The Newsletter of

The Cliometric Society

July 1994 Vol. 9 No. 2

Summary of Papers and Discussion from the 34th Annual Cliometrics Conference

University of Arizona
May 20-22, 1994

by
Kerry Odell, Scripps College, Claremont, and
John Wallis, University of Maryland

(Tucson AZ) Without benefit of plate tectonics or discernible acts of god, the Cliometrics Conference traversed the Great Plains and the Continental Divide to the western United States for the first time in its 34-year history. With humidity levels in the single digits, only the paper givers had to perspire, and all participants enjoyed the resort setting arranged by our hosts Price Fishback, Barbara Sands, Shawn Kantor, and Gary Libecap.

First at bat was Randall Kroszner (Chicago) who attempted to explain the geographical pattern of investment in banks by the Reconstruction Finance Corporation between 1933 and 1935. His model included economic variables (income, urbanization, failing banks, bank capital-to-asset ratios) and a set of political variables (state representation on key Senate and House committees, state RFC board members, Roosevelt's state shares of the popular vote in 1932, and Gavin Wright's 'political productivity'). After controlling for the severity of banking crises in each state, Kroszner concluded that RFC investment patterns were influenced by politics.

Exercising host rights, Kantor opened the discussion by pointing out that Kroszner's analysis lacked an underlying model of political behavior. Both Don McCloskey (Iowa) and Joe Reid (George Mason) remarked that such a model would strengthen the paper and provide a better method for evaluating political influences on RFC investment. Participants also focused on the appropriate measure of influence, offering alternatives to Kroszner's use of tenure of politicians. Samuel Williamson (Miami) suggested that seniority might be a more appropriate guide to legislators' influence; Lou Cain (Loyola and Northwestern) commented that chairmanship of Congressional Committees was probably more important than the tenure of individual committee members. Sumner LaCroix (Hawaii) took the discussion a step further, proposing that legislative logrolling might introduce influence from legislators who were not members of the Banking committees, skewing Kroszner's results. Both Ruth Dupré (École des Haute Études Commerciales) and Joan Hannon (St. Mary's) wanted to know just how much influence legislators could have had on RFC investment in individual banks. Janice Kinghorn (Washington U.) suggested that the RFC might not have been any more politicized than other New Deal banking programs.

The forcible dissolution of conglomerates during the American occupation of Japan after World War Two was examined by Yishay Yafeh (Harvard). Yafeh's main hypothesis was that the American policy of dispersing ownership rights made it

(continued on page 20)

What's Inside

Executive Director's Notes

Report of Trustees' Meeting

The Trustees of The Cliometric Society met on Sunday, May 22, 1994, in Tucson, Arizona, to discuss the following items of business:

The Society will sponsor a C-session at the International Economic History Congress this September in Milan, scheduled for Thursday, September 15, 9:00 am - 12:30 pm. Summaries of the five papers are included as an insert to this *Newsletter*. Our fileserver will be demonstrated during the session, or at another time to be announced at the Congress.

We will also sponsor sessions at the 1995 ASSA meetings in Washington DC in January. Program chairs Barbara Sands and John James have selected 18 excellent papers from the 29 submitted. Summaries will be published in the October *Newsletter*. In addition to our joint session with the AEA, we will co-sponsor a session with the American Finance Association. A 'Members and Friends of Clio' party Saturday evening was approved. Proposed time is after 8:30 pm, and time and location will be announced in the ASSA program. Please plan to attend.

THE CLIOMETRIC SOCIETY
Miami University
Oxford, Ohio 45056 USA
(513) 529-2850
Fax: (513) 529-6992
E-mail: CSociety@cs.muohio.edu

The Newsletter of The Cliometric Society
is published three times per year.

Samuel H. Williamson, Editor
John Lyons, Associate Editor
Louis Cain, Associate Editor
Debra Morner, Assistant Editor

The 1995 Cliometrics Conference will be held at the University of Kansas, May 19-21, hosted by Tom Weiss.

Executive Director Williamson concluded with several brief reports. In December 1993 The Society published its first book, *Two Pioneers of Cliometrics*. An annotated announcement will appear in the next edition of the annual *Subject Guide to Books In Print*. The *Newsletter* will continue its interview series into 1995; several interviews have recently been completed or are in progress. The *Newsletter* also has received some income from book advertising; the Trustees ask all members to promote their work in the *Newsletter*. A summary report of the Society's fiscal year was submitted, with a projected deficit of about $450 for the fiscal year ending June 30, 1994. A full report will appear in the October *Newsletter*.

E-mail, Server, and Lists Update

In the last *Newsletter* we reported on the Society's Internet capabilities; our new machine allows us to expand existing services and to offer some new options.

Our first service is the mail-forwarding program. If you want to e-mail a message to any member of The Cliometric Society and do not know the address, you can send it to 'first initial and surname' at **@cs.muohio.edu** (*E.g., swilliamson@cs.muohio.edu*) If the member has an e-mail address on file, the message will be forwarded. Most messages will be forwarded successfully, since we have spent much time and effort updating addresses. If the member does not have an address on file, you will receive a message to that effect from CS.

The second service is our file server (often called a 'gopher'). You can reach it in several ways: telnet to **cs.muohio.edu**, type *server* when asked to login and hit return; with a gopher client, open a 'bookmark' at **cs.muohio.edu** Now you may also reach the server by WWW: **http://cs.muohio edu/** Through MOSAIC interfaces, you can obtain pictures and graphs as well as ASCII text files. The MOSAIC client software is free; a direct connection to the Internet allows you to use this new medium.

The third service provides The Cliometric Society 'lists'. (Listservs and ListProcs are 'computerese' for electronic

(continued on page 35)

AN INTERVIEW WITH W. W. ROSTOW

Editors' Note: Walt W. Rostow is Rex G. Baker, Jr. Professor Emeritus of Political Economy at the University of Texas at Austin, but still teaches "The World Economy: 1750-1994" in two terms. He was educated at Yale ('36; Ph.D., 1940) and was a Rhodes Scholar at Oxford (1936-38). During the 1940s he alternated academic with government service: teaching at Columbia in 1940-41, Oxford in 1946-47, and Cambridge in 1949-50, and working with the Office of Strategic Services in Washington and London during the war years (1941-45) and at the United Nations Economic Commission for Europe in 1947-49. In 1950 he joined the economics faculty at MIT for a decade, during which time he served also as consultant to the Federal Government. He re-entered full-time government service in 1961 for the Kennedy and Johnson administrations, and returned to academic life at the University of Texas in 1969.

Our interview took place in mid-March 1994 at Rostow's home in Austin, and was conducted by John V. C. Nye (Washington University in St. Louis), who writes: W. W. Rostow has been one of the most influential, imposing and controversial figures in the fields of economic history and development for over half a century. He is best known for his book, ***The Stages of Economic Growth*** *(1960), in which he characterized the process of modern growth through a series of five stages. The book introduced the term 'take-off into sustained growth' to the jargon of economic development and had an enormous impact on the development policy literature. His first works on the growth and development of early industrial Britain, partly in collaboration with Gayer and Schwartz (1953), served as pioneering works of cliometrics before the term was even invented. Despite his early interest in quantification, Rostow remained outside the cliometrics movement of the late '50s and early '60s. He has often referred to himself as a maverick in the profession. His most recent work is a 700+-page history of modern economic thought,* ***Theorists of Economic Growth from David Hume to the Present*** *(1990).*

You were an early exponent of a quantitative approach to economic history, particularly in your work on the Industrial Revolution. How did you become interested in that work, and also the joint work with Gayer and Schwartz?

It began while I was an undergraduate at Yale. I did my freshman work on a scurrilous journalist of the French revolution, Hébert, and his newspaper *Le Pere Duchène*. The files of that were in the library. During my second year I worked on the character of the English revolution of the 17th century centered on Winstanley and the Diggers. At just this time, I took a black market seminar in economic theory with Dick Bissell and three others. The seminar took place on Thursday nights. Dick was fresh back from a year at the London School of Economics where he read Wicksell, Marshall, Wicksteed and the Austrian theorists of capital. And so did we. He had one of the five greatest gifts of exposition of anyone I have ever known. He was doing a thesis at Yale on the theory of capital. My first introduction to theory was mathematical and both micro and macro as we now call it; and out of that seminar I posed for myself two questions. One, suppose you were to take economic history, which was at that time a rather descriptive and institutional field, and apply to it modern economic theory and modern statistics. Two, there was a larger question, in effect the Marxist question. That was the relationship between the economy and the society, the social structure and political culture. I had already decided they were interactive rather than linear, as in Marx's formulation. Those are the two questions that have interested me ever since I formulated them at age 17.

The first work I did was in 1934, on the inflation during the Napoleonic wars and the deflation afterwards. I found that the major characters in monetary history all wrote about that episode. But I also read Tooke. I found that the monetary theorists explained only a very small part of the process that affected prices during and after the Napoleonic wars. You had to look at the supply side and you had to take Tooke very seriously. In fact, Tooke was much more careful about monetary analysis than the monetarists. They were quite content to deal with the process by making a correlation between the rise in discounts and country bank notes on the one hand, and prices on the other. Ricardo's analysis was superficial: why prices rose at that time and then fell afterwards from 1812. This was a desperate part of the war. You couldn't understand what happened except in the context of the Continental System and the Orders in Council, the closing off of Hamburg, and the routes to the Mediterranean and Scandinavia, and so on. It was out of desperation they started the boom in Latin America at the same time. This was the kind of a war it was. By throwing myself into the whole process, I learned a great deal.

Did you feel you were making a break with traditional economic history, or that you were doing a better job?

I didn't think of a job. I didn't think of it. This was what I wanted to do. I viewed the British economy as part of the whole society of Europe and the Américas. Well, I was conscious I was breaking away, but that was not nearly as important as following my own bent. I wrote three essays in economic history as an undergraduate. In my sophomore year I did 'Inflation and Deflation: A Chronicle of the Napoleonic Wars in England'. That was in 1934. Then as a junior I did '1873: The Study of a Crisis'. Then as a senior in 1936 I did 'Outline for an Economic History of England: 1896-1914'. In each of those cases I devised a method which I ultimately used in my thesis. I told the story of a whole economy in motion by doing it year by year, cycle by cycle. I think I differ from a great many economists in that, from the beginning, the questions I posed demanded that I look at the whole economy and not some part of the economy.

Now what happened on the Gayer study was that I had finished my Yale thesis, which was largely written at Oxford.[1] Gayer, it must be said, had done at Oxford a very good book about this period, 1790-1850. In New York he conceived of doing this same period with the full techniques of the NBER. He had Anna Schwartz and Isaiah Frank working with him. He had collected a good many of the series, and he put them through the National Bureau method. Meanwhile, Anna did a price index, the best there is, with good careful weights. Isaiah Frank was doing the stock exchange and related institutions. But they had no method for putting together the price index, the stock exchange index, and the statistical data which they had put through the NBER cyclical and trend analysis. Gayer called me down from Yale where I had just finished my thesis. So in 1939-40 I did Volume One, the history, and began work on Volume Two with Anna Schwartz. I used the NBER method which I took apart in the light of history. The reason that I could take apart the average figures and the deviations from the average was I that knew every case in these 60 years. The econometricians who work with averages do not know what they're talking about. I mean that not in the cheap sense, but that they don't know what the deviations mean. Part Two fulfilled Wesley Mitchell's dream. We had the history, the theory and the numbers. There is only one other book like that. (Well, there is one for the 1830s, which Robin Matthews did, and one for the 1850s, which Jon Hughes did). But the only other first-rate study of that kind is Svennilson's study (1954) of the interwar years. It is a beautiful book; but it was not popular after the war.

The link between the Gayer study and my past was that I'd been doing these finger exercises as an undergraduate and graduate. By 1939, I was ready to play my part in the Gayer study. I was able to do it in a year plus because I taught 1940-41 at Columbia. Anna Jacobson Schwartz, who was a young mother, used to go home at noon and nurse her baby. She was a splendid statistician. I liked her. She defected to Friedman. And she defected away from the explanation of the price increases and price decreases we used in the Gayer study. So you will find in the Harvester edition of the Gayer study (1975), in the Preface, a good summary which brought together the literature on the period 1790-1850, since we'd finished the study before the war. And there she takes her distance from our explanation of the price fluctuations. I stick with the 'old time religion'.

Did you see a link between your work and the cliometrics movement when it came around in the 1950s?

I gave a paper at Williamstown in 1957 which still is my final word on this subject: 'The Interrelation of Theory and Economic History'. I urge you to read that because I talk of that relationship exactly.

[1]The thesis was published as late as 1981 in the Arno Press Ph.D. series: *British Trade Fluctuations, 1868-1896: A Chronicle and a Commentary.* It contains introductory remarks written some four decades after the event.

Could you state in a nutshell what you thought?

I took it for granted that other people have the right to make their own decisions about what they do their research on and how they do it. My own view of the different kinds of cycles concerns concurrent interactions, which is my view of how history unfolds. For example, long demographic cycles, the short demographic cycles, Kondratieff cycles and trend periods, housing cycles, major cycles, inventory cycles. The problem that the historian faces is dealing with all the forces in play, not a mono-causal world.

Among the cliometricians there was a feeling they were doing something different, that there wasn't enough systematic use of economic theory. Did you agree, or did you feel that the body of historians writing at the time were doing a pretty good job?

I felt very warmly towards them, and I wrote, I forget where I wrote it, but I wrote in favor of cliometrics. The only thing I held against them was that, with the exception of Landes – and David, who broke out of it – they were victims of the reigning neo-classical economists.

You said David Landes and Paul David?

Yes, they broke out, especially Paul David in his criticism of Fogel's slavery book.

What did you think, for instance, of Conrad, Meyer, North, Davis, Hughes, Parker . . . ?

I liked them. I just took a different tack. I did my own thing. Meyer and Conrad did the first work on slavery, for example; North has worked on institutional influences, and Hughes did the 1850s, and for Parker it was productivity. I felt in my bones that cliometricians in general, Fogel particularly (whom I like very much and who has done so well by his students), were too much in the grip of the neo-classical economists and this mainly accounted for their failure to deal with the issue of technology.

Let me give you a simple example. I put a footnote in *The World Economy* where I take apart the Fogel analysis of railroads (pp. 746-47, note 53). Fogel says if anything was a big factor it was the nail industry, which meant more in the 1840s and the 1850s than the volume of output of iron for the railways. But the point was not at *all* quantitative. The point was that the railways induced both France and the United States to get out of the farmer's iron business and to bring in the blast furnaces and then the modern methods of making iron from coke. Steel came along in the late 1860s and '70s. But the iron came first. It was not a question of the quantities of iron used by the railroads, but the fact that technological change came about through the railroads and the iron industry. The same thing happened to Russia. Up in Siberia they gave up making iron out of timber, which was plentiful, and took to modern iron and steel manufacture when the railways united Donets coal and Krivoi Rog iron.

The new commitment to neo-classical economics was what kept cliometricians from doing what they should have done. They should have done the economic history of the United States when we did the Gayer study. There was no successor to Smith and Cole (1935), the pioneer book on the early American economy. They were kept from dealing with the American economy as a whole because they were in the hands of the mathematical economists. In his book, *Unbound Prometheus*, Landes deals with the question of technology right head on.

But does he really deal with it? If I might push you on this: it seems that he asserts things. He doesn't check for alternate theses, he asserts, and judges success by use of what he feels were the leading technologies.

There is no way to deal with technology except by description, which Landes did. Why is it that technology came in four batches? In the 1780s, the steam engine, Cort's method of making iron, and the factory method of making cotton textiles – these all came together. The railways came concurrently in Britain and the United States, with Germany and France only a little behind, and they induced the steel revolution - at the end of the 1860s. Electricity was spread about, but it explains the rise of France. They were poor in iron and coal as compared to the Germans, but the Alps were a great source of hydro power. Due to hydro power, the French had a higher rate of growth than the Germans or Americans before the First World War. So first you had the textiles, then you had the railways and the iron revolution and steel, then you had the electricity and the internal combustion engine and chemicals. You really didn't have another technological revolution until the 1970s. In the middle of that decade you began to get microelectronics, the new genetic engineering, the laser, and the new methods of producing physical objects with plastics or ceramics.

Towards the end of *Theorists* I go through the mystery of why it is these technologies come in bunches; why they come about every 60 or 70 years. Now, Schumpeter made a decisive error by linking the Kondratieff cycles, which are cycles in relative prices, to the technological cycles. To this day . . . it [has] shocked me that he made that mistake. He was a fascinating figure, Schumpeter. He was absolutely right in making his pitch about entrepreneurship, but he had no theory of growth. In his youthful volume (1912) – he didn't deal with population, technology and investment, and the late-comers and the early-comers. . . This bunching issue has stirred a considerable literature. But to this day I don't know of a conventional theorist who has contributed to that literature.

To return to the question of theory, I have six variables which I ask my students to use. Now if you wish to characterize my work compared to conventional economics, here it is. Conventional economics evades these six variables: population; technology and investment; relative prices, which embrace the Kondratieff cycles; business cycles, but seen as a form which growth took – not abstracted from the whole system; the stages of growth, which repeat in a sense the technological revolutions, but from a different perspective – the perspective of a single country; and the non-economic variables which affect the world economy. Among these are perfectly obvious ones like the traumatic effects of wars – the Napoleonic Wars, the Civil War, and the World Wars. But the economy is also affected, for example, by how the ruler disposes of his limited resources. . .There are three directions that rulers could take: they could dispose of resources to redress old wrongs; to build up the center *versus* the regions; and there was the question of welfare . . . It's very important to be clear about the primacy of politics, generally, notably in modern economic development.

Yes. The work of the 1950s, particularly yours, but also Gerschenkron's, was often self-conscious about communism as a tempting alternative model for developing nations. Can you comment on the relevance of your 'non-communist manifesto' to the eventual economic and social collapse of the East?

That's the title *The Economist* gave to the book.[2] I didn't mind at all; but it was a 'non-communist manifesto' in the sense that it was an alternative to Marx's theory of development. It was not a polemical book except at the very end where I state my assumptions about human beings and the process of growth, as opposed to the communist view.

I think that I should tell you, though, that I was anti-communist from a very early time. My father came over in 1904 from Russia. He had already been in the Social Democratic movement and he had fought the communists over the issue of 'What Is to be Done' in 1902. And he didn't like the communists because they wanted to seize power even though they were a minority. I remember we had at our house a visitor from the Soviet Union. He was charming, wearing a leather jacket in the early '20s. I couldn't have been more than six. Afterwards, Father was asked what our visitor's view was. And he said no good would come of them. They took over the czarists' police, but they made them tougher. The czarists at least did not go after the families. They only sent off to Siberia the political dissidents. *These* fellows took the families. My father taught us from that early time: in politics the methods used were as important as the aims you nominally sought. I never forgot that lesson. I dealt with it in the *Theorists* book in the analysis of Marx.

I had the great pleasure in Moscow in 1958 of quoting Charlie Curtis [a Boston lawyer]. I said in the Institute of World Politics the problem with Marx was that he did not understand Charlie Curtis's Law. Charlie Curtis's Law was that the end of a discussion was not 'By golly, you're right', but 'I've got to live with the S.O.B., don't I?' And that's what Marx did not understand. He had a blood lust in him. He was very harsh to his wife and daughters and indeed, in the end, with Engels. He was a troubled man. He had never built a theoretical structure out of his Manifesto. He spent his whole life trying. Now Gerschenkron and I were anti-communists, but I dealt with the Soviet system and with Soviet diplomacy in an unemotional way. But I did take the Cold War seriously.

Although you and Gerschenkron seemed aware of the human cost of the communist system, my impression is that you overestimated their industrial success.

This should interest you as a historian: even their own experts had predicted the key sectors of the '50s and '60s would decline. Read over the passage in *The Stages* which begins: 'Beware of linear projections!' The theory of the demise of the Soviet Union which I put at the beginning of the third edition of *The Stages* details their failure to pick up the automobile revolution, but they were *terrorized* by

[2] In the autumn of 1958 Rostow gave a series of undergraduate lectures on "The process of industrialization" while on sabbatical leave at Cambridge University. He was persuaded to publish his lectures in abridged form in two numbers of *The Economist*, August 15, 1959, pp. 409-16, and August 22, 1959, pp. 524-31. The articles were called "Rostow on Growth; A Non-Communist Manifesto", thereby providing the sub-title for *The Stages of Economic Growth*, published in 1960.

their failure to pick up the computer revolution in the mid-1970s. So they missed two technological revolutions in a tragic effort to dominate the world and to behave like a superpower. After the Second World War they fell off their growth curve, but you can't understand the process unless you look at the sectors. At the end of the analysis of Russia and the Soviet Union in *The World Economy* I describe the deceleration of the economy as early as 1978, and I give the major reasons for it. They diverted their best scientists and engineers to military purposes. I visited the Soviet Union in 1990 and I observed they are still, on the whole, palpably tied to the technology of the 1950s.

The Soviets were clearly unable to catch up with the technological wave, but to what extent did they *not even* take full advantage of technology of the '50s? Even ignoring linear projection, how much did we overestimate their success even in the earliest periods?

Well, you've got to be very careful about that. They kept a high rate of growth in the steel industry, they allocated 30% of their output to military production, to which they allocated their best engineers and scientists, and they produced a hell of a lot of bombs. They were very dangerous. They lagged us in the quality of their aircraft by two or three years. Their tanks were very good. But they came unstuck out of a mixture of a fall in their rate of growth, which the experts predicted to be on the way in the '50s, and in Afghanistan they took on problems they couldn't handle, militarily, given the opposition of the Muslim world – in fact, the whole noncommunist world. Then they were hit by an absolutely new phenomenon – the success of China under Deng Xiao Ping in the late '70s.

Yes, shouldn't we talk about that? Because China seems to be a contradiction – they missed more of the technological breakthroughs than the Russians.

No, no. The biggest thing the Chinese under Deng Xiao Ping had going for them...

Agriculture is what developed the industry . . .

That's the point. Family responsibility is the key thing in the late 1970s under Deng Xiao Ping. From the families you could lift off the communes, the bosses, the cadres – families had to turn over a certain amount as taxes and rents, and the *rest* they could decide for themselves. When I visited China in 1983 people in the cities – for the first time in Chinese history – were outraged that people in the countryside were doing better than they were.

But isn't that a purely market phenomenon – not a technological one?

It's not technological at all.

So it's the market, right? It's institutional . . .

Wait a minute. Wait a minute. It's institutional, but it's not market. Because the Russians had treated their peasants like animals – they killed off 10 million – they set up a system of collective farms and the best people on those collective farms went to the cities, where they did better. So on the collective farms were the old people and children. This was well-captured in a cartoon that came my way when I did a book on Soviet society.[3] They had these garden plots which were terribly inefficient, but they got 30% of the food for the cities from these garden plots. They were caught between the market gardening of this kind and the slovenliness of the state farms. They threw tremendous amount of capital into agriculture. A vast number of tractors were down for lack of spare parts, because their factories were measured, and given their instructions, in terms of complete units, not spare parts. So they cannibalized their farm machinery. On collective farms machinery belonged to everybody; therefore, it belonged to nobody. Nobody felt responsible.

Now in China, on the other hand, they still had the family – intact. They still had the small farms except in the North, in Manchuria, where there were collective farms. But the reason that China has done very well is that it was an agricultural country; they freed up agriculture, and then they went into light industry, which was easy to do. And so the south grew and industry moved inland from the coastal areas. The south is alive with construction and light industry. They jumped from textiles to computers. Now the biggest problem that the Chinese face is what the hell are they going to do with the old factories which they got from the Russians in the 1950s. The Party still gives them subsidies. They are gradually going to bring in the Japanese and others to modernize their old factories. That's the policy the Russians ought to pursue but they haven't been able to do it. They can't bring their 1950s factories up-to-date except by bringing in foreign firms. They ought to bring them in for 20 years – and make profits and try to sell abroad – and then at 20 years they would have the right to buy the foreigners out.

[3]The cartoon depicted a sturdy family at the gate, waving to the frail grandmother, and saying, "Goodbye, grandmother. Do the family's work on the *kolkhoz*."

Right. Let's go back to a subject you mentioned earlier: your abiding interest in the economics of long cycles. How much emphasis should economic historians place on such work, and what suggestions do you have for questions worth pursuing along this line?

Relative price movements – of foodstuffs and raw materials relative to manufactures – play a very large part in the multiple forces that enter economic history. You have the upswing from 1790 to 1813. There's a fall again when Napoleon was forced to reform his army after the debacle in Russia, and he re-opened foreign trade. The downswing continues after the war and extends to the shortage of grain in the 1840s and 1850s. Then you have the upswing until 1873. You have the downswing 'til '96, which is *not* a depression in terms of employment, if you do it cycle by cycle and measure it carefully, but it *was* a depression of prices. Then you have the upswing to 1920, and the downswing to '33. You have the upswing until 1951. The subsequent downswing plays a very important part in the '50s and '60s. All the industrial countries benefited from the downswing in the prices of foodstuffs and raw materials. And then stocks begin to attenuate in grain as well as energy, and you have the upswing in the '70s.

These long cycles were an important part of the story from 1790 to the 1980s; but I reiterate, they are among the key variables you have to track. They don't stand alone in economic history. In my first post-war book, actually my first published book, *The British Economy of the Nineteenth Century*, I said at the beginning that the long cycles of fairly uniform time sequence were a product of the 19th century. I did not predict they would continue into the second half of the 20th century. I said that improvements in agricultural productivity would be more even than the opening up of new territories, which was the method used in the 19th century to deal with shortages of grain. I reckoned without the switch to oil, or that it would take 10 years to open up the Norwegian and British reserves, and 10 years to get the North Slope going. But I don't think there is any reason for the rhythm to continue. But you can't understand history without understanding these long movements of relative prices. I did a mathematical model of the Kondratieff cycle, quite different from the one that appears in the *Business Cycles* (1939) of Schumpeter, and quite similar to Arthur Lewis's explanation. Long movements of commodity prices, raw material prices, have had a lot to do with the contours of economic history. But as I say, I don't deal with them alone. I tell my students to look at population movements, technology and investment, relative prices, business cycles as part of growth. The thing that's so much fun about economic history, and what's so interesting about the Ricardo-Malthus debate, is that Malthus was aware there were so many things operating at the same time . . .

Is that what you called in an earlier essay 'the problem of economic history' – the debate between your sort of real-world theory and abstract theory?[4] You pose Malthus as representing the real world to some extent and Ricardo as representing abstraction.

Yes, I do indeed. Milton Friedman asserted flatly in one of his books or one of his essays, that he's going to stick with his theoretical view dominated by one variable. Some people are gifted that way; some are gifted to look at the whole of reality. What I would assert is that a historian is bound, by his profession, to deal with multiple variables. In that sense, I'm a historian. But, for each of my variables, I have a theory: a theory of demographic cycles, a theory of innovations and investment. I keep saying it very politely, but the proportion of income invested isn't at *all* the product of a Keynesian system. It isn't at *all* the product of the consumption function. It's a product of how much of the backlog of technology you missed and are as a society willing to make up.

Would you elaborate on that? This notion of the backlog of technology.

Yes. After the war the Japanese ran a 30% investment rate. After the war the Europeans ran a 22 to 24% investment rate. In the United States it hovers around 15 to 17% and the rates of growth accommodate themselves to the investment rates. Well, the Japanese were furthest behind. The Japanese were impoverished; they had a broken society. But they came up like a rocket. They set about acquiring every type of technology. Along the way they started with cameras. The cameras arose out of their expertise at making bomb sights, and they were very good at it. And they went right up. They planned a technological chain. They were a well-educated people. They climbed a chain right up to the present crisis. And they ran, in so doing, a 30% investment rate for most of that period, although it fell off towards the end. The Europeans also came up. They had the war damage to make up, and they hadn't had the mass automobile age or the age of consumer durables. The automobile age came in the '50s

(continued on page 26)

[4]See Rostow (1992), pp. 224-5; see also Rostow (1986).

Stanford Economists Consider the Economics of the 21st Century

by

Gavin Wright, Stanford University

(Stanford CA) On June 3rd and 4th, a conference was held at Stanford University entitled 'Growth and Development: The Economics of the 21st Century'. The sessions marked the opening of the Donald L. Lucas Conference Center, located in Stanford's new economics building, just opened in the Spring of 1994. Although the subject matter was not limited to economic history, historians and historical topics were well represented. Organizers were Gavin Wright, Director of Stanford's Center for Economic Policy Research (CEPR), and Ralph Landau, Co-Director of CEPR's Technology and Economic Growth Program.

The first session was devoted to 'Overviews of Economic Growth and Development,' and began with a paper by Moses Abramovitz (Stanford) and Paul A. David (Stanford and Oxford), 'The Bases of Productivity Leadership and the Limits of Convergence'. Summarizing many years of research on the sources of American economic growth, the authors argued that the US had surged to world leadership during an era when progress was biased toward natural resources, tangible capital, and economies of scale. The ability of other advanced economies to follow the American lead was constrained, not just by a technology gap, but by the problem of 'technological congruence': large international differences in factor proportions and features of the market, which required an extensive process of local adaptation before US technology could be successfully transferred. These barriers to convergence have greatly receded in the 20th century, partly because of a growing similarity in conditions of production, but more fundamentally because of the transformation of technology, whose bias has shifted towards unconventional and intangible factors of production. Discussants Dale Jorgenson (Harvard) and Paul Romer (UC-Berkeley) praised the paper for the sweep of its vision, noting that the historical perspective encouraged a healthy skepticism about the notion that convergence is an easy, automatic process.

The session's other papers reflected different historical perspectives. Lawrence Lau (Stanford) summarized many years of econometric analysis of sources of growth in developing countries. Lau drew a parallel between the Abramovitz-David finding that US total factor productivity grew very little during the 19th century, and his own controversial finding that capital accumulation, not technological change, has been the overwhelming source of the recent high rates of productivity growth in east Asia. Henry Rowan (Stanford Business School) presented a visionary paper entitled 'World Wealth Rising: Why and How a Rich, Democratic, and (Perhaps) Peaceful Era is Ahead.' The paper suggests that, contrary to widespread pessimism about development prospects in the Third World, most of the important indicators for the world's most populous countries are strongly positive. He discussed such factors as liberalized economic policies, literacy rates, and population growth, and argued that these positive trends are correlated with democracy and a reduced tendency to go to war. Discussants had no difficulty pointing to counter-examples, but Rowan maintained that a quantitative assessment shows that the positive trends will dominate over the next four or five decades.

Several papers were devoted to technology and the future of technology policy. In a paper which has received widespread media attention (*e.g.*, in *The Economist*, June 18th, 1994), Nate Rosenberg (Stanford) recounted a number of historical examples of major technological breakthroughs whose significance was completely underestimated at the time, even by their inventors. Marconi thought that radio would be used mainly for point-to-point communication where laying wire was difficult; the concept of 'broadcasting' was beyond his imagination. The laser was developed at Bell Labs, and its uses have expanded rapidly, from surgery to music to telecommunications and more; yet the lawyers at Bell Labs were reluctant even to apply for a patent, seeing no conceivable relevance to the telephone industry. In 1949, IBM thought that the entire world demand for computers could be satisfied by perhaps 10 or 15 machines. These stories might well be regarded as merely amusing examples of shortsightedness by smart people, but Rosenberg argues that this pattern is more the rule than the exception, and applies to most major technologies of the 20th century. Why should this be so? Systematic reasons include: (1) the need for continuing improvement in the technology itself, (2) the gradual discovery of new uses as the technology develops, (3) the dependence of new uses on complementary developments elsewhere in economy or society, and (4) the path-dependent character of the emergence of entire technological systems, generally difficult or impossible to foresee in advance. The high level of uncertainty about long-term payoffs to new technologies points to a need for socially-constructed institutions to support such investments. Two other papers raised the possibility that the political and institutional foundations of US technology policy may be at a crossroads.

Linda Cohen (UC-Irvine) and Roger Noll (Stanford) argued, in 'Privatizing Public Research', that the previous national security rationale for government policy is being replaced by a new 'competitiveness' strategy. But the results are far from satisfactory, with strong tendencies toward pork-barrel inefficiencies, and even when projects are successful, they come under attack from commercial rivals. Cohen and Noll believe that the US is a long way from an appropriate match between political strategies and constituencies on the one hand, and a sound rationale for technology policy on the other. Discussant Richard Nelson (Columbia) suggested that relationships between industries and government agencies have typically evolved over a long period, citing the case of agriculture; as a result, they display much more stability and direction than Cohen and Noll allow.

Another tack was presented by A. Michael Spence (Dean, Stanford Business School) in 'Science and Technology Investment and Policy in the Global Economy'. Spence believes that because of the end of the Cold War and the loss of technological leadership, US science and technology structures are losing political support. He predicts growing demands to 'close' the research and university systems, *i.e.*, to channel their services more exclusively toward US citizens and industries. The result, Spence argues, would ultimately be destruction of their effectiveness. A better approach would be to develop new international institutions to support basic research, since the outputs in fact are global public goods. His analysis was seconded by discussant Robert Hall (Stanford), who pointed out that technology development is highly concentrated in specific locations around the world, and that such specialization should not be a cause for concern. Training students and developing science-based technologies are things the US does especially well, argued Hall, and reflect a global pattern of specialization.

The concluding paper, by co-organizer Ralph Landau, was a broad historical look at the chemical industry as a case study of the complex factors behind changing national comparative advantage and economic growth. Landau, a successful chemical engineer and winner of the National Medal of Technology, has devoted his retirement to the study of economic growth. One of his central conclusions is: 'History Matters.' For example, Landau notes that the three largest chemical companies in the world in 1929 (DuPont, ICI, and I. G. Farben) are still major factors in the industry today (I. G. Farben having been decomposed into three successor companies, Bayer, BASF, and Hoechst). This persistence supports Alfred Chandler's emphasis on firm-level performance. But this is not sufficient, argues Landau. The historical record of the industry has also been shaped by national policies and infrastructure, as well as global and socio-economic forces beyond the reach of policy.

This multi-level perspective will inform the conference volume, now being prepared under the editorship of Landau, Wright, and Timothy Taylor, managing editor of the *Journal of Economic Perspectives*. Other contributors include Stanford economists John Taylor, Masahiko Aoki, Anne O. Krueger, Ronald McKinnon, and three author teams: Sylvester J. Schieber (The Wyatt Company) and John Shoven (Stanford); Tim Bresnahan (Stanford) and Shane Greenstein (Illinois); and Tom Campbell (Stanford Law School), Daniel P. Kessler (MIT), and George Shepherd (Stanford).

Conference Report: Fiscal Crises in Historical Perspective

by
Jocelyn M. Hammaker, University of California-Riverside

(Oakland CA) The 1994 Spring Conference of the All-UC Group in Economic History was held at the Claremont Hotel, April 8-10. Additional sponsors were the Burch Center for Tax Policy and Public Finance, UC-Berkeley; the Center for State and Local Taxation, UC-Davis; the California Franchise Tax Board; the Institute of Business and Economic Research, UC-Berkeley; the Institute of Governmental Affairs, UC-Davis; the Agricultural History Center, UC-Davis, the Department of History, UC-San Diego; and the Provost for Research, UC-Berkeley. Program Committee members were Chair Michael Bernstein (UC-San Diego), with Elliot Brownlee, Alan Olmstead, John Quigley and Steven Sheffrin. Bob Barde (IBER, UC-Berkeley) and Mary Davis (Agricultural History Center, UC-Davis) provided staff support.

Ed Perkins (USC) chaired the first session, on state finance in California. Keith Gallagher (UC-Berkeley) began with a paper on 'California's Taxing Tradition, Nevada's Unretiring Debt: the Bullion Tax Fight, State-Building, and Labor Politics on Nevada's Comstock Lode'. Gallagher blends labor history with policy studies to outline 'state-building' in Nevada in 1863-1865 with an explicit look at labor's involvement. He considers the emerging tax policy for mining operations and labor's response, represented by the actions of the Comstock miners' union. The working class exhibited considerable autonomy, he argues, and significantly challenged the dominant political culture.

Thomas Laichas (UCLA and Crossroads School, Santa Monica), in 'California and the Budget Idea, 1850-1916', considers the relationship between California Governor Hiram Johnson's first state budget in 1913 and previous systems of program planning, fiscal control and administrative oversight. Laichas identifies three distinct fiscal regimes in California's history: 1) when local and corporate interests prevented parties from establishing strong legislative control over state finances (1850s to 1880s); 2) when the Republican Party reformed fiscal and administrative systems, empowering party leadership to broker differences between its 'reformist' and 'regular' factions (1890-1910); and 3) when the Republican reformists were in control (1910-1916) and rejected political negotiation in favor of a system which, ideally, would develop and implement executive policy.

Daniel Klein and Chi Yin (both of UC-Irvine) examine non-governmental (market) alternatives prompted by fiscal crises with a look at 'Toll Roads in California, 1850-1902'. According to Klein, who presented the paper, with county and state governments unable to effect road improvement, California lawmakers immediately opened the field to private initiative. Some 115 private companies successfully built and operated toll roads in California. Klein and Yin compare three eras of toll road operation in the US, the Eastern Turnpike Era (1791-1845), the Plank Road Boom (1847-1853), and California's Toll Road Era (1850-1902). They argue that, compared to earlier toll road experiences in the East, California road enterprises were more profit-oriented, built longer roads, and were more concentrated in ownership. The authors also emphasize the role of indirect gains, such as community pride and collateral business motivations, in generating voluntary private support for California toll roads.

In light of the current fiscal crisis in California higher education, John A. Douglass (UC-Santa Barbara) provides an analysis of 'The University of California and Four Periods of Fiscal Crisis: 1868-1960', with the final crisis leading to the 1960 Master Plan for Higher Education. Douglass argues that the character of the University has been shaped by its strategic responses to these crises, which transcended simple financial concerns, calling into question the purpose of the University, its management, and its relationships to state government and the public. Douglass cites two 'constants' present in each crisis, including that of the 1990s: tremendous population growth in California and an ideological commitment to expanding higher education. He focuses particularly on the 1930s, on plans of the Warren administration to expand higher education to deal with an anticipated postwar recession, and on the circumstances that led to the 1960 Master Plan. In his presentation, replete with slides of the original buildings (Gilman, Sproul, Warren and more), Douglass argued that the University of California-Berkeley is just part of the system of higher education, and that its policies are symbiotic with the rest of the state school system.

Joan Hannon (St. Mary's) led the discussion of the first two papers. She thought the Gallagher paper was in fact two: one tracing political battles over taxation in Nevada, and the other detailing the efforts of the miners' union to win and defend a $4-a-day wage for all underground workers, regardless of skill. Although Gallagher characterized the union's motivation as egalitarian, Hannon suggested that the union's struggle for equal wages reflected solidarity against management rather than promotion of equity. Her viewpoint was endorsed by others, who suggested that Gallagher consider union demands in terms both of claims on profit and on how union policies redistributed income among the working class. In Hannon's reading, the story was that transitions from one fiscal regime to another were driven by changes in political economy and in constituencies, not by fiscal crises. Further examination of the relevant constituencies in each period was needed to address the relative importance of a simple march of administrative progress *versus* conflicts over budget process and outcomes between winners and losers. John Wallis (Maryland) apprised Laichas of complete computer datasets of California's Controller Reports from 1850 to 1917, which confirm the paper's finding that, although there was no formal budget process over this period, systems of fiscal control and oversight were in place.

In his discussion of the latter papers, Bruce Cain (UC-Berkeley) uncovered an issue implicit in both: fiscal crises prompt debates about the definition of public goods. For example, Klein and Yin show that toll roads were sometimes defined as private goods, and at other times as public. Cain noted that over time the boundary between pure private and public goods tends to shift, and suggested that the dynamics behind these shifts should be explored more fully: why and under what conditions were toll roads considered private or public? Likewise, why has the University of California been considered a public good, and how is that perception changing as the utilitarian ethos seems to give way to a focus on enrollment as a source of funds? For the current situation, Cain cited 'new' problems which complicate and define this crisis: diversity in culture and the population base, expenditure limits and the initiative process, a decline in faith in representative government, and a need for alternative sources of funding. Given these issues, he suggested higher education in California be redefined as both public and private good in order to sort out the differential benefits it offers users. A participant asked Douglass about who pays for and who gains from the UC system and how this has evolved over time. Another thought that Klein and Yin claim implicitly that a new ideology of central planning toward the turn of the 20th century had 'done in' the private toll road system.

The second session, on state fiscal crises in historical perspective, was chaired by Harry Scheiber (UC-Berkeley). The first paper, 'The Political Economy of State Sales Tax Adoptions: 1930-1938', was by Kim Reuben (MIT, NBER-Stanford), who said the Depression was a period of rapid fiscal change for state governments as they looked for alternative revenue sources to meet rising expenditure. The first general sales tax was introduced in 1931; by 1938 28 states had adopted sales taxes. Reuben analyzes relationships among political and economic factors to estimate the probability a state would adopt a sales tax. States experiencing the sharpest employment declines between 1929 and 1932 were most likely to enact sales taxes, as were those unable to issue debt to cover deficits. Finally, states in which the same party controlled both executive and legislative branches were more likely to pass sales taxes, but the probability of adopting a sales tax decreased in election years.

Steven Sheffrin and James Hartley (both of UC-Davis) and J. David Vasche (California Franchise Tax Board) also focused on the Great Depression in 'Reform During Crisis: The Transformation of California's Fiscal System During the Great Depression'. Sheffrin, the presenter, outlined four important factors in this period; first, government expenditure grew rapidly during the late 1920s and early 1930s, some of which growth was deemed 'uncontrollable'. Second, the method of taxing public utilities was widely viewed as unsatisfactory, and there were strong advocates for change in the name of pure tax reform. Third, intergovernmental relations regarding educational finance were politically controversial. Last, while there was support for property tax relief, there were disputes over both revenue sources and the mechanisms needed to ensure the permanence of reform. Sheffrin *et al.* examine these factors from a number of perspectives: an analysis of the growth of state expenditures, studies of voting behavior on initiatives, counterfactual fiscal histories, and comparisons with more recent fiscal crises. They conclude by citing three fiscal legacies of the Depression in California: changes in the state-local intergovernmental fiscal relationship, a decline in the importance of special funds with a concomitant increase in local assistance, and the birth of the current basic state tax system.

In the session's third paper, 'Debt, Revenue Structure, and Default: State Government Finances in the 1840s', John Wallis, Arthur Grinath (Maryland), and Richard Sylla (NYU) consider the depression of the early 1840s, when nine states defaulted on some or all of their bonded debt. Several ultimately repudiated their obligations, while others repaid in full, including interest accumulated on late payments. Given their high debt levels, it is not difficult to explain why these states defaulted, but the authors ask why they had borrowed so much. Wallis, the presenter, and his co-authors look at the structure of revenue for a cross section of states in the 1830s and '40s and find that two completely different types of states defaulted. Two wealthy, commercially-developed states, Maryland and Pennsylvania, borrowed in an attempt to avoid enacting property taxes. The other group of states was on the frontier, where property taxes were very high, but where taxable commercial properties were inadequate for their needs. Maryland and Pennsylvania ultimately raised taxes and repaid their debts; frontier states that borrowed to make permanent improvements were unable to raise other taxes and thus repudiated their obligations.

Peggy Musgrave (UC-Santa Cruz) and Gavin Wright (Stanford) led the discussion. Musgrave commented on the papers' use of a limiting public choice framework to examine how states reacted to two periods of fiscal stress; Wallis *et al.* consider the 1840s, a period of rapid development and slim tax bases, while Sheffrin *et al.* and Reuben consider the 1930s, a period of declining tax revenues. Musgrave maintained that fiscal history is tied to social and demographic evolution. Discontinuities, such as depressions, are reflected in the fiscal structure, while institutions constrain government's ability to respond. She suggested that examining the relationships between economic development and social and demographic history would improve our understanding of these periods. Further, Musgrave wished the authors had distinguished carefully between capital and current budgets, between what is and what is not being financed by debt, and why. Wright also stressed the importance of demographic change in explaining government's reaction to crises; for example, as the electorate became more democratized, changes in its demographic structure may have influenced voting behavior. Wright suggested that, if the aim of the Reuben paper is to explain the evolution of the sales tax, more 'historical' explanatory variables are warranted. In general, Wright faulted the papers for ignoring the Southern states, citing a need for histories of public finance for both the North and the South. Finally, regarding Sheffrin *et al.* and Reuben, Wright maintained that neither paper explicitly considers the major change in the mix of local and Federal spending in the 1930s.

These comments sparked a mini-debate on the property tax, what Sheffrin termed a 'cultural deconstruction of the property tax'. Sylla called the property tax 'a cultural theme', stating that Americans have not always been against it; until recently, property taxes had not been challenged at the local level. Another participant, however, cited cross-national evidence indicating that longstanding tax revolts occurred only in those countries where property taxes were high (*e.g.*, the USA, Great Britain and Denmark). The absence of a well-specified underlying model for research on government structure during these periods was noted. Lastly, responding to complaints that the fiscal roles of different levels of government had not been clearly demarcated, Wallis asserted that 'All government spending is local', despite the obvious trend towards centralization in the 20th century.

Robert Triest (UC-Davis) was chair of the third session, on public sector retainers and fiscal pressures. James M. Poterba (MIT, NBER-Stanford) and Kim Reuben argued, in 'Fiscal Institutions and the Compensation of Public Sector Workers', that state and local fiscal crises in recent years have sparked a debate on the role of public sector pay in contributing to fiscal stress; several studies suggest that rising compensation of public sector workers has been a key factor in state fiscal crises. Poterba and Reuben document relative wages of public and private sector workers during the 1980s, controlling for worker characteristics, and find that the public sector wage premium evolved differently for men and women: for men the premium increased, while it declined for women. They analyze the effect of state fiscal conditions and institutions on the wage differential, using data from the *Current Population Survey* for 1979 to 1991, and conclude that public sector employees in states with restrictive fiscal constitutions experienced slower growth in the public : private relative wage than did employees in states with less restrictive rules.

Martin Shefter (Cornell), in 'Urban Fiscal Crises', considers the political circumstances of municipal governments, arguing that since municipal governments cannot print money, they are completely dependent on credit markets to cover budget deficits. If a city is excluded from credit markets, it will experience a fiscal crisis, thus giving financiers enormous political leverage and imposing restrictions on local democracy. To obtain the financing necessary to bail a government out of crisis, the people may be excluded from influencing important decisions or may be compelled to discipline themselves.

The hypothesis is discussed in terms of six fiscal crises in New York City from the 1870s to the 1990s.

In 'Can We Decentralize Our Unemployment Policies? Evidence from the United States', Robert P. Inman (Pennsylvania) and presenter Daniel L. Rubinfeld (UC-Berkeley) question a standard assumption that factors of production are highly mobile within nations, that factor mobility across subregions of a nation and the high propensity of a region's residents to purchase 'imported' goods have been used to support arguments against the use of sub-national policies to manage unemployment. However, surprisingly little evidence has been presented to verify these claims. Inman and Rubinfeld suggest two prerequisites for a regional-state fiscal policy to be effective: 1) that regional-state labor markets adjust slowly to shocks in other regions and states, and 2) that effects of a shock in one region-state do not extend to different regions or states; for example, the unemployment rate of one state is not primarily a function of that of another state. The authors test these hypotheses by examining the persistence of unemployment within individual US states and the relationship between unemployment in one state and neighboring states. Their evidence suggests significant persistence, rejecting one of the key assumptions above, and also suggests that effects among neighboring states are not substantial.

Discussant Perry Shapiro (UC-Santa Barbara) began by suggesting that the focus by Poterba and Reuben on wages rather than on total compensation may ignore important relative changes in public- and private-sector compensation, since fringe benefits make up a large percentage of total compensation, and noted that the list of worker characteristics for which the authors control omits differences in marginal productivity or ability. Shapiro maintained that changes in the wage differential over the period reflect wage trends in both sectors: since private real wages declined so much in the 1980s, the relative wage trend is partly an artifact of this decline. Other participants suggested that job security is measurable and presumably has some impact on relative wages, and that, since differences in enforcement of affirmative action laws should influence wage differentials, trends should be considered separately by race. On Inman and Rubinfeld, Shapiro noted a bias in the fiscal federalism literature: that the lower the level at which a policy can be implemented, the better. One basis for a national policy is the presence of externalities; however, the authors' evidence suggests considerable local separation and relatively weak externalities. Shapiro said that policies should be implemented where they will be most effective, but thought that certain state fiscal policies might violate the Commerce clause of the Constitution. Lastly, Shapiro observed that states can run counter-cyclical policies, such as full employment expenditure policies, but seem hesitant to do so. Richard Sutch (UC-Berkeley) pointed out that there was an attempt at regional policy during the Kennedy and Johnson administrations, but that states could not overcome political obstacles.

The second discussant, Teresa Hannah (UC-Davis), stated that the Shefter paper fills a gap by identifying a link between highly redistributive policies and tradeoffs between constituents and financiers. She suggested that Shefter consider the importance of capital mobility, wondering whether the city is the level at which redistributive policies should be implemented. According to Hannah, fiscal crises are constraints on the political agenda at all levels of government, and municipalities should not expect to be exempt from fiscal accountability. She asked that Shefter identify the 'controllability' of crises: whether a crisis was due to mismanagement or to structural problems. Another participant took issue with Shefter's characterization of the fiscal accountability of municipal governments as 'undemocratic', maintaining that in the long run, if cities do not wish to be beholden to financiers, they must accumulate surpluses.

The final session of the conference, on national budgetary processes and fiscal crises, was chaired by Barry Eichengreen (UC-Berkeley). Kevin Hoover and Mark Siegler (both of UC-Davis) opened with 'Two Centuries of Taxes and Spending: a Causal Investigation of the Federal Budget Process, 1791-1990'. Using techniques developed in earlier work by Hoover and Sheffrin, the authors attempt to identify the direction of causation between taxes and spending under the various regimes that have characterized the Federal budgetary process. In particular, they attempt to isolate instances in which causation changed radically, as in the early 1970s. Their approach coordinates narrative historical and institutional knowledge with econometric techniques to identify structural change. Hoover and Sheffrin conclude that no simple economic model can be applied to explain the causal relationship between taxes and spending over the history of the US; changes in the structure of causation are not related in any simple way to political parties or to budget reform.

In 'Tax Regimes and National Crises in the United States:

(continued on page 32)

Report on the Canadian Cliometrics Conference

by
Karen Clay and Gillian Hamilton, University of Toronto

(Montreal PQ) The 19th conference on the use of quantitative methods in Canadian economic history was held in Montreal on April 8-9, 1994. Mary MacKinnon (McGill) and Rick Szostak (Alberta) organized and hosted a conference dedicated to Malcolm C. (Mac) Urquhart's contributions to Canadian economic history, and honoring his leadership of the remarkable 15-year collaborative effort that culminated last year in publication of the new Canadian national income series, *Gross National Product, Canada 1870-1926: The Derivation of the Estimates.*

To honor Mac, academics had arrived from across Canada and the United States – even from the United Kingdom – sparking the usual banter about relative living conditions. Weather was the principal bone of contention. Those of us from central Canada, where the snow had almost disappeared, lorded our relatively balmy conditions over those from Quebec City (still digging out from the previous day's two-foot snowfall), but we experienced the usual envy (and loathing!) of those West Coasters who relentlessly described the flowers already blooming in their gardens.

The first session, chaired by Almos Tassonyi (Ontario-Municipal Affairs), opened with Gillian Hamilton's examination of the determinants of contract duration in early-19th century craft apprenticeships in Montreal. She argued that, while variation in length should reflect human capital differences (as in the case of indentured servants), the duration of an apprenticeship contract involving a young boy should also reflect how uncertain a master was about his candidate's true quality. Although uncertainty is not directly observable in the data, she analysed its role by examining probationary periods and annulment behavior. Lou Cain (Loyola and Northwestern), one of the discussants, wondered what the reduced-form estimates masked, and called for a more complete presentation of descriptive statistics to help separate supply from demand factors. Jose Igartua (Université du Québec, Montréal), the second discussant, questioned the assumption of competition, since many masters employed just one or two apprentices at a time. Ruth Dupré (École des Hautes Études Commerciales) asked why trial periods weren't used more frequently.

Michael Huberman (Trent) and Denise Young (Alberta) followed with their paper on the effect of unions on strike behavior: 'What Did Unions Do? An Analysis of Canadian Strike Data, 1901-14'. Their aim was to assess the divergent approaches to strike behavior taken by labor economists (who emphasize screening and the role of acquiring information) and historians (who stress issues of power). They use micro data on individual strikes in a hazard model of strike duration. Their results do not support the screening model, but instead are more supportive of the historians' viewpoint. They conclude that economists' findings on the determinants of more recent strikes do not apply directly to earlier periods. In her discussion, MacKinnon suggested they include location variables which might capture differences in behaviour across city size. She also suggested including seasonal dummy variables; after all, she queried, 'who, in Canada, would strike in the winter?' Igartua, responding to a call for yet more dummy variables, quipped that 'maybe we are the dummies using the variables put out by the Labor Department'.

Herb Emery's (Calgary) paper was on the decline of fraternal sickness insurance, using data on British Columbia Oddfellows (1891-1950). Asking whether private insurance was crowded out by public insurance, or whether some other change affected the viability of private insurance, Emery focused attention on the demographic requirements of a stable fraternal insurance scheme and on a cohort effect associated with the Great Depression. During the Depression few new members joined, and the existing membership aged. After the Depression, it became very difficult to attract the necessary representation of young (healthy) members. Emery argued that the decline allowed, and was not caused by, the growth in public insurance. Cain, Igartua and MacKinnon were all concerned about the many reasons for joining a social club and called for more information on the Oddfellows' organization and their membership. 'How odd are these fellows?' Cain asked, raising the possibility that the age distribution had changed over the relevant period, given the cohort effect of World War I. Urquhart (Queen's), drawing on his Prairie upbringing, wondered whether the British Columbia experience was representative. Farmers, he argued, typically worked such long days they had little time for a social club. He also shed light on other reasons for joining such clubs: many had their own bars at a time when liquor laws were quite rigid. Knick Harley (Western Ontario) wondered why young people didn't set up their own clubs.

After a lunch break, George Emery (Western Ontario) brought the first afternoon session to order. Marvin McInnis (Queen's) launched the session with a brief version of his lengthy study on 'Immigration, Emigration and the Canadian Economy of the late Nineteenth Century'. He re-examined economic performance in light of the Urquhart series and addressed the confusing co-movements of high immigration and high emigration to the USA. McInnis argued that growth was comparatively slow over the period, so the main puzzle is the high immigration. The immigration data, he contended, suffers from egregious double-counting. His revision of the series to correct for this rectifies the previous confusion. Tim Hatton (Essex) suggested that Canadian immigration should be compared with British emigration to Canada, since this would capture most people entering Canada at this time. A cursory examination of these data had led Hatton not only to agree with McInnis's claim regarding the inflated immigration series, but also to suggest McInnis's revisions were conservative. Hatton also asked about sources of emigration. McInnis responded that the rural population was largely responsible for the out-migration to the USA, reflecting an inability of Canadian urban centers to absorb the growing population. This sparked a flurry of comments on rural-urban issues. George Grantham (McGill) asked whether the types of rural-urban emigration differed between Quebec and the rest of the country. Kris Inwood (Guelph) wondered why the urban sector had failed to absorb the excess of rural workers, and Ann Carlos (Colorado) raised the issue of endogeneity of family size.

Alan Green (Queen's) followed with 'The Nature of Technological Change in the Canadian Railway Sector, 1875-1930'. He began by informing us that, unlike Marvin's 'tip of the iceberg' paper, his paper was 'the whole iceberg'. The impetus for Green's paper was Urquhart's article in the 1959 *Canadian Journal of Economics and Political Science* on capital accumulation and technological change, where he had observed that labor's factor share in the US railroad industry had risen dramatically between 1870 and 1920. Using Urquhart's new data, Green finds the same overall trend for Canada, but he is able to detect two sub-periods: labor's share fell between 1875 and 1899, but grew sharply over the first two decades of the 20th century. Relating these movements to technology, he suggests that the nature of railroad technology altered over the period – from saving both capital and labor equally before 1900, to principally labor-saving technology after 1900. This regime shift is consistent with a decline in the interest rate:wage rate ratio, which began shortly after 1900. Bill Marr (Wilfrid Laurier), discussant, raised several questions concerning inferences Green draws from his tables. For example, while Green argued that labor's share rose between 1900 and 1920, Marr noted that the series was quite flat between 1900 and 1915 and rose quickly in the remaining five years. What, then, Marr asked, was unique to the latter period? T. K. Rymes (Carleton) commented that Green's paper pointed out the problem with Solow's attempt to measure the 'residual' in a growth accounting framework.

The theme of the final session of the day, chaired by Dupré, was the use of microeconomic data to study historical issues. Livio DiMatteo (Lakehead) presented a controversial paper written with Peter George (McMaster) on the use of historical micro data to interpret Canadian economic history. Their paper reflected on the value of and the future for such research. They suggested that while micro studies offer a source of comparison to macro series, the 'successes' have been few. They speculate that this might be attributed to the nature of most Canadian micro data sets, which are more likely to have been constructed to address sociological questions raised by historians or demographers. They urged economic historians to take a more active role in data collection. In discussing their paper, Fazley Siddiq (Dalhousie) suggested they examine the appropriateness of the tools and methods used to interpret various data sets. Szostak, the second discussant, thought everyone should consider whether the incentives academics face should be adjusted in light of the fact that time-consuming micro data collection conflicted with the 'publish or perish' paradigm. Several people objected vigorously to the notion that there had been few successes. Ron Shearer (UBC), for example, argued that they focused almost exclusively on data concerning people, while ignoring studies of businesses – savings banks, the Hudson's Bay Company, the fur trade, etc. Marilyn Gerriets (St. Francis Xavier) called for people to focus their attention on micro studies during the years when the censuses are particularly poor.

Morris Altman (Saskatchewan) followed with 'Land Tenure, Ethnicity and the State of Agricultural Income and Productivity in Mid-Nineteenth Century Quebec', which quantifies a long debate on the relative position of French *versus* English farmers in Quebec. Using the 1851-52 census, he finds that, while farm income, partial factor productivity and farm size were lower for French Canadian farmers, *Les Habitants* were capable of profitable farming as their farms around Montreal earned relatively large agricultural surpluses. His findings challenge the view (attributed to Fernand Ouellet) that French-Canadian farmers earned lower incomes than English-speaking farmers because of an intransigent *mentalité*. He also challenges arguments attributing the malaise of French Canadian agriculture to the existence of the seigniorial system. Although Altman's purpose was to measure the relative positions, most of the ensuing discussion centered on trying to explain them. Clay, the discussant, wondered if ethnic groups had differential access to capital markets, McInnis suggested the difference lay in relative access to product markets, and Hamilton raised the possibility that French Canadians had enjoyed better access to certain labor markets.

Kris Inwood closed day one with his paper on the views of industry presented in the Canadian census between 1871 and 1891. He stressed the pitfalls of trying to compare these data across censuses without carefully reconstituting them, and focused attention on the difficulty of this task. The late 19th-century censuses counted repair work as manufacturing, for example, and also enumerated different processes within an establishment as separate establishments, despite the fact that the enterprises used the same building, labor and other inputs. Siddiq and Szostak, the discussants, applauded Inwood's efforts. Siddiq asked Inwood to consider how his reconstituted data could be compared with subsequent series. Rymes suggested another technique for addressing the vertical integration problem.

The evening's festivities included dinner and speeches at a local French restaurant. Mac Urquhart was toasted by Lance Davis (Cal Tech), who recounted tales of Mac's role at early Cliometrics Conferences, and by Alan Green, who lauded Mac's ability to bring people together, to lead and guide collaborative efforts through to completion. On William Parker's behalf, Alan presented Mac with a book of Scottish toasts. Mac responded in kind, regaling the group with a centuries-old Urquhart family toast.

Saturday morning began with the 'Work in Progress' session, chaired by Michael Hinton (Entreprises Economica). Sung Huh (Carleton) and T. K. Rymes discussed their research on the movement of the Canadian banking system from bilateral clearing in the 1870s to a strictly multilateral system after 1901. Their question is whether bank reserve behaviour conformed to the Edgeworth Hypothesis, which predicts that banks would not need to hold any cash. Tony Ward (Brock) then argued that an important component of the Wheat Boom in the Canadian Prairies was climatic change in the late 1880s and 1890s, which raised the average temperature and lengthened the growing season. Next, Nancy South (Victoria) provided evidence that the Britannia (B.C.) copper mines awarded incremental wage bonuses tied to the price of copper, asking why the firm followed this practice and how closely wages followed changes in copper prices. Patrick Coe (UBC) in his turn presented preliminary results suggesting that the introduction of the FDIC and the fixing of the price of gold in January 1934, not Roosevelt's New Deal, were the key elements in inspiring investor confidence and recovery. Finally, Neil Quigley (Western Ontario) examined the claim that implicit deposit insurance was present in Canadian banking between 1890 and 1966, concluding that although losses to depositors post-1900 were very low, mergers of struggling banks with stronger banks, not government payments, played a key role in limiting losses.

The Monetary Economics session, chaired by Kieran Furlong (Toronto), featured a paper by Michael Bordo (Rutgers), Angela Redish (UBC), and Hugh Rockoff (Rutgers) on the Canadian and United States banking systems in the 20th century. Examining the two systems between 1920 and 1980 along the dimensions of stability and efficiency, the authors shed light on the relative strengths and weaknesses of nationwide branch banking *vis-à-vis* unit banking. During this period, Canada recorded a single failure; in contrast, the United States experienced continual bank failures, often at high rates. The consolidation of Canadian banks prior to and during the period of study, however, led to a small number of banks controlling a very high proportion of bank assets. Based on available data on the interest paid on deposits, rates charged on loans, and return on equity, Bordo, Redish, and Rockoff concluded that the higher net rates of return on equity were attributable to the higher asset-equity ratios of Canadian banks and not to exploitation of their oligopoly power. Pierre Siklos (Wilfred Laurier),

(continued on page 33)

Consider the Contrapositive

It has become the custom of the cliom nation to gather and hear the word of the Mullah Nasr-ed Din's faithful servant, the most senior of the jayhawk tribe. Unfortunately, this year he boarded the great silver bird for the place called Europe searching for other truths. One hopes his search was not in vain. Our search led us to the desert home of the southwestern wildcat tribe, whose warm welcome and bounteous offerings caused the slioms to hope they would return soon.

Consequently, it has fallen to a less trustworthy, but no less obedient, servant to report on this year's search for the statement uttered in the passion of the moment that best reflects universal truth. Many members of various tribes instructed your servant to write things on the list. In obedience, it was done. This, however, should not be taken to mean that such utterings even begin to conform to the ideal.

The hirsute hawkeye (he who would instruct us on speaking persuasively) said, then pronounced as good, 'You can do lots of things with invisible charts.' While this has a certain universality, to paraphrase the youthful member of the crimson tribe, 'I am sorry to say it is demonstrably false.' The demonstrator was none other than the chief of the cliom nation whose scheduled 10-minute display of high-tech wizardry was reduced to a primitive visual aid. Were this statement true, he would have dazzled the assembled throng before a lighted, albeit blank, screen.

He who teaches at the university named for a jar maker noted, 'Death is the perfect excuse for not working.' This is suspiciously close to last year's winner, 'Single people do worse, because they die before they have a chance to get married.' Surely the utterer realizes the competition will not be skewed toward parallelism. More to the point, the statement leaves unspecified who offers this as justification for idleness, and who receives the approval. Can the excuse be offered in perpetuity, or is this only a short-term apology, with the impression left she or he may return at some uncertain future date? Can a firm lay off in perpetuity one who is deceased? This same chap commented, 'There are not too many humanitarians in the 18th century.' This is appealing because of what economists believe about the relevance of scarcity over time and space. Unfortunately, it suffers from a McCloskey type-I error. How many not too many is not too many enough? How can we identify when we have enough? If we have not too many, how big a bribe does society require to produce more? What kind of world would this be if we had too many?

A disturbing definition was offered by he who shies from bulldogs and laments over bulls, 'We made these and these and these assumptions, and out came these numbers. That's Cliometrics!' This is new economic history in an old black box. Why, pray tell, is the box always black? Would a box of any other color produce the same results? Further, it would appear from the definition that the investigator must assume numbers. Do we infer that the conclusions must also be assumed? Can this be what is meant by 'Explaining the fullness of the human condition'?

One member of the golden bear tribe, he of significant stature and adequate nutrition, noted, 'You don't need big shovels to build great highways.' This is universally true for those who appreciate the asymptotic properties of isoquants. It does, however, raise the question whether this restaurateur gathers provender for his establishment from a flowerpot. Perhaps because of his stature, perhaps because of his moonlight calling, he also remarked, 'Canned food lowers living standards because it tastes terrible.' While interpersonal comparisons of utility are strictly forbidden, it seems unkind as the fiftieth anniversary of D-day approaches to remind those in their golden years that the calories were courtesy of Spam. Those planning to duplicate their parachute jumps into northern France might note he also commented, 'Increased mortality is good.'

We come thus to this year's finalists. The third runner-up, uttered by the aforementioned gentleman from Jar Maker U. is, 'Firms that are not observed, don't survive.' While this is potentially true, it is not universal. No matter how hard some firms, some individuals, try not to be seen, no matter how important it is to be unimportant, if they choose not to survive, if they call Dr. Kevorkian, almost certainly they will be observed. Consider the obverse; being observed is no guarantee of survival. Consider the converse; survival is no guarantee that you will be observed. Once again the conference volume is devoid of any reference to your servant's published works.

The second runner-up, articulated by the eastern practitioner of public choice, he whose university in part is *named* for a jar, has a universal quality, but it is not the truth. It is a question: 'If r-squared pops up a bit, does government matter?' Once again it is relevant to inquire into the standard to be used. Will an increase in tax rates for wealthy individuals produce a pop? Will a coherent foreign policy produce a big bang? In the 1950s, Jonathan Hughes visited Columbia, where one of his colleagues averred, 'Whenever you find an r-squared of 0.85 or more, you are in the presence of an economic law'. One hundred pairs of series randomly selected from the *Historical Statistics* resulted in r-squareds of 0.85 or greater more than 80 percent of the time. Is there still room for a pop here, or does government not matter? Does anything matter? Is anything significant?

The lassie from the mountainous town of the north with the university named after a fellow Scot insisted, 'If you don't know where it is, it's in Saskatchewan.' This could well have been true in the 1930s when rail travel was still in vogue, but it is not universally true. In particular, it fails to consider the significant role played by transportation costs. Have you ever tried to get to Saskatchewan? Many of us may know someone who claims to come from Saskatchewan, but have you ever met anyone who claims to be going to Saskatchewan? Pilots giving airborne geography lessons may insist you are flying over Saskatchewan, but it looks exactly like Manitoba, and there never seems to be any there there.

Finally, this report turns, as it inevitably must, to the one who inspired this award, she who served as hostess this year, she who has come close to uttering the winning phrase on other occasions, and she who is this year's winner – the first to have her wisdom inscribed twice in the book. 'I didn't know I was going to know the answer to the question, but now it's a different question.' How universal! How true! While this does not open a can of worms larger than the universe, it clearly describes how that could occur. It concisely describes the process of research; the answer is present long before it is recognized, during which time one's mind continues to conjure new questions. And isn't this a more accurate measure of academic productivity than two additional students sleeping in the rear of a classroom?

Next year the климs will assemble at the campfire of the jayhawk tribe, at the home of the Mullah Nasr-ed Din's faithful servant. Next year, this year's most deserving winner of the Clio Can, he who bears a close resemblance to Rusty Trawler, must face the daunting prospect of preparing a musical tribute to past winners. As a tribute to his style and the elegance of his work, one tune stands out, 'Top Hat, White Socks, and Tails.'

In closing, two additional truths from our discussions command attention. The sandy-bearded youth from the levee above the big muddy noted, 'That question is more interesting than the one I'm asking.' Each year, when the clioms gather, they ask penetrating questions of each other. Each year the clioms return home stimulated, refreshed, renewed, ready to ask more penetrating questions of their own research. He of the southern tiger tribe reminded us, 'The world isn't a two-period place.' Clearly not; it's three. And so it goes . . .

Your servant from the
ramblin' wildcat tribe

The Ballad of The Can

Lyrics by Don McCloskey
(Sung to the tune of 'The Rebel Soldier' , *c.* 1865)

The Cliometric Nation has given some The Can:
McCloskey, Sutch, and Joel Mokyr, they got the thing and ran;
Ransom, Weiss, and Larry Neal, Pete and Jeffrey, too;
Sam Williamson was number nine – he's not inclined to sue.

Chorus:
I'm a clio'trician, now that's just what I am.
For theorems, *t*-tests, unit roots I do not give a damn.
I find my own statistics; I know them like my hand.
I ask important questions, 'bout K and L and Land.

But remember now the story of how our tribe began:
The teachers of that 'youngish' group thought worthy of The Can,
Pioneering generation, the giants to this day,
The Fogels, Norths, and Gerschenkrons must figure in the lay.

Chorus

And chief among the giants, with Engerman and Jon,
With Parker and with Gallman in that tremendous dawn,
A student of Yank banking, capital's sweet dance,
Whaling, British burdens: Sir Davis *de la* Lance.

Chorus

Clio Report *(continued from page 1)*
difficult for owners to monitor managers effectively. However, after the Tokyo Stock Exchange reopened in 1949, share ownership again became concentrated and the monitoring problem was further reduced by firms' ties to major banks. He tested this hypothesis by examining a group of reorganized firms and found that firms with dispersed ownership performed less well than firms with more concentrated ownership.

Joel Mokyr (Northwestern) raised the first of many questions about Yafeh's choice of 'firm performance' variable, arguing that return on assets was preferable to Yafeh's profits-to-sales ratios, since a decline in profits-to-sales is not necessarily evidence of a poorly-managed firm, especially if that firm is trying to maximize sales or market share. Furthermore, Kinghorn and Randall Nielsen (Washington U.) pointed out that if the occupation authorities had been successful in breaking up monopoly firms, then one would expect a declining profits-to-sales ratio. Ken Sokoloff (UCLA) and Libecap suggested that variables other than mismanagement, such as increased transactions costs, could result in a decline in Yafeh's performance measure. LaCroix and others claimed that the ratio of profits to value-added would be better. The monitoring question was the other main target for comments, with Lawrence Boyd (West Virginia) asking what information was available to banks but unavailable to individual stockholders. David Zalewski (Providence) commented that secondary securities markets could have served to concentrate stockholding and monitoring; Gillian Hamilton (Toronto) pointed out that monitoring could also have been accomplished with performance-based contracts for firm managers.

Gerald Dwyer (Clemson), presenting a paper written with Iftekhar Hasan (New Jersey Institute of Technology), investigated the reasons for divergent bank failure rates in Illinois and Wisconsin in 1861 when the prices of Southern bonds (used as backing for banknotes) fell precipitously. Their hypothesis was that the Wisconsin banks jointly restricted payments from April 1861, while the Illinois banks did not. They develop a model of the restriction process, showing that banks may benefit from a restriction which enables the banking system to sort out relative bank soundness. They conclude that payments restrictions explain the lower proportion of banks closing in Wisconsin than in Illinois.

Following a conference-long practice of noting the absence or presence of models, Warren Weber (FRB, Minneapolis) questioned the authors' use of the Diamond-Dybvig intertemporal model. Both Kroszner and Eugene White (Rutgers) suggested that a 'political economy' model of payments restrictions would be useful and more appropriate. Paul Rhode (North Carolina) pointed out that Wisconsin in particular might have borne some costs as a 'restricting' state. Others suggested potentially relevant differences between Wisconsin and Illinois aside from their records of payments restriction. Jeremy Atack (Vanderbilt) asked about the bank asset mixes in the two states; Wallis, in a comment seconded by Roger Ransom (UC-Riverside), pointed out that Illinois was more firmly tied to Southern railroads, which may have made its banks more prone to fail. Sokoloff wondered about links between dates of bank closure and 'saving legislation'. Greg Clark (UC-Davis) and Larry Neal (Illinois) wanted more disaggregated information on bank location, asset mix and capital, better to sort out variables which could cause differential failure rates. Finally, Stephen Quinn (Illinois) raised the counter-factual: Could it be that Illinois banks were forced to fail, rather than that Wisconsin banks were saved from closing?

For her investigation of the determinants of apprenticeship contract structure, Gillian Hamilton used a set of contracts notarized in Montreal between 1791 and 1820. The central issue of her paper was masters' uncertainty about worker quality and the likelihood of completing an apprenticeship. One way around the uncertainty was a probationary period before an apprenticeship contract was signed. Hamilton claimed that uncertainty usually led to longer contracts as masters equated expected benefits and costs, but that use of a probationary period tended to dissipate uncertainty and thus to shorten apprenticeship contracts.

Sands asked about the prospective apprentices' uncertainty about masters, and Phil Coelho (Ball State) suggested that information about masters was readily available since Montreal was a relatively small city at the time. Sokoloff proposed that data on the master's 'track record' could be added to the regressions, and that uncertainty could also be estimated by looking at whether masters or apprentices were more likely to annul contracts. Atack wondered whether probation was offered by apprentices as a signaling device, rather than always being a risk-reducing demand of masters; Williamson suggested a look at data on apprentices failing probation. Dupré and Kantor asked about non-probationary means of addressing uncertainty, such as end payments and contingent contracts. Claudia Goldin (Harvard and Brookings) brought the discussion back to a more theoretical level, asking whether uncertainty in the apprenticeship process resulted in suboptimal amounts of training.

In a session safely distanced from meal breaks, Joel Mokyr and Rebecca Stein (Northwestern) argued that the discovery by Pasteur of the 'germ theory of disease' in 1864, and its refinements by others, ultimately provided households with information motivating them to change their lifestyles and reduce mortality and morbidity significantly. That is, people were convinced that changing behavior would improve health, and household organization changed to provide more home labor inputs for cleaning and food preparation. Mokyr and Stein tied improvements in knowledge to declining mortality rates and concluded that 'some eight to 39 percent of all welfare improvements during this time' can be attributed solely to change in health technology.

Those attending a Clio Conference for the first time: (l. to r.) Stephen Quinn, Josh Gotkin, Randall Nielsen, David Zalewski, Janice Rye Kinghorn, Yishay Yafeh, Joyce Burnette, Lawrence W. Boyd, Gillian Hamilton, Warren Weber, Rebecca Stein.

Most participants, like Barry Eichengreen (UC-Berkeley), wanted more convincing that Pasteur *et al.* were as important as Mokyr and Stein assert. Sokoloff said he would put more weight on the public health measures instituted well *after* Pasteur, but McCloskey attributed much of the observed decline in mortality to pre-Pasteur 'evangelical cleanliness' as preached by Florence Nightingale, while Clark thought the 'revolution' could adequately be subsumed by previous stories of technological change. In any case, Wallis was suspicious of the chain of assumptions leading to the authors' estimates of the welfare effects of the Pasteur revolution. Other participants were dubious about the model of household production and consumption employed by Mokyr and Stein. Lance Davis (Cal Tech) pointed out that the model could not explain consumption of goods, such as cigarettes, known to increase mortality. Boyd wanted more explanation of how the 'macro-invention' of germ theory might translate into household-level production functions. John Komlos (Munich) thought some attention should be paid to the time profile of consumption, since nutritional advances would make more difference to infants than to adults. Finally, while Ransom thought appealing to Pasteur was important as a rhetorical device for persuading people to improve their quotidian hygiene, Dupré pointed out that Rousseau – cited by the authors as an influential proponent of breast-feeding to reduce infant mortality – had nonetheless abandoned his five children.

Roger Ransom, Richard Sutch (UC-Berkeley) and Samuel Williamson presented what manv saw as three short papers, all related to provision of government pensions to Union Civil War veterans. First, the authors pointed out that these pensions were the single most important item of non-military Federal expenditure in the decades at the turn of the 20th century and that tariff revenues were used to finance the pensions. Second, they suggested that pensions had reduced labor force participation rates. Finally, they asked whether pensions had delayed the rise of a national social welfare system, but concluded that institutional factors, as well as high personal savings rates, were responsible for the delay.

Addressing the first of the three papers, Gavin Wright (Stanford) and Wallis asked about the difference between actual tariff levels and political rhetoric regarding tariff levels. Jim Shepherd (Whitman) wondered why the authors tied continuing tariffs to pensions rather than looking at all the other types of government expenditure which had to be financed during this period. Joyce Burnette (Northwestern) inverted the question, suggesting that pensions were only a vehicle for spending revenues from desired tariffs. Fishback, Weber, and Libecap questioned the authors' assertion that veterans joined with protectionists to provide the Republican party with a strong political coalition; Neal was concerned that World War I was not addressed at all. On pensions and labor force participation, Odell pointed out what appeared to be an anomaly in the regression results which suggested that Confederate veterans – who did not receive Federal pensions – were more likely to retire than Union veterans. Mokyr, Dwyer, and Coelho followed up, asking what was so different about the Confederate veterans' world that could create this anomalous result. Odell and White asked whether the availability of pensions would have been likely to raise Southern and reduce Northern savings rates. On the final paper, Goldin commented that the authors' linkage of Civil War pensions to the delay of universal pensions was tenuous, especially for the 1920s. Kantor pointed out that voters rebelled against workmen's compensation legislation in the early 1900s, and that the same attitude might have prevailed toward legislation on pensions.

Friday night participants boarded vans and rental cars bound for the original Tucson, near the present-day downtown. After the Mexican buffet hosted by the University of Arizona, many had planned to browse the shops surrounding the courtyard, but attempts to conclude the evening buying souvenirs of the old Southwest were thwarted by their discovery that all the shops had closed.

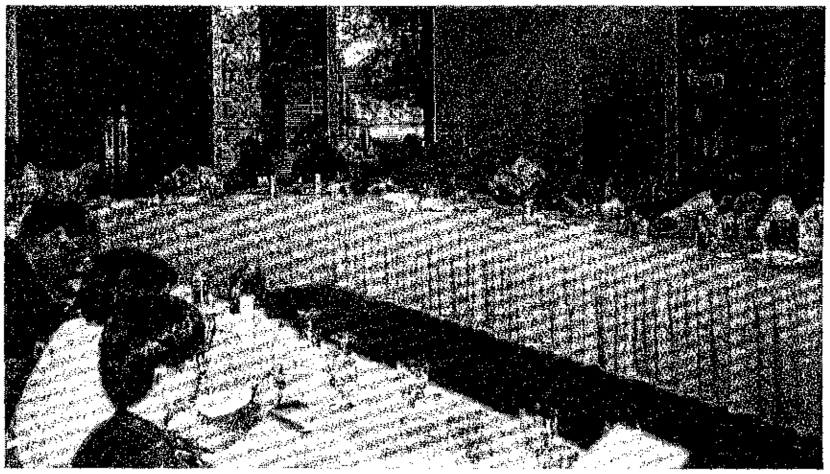
Phil Coelho and Robert McGuire (Akron) began day two with a paper asking why European indentured servants and African slaves ended up in geographically separate parts of the New World. They argued that with the importation of the first African slaves, African tropical diseases became Caribbean tropical diseases, and Europeans began suffering from higher mortality. On the other hand, Africans suffered disproportionate mortality in the colonies north of the Chesapeake, where the disease environment was dominated by 'European' diseases: measles, influenza, and whooping cough. Under assumptions about slave and indentured servant life expectancies, maintenance costs, and productivities, they show that differences in life expectancy could explain why slaves went south and servants went north.

Comments centered on two issues. First, factors other than disease resistance might have been at work. A number of people wanted the authors to place greater emphasis on the labor potential of indigenous peoples, with Mokyr citing the Dutch East Indies, Libecap citing Brazil, and Kinghorn citing Peru. Stein then urged the authors not to dismiss differential fertility rates in different areas. Sokoloff claimed that enslaved peoples could not be separated from the methods of production – particularly the gang labor system. Goldin suggested that Africans might have been preferable as slaves because, in a white population, they were easier to identify as runaways. Reid brought up the possibility that culture also played a role since there was hesitation to enslave Christian populations. Finally, Sutch argued that racism be kept in mind as major factor in determining which peoples were enslaved. The second line of questioning related to human adaptation to new disease environments. Atack referred to recent literature which argues that evolutionary adaptations can occur in as little as two generations. White wondered if acquired immunity could account for the 20,000 African slaves in New York in 1790. Sutch rounded out by asking if any of the New World locations could have become more healthful as they developed economically.

Joyce Burnette's paper provided an analysis of labor market sex segregation and competition during the Industrial Revolution in Britain. Noting the average differences in strength between men and women, she used a model of a competitive labor market to determine whether or not male and female workers were occupationally segregated. Using data on English farm servants from 1768-1770, she estimated the cross product effects of men's wages on women's employment (and vice versa) and found that, accounting for comparative advantage based on strength for unskilled labor in a competitive market in which both men and women had participated.

The first round of questions addressed exogeneity in Burnette's analysis. Sutch wondered what inputs were taken as given – was there a prevailing 'strong man technology' that was not altered to fit female strength levels? And why was it assumed that technology is fixed and labor is mobile, rather than assuming that technology is changed to fit available labor? Goldin followed up by asking how Burnette would account for the observation that different practices were used at different times. Sokoloff and Clark suggested that wage determination should control for clearly exogenous variables such as soil type and geography, but Clark cautioned that assuming exogeneity of the number of farm animals was inappropriate, since these animals could produce different outputs requiring different types of labor. Others questioned Burnette's measures of wages and employment:

Wallis pointed out that the wages used were town-level, but employment was farm-level; Komlos wanted to see data on real wages; Coelho asked about non-wage remuneration. Finally, the possibility of other sorts of discrimination was raised, with Boyd addressing occupational segregation within the household, Fishback suggesting limitations which kept women in unskilled jobs, and David Greasley (Edinburgh) mentioning legal restrictions on types of work women were allowed to do.



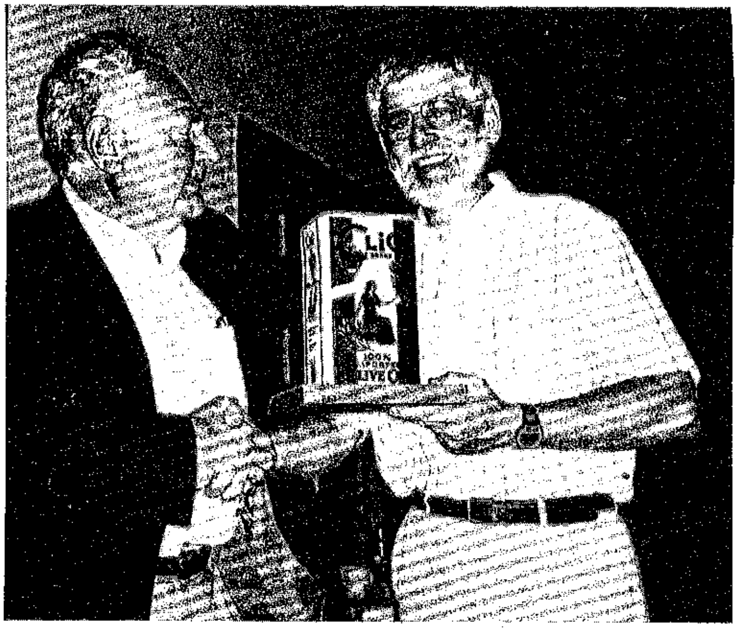
In search of the well-defined variable.

Randall Nielsen's paper questioned work by Robert Fogel (among others) on private grain inventories in Britain, and government policy regarding their distribution, between 1600 and 1640. Nielsen substituted a dynamic rational-choice model of inventory control for the previous assumption that inventories were a fixed proportion of a normal crop. His model explains the significant reduction in the variance of grain prices in Britain without reference to 'paternalistic' government policy aimed at avoiding famine. Nielsen claimed that the observed smoothing of grain prices during the period was, instead, part of a more general period of price stabilization for grain throughout western Europe and for many goods within Britain.

Although Nielsen argued that British price stability was not due to British government policies, Komlos, White, and Dupré wondered whether other European governments might also have been undertaking price-smoothing policies. Clark added that inventory confiscation schemes may have been in place at the local level in Britain years before the national policy. On the other hand, Kantor suggested that confiscation policies might have been more rhetorical than real and that such a policy would be likely to increase rather than decrease uncertainty. Sokoloff and McCloskey argued that observed price correlations were likely anyway in light of the international grain trade. A number of participants were puzzled by Nielsen's assertion that price variation for other commodities in Britain was also diminished. LaCroix wondered about changing inflation rates, while Sokoloff asserted that grain prices were likely to drive all other prices during this period. Clark asked for a better-articulated inventory model, which would then allow Nielsen to separate the effects of changing weather and changing storage behavior. Mokyr and Neal also asked how Nielsen could explain price stability during the Baltic war early in the 1600s.

Using detailed pension records of the Canadian Pacific Railway (CPR), Barton Hamilton and Mary MacKinnon (both of McGill; MacKinnon presenting) showed that the composition of employment separations changed substantially between 1903 and 1938. Over half the employees hired between 1903 and 1913 who left the firm had quit; that share fell to only 16% for workers hired between 1930 and 1938. Hamilton and MacKinnon attempted to determine whether the shift was due to changes in attributes of the labor force or changes in the employment policies of the railroad. While worker characteristics did affect the probability of a separation, they found that the main source of the shift from quits to layoffs was company policy to promote employment stability.

As with earlier papers, definition of variables was foremost in many participants' minds. Goldin wanted to know what a layoff meant, while Sutch wondered what constituted a 'continuous job spell.' Mokyr argued that the numbers of quits and layoffs might be underestimated in the data, since some workers could have been transferred between departments of this large enterprise rather than face a job separation. Komlos again wondered how wages fit into the story, and Kantor proposed that wages might have been changed to compensate employees for unemployment risk. Seeking a refined model and story through use of disaggregated data, Fishback asked if workers in different jobs at CPR had been treated differently with respect to layoffs, and Neal suggested the degree of unionization in different jobs might have affected the ratio of quits to layoffs. Other proposed influences on this ratio were the degree of firm-specific training (Nielsen) and the number of external job opportunities (Williamson). Cain suggested the authors look at the rise of the trucking business as a factor in CPR's decision-making. Hannon warned against assuming that the shift toward layoffs was a mistake or an unintended consequence, and Hamilton pointed out that unemployment may have served as a new form of worker discipline.


Lance Davis accepts The Can from last year's winner Sam Williamson.

Stephen Quinn's paper on goldsmith-bankers in 17th-century London illustrated how a network of personal contacts substituted for a central clearinghouse for notes, such as the one in Amsterdam. Quinn found that the London network, in which bankers would extend or accept overdrafts from other bankers, increased liquidity and promoted the spread of banknotes and checks in the period before the Bank of England was founded. Using the accounts of Londoner Edward Backwell, Quinn related specific note-clearing patterns among bankers to the nature of their personal relationships.

Interest in the details of this network was high. Mokyr pointed out that such 'insider' networks were typical of Jewish communities. McGuire emphasized the trust that was built up between Backwell and his masters or apprentices; he and McCloskey wondered if the Dutch had resorted to a formal clearinghouse because they were less trusting. Kroszner asked about the role of the Goldsmiths Company in promoting or regulating these networks. Zalewski speculated on the optimum and maximum sizes of banker networks. Wondering what Backwell had gained by holding significant no-interest overdrafts, Mokyr thought that other 'goodies' might have been offered. Weber wanted to know what losses on demand notes occurred in this unregulated system. Finally, Wallis urged Quinn to place his analysis within the rise of fractional reserve banking and increased lending by bankers to the British government.

With a question queue that had begun to form three hours before his paper was given, Greg Clark closed the proceedings in true Cliometrics Conference style. The paper brought us back to British government finance, but this time to war-related borrowing between 1720 and 1837 and the crowding-out question. New data on returns from owning land and houses, private perpetuities (rent charges) and returns on bonds and mortgages (recorded in the Charity Commission reports) allowed Clark to look more closely at interest rates paid on assets for which there were no usury restrictions. His results showed that neither government debt nor military expenditures had a large effect on rates of return; Clark concluded that government debt must have been financed from increased domestic savings, rather than by crowding out private investment.

Clark was immediately chided for what Williamson argued was a misunderstanding of the distinction between debt and deficits and their roles in crowding-out. Sokoloff and Neal both had reservations about the data from the Charity Commission, which they felt may have been given under duress. Beyond these definitional and measurement issues, the main theme of participants' questions was Clark's counterfactual world. Dwyer cited other studies which had shown no relationship between increased levels of government debt and higher interest rates; in that light, he thought the one percentage point increase in interest rates that Clark had found amounted to a significant financial impact from the British debt. Fishback then noted that Clark's data showed the government debt accounted for half the change in interest rates. Both McCloskey and Neal (proprietarily) felt that Clark too easily dismissed the international financial market and the possibility that British interest rates remained steady due to large inflows of capital from abroad. Mokyr agreed that resources to finance the Industrial Revolution could have come from other countries, but stressed that there have been other instances – such as railroad building – when massive domestic investments had been financed from domestic saving.

Following the rigors of the final session, participants convened on the patio for Saturday dinner and the annual awards ceremony. MC Price Fishback orchestrated the awards with the timing of Billy Crystal, thus revealing why he receives more invitations to announce national swim meets than to lecture about coal miners. Lou Cain (another of Clio's distinguished raconteurs) reminded Conferees of the essence of this year's revealed wisdom. The climax of the ceremony was the tenth presentation of The Can: this time by Sam Williamson to Lance Davis (co-founder, in December 1960, of the Cliometrics Conference). A group rendition of the commemorative song, penned by Don McCloskey, the first recipient of The Can, ended the rite.

Participants departed the torrid Southwest still bemused by the numerous questions dancing in their heads. Is a bad model better than none? Would Florence Nightingale have had sufficient upper body strength to knock out Jean Jacques Rousseau? How could I possibly be overdrawn if I still have cheques? Is the world a smaller place than we think? And if so, shouldn't we pay more attention to war (at least in the Baltic or Prussia) and pestilence (at least malaria)? How big is big, and is it either significant or important? Are Confederate veterans and the Dutch really different from you and me? How far and how fast can you disaggregate? And has anybody yet asked the right (or, at least, the relevant) question? Clearly, there will be a full agenda next year in Kansas.

Two Pioneers of Cliometrics

The Cliometric Society announces a commemorative volume published to celebrate the award of the 1993 Nobel Prize in Economics to Robert Fogel and Douglass North. The five reprinted essays provide an *ex ante* perspective on the achievements in economic history recognized by the Nobel Committee.

The volume contains "Douglass North as a Teacher" by Jonathan Hughes, "Douglass North and the New Economic History" by Richard Sutch, "Robert William Fogel: An Appreciation by a Co-author and Colleague" by Stanley L. Engerman, "Robert William Fogel: An Appreciation by an Adopted Student" by Donald N. McCloskey, and "The History of Cliometrics" by Samuel H. Williamson.

Two Pioneers of Cliometrics is available at cost, an inexpensive way to introduce students in economic history or historiography to the field and two of its founders. Multiple copies may be ordered for class use; please contact The Society for information about large orders. The book has an ISBN number and is appropriate for library purchase.

To Order: Mail or fax the order form below or e-mail the following information to:

The Cliometric Society
Miami University
Oxford, Ohio 45056 USA
Fax: (513) 529-6992
Email: Csociety@cs.muohio.edu

Two Pioneers of Cliometrics

______ Paperbound copies (ISBN 0-9640068-1-2) @$7.50 ________
______ Hardcover copies (ISBN 0-9640068-0-4) @ $17.50 ________
Single Copy Postage: Add $2.50 North America/$4.50 Overseas ________
Total: ________

Name:____

Address:______________________________________

____

Visa or Mastercard #______________________________Expiration date____________

Please make checks payable to **The Cliometric Society**

Rostow Interview *(continued from page 8)*

and '60s. It was interesting to see the British and the Continental factories where the bicycle racks gave way to the parking lots. They didn't have American automobiles at the time. They had smaller automobiles and they had a big tax on petrol. But they had the automobile age, and they had refrigerators, and they used oil instead of coal, and they got rid of smog. In other words, the investment rate is a function of how close you are to the technological frontier and how acquisitive your entrepreneurs are.

This sounds very Gerschenkronian. Is it similar to Gerschenkron's ideas of backwardness?

No, it isn't backwardness. You're assuming that the stage of education of the populace is the same; you're assuming that the entrepreneurial acquisitiveness is the same. If that were the case, the advantages of being late would be uniform. In Russia you had not only the backlog of technology, but a population that was not very well-educated and a system that was counterproductive entrepreneurially. But if you assume other things are the same – which an economist normally assumes – then indeed Gerschenkron falls apart. He said something important about Russia, Germany, France, Britain. But let's take the Swedes and the Italians. They had takeoffs about the same time as Russia. The Swedes moved right up in the '30s to the technological frontier; the Russians didn't. So you have to factor in other variables, along with the size of the technological backlog. The Japanese were very well-educated people; so are the Koreans. The Koreans are the real miracle. They were the poorest kids on the block. The latest issue of the *Korean Business Review* reports the six major industries for export and they all have a high rate of growth. All are high-tech industries. That's what you can do if you've got an entrepreneurial system that works, and well-educated people, and an organization of society that gives them their head.

Since your interest is very strong both in cycles and in trying to fit technology into economic history, what do you think of attempts to introduce evolutionary elements into economic history? I think specifically of Paul David's work, Doug North's work, and Joel Mokyr's work. Three very different approaches, but all three in the last five or 10 years have tried to come to grips with technology, with evolution rather than mechanical notions, and learning and change.

I think that's the only way you can go and make progress. The only one of those books that I've kept up with is Mokyr's book on the Industrial Revolution (1985). It has a long introduction, which I thought was very good. I have sympathy for people who approach history this way, who take into account the institutions, the technology and other things which lend themselves *only* to treatment by historical methods. Now you can formalize that by bunching together the technologies. I do that towards the end of the *Theorists* book in the mathematical appendix. But I have great sympathy with a broader approach to economics; and I think that those who cut economics down to the size of the differential calculus lose an awful lot.

Since we've started on the education of an economist, can you comment on the role of economic history in that education? What should it be? How has its influence varied over the past half century or so, and what about the future of the profession?

I can say no more than I did in the preface to this book, *Theorists*. It is dedicated to the next generation. It's the way I feel about the next generation, that they're missing so much. 'To the Economists of the Next Generation: in the hope that, without abandoning modern tools of analysis, they may bridge the chasm of 1870 and reestablish continuity with the humane, spacious, principled tradition of classical political economy.' That's what I think they ought to do. I don't feel sore at anybody. I think that at the moment a lot of talent is wasted. The economists are sidelining themselves; but that is what I hope for the next generation. You see, I've put the appendix in the *Theorists* book to show what mathematics is *good* for. Mathematics is good for isolating certain forces at work. But if you're going to tell the whole story of an economy in motion, which is what I've tried to do in my lifetime as an economic historian, you have to remember Malthus and Ricardo. The historian is bound to deal with many variables operating at the same time. And that's what Malthus and Ricardo split over. I want the economist to deal in an orderly, logical way with each of the variables. He then deals with the unfolding of history. And history is never linear. I'm undogmatic with my students. I make them the greatest living experts on the critics of Rostow. I tell them that this isn't at all to score off them – my critics. I tell them to read the critics because that's the way to expose the problems in economic history: the conflicts between economists.

Why do you think there was this change? You suggest that in your youth there was a coexistence of both the narrow and rigorous with the broader, wide-ranging views. Why was there a split so people feel that

economic history – to quote *Science* magazine – became a 'backwater field' in the profession?[5]

That's simply the triumph of Samuelson and *The Foundations of Economic Analysis*. The triumph of that kind of economics took the whole profession out of the game.

But why?

Why? Because the differential calculus could not deal with *these factors* that matter: with population, with technology and investment, with relative prices, business cycles as an aspect of growth. So economics became everything you could deal with through the calculus.

Isn't that odd – if economics couldn't deal with them, that should have created a greater demand for historical work? What is it about the university system? You, yourself, were a colleague of Samuelson . . .

I like Samuelson. I regard him as a friend. I would never, never question his right to deal with economics his way. I wish wistfully that he'd understood not only that 'mathematics is a language' – which is a direct quote – but also the wonderful wisdom that 'nature is much more complex than could be dreamt of by a single mind', or revealed by a single technique or variable. Despite what Keynes said about bridging the gap in economics between micro- and macro-analysis of prices, he didn't bridge it. His was a Marshallian short-period analysis in *The General Theory*. What is missing from the way we teach economics is the *sector*. It's the *sector* in which technology comes. It's the sector you must study to understand high prices of raw materials and the low prices of raw materials. There is no theory of the *sectors*. Think about it.

Tell us some more about that. What should the theory of the sector be about?

The theory of the sector should be the rise of a technology. Kuznets's early book, *Secular Movements in Production and Prices* (1930), caught the sectors very, very well – the life of the sector is a life of deceleration, and he specifies the reason for the deceleration in that book, and the counterpoint to this in prices – acceleration, deceleration, and the leveling off of prices. And growth consists . . . that's what section 5 of *The World Economy* tells, that there are aggregate figures of industrial production, GNP and population – and underneath the smooth aggregate curves you have the coming in of the new technologies. *That* you can only catch by looking at the sectors.

From time to time you have referred to yourself as something of a maverick. Yet you've obviously been a successful maverick within the profession. How has that worked?

I don't know whether I was successful or not; I've had a lot of fun. When I wanted to take the time off to do something else, I did. For example, I thought even before the war I would write, someday, a book on the world economy, because you couldn't understand Britain without understanding the world economy. After I left the government in 1969 I caught up with economic history. I felt I knew as much as I was likely to absorb. I planned a 700-page book and I suddenly decided that 1790 was a curious year, because the Industrial Revolution was underway. So, I'll take some time off to write a chapter on how it all began. And that became a book. I found that there was no satisfactory theory of the Industrial Revolution. I worked it like a detective story. I took traditional societies first and asked why didn't they experience sustained growth. They had odd inventions scattered through their history. Then at last came the breakthrough in the 18th century. And when I was finished with *How It All Began*, I felt I had done enough on the Industrial Revolution and I was ready for *The World Economy*, which occupied me from 1973 to 1978.

Going back to being a maverick: Landes has written about my pleasure, my easiness, at the Konstanz conference when Kuznets mounted the attack on *The Stages of Economic Growth*. And the answer is that I never was sore at anybody. I just had fun doing my own thing; and if I wanted to take time off to do this series of books on 'Ideas and Action' after 1978, I did it.[6] It was in 1985 that I was ready to do another 700 page book – *Theorists of Economic Growth*. When I was finished with it in 1989, my wife said to me, 'You don't look pregnant with another 700-page book.' That starts something we called the joint venture. There it is. [He points to a box.] JV – we had a whole seven boxes on JV, joint venture. We thought of a number of things we might look at in the post-Cold War world and did an essay or two.

[5] Commentary on Economics Nobel awards to Fogel and North; *Science* 262 (22 October 1993), 508.

[6] A series of six brief monographs intended to "explore the relationship between ideas and action", where ideas are "the abstract concepts that public officials and their advisers bring to bear in making decisions". Quotation from the Preface, p. ix, to Rostow (1981).

And who is this 'we'?

My wife and myself. [The Rostows have been involved in an experimental approach to the urban problem in Austin.] We looked at the American economy and finally we decided on the problem of the cities. We spent five years on that, two years of study, a year of writing and clarifying the operational hypothesis, four months of planning and then 15 months of making it work and bringing it to scale, and going to the foundations. But why did we take five years off at this stage of our lives to study the cities? Because we felt it was a very serious problem. We wanted to make as much of a contribution to it as we could, even though we are both over 70.

So I treasure my colleagues and am delighted that they did what they wanted to do and I've done what I wanted to do. I wouldn't ask for anything else. The reply, which I wrote while I was in government, that's in the opening pages of the Konstanz book, says that it is true that economists are like other people. [See Rostow, ed., 1963] It's not a monopolistic market; but it's not a free market either. The coming in of a new vocabulary which I used in *The Stages* cuts into the attention paid other people. There is a hard test of the usefulness to others of the views which are put forth in there. This and the other original views I've fostered must look after themselves . . .

I have a theory about Ph.D. theses. They ought to be done soon. The bad thing is to stretch out the time, hang around graduate school. The Ph.D. thesis should be a great book, because you will be satelliting off it for years to come: You have a wife and child, and the child has its teeth straightened, etc., and so you want to state your goal in the world. There's something in Schumpeter's emphasis on the 20s being the critical decade. I have a table in the *Theorists* book as to the time when ideas were formed. And it was, almost without exception, before they were 30; Marx, John Stuart Mill and so on. It's a shame to waste that great period on a trivial subject, and therefore, I discourage students from being disciples – my disciples or anyone else's. M. M. Postan felt the same way about his students; he did *not* want them to be disciples of his.

Yes, I understand that Postan spent a good portion of his last years visiting you here in Texas. Would you tell us something about that interchange, and about his influence on your thought and work.

Well, he didn't have a great influence on my thought and work. I knew him from the time in 1938, when I sent him an article based on my Oxford thesis. I was asked by Postan to come to Wiltshire and visit him and his then wife, Eileen Power, a wonderful woman, and a great Medieval scholar. She died in 1940, suddenly. Postan was an authentic scholar, but he also was much involved in the current world. He did the official history of the aircraft industry, one of the series of books Hancock edited. He understood *exactly* what I was trying to do. He didn't try to influence me at all. But we liked each other, we enjoyed each other, we enjoyed each other's company. And he was a friend. He came three times to Texas in the '70s before his death. He was marvelously productive in the 15 years after age 65. Until then, he ran economic history at Cambridge. He also ran the International Economic History Association and much else. Then he retired and he had time to write, and he wrote about the medieval period; that was his thing. We differed about the modern economy somewhat, but he may have been right. I didn't disagree with him. But I pointed out that it was not inevitable. He feared we were going to have a period of chronic, high unemployment. He feared the new technology. He felt we wouldn't be fast enough in training people in the technology. And he was right.

So he felt there was a transition problem caused by the technology.

I said the major danger lay elsewhere. Computers didn't worry me so much, because I'd studied the logistical chain of computers *à la* Leontief. There are jobs all the way up and down the chain. But robots might put people out of work. I talked to him about where the workforce should be employed: in the infrastructure, which is poor in Europe, poorer still in the United States. It took longer to get from the Charles DeGaulle airport to my hotel than it did to fly in from Luxembourg to Paris. He worried about the *lumpenproletariat* who would be unemployed. My answer was that there was no reason for them to be unemployed, that there were ample jobs to make a decent infrastructure. But I didn't wholly rule out his anxiety. Postan was a friend and we enjoyed each other. We didn't influence each other, but I took him very seriously, indeed.

What would you say is the biggest change in emphasis in your thinking about the stages of growth in the last 30 years? That is, what had you not quite anticipated when *The Stages* first came out?

The Stages proved a salutary method for giving shape to the foreign aid field. The consortium method focused around a country plan, buying time through the preconditions, getting them into take-off. And then we say good-bye as they were far enough advanced to get their loans from the private market. I said at the White House the other day, March 4th, I said there are very few of us who remember the year 1958. A junior senator from Massachusetts, who was John Kennedy, and a Republican senator from Kentucky, John Sherman Cooper, made common cause to pass the resolution in the Senate in favor of the support for the Indian Second Five Year Plan. Three bankers were sent abroad. Herman Abs of West Germany, Alan Sproule from the United States and Oliver Franks from Britain went out to India and Pakistan. From their report came the first World Bank consortium. It brought everyone together in a unified way around a country plan: the Japanese, the West Europeans, and the Americans. The Indians had been thought to be fit subjects for triage. [*I.e.,* Aid could not help them.] Now they have a middle class of 200 million. They surely still have people sleeping on the streets of Calcutta, but that's because of the excessive birth rate, although it's falling. So the take-off hypothesis served its purpose in its time.

What you ask is a good question though: what is it that I hadn't anticipated that we have learned? In *The Stages* I didn't write enough about the differences in the length of time of the preconditions. It was something I taught, but I didn't put it in. It took the Mexicans from their independence, let's say in 1820, until *1940* before they took off. It took so long because they had to go through the political problems and define the law of the land, and decide what color they were, and who would rule, etc. It took the Chinese from, let's say, Hong Kong and the Opium Wars of 1842-43; and they didn't get going until the 1950s. The Japanese were intruded upon in 1851-53; and they took off in 1885. Why did the Japanese take off promptly and the Chinese have such a hard time? Why are the Africans having such a hard time?

One of the two best questions put to me on a trip around the world we took in 1983-84 was at a World Bank agricultural technicians' college in India. They had an African there. He stood up and said, 'We black Africans obtained our independence in 1960. We still haven't taken off. What's wrong with your theory?' I laughed at a good question, and took him through the length of time people take for the preconditions. Africa is peculiarly difficult because they've been divided up by the map, not by tribe. It will take a long time before they work through the generations and eventually pull their countries together in growth. I don't think they'll take as long as the Chinese or the Mexicans, but it will take a few more generations before they square themselves away. I would have given more weight in *The Stages* to the difference in the length of time of the preconditions, the cultural and political problems people face. I do spend some time at the end of the *Theorists* book on this problem.

The World Bank and the IMF – to judge from their recent pronouncements – seem to think that they have a very mixed record of helping out in development. How sanguine are you that development assistance can be undertaken properly in the future? Can we learn lessons from what we have done well, and from what we have done badly?

I wouldn't dogmatically draw conclusions. The different parts of the world vary a great deal. It started off in East Asia. We did very well. We bought time for the Koreans to find their feet and to find a generation which really wanted to develop, and we found that generation in the 1960s. One of the two times that I spoke up as an agent of the President in opposition to cabinet level people was on Korea in 1961. I asked permission from the President to speak as a development economist rather than as an aide to the President. And I said that everyone – military, civilian – was predicting that at the end of the '60s they'd be in as much trouble economically and politically as they were in 1961. They would not expand their exports. They wouldn't increase their GNP. There would be political turmoil. I said no, that's too pessimistic; it's a question of the generations. A new generation was coming to life, represented by Park and his people. They were going to do things. We see a very different South Korea today. The President came around to my side of the table afterwards and said quietly to me in his usual humorous style, 'You were one of the only ones who predicted last year that I'd beat Nixon, so I'll take you seriously.'

But it's interesting in Asia, because it started off well. The usual argument was that the Chinese are behind all of this ferment in Thailand, and the other Confucians, the Japanese, the Koreans are doing well. But now Malaysia has taken off. Now Indonesia's taken off, and India is far down the road. Bangalore is one of the great international centers for software, with satellite hookups to big companies in America and elsewhere. It isn't simply overseas Chinese, although they played a big part in this story. The Middle East is much more troubling. They've made a lot of progress in education and technology. It's partly the political problem posed by Israel, but it's not only that. You can see that in Iran and in Egypt, which don't really take the Israeli issue very seriously. They've had trouble finding their way into the modern world. I think the Egyptian case is baffling. They've had excessive birth rates. Yet *anyone* can go to the universities. They have overwhelming bureaucracies. And they stultify their own development. Turkey, on the other hand, has done well. So East Asia's a success; the Middle East has cultural problems they'll have to overcome; Africa will have to wait several generations; Latin America is finding its feet now. The middle size and smaller countries worry me a lot, because they are not big enough to be a critical mass and to have an MIT. Brazil, Mexico, barely Argentina, are big enough to become part of the modern world, but for the others I think the technological issue will do better than trade in bringing them together. They're on their way. They're going to be somebody. In short, it's not helpful to generalize. One must look at each region. But on the whole, by an economic historian's standards, the latecomers have done well.

Let's move now from developing nations back to the West. In discussing America's future in a number of essays you've focused on the need to shift from an emphasis on zero-sum pie-sharing to cooperative growth promotion. How can we *do* that, given the expectations developed by over three decades of the welfare state, especially in Europe and Japan?

It's a serious problem. You see the '50s and '60s – and partly the economists are to blame for this: Swan, Tobin and Solow didn't understand the '60s. They thought that the contemporary three times the average rate of growth since 1820 was permanent, when it was the product of a convergence of the factors I cited earlier, that neo-classical economists didn't take into account. That was when, with a certain *noblesse oblige*, governments piled up welfare state guarantees. In the long run, they will have to make accommodations. The welfare state has to be taken apart. Certain things *can* be guaranteed, like education. Education is a tremendous factor on the Continent and in the United States, but that involves, only in part, the welfare state. You've got to link the private sector and the education system to educate people for the new jobs in the modern high-tech world. You could bring the people up from the South, for example, from sharecropper farming to work on the assembly line for Ford or Chevrolet. And they could make that transition. You can't do that any longer. There are more chips in an automobile than there are in a computer and you need education. Education is sluggish because it hasn't been subjected to Japanese competition. We owe the Japanese a great deal. They've forced a revolution in administration in the United States. But the instruments of government have not been subjected to that kind of competition and they're still way out-of-date. That's one of the things we're fighting for in dealing with the urban problem. But one problem worries me.

One problem?

Yes, it worries me more than any other: the fall in the birth rate in Japan and Russia and Germany. This means that these countries will hollow themselves out. Now improvement in medical science gives, in part, an out. We can extend the length of retirement from 60-65, which is arbitrary, to 75. These people can improve the work force. We can improve the productivity of the work force, as we're doing now in the United States. We also need to improve education. Eighty percent of the people in our public school system do not go to college for four years. They must be trained for the modern work force. Higher productivity of labor will help solve the problem. But still I fear for the older industrial countries versus the younger industrial countries on demographic grounds. The Russians are terrified, for example, that the Chinese will take Siberia one day. That played a part in the revisions of foreign policy of Gorbachev. I worry a lot about people not being aware of the implications of the demographic revolution for the social security network we now have. Older people take a tremendous share of medical expenses. And the medical expenses will increase as the population ages. You're right to raise the social welfare gap. Because social welfare immediately comes under pressure, if you don't have a high rate of growth and people then are caught between cutting down on investment in infrastructure and cutting down on their allocations to social welfare. The social welfare system is wrong in that most of it goes to deal with remedial damage control. You want to get the causes of it; and our whole program addresses the causes – to prevent problems.

I think that somewhere along the line, there will be another surprising surge in the population growth rate of the kind there was after the Second World War till about 1960. But you asked me what worries me most: it's the demographic issue. The other thing that worries me is that we use APEC well, so that China and India don't repeat the German and French folly.

APEC?

The organization of Asian Pacific Economic Cooperation. It was founded at the same time the Berlin Wall fell, but it surfaced in Seattle. That will be the instrument for making the Pacific Basin really peaceful. The 20th century is a *dreadful* century. It's dominated by the First World War, the interwar bad period, then the Second World War and then the Cold War. We can't afford to have that happen again with the weapons of mass destruction. Therefore, I worry a lot about the 21st century. At the same time, it was the era of the end of colonialism and diffusion of technology to Asia, the Middle East, Africa in time, but Asia particularly. Asia and the United States will have to work this out. Let the Chinese come forward and join the collectivity and the Indians; let them remember that in the 20th century we spent the bulk of our time beating the Germans and the Japanese and the Russians into some reasonable proportion to their real places in the society of nations. And that was a hell of a way to spend a century and I don't want to see that happen again.

Is there any possibility that political problems might lead to reversal of the stages, so large portions of the world fall into war and revert to a traditional economy?

You can't say no and never. Look at Yugoslavia. But that will burn itself out in time, this phase of nationalism, and parts of the world will go on. But the Nations are quite right to hold a summit on unemployment right now. Europe worries me a great deal. Nationalism is rising in Europe and it's a new version of the inter-war period. The central question is: how do we get that machine going? You don't want to blame it entirely on Reagan. From the mid '70s on, the tendency to cut social welfare expenditures is world-wide, but it was also because of political pressure, a time of reduction in investment in infrastructure. This period of stagnation means you can't employ people who can work on the social infrastructure.

Would you like to sum up, or give any advice to the younger generation?

The younger generation – I've given them all the advice that I want to give in expressing the hope I did in the beginning of the book on the *Theorists of Growth*.

References:

Gayer, Arthur D., "Industrial Fluctuation and Unemployment in England, 1815-1850", D. Phil. dissertation, Oxford University, 1930.

Gayer, Arthur D., W. W. Rostow, and Anna Jacobson Schwartz, with the assistance of Isaiah Frank. *The growth and fluctuation of the British economy, 1790-1850 : an historical, statistical, and theoretical study of Britain's economic development.* 2 vol. Oxford: Clarendon Press, 1953. (Reprinted, with a new introduction by W. W. Rostow and Anna Jacobson Schwartz; Hassocks, England: Harvester, 1975.)

Hughes, Jonathan R. T. *Fluctuations in trade, industry, and finance; a study of British economic development, 1850-1860.* Oxford: Clarendon Press, 1960.

Kuznets, Simon S. *Secular movements in production and prices; their nature and their bearing upon cyclical fluctuations.* Boston: Houghton Mifflin, 1930.

Matthews, R. C. O. *A study in trade-cycle history: Economic fluctuations in Great Britain, 1833-1842.* Cambridge: Cambridge University Press, 1954.

Mokyr, Joel (ed.). *The Economics of the Industrial Revolution.* Totowa NJ: Rowman and Allanheld, 1985.

Postan, M. M. *British war production.* London: HMSO, 1952. In the series *History of the Second World War*, W. K. Hancock (ed.).

Rostow, W. W.

British economy of the nineteenth century; essays. Oxford: Clarendon Press, 1948.

The dynamics of Soviet society. [with Alfred Levin *et al.*] New York: Norton, 1953.

"The interrelation of theory and economic history", *Journal of Economic History* 17:4 (December 1957); reprinted as ch. 2 of *History, policy . . .*

The stages of economic growth: A non-communist manifesto. Cambridge: Cambridge University Press, 1960. (second edn., 1971; third edn., 1990)

How it all began; The origins of the modern economy. New York: McGraw-Hill, 1975.

The world economy: history & prospect. Austin: University of Texas Press, 1978.

Pre-invasion bombing strategy: General Eisenhower's Decision of March 25, 1944. Austin: University of Texas Press, 1981. (Ideas and action series; no. 1)

"Professor Arrow on Economic Analysis and Economic History", in William N. Parker (ed.), *Economic History and the Modern Economist*, Oxford and New York: Blackwell, 1986; reprinted as ch. 4 of *History, policy . . .*

Rostow, W. W.
History, policy, and economic theory: Essays in interaction. Boulder CO: Westview Press, 1990.
Theorists of economic growth from David Hume to the present: with a perspective on the next century. New York: Oxford University Press, 1990.
"Reflections on Political Economy: Past, present, and future", in Michael Szenberg (ed.), *Eminent Economists: Their Life Philosophies*, Cambridge: Cambridge University Press, 1992, pp. 222-35.
Rostow, W. W. (ed.) *The economics of take-off into sustained growth; proceedings of a conference held by the International Economic Association.* [Konstanz, 1960] New York: St. Martin's Press, 1963.
Schumpeter, J. A.
The theory of economic development; An inquiry into profits, capital, credit, interest, and the business cycle. Cambridge, Mass.: Harvard University Press, 1934. [First German edition, 1912.]
Business cycles; a theoretical, historical, and statistical analysis of the capitalist process. 2 vol. New York and London: McGraw-Hill, 1939.
Smith, Walter Buckingham and Arthur H. Cole. *Fluctuations in American business, 1790-1860.* Cambridge: Harvard University Press, 1935.
Svennilson, Ingvar. *Growth and stagnation in the European economy.* Geneva; United Nations Economic Commission for Europe, 1954.

All-UC Report *(continued from page 14)*

an Institutional Overview', Elliot Brownlee (UC-Santa Barbara) argues that the overarching explanation of US tax history has been left largely to political scientists. In contrast, Brownlee poses a 'democratic-institutionalist' interpretation of American federal taxation as an alternative to the 'progressive,' 'capitalist state,' 'neo-conservative,' and 'pluralist' interpretations employed by political scientists. His new view incorporates civic values, with the State and interest groups seen as autonomous actors. In addition, Brownlee argues that both historical contingency and political entrepreneurs shape the flow of events; irreversibility or path-dependency characterizes the history of institutional change. In his model, political conflict over control of the public sector is intense, and outcomes highly uncertain.

Robert M. Collins (Missouri) followed with 'The Forgotten Economic Crisis of 1968'. Citing an emphasis on the Vietnam War and the politics of protest in conventional analyses of the 1960s, Collins examines the 1968 crisis by analyzing its origins and studying the responses of major institutions to what he considers the most serious test to date of the postwar political economy. When the crisis peaked in March 1968, there were speculative attacks on the dollar, a worsening balance of payments, a threatened breakdown of key institutions in the international monetary system, and the Johnson administration's failure to dampen inflationary pressures generated by the Vietnam War and the Great Society. Causes of the crisis included the stirring of global competition, the overreach of 'growth liberalism', and the role of personalities in politics. Collins maintains that the results of the crisis were profound, leaving marks on the Johnson administration, capping the US escalation of the War and the Great Society, and helping to shape the subsequent economic order.

In the closing paper, 'Fiscal Crises and Regime Survival in Latin America: Chile and Mexico in the Great Crash, 1924-1935', Michael Monteon (UC-San Diego) notes the paucity of comparative history in detailed studies of Latin America's political economy. According to Monteon, Chile and Mexico each entered the Depression in the midst of a corporatist political experiment, headed by a strongman, a 'modernizing *caudillo*'. Although Calles in Mexico seemed to face a more difficult situation than did Ibáñez in Chile, when the crisis began Ibáñez was driven from government, while Calles created the political party that has dominated Mexico since 1929. Monteon argues that a major part of the explanation for these outcomes is the importance of each government in the overall economy at the start of the Depression. Chile's regime was spending 10% of GDP, while Mexico's 'revolutionary' government, cut off from foreign lending, was spending only 5%. As a result, the Depression hit Chile's political system much harder than it did Mexico's.

Stephen Haber (Stanford) and Richard Musgrave (UC-Santa Cruz) led the discussion. Haber first commented that each paper illustrates the importance of politics to understanding economic history. Haber praised Collins for 'bringing economics back into the 1960s' and the detailed exposition of the crisis in the White House, but questioned his characterization of the 1968 crisis as domestic and 'economic', since it should be seen as international and monetary. Haber then faulted Brownlee for suggesting that tax progressivity in the US is inviolate; it is 'too gentle' to say that the Tax Reform Act of 1986 retained progressivity while curtailing the privileges of the upper class, and asked to see hard evidence about the real incidence of taxes over the regimes discussed. Haber suggested Monteon clarify the purpose of his paper. At present he considers three dependent variables: differences in policy regimes, in severity of crises, and in political survivorship, emphasizing the roles of contingency and human agency in explaining the differences, but Haber said he could just as easily conclude that political survival in Mexico or Chile was a function of politics, not necessarily economics. Finally, he warned Monteon about possible inaccuracies in the Mexican data, especially official unemployment rates, and argued that real wage data for the Depression must be considered together with the short work week and the steep decline in prices during the period.

Musgrave observed that the arguments of the session's first three papers were to some extent based on causal relationships: between societal and fiscal events for Brownlee or Collins, and between taxes and spending for Hoover and Siegler. Musgrave took issue with the 'troublesome' concept of causality between aggregate variables in the last paper, arguing that a complex set of intermediate factors makes it difficult to ascertain exact causation in aggregate outcomes. The impact of an event on an aggregate variable may reflect either a change based on constant responses, such as to an external shock, or it may reflect changes in the nature of responses because of economic advice, theory development, etc. In reply, Hoover maintained that the inference techniques used in the paper do not assign causation between shocks and outcomes, but merely evaluate the fiscal response to shocks, and consider only the direction of causation between taxes and spending.

The Conference was widely attended by economists, historians, and graduate students from UC campuses and others. The topic was thought timely, since numerous state governments, as well as the Federal government, are now experiencing fiscal crises. Of special interest was a report on computerized data compiled by John Wallis and Richard Sylla, a set including detailed Controller's Reports for most states from the 1850s to the 1930s. A highlight of the conference was the thoughtful and humorous talk given by Eugene Smolensky, Dean of the Graduate School of Public Policy at Berkeley. The next conference of the All-UC Economic History Group will be held in Pasadena, November 11-13, 1994, organized by Program Chair R. Bin Wong (UC-Irvine) on the theme 'Divergence and Convergence in the Global History of Economic Development'. A call for papers was recently distributed *via* the Society's Listserv, Econhist. Readers may see this announcement by consulting our gopher. [See page 2 for procedure.]

Canadian Clio Report *(continued from page 17)*

the discussant, inquired about the literature's emphasis on stability, and wondered whether the United States might not be seen as more innovative as well as less stable. Shearer asked if the exclusion of state banks biased the results. Quigley pointed out banks had hidden reserves not reported in the data and noted that the failure of return on equity to be equalized due to competition is problematical.

The conference resumed after lunch with 'Topics in Canadian Economic History, 1700-1870', chaired by Gerriets. Ann Carlos and Frank Lewis (Queen's) examined the interrelationship between the Indians, the Hudson's Bay Company, and French trappers in order to understand beaver harvests in the hinterlands of three Hudson's Bay Company trading posts between 1700 and 1763. Carlos and Lewis noted that while Indians had individual incentives to overharvest, the Hudson's Bay Company had incentives to conserve stocks and avoid overharvesting in regions over which they held monopolistic control. In regions where the Company engaged in a duopoly game with French traders, competition led to overharvesting and eventual depletion. Estimates of the beaver population, the prices paid by the Hudson's Bay Company at each fort, and the prices in foreign markets support the authors' hypothesis that in the Fort Churchill hinterland the Hudson's Bay Company maintained a monopoly throughout the period, and in the Fort Albany and York Factory hinterlands the Company competed with French trappers during part or all of the period. Jim Irwin (Central Michigan) wondered whether Cournot supplied the right model, given the possible complexity of feedback between the Hudson's Bay Company harvest and the stock. Don Paterson (UBC) asked about other furs being brought into the forts and their depletion over time. Bob Gallman (North Carolina) questioned whether the Hudson's Bay Company knew what the maximum sustainable yield was and the usefulness of this concept when applied over such a large area.

Don Paterson and Ron Shearer, in a paper befitting a conference honoring Mac Urquhart, examined the importance of the starting point of 1870 used by Urquhart and his team of scholars. Paterson and Shearer characterized 1870 to 1896 as years of 'relative stagnation in aggregate output, but continued improvement in *per capita* output'. They then asked whether the two decades that preceded 1870 exhibited a similar pattern of growth or whether they represented a distinct phase. Given the impossibility of constructing GNP estimates for the 1850-1870 period, the authors examined a variety of other data including the money supply; Canadian railway mileage, investment expenditure, and equity prices; Canadian terms of trade; and timber and grain exports and prices. The evidence suggested that the boom of the early 1850s had died out by 1857, that growth continued slowly if at all between 1857 and the mid-1860s, and that the pace increased significantly between Confederation and 1873-74. Doug McCalla (Trent) noted the authors' findings for the period of the American Civil War ran counter to the conventional wisdom that the war brought some measure of prosperity to Canada, but added that he was inclined to accept their results. Urquhart told the audience how pleased he was that the authors had undertaken this project and noted the pre-1873 period was quite different from what followed because it was the peak in the market for grain and horses in the US.

Catherine McDevitt (Central Michigan) chaired the final session, 'Canada in International Comparison'. Bob Gallman and Lance Davis discussed their ongoing research on domestic savings and investment, international capital flows, and the evolution of capital markets in four frontier economies: Argentina, Australia, Canada, and the United States. In comparison with the world economy, which experienced slow economic growth from the mid-1860s to the 1890s and a boom between the 1890s and World War I, the Canadian experience of strong growth followed by very rapid growth in the second period is quite striking. Canadian domestic capital formation during the first period was funded by domestic and foreign savings in roughly equal proportion. In and after the 1890s, Canadian rates of domestic savings increased dramatically. The authors attribute this increase to a capital market innovation, bond dealers, who were able to promote high savings through education of domestic savers. Trevor Dick (Lethbridge) asked why Davis and Gallman preferred to capitalize interest and dividend payments to get estimates of the capital stock rather than using more direct estimates. Harley expressed concern over differing interpretations of Canada in the 19th century which range from those of Davis and Gallman, who view it as doing reasonably well, to McInnis, who views it as not doing well, to Maddison, who finds GNP *per capita* in Canada to be only 55% of the US figure. McInnis commented on the need to correct the authors' estimates of the savings rate.

Mark Thomas (Virginia) examined the experience of Canada and Australia from 1890 to 1913 to determine how to account for Australia's failure to grow during this period of world economic expansion – particularly in Canada. While much of this relative failure is attributable to the Australian depression of the early 1890s, which coincided with the Canadian wheat boom, Australia failed to grow extensively because of its declining ability to attract migrants and capital. Thomas identified Australia's failure to attract migrants, despite persistently higher real wages than in Canada, as the result of expectations of future performance, which worsened as Australia entered the recession. Eventually, they became a self-fulfilling prophecy. He attributes the shift of British investment to Canada, despite high real rates of return and optimism in Australian financial markets, to the volatility of the mining sector and the severity with which the depression hit pastoral companies and financial houses. Hatton suggested that the slower movement of migrants and capital to Australia might have been appropriate if that country had been unable to absorb them.

The conference ended with thanks to the organizers, Mary MacKinnon and Rick Szostak, and to Kris Inwood who made the financial arrangements. The next conference, slated for October 1995, will be at the University of Western Ontario, with Knick Harley and Neil Quigley handling local arrangements and the program.

Call For Nominations For The Second Jonathan Hughes Prize For Excellence In The Teaching Of Economic History

The Economic History Association has established an annual award in memory of Jonathan Hughes to recognize excellence in teaching economic history. Jonathan Hughes was an outstanding scholar and a committed and influential teacher of economic history. The winner of the Hughes Prize for 1995, which includes a $1,000 cash award, will be selected by the Committee on Education and Teaching of the Economic History Association.

The Committee now 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. A nomination must include a statement describing how the nominee has exemplified excellence in economic history teaching. Finalists for 1995 may include nominees not selected for the 1994 award.

There are no restrictions on a statement's format or topics, but the following considerations should be borne in mind. Teaching can be regarded as having three components: teacher, subject matter and students. A statement might describe how the nominee excels, discussing a) the qualities of character and personality, both professional and personal, the nominee exhibits; b) how the nominee's teaching offers insight into the discipline of economic history; and c) how the nominee has recognized potential and motivated superior performance among students.

Specific further evidence of the nominee's teaching abilities (*e.g.,* imaginative course materials or names of some of the nominee's current or former students willing to support the nomination), while not required, would be welcome.

The deadline for nominations is March 20, 1995.

Nominating statements (3 copies) or inquiries about the Hughes Prize should be directed to:

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

Executive Director's Notes *(continued from p. 2)*

bulletin boards.) Since last summer, we have run a listserv named Econhist, with about 200 subscribers. Traffic has grown to five to 10 postings each week. Most subscribers are Society members; others are interested in the field and want to follow our discussions. I recommend that you subscribe to this list, as it is a place to share ideas and to help your colleagues. For the past month, Econhist has not been moderated – every posting is forwarded immediately to all subscribers. This has worked well so far, with everyone following courtesies for the list.

We have recently added lists on specific topics in the field, and plan to add further topics. Each list has two or more moderators or editors, to facilitate discussion and periodically to provide summaries of postings to Econhist. Moderators control the content of their lists, follow general Society guidelines, and appoint an editorial board to help them make the list effective. The new lists are in the economic history areas of teaching, macro, business, global change and recurrent phenomena.

You can easily get an update on Society lists by sending the two-word message *info lists* to **lists@cs.muohio.edu** The reply will include a summary of the list topics, editors' names, instructions on how to subscribe and how to find more information about each list. If you are not sure you want to subscribe to a list, we can provide a weekly record of postings. Complete postings can be consulted in the file server, under 'Lists Archives'.

Questions or comments regarding e-mail, the server or the lists, should be sent to our 'helpline': **help@cs.muohio.edu** The machine will acknowledge receipt of your message with a standard reply, including answers to frequently-asked questions, and you will be sent a personal response from the Systems Administrator.

Call for Papers

20th Annual Meetings
Economic and Business Historical Society
April 27-29, 1995

The Economic and Business Historical Society encourages interdisciplinary scholarship in economic and business history. Its international membership includes scholars with interests in economics, history, business, social science, geography, and related disciplines. The Society is particularly interested in soliciting papers both from new scholars and graduate students and from those working in such areas as business and the environment, business and culture and political economy.

EBHS will hold its 20th annual meeting in Boulder, Colorado. Members and nonmembers are invited to submit papers, offer their services as session chairs or discussants and/or make program suggestions. Please enclose a two-page abstract with proposals for papers.

The deadline for abstracts is January 15, 1995; for papers, March 1, 1995.

Correspondence, abstracts, and papers should be addressed to:

Professor Ann Carlos
EBHS President and Program Chair
Department of Economics
University of Colorado
Boulder, CO 80309 USA
Fax: 303-492-8960

THE CLIOMETRIC SOCIETY
MIAMI UNIVERSITY
OXFORD, OH 45056 USA

OXFORD, OHIO 45056

NON-PROFIT ORG.
U.S. POSTAGE
PAID
Permit No. 40