campus version of *Esquire*, or *Life*, or *The New Yorker*. Our one acquaintance held in common was Paul Robeson, Junior – Bob and Paul went to political meetings together, and Paul and I played basketball together. It would not have taken a very astute observer to identify which of us, Bob or I, was the more serious – the more likely to win a Nobel.

One image of Bob remains clearly in my mind from that time. I was sitting in Willard Straight Hall, desperately trying to sell the magazine, to make enough money to pay the printer's bill – otherwise, the money would have to come from my own pocket, which, while not empty, did not contain nearly enough to keep the printer happy. The front door to the Hall opened, and in swept Bob. He wore an elegant Chesterfield coat with a velvet collar, and he was surrounded by friends and well-wishers, all off to a meeting of the Marxist Discussion group. I regret to say that none of them bought a magazine from me.

Bob more than made up for that failure on the next meeting between us that I remember. That was in Lance Davis's living room in West Lafayette, on the occasion of the first Cliometrics meeting. I was taken from the airplane to Lance's house, and when I walked in, Bob saw me and walked over to greet me. His first words were: 'I

have read your dissertation.' Up until then I had believed that no one, except my dissertation supervisor, had actually looked at the thing. With the first words he spoke to me, Bob won my undying affection and loyalty.

I met Doug in the fall of 1956. I was spending the year at Hopkins, and Doug and Ken Buckley came down from the NBER, where they were in residence for the year, to visit Simon Kuznets and tell him what they were up to. Simon asked me to sit in on the talk. I should tell you that my conversations with Kuznets were at that time handicapped – from my side – by my sense that when I spoke with him I was talking with God. Bob tells me he had the same difficulty for a time. But not Doug. He breezed into Kuznets's office with all the confidence – indeed, insouciance – of Professor Harold Hill, about to sell a set of band instruments to River City. He was writing a book, he said, and he told us about it with infectious enthusiasm and excitement. The experience was unforgettable.

We also met again at the first meeting of Cliometrics, and at a later meeting of that group I observed an unusual – possibly unique – event. It has been said that Doug has never responded to a critic. That is not true. He once responded to me. The occasion was a Cliometrics meeting in which I was giving a paper on the self-sufficiency of the antebellum cotton economy, a paper that called into question an important element of Doug's growth model – the model in that exciting book he had described in Simon Kuznets's office in 1956. The paper dealt with earthy matters – corn, pigs, things like that – and Doug's response was properly earthy. It was long on emotion, but short on analytical elegance. In brief, he compared my paper – to the paper's disadvantage, as I recall – to the droppings one might find on the floor of a pig sty.

So much for their salad days – at least as I observed them. What can we say of their careers and personal qualities? First and foremost, they have been leaders in the modernization of an important branch of history and of economics. I think the process of modernization has gone through two clear stages. In the early stage, exponents of the new economic history urged the collection, the systematic assembly, and the analysis of quantitative evidence; the analysis, it was held, should make use of standard economic theory. Doug's papers on the balance of payments and on shipping costs exemplified the best of this kind of work. His book on U.S. economic growth, 1790-1860, was a different kettle of fish. The difference is reflected in the fact that historians, who had many reservations

about the early work in the new economic history, seem to have liked Doug's book from the beginning, and their patronage has kept it in print to this day – thirty-three years and still counting. Why this popularity I do not know, with any degree of certainty, but I believe it stemmed from three features of the work. The most important is that in this book Doug attempted an over-arching interpretation of American economic growth, of a style familiar to historians of that day. Second, unlike other pieces of the new economic history, the quantitative evidence in that book was largely of an illustrative character – it was not closely analyzed, econometrically. Finally, Doug paid some attention to cultural and social features of the economic regions he was treating. This was not exclusively a book in economics. So the book was not off-putting to historians, even those who were not of the new economic history persuasion. Nonetheless, it was a piece of new economic history. It contained a clear model of economic growth, and the evidence assembled was intended to show that the model represented an appropriate view of American history. In a way, North's first book is more representative of the second stage in the modernization of economic history, than it is of the first. It represents a closer merger of economics and the history of change over time than do most of the other works in the first stage of Cliometrics.

Bob's early books on railroads and his volumes with Stan Engerman on slavery, while they dealt with major empirical questions, were equally important for raising fundamental methodological issues – and raising them in so provocative a way that the professions – history and economics – had to pay attention. It seems to me that the provocativeness reflects a fundamental feature of Bob's personality and character. He has, in a sense, grasped us all by our shirt fronts and made us think about and debate the issues in which he was interested. He set the agenda not only for himself and those who agreed with him, but also for those who disagreed with him. Lance, in his remarks, has pointed out that Bob is an indefatigable debater – if you begin with him, count on it: he *will* outlast you. What is so interesting about him, however, is not his boundless strength and his use of it in debate, but that he always finds ways of drawing worthy opponents into serious exchanges with him – opponents who seem quite willing to drop whatever they have been up to in order to devote weeks, months, even years to arguing with Bob about his work. Bob, then, recruits his opponents in the search for answers to questions he finds compelling.

The second stage of the modernization of economic his-

tory, it seems to me, is as yet incomplete. It has involved a closer attention, than in the first stage, to the relationships between economic history and other elements of history, as well as a direct concern with economic changes across historical time. Compare the first volume of *Without Consent or Contract* with *Time on the Cross*. The later volume is much more clearly a work of history than is the earlier. Bob's extraordinary project on mortality and morbidity is involving him in analyses concerned with changes across, not years or decades, but centuries. This project has also led him to deal more directly than ever before with questions that abut closely on policy issues.

Doug's early book on American economic growth presaged a series of books that have focused on long-period change. This work began with a volume done with Lance Davis, on institutions, a subject that has continued to engage him, first in a book done with Bob Thomas, and subsequently in volumes produced alone. He has been drawn into a consideration of transactions costs, into political theory and political history, and now, most recently, into issues of cognitive theory. The body of his work is coherent, and shows a persistent search for the roots of long period change in human societies. He continues to entertain new ideas and new approaches to the issues with which he is concerned.

Bob and Doug have not only led in the modernization of economic history, they have also helped to legitimize the field to the neighbors in economics and history. Both have carried the word actively to the neighbors. The major role Bob has played in this organization is part of the story; Doug's continuing conversations with economic theorists is another part.

Finally, one should not ignore the fact that, in addition to their research prowess, both of these two have been great teachers. Doug's teaching performance was recently recognized by the Economic History Association, which awarded him the first Jonathan R. T. Hughes teaching prize. The best evidence, however, lies in the quality of the work of the students of Bob and Doug, and the obvious influence that Doug and Bob have had and continue to have on their students.

So much for their work. Lance, in his talk, speaks of the willingness of each of these two to say what they have to say and to accept the intellectual knocks that often come to those who speak their minds. They have exhibited courage and a certain toughness. In something I

ited courage and a certain toughness. In something I wrote about them I mentioned the iron will that seems to have driven them to do their work, even in circumstances that have not always been favorable. They have taken their knocks and then forged ahead. Keynes, in treating the intellectual friendship between Malthus and Ricardo points out their continuing disagreements – in his last letter to Malthus, Ricardo says: ‘We will never agree, but we will always be friends.’ Bob and Doug have had the ability to fight fair and, so far as I can tell, they carry no grudges. To them, as to Ricardo and Malthus, the important thing seems to be to seek the truth. They have been, then, good intellectual warriors; each also has the ability to extend warm friendship. Both have picked up my spirits more than once, and my guess is that my name is legion.

After the tributes, Doug rose and thanked the four speakers on behalf of himself and Bob Fogel, who was ill and unable to attend. Williamson thanked everyone for coming, thanked Robert Whaples for organizing the session – and then all adjourned to the bar for drinks.

#### References

- Davis, Lance and Douglass C. North, 1971. *Institutional Change and American Economic Growth*, New York: Cambridge University Press.
- North, Douglass C., 1990. *Institutions, Institutional Change and Economic Performance*, New York: Cambridge University Press.
- Parker, William N., 1984. *Europe, America, and the Wider World, Volume 1: Europe and the World Economy*, Cambridge: Cambridge University Press.

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## Call for Papers 1995 E.H.A. Meetings September 8 – 10, 1995

The Economic History Association will hold its annual meetings at the Inter-Continental Hotel, Chicago, Illinois. The theme of this year's meeting is Evolution of the Global Economy.

The Program Committee Co-Chairs Peter Lindert and Kenneth Snowden, and Steve Haber and Christy Romer, invite proposals for papers and suggestions for sessions. Please send two copies of abstracts of papers or session suggestions to each Co-Chair. For full consideration, proposals must be received by **January 24, 1995**. Notices of acceptance will be sent to authors by **March 12, 1995**.

Peter H. Lindert, EHA Program Co-Chair  
Agricultural History Center  
University of California-Davis  
Davis CA 95616

Kenneth Snowden, EHA Program Co-chair  
Department of Economics  
University of North Carolina-Greensboro  
Greensboro NC 27514-5001

Persons expecting to receive their Ph.D.s in the academic year 1994-95 are invited to apply for inclusion in the dissertation session. Convenors are Winifred Rothenberg of Tufts University and Cormac Ó Gráda of University College, Dublin. Dissertations on U.S. or Canadian history chosen for presentation at the session are finalists for the Allen Nevins Prize. Dissertations on non-North American history chosen for presentation are finalists for the Alexander Gerschenkron Prize.

Applicants must send two copies of completed dissertations by **June 10, 1995** to

Winifred Rothenberg  
Department of Economics  
Tufts University,  
Medford, MA 02155

**Rosenberg Interview** (continued from page 6)

degree of political stability, as well as a society which had legal institutions and protections of property and contract that were more advanced and more developed than those of other countries. I would take it that one of the most distinctive features of technological innovation if you look at it *ex ante*, not *ex post*, is that it involves decision making under very high degrees of uncertainty. When on the top of the inherent uncertainty you pile on political and legal uncertainties, then it seems to me that the willingness of people to make long-term investments in highly uncertain projects is going to be looked at in a very different way. Perhaps economists don't have a great deal to say about political stability in and of itself, but I think an essential consideration is that this is a decisive factor in shaping the environment in which people make decisions that will, or may, lead to new technology.

Now, at the same time, I would characterize much of my own work as really being in a certain sense conceptual in nature. How do you think about technological innovation? What is it? How does it differ in different sectors of the economy? These questions certainly do not lend themselves readily to quantitative analysis. But yet, at the same time, some of the most important work in the field – again I would cite in particular the work of a guy like Jack Schmookler – used quantitative data to shed some very powerful light on what drives inventive activity, both with respect to its rate, its timing and its direction, and I have to think that Schmookler was a very undervalued member of this profession. I think he had a very deep understanding of what drives technical change.

I do think it's short sighted, in spite of what I've just said about Schmookler's importance and his undervaluation, to concentrate your thinking purely upon activities for which there are readily available measures. And technological change is a peculiarly difficult subject to get a hand hold on directly. So we treat it as a residual or we take proxies like patent data; unfortunately there are no really good data bases on which to draw, and furthermore it's a very subtle thing.

One reason we have so much difficulty in modeling technological change is that it isn't just one thing, it's not even just one big thing, it's a great many small things. And a lot of my own work has been an attempt to identify what some of those small things are, what some of the more subtle interaction effects are. It's one thing to identify these things; it's another matter to find good proxies or surrogates for them. I'm content to say simply

that that has not been my department, but at the same time I think the concerns that have been central to my own work are at least as important as the sort of things that my cliometric colleagues work with. I would make no claim beyond that. That is a very substantial claim.

**Let's go back to Marx. You've taught history of thought over the years. How important is it still to study Marx, say, for understanding technological change, or, for that matter, to study other figures in the history of economics? Who are some of the essential characters? What role should studying them have in an economist's education – in particular an economic historian's education?**

Well, I would say everybody should read chapters 13 through 15 of the first volume of *Capital*, for a variety of reasons. But let me back up. On the question of Marx: Marx was certainly the first major figure in economics who placed the phenomenon of technical change in the very center of his economic analysis. Because Marx was attempting to identify what he called the 'laws of motion' of capitalism, I would argue that it is a very common mistake to think of Marx as a technological determinist. I think the real Karl Marx is without question an economic determinist. But he is an economic determinist where economic forces shape technology and create a high degree of technological dynamism. I think Marx captured a very important part of the picture of how capitalism shapes this rapid rate of technological change. And, of course, Marx regarded capitalism as being unique in history as the only social system where the economic interests of the ruling class are apparently tied, not to conservation of the old mode of production, but to change. I think Marx offered, and still does, some very profound insights into the performance of technology.

The problem is, of course, that Marx comes loaded with an awful lot of other freight. Let me tell a little story here. Two years ago I wrote an article for *Scientific American*. In *Scientific American*, the very last page is a one-page essay every month. The editor was out here, I had lunch with him, and made some references to Marx, and he asked me to write a one-page essay, which I did. A large part of what I was saying was, 'Look, whatever is going on in Eastern Europe today, Marx had very little to do either with the particular form that the social systems took there or with their present collapse.' I went on to argue that Marx still needs to be read as certainly the most important economic historian of the 19th century, which is a description of him that I would seriously make. The

editors of *Scientific American* gave my little article the title, 'Marx wasn't all wrong'. Now, as it happens, it was an excellent title. It was not my idea, but it did in fact capture the essence of what I was saying. Marx was not all wrong. In fact, when you think about it, it is hard for anybody to be all wrong. Even a broken clock is correct twice a day. You'd be amazed at the crank mail that I got from that article because there was a large number of people out there for whom Marx *had* to be all wrong.

I teach courses where I still require the reading of Marx. In terms of my own priorities I think that is an important answer. But you know, if you are as preoccupied as I am with technological change, you're kind of reluctant to lose Marx because there's not an awful lot out there. You've got a page [in Smith] on the pin factory and you've got Charles Babbage. You've got a little bit in John Stuart Mill, but then you've got to jump to the 20th century. You've got to look at Schumpeter and Kuznets and Schmookler and Abramovitz. I don't even know what to do with Solow, because the so-called neoclassical growth model is peculiar in its handling of technology. In the Solow model technological change is, of course, totally exogenous. It appears as manna from heaven. That's Solow's metaphor, not mine.

**Maybe we should take a look at what you've been up to lately. What intrigues me about your career to this point is that you still mix a lot of things that I don't think very many people do: some history of thought and some economic history but also contemporary technological issues. So would you say a bit about that context of your work and where it's headed?**

All right, let me say first of all that much of the context of my work has been for some time in the 20th century. I would also add that the 20th century is 94% history. Everything I do, I do by looking at the phenomena that interest me in the context of history. I guess I differ a lot from some people, many economists, who say, I always think rather patronizingly, economic history is very useful because we go out and test our theories. My view is: what in the world is economics about if it isn't about a process of economic change that takes place over historical time, and it seems to me that the fundamental responsibility of the social scientist is basically to deal with the question, 'How did we get here?' How did we get to this present juncture in human and in social affairs? And so, a lot of what I've done, that might qualify as economic history, was done simply because I felt that to understand almost any phenomenon one has

to understand it in terms of its history. A fancy term that we use now – we call that path-dependence.

As for my present work, much of it deals with the interface between science and technology. Some of it very specifically is concerned with university research as it affects technological advance in industry. I'm very much interested in the economics of science. I believe that not only is technology largely shaped by economic forces but (we touched on this earlier) I think an awful lot of what goes on in the scientific world is shaped by economic forces as well. I'm surprised, given the imperialistic tendencies of modern economics – you know, look at work of somebody like Gary Becker who has so much expanded the range of problems that can be explained in economic terms – I am surprised that it has taken the economics profession as long as it has to look at science, scientific research, as if it were an economic enterprise or an enterprise which we are willing to finance, at least in considerable measure, because we anticipate there will be economic payoffs drawn from it.

So a lot of my work is concerned with those kinds of interactions between the research community and industrial innovation. At the same time, I've gotten rather heavily into technological change in medicine which, as I hear pointed out at least once a day, accounts for one-seventh of our GNP and will very likely soon be more. And the forces that shape technical change in medicine are, believe me, quite unlike the forces that shape technical change anywhere else in the economy. It is a sector where, until very recently, budgetary constraints were quite simply not important. You know, there is another sector where one can say that that has been so until recently, and that of course is the military sector. But even there at least, there is talk about increasingly tight budgetary constraints.

I'm particularly interested in looking at how useful knowledge grows. In a way, that is my ultimate interest, looking at ways different bodies become institutionalized, and how they form new disciplines. The 20th century has given birth to chemical engineering, aeronautical engineering, computer science, the development of electricity – all that began in the late 19th century. These bodies of knowledge then become institutionalized at universities. Universities hire people to teach in these subjects, to do research in these subjects, to certify that students, after they've spent four years in an institution, are in a certain sense professionally qualified.

I try to look at the development of new engineering and applied science disciplines as part of the process by which useful knowledge grows and becomes institutionalized, markets get formed – you don't begin teaching electrical engineering, after all, until you know there are things that people who have EE stamped on their foreheads can go out and do, until there is a market. So one question I'm asking is, how do markets get formed for new professionals? And does that process work very differently in different countries? You almost intuitively know that it does. If you look at the engineering disciplines in Great Britain, and the way they failed to become institutionalized at the great universities there, how does that in turn affect economic performance? Can we get some important insights on the performance of national economies from that particular angle of vision?

I also occasionally go back and write something in the history of economics, Babbage being the most recent. You think of Babbage as the father of the computer, and that's a fair enough label for him, but he was also, I think, a very considerable economist in the specific sense of trying to understand what the Industrial Revolution was all about. And let me come full circle here by pointing out that although Babbage has not been widely read by economists, Babbage had an enormous influence, simply an enormous intellectual influence, upon two people in the 19th century: John Stuart Mill and Karl Marx. If you read either of those people, you will find that they quote shamelessly, page after page, from Babbage and I think with good reason.

**Well, I always thought his explanation of why the division of labor was efficient was better than all three of Smith's put together.**

You're damn right it was. You're damn right it was. And I've written a paper making exactly that point. It's very curious, but if you look at two of the most distinguished books in the history of economics of the 20th century, which is to say Schumpeter's *History of Economic Analysis* and Mark Blaug's *Economic Theory in Retrospect*, they both have one peculiar feature in common with respect to Babbage: they both describe his book as being a remarkable work and both of them devote no more than one sentence to it. But Babbage was one of the great polymaths that England tends to throw up periodically. I mean, he was a genius. And what he understood about the division of labor was in a way very simple. Simple in a sense that many profound observations are simple once somebody has finally made them.

**Let me raise one last issue: technology policy. Does history tell us anything ... you know, since the Clinton years have started, things like industrial and technology policy are back in the public discourse. Any comments you want to make?**

I guess the short answer is 'No'. But don't quote me on that.

*Interviewer's note: I would like to thank Sharon Squyres for transcribing the audiotape of the interview.*

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## Call for Papers

### Canadian Economic History Conference

#### October 13 - 14, 1995

The 20th Conference on the Use of Quantitative Methods in Canadian Economic History will be held at the University of Western Ontario. The conference theme is Markets and Institutions.

Knick Harley and Neil Quigley invite expressions of interest in presenting papers at this conference. Papers addressing the conference theme, and any papers in Canadian economic history, are welcome. Please send an abstract of up to 300 words before **May 15, 1995**. Authors of accepted papers should plan to submit complete texts by **September 15, 1995**.

For further information contact  
Neil Quigley, Programme Co-ordinator  
Department of Economics  
University of Western Ontario  
London, Ontario N6A 5C2 Canada  
Quigley@sscl.uwo.ca



## Summary Budget Report

Fiscal year 7/1/93 - 6/30/94

(Includes carry-over balances from last fiscal year and known pending debts)

### Income:

#### Membership Revenue:

Dues: 5,848.00  
 EEH: 12,284.00  
 EHES/Other: 982.00  
 Total: 19,114.00

#### Other revenue:

Endowment fund interest: 196.19  
 Newsletter ads: 111.95  
 Miami University Subsidy: 2,000.00  
 Publishing: 1,212.62  
 Cost sharing: 836.14  
 Total: 4,356.90

**Total Income: 23,470.90**

### Expenses:

#### Membership:

'Bad debts' (credit cards): 186.00  
 Credit-card processing fees: 157.56  
 Membership Mailings: 364.12  
 Academic Press: 13,991.00

EHES: 720.00  
 Hospitality: 816.26  
 Total: 16,234.94

#### Newsletters (two of three):

Printing: 1,302.32  
 Copying/Postage/Other: 611.22  
 Total: 1,913.54

#### Office:

Phone and fax: 597.35  
 Administrative Assistant: 4,830.00  
 Student assistants: 311.31  
 Total: 5,738.66

Publishing:  
 Printing: 1,069.00  
 Other: 477.73  
 Total: 1,546.73

**Total Expenses: 23,887.14**

**End of Year Balance: -416.24**

### Have you forgotten a member's e-mail address?

A reminder to those who are looking for the e-mail address of a member of The Cliometric Society: We update the Membership Directory every month, so you can use it to search for members' addresses, phone numbers and research interests. An easier method is to use the 'forwarding service' we provide. You can contact *all* members who have e-mail addresses in our directory by addressing messages to their *first initial, last name@cs.muohio.edu*. For example, Clio Muse's address is **cmuse@cs.muohio.edu**

## Call for Nominations The Jonathan Hughes Prize

The Economic History Association has established an annual award in memory of Jonathan Hughes to recognize excellence in teaching economic history. The winner of the Hughes Prize for 1995, which includes a \$1,000 cash award, will be selected by the EHA Committee on Education and Teaching. The Committee invites nominations for the 1995 Hughes Prize. Anyone may submit a nomination, and teachers of economic history at any level of instruction are eligible for the prize.

The basic requirement for nomination is a statement of how the nominee has exemplified excellence in the teaching of economic history. The Committee will select a list of finalists based on the cogency of the nomination statements submitted. Finalists for 1995 may include nominees for the 1994 award.

It is in the nature of courses in economic history, especially at the undergraduate level, that a large proportion of students completing these courses do not do advanced work in economic history or maintain contact with the profession. For this reason, the EHA Committee on Education and Teaching is eager to ensure broad circulation of the call for nominations for the Hughes Prize. In particular we wish to enable teachers whose students rarely continue their studies in the field to have an opportunity to be nominated for the Hughes Prize. Accordingly, the Committee requests that those who see this announcement disseminate it as widely as possible within their own institutions.

Queries may be addressed to Mary Schweitzer at any time. Three copies of nominating statements should be sent before March 20, 1995, to

Professor Mary Schweitzer  
Department of History  
Villanova University  
Villanova, PA 19085

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## Review of List Commands

### Information:

For expanded descriptions of all lists, send the e-mail message

*info lists*  
to **lists@cs.muohio.edu**

For more detailed information about a specific list, send the message

*info <name of list>*  
[for example, *info econhist*]  
to **lists@cs.muohio.edu**

### Subscription:

To subscribe to a list, send the one-line message

*subscribe <name of list> <First name Surname Name of Institution>*  
[for example, *subscribe econhist clio muse olympus u*]  
to **lists@cs.muohio.edu**

To unsubscribe from a list, send the message

*unsubscribe <name of list>*  
to **lists@cs.muohio.edu**

### Posting to a List:

To post a message to a list, subscribers should send e-mail messages to

*<name of list>@cs.muohio.edu*  
[for example, *econhist.macro@cs.muohio.edu*]

### Help:

For any type of help with lists, send your e-mail questions to

**help@cs.muohio.edu**

### Too much clutter?

Subscribers who feel that too many daily postings clutter up their mailboxes should use the 'digest' option. Daily postings arrive only once a day in 'digest' form containing the titles of the postings, followed by the postings themselves. Send the message

*set <list name> mail digest*  
[for example, *set econhist.teach mail digest*]  
to **lists@cs.muohio.edu**

# CALL FOR PAPERS

## 1995 CLIOMETRICS CONFERENCE

The Thirty-Fifth Annual Cliometrics Conference will be held at the University of Kansas, May 19-21, 1995. Thomas Weiss and Joshua Rosenbloom will serve as hosts.

The National Science Foundation continues its sponsorship for 1995. As in recent years, the Conference will be able to pay most expenses for the majority of the participants. Membership in The Cliometric Society is not a consideration for Conference participation. We encourage *Newsletter* readers to share this announcement with colleagues and students.

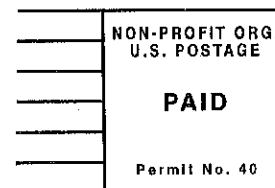
Paper Proposals and Requests for Invitations Due	February 1, 1995
Invitations Mailed to Participants	March 1, 1995
Complete 20-Page Papers Due	April 5, 1995
Conference Books Mailed	May 2, 1995

Anyone who wishes to attend the Conference should request an invitation by writing to the Conference Secretary. Proposals for papers should be three to five pages in length. Three copies of proposals should be sent to:

The Cliometrics Conference Secretary  
Laws Hall Box 35  
Miami University  
Oxford, OH 45056 USA

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**THE CLIOMETRIC SOCIETY**  
MIAMI UNIVERSITY  
OXFORD, OH 45056 USA



## Cliometric Society Sessions at the 1995 ASSA meetings

The Cliometric Society will sponsor five sessions and a cocktail party for members and friends at the 1995 annual meetings of the Allied Social Science Association in Washington, January 6 through 8. All events will be held at the Washington Hilton and Towers Hotel. The Friday afternoon session is a joint session with the American Economic Association, and the Sunday morning session is a joint session with the American Finance Association. Program Chairs John James and Barbara Sands selected and grouped papers sure to hold your interest. All sessions will be conducted in traditional Clio style: Authors and discussants will begin with a 15 minute presentation, with the remainder of the session devoted to open discussion of each paper in turn. Summaries of the papers are attached so you can prepare to participate in the discussion. Summaries are also available in *Conferences* Directory of The Cliometric Society server: Gopher/Telnet address [cs.muohio.edu](mailto:cs.muohio.edu) WWW address <http://cs.muohio.edu/> Complete papers, references and bibliographies are available on request from the authors of correspondence, whose addresses appear on the overleaf.

### Migration and Well-Being in 19th Century America

Friday, January 6 8:00 am

Presiding: Joseph Ferrie, Northwestern University

Farragut Room

Herscovici, Steven (Chicago): *Migration and Economic Mobility: Wealth Accumulation and Occupational Change Among Antebellum Migrants and Non-Migrants*  
 Katzenberger, Paul, John Komlos (Munich), and Peter Cochran (North Carolina-Chapel Hill): *What Do the Heights of Georgian Convicts Tell Us About the Antebellum US Economy?*  
 Wegge, Simone (Northwestern): *Self-Selection and 19th Century European Emigration: The Case of Border-Crossing Hessians, 1832-1857*

Discussants: George Borjas, University of California-San Diego  
 Jonathan Pritchett, Tulane University

### American Monetary and Financial History

Joint session with American Economic Association

Friday, January 6 2:30 pm

Presiding: Bennett T. McCallum, Carnegie-Mellon University

Farragut Room

Guinnane, Timothy (Yale), Harvey Rosen (Princeton), and Kristen Willard (Columbia): *Turning Points in the Civil War: Views from the Greenback Market*  
 Kroszner, Randall (Chicago): *The Role of Firewalls in Universal Banks: Evidence from Commercial Bank Securities Activities before the Glass-Steagall Act*  
 Mason, Joseph, and Charles Calomiris (Illinois): *Contagion and Bank Failures During the Great Depression: The June 1932 Chicago Banking Panic*  
 Weber, Warren, Arthur Rolnick (Federal Reserve Bank), and Bruce Smith (Cornell): *Establishing a Monetary Union: The United States Experience*

Discussants: Peter Ireland, Federal Reserve Bank of Richmond  
 Richard Grossman, Wesleyan University

### Institutional and Technological Change

Saturday, January 7 2:30 pm

Presiding: Samuel H. Williamson, Miami University

Edison Room

Carlson, Leonard (Emory): *The Economics and Politics of Irrigation Projects on Indian Reservations, 1900-1940*  
 Grove, Wayne (Syracuse): *Farmers' Choices: Competing Cotton Harvester Technologies*  
 Morris, Cynthia Taft (Smith): *The Secret is in the Brew: No Quantitative Approach to Institutional Performance Can Go It Alone*

Van Vleck, Va Nee (Grinnell): *An Alternative Hypothesis on the Capacity of British Coal Wagons: An Inventory Management Tool*

Discussants: Alan Dye, University of Michigan  
 James Eaton, Bridgewater College  
 William Sundstrom, University of California-Santa Clara

**Cocktail Party: Members and Friends of The Cliometric Society**  
 Saturday, January 7 8:30-10:30 pm  
 Conservatory Room

### The Micro-Structure of the First Capital Markets

Joint session with American Finance Association

Sunday, January 8 8:00 am

Presiding: Lance Davis, California Institute of Technology

Monroe East Room

Neal, Larry (Illinois): *The Response of the London Stock Market to New Issues: Canals, Railways and Empire in the 19th Century*  
 Sylla, Richard (NYU): *The US Securities Market at Independence, the Civil War, and the Rise of Big Business: The Development of Microstructure*  
 Tilly, Richard (Muenster): *The Berlin Securities Exchange in National Context: Actors, Rules and Reforms to 1914*  
 White, Eugene (Rutgers), and Emmanuel Thivieaud (Harvard): *The Paris Bourse before 1815*

Discussants: Lance Davis  
 Robert Schwartz, New York University

### The Rise and Fall of Bimetallism

Sunday, January 8 10:15 am

Presiding: Ron Michener, University of Virginia

Jackson Room

Flandreau, Mark (CNRS, Paris): *An Essay on the Emergence of the Gold Standard*  
 Oppers, Stefan (Michigan): *Arbitrage in Bimetallic Money Supplies: Evidence from the Exchange Rate*  
 Quinn, Stephen (Illinois): *Gold, Silver and the Glorious Revolution: International Bullion Arbitrage and the Origins of English Gold Standard*

Discussants: Warren Weber, Federal Reserve Bank of Minneapolis  
 Peter Garber, Brown University