The Newsletter of

The Cliometric Society

February 1993 Vol. 8 No. 1

The Cliometric Society Sessions at the ASSA Meetings
January 5-7, 1993

by Bill Sundstrom (Santa Clara) and Louis Johnston (Bowdoin)

There once was a conference in Anaheim,
Where the sun is reputedly most sublime,
But the conferees were shocked,
At what Nature could concoct,
For it rained the whole bloody time.
—*Anon.*

(Anaheim, CA) Dampness notwithstanding, the Cliometric Society's four sessions at the 1993 meetings of the ASSA in Anaheim were lively and well attended. The first session, "New Lessons from Old Monetary Regimes," was held the morning of Tuesday, January 5, and was chaired by Richard Sylla (New York University). Warren Weber (Federal Reserve Bank-Minneapolis) began the session with a paper co-authored with Arthur Rolnick and Bruce Smith (also of the Minneapolis Fed) on the origins of the U.S. monetary union. Their paper focused on the problems created during the colonial and Confederation periods when individual states could issue their own fiat currencies. The first problem was that multiple currencies created "unnecessary and socially wasteful exchange rate variability." Rolnick, Smith, and Weber argued that fixed exchange rates could have solved this problem, but Article I, Section 10, of the U.S. Constitution prohibited states from issuing their own currencies. Why did the framers go this far? The authors pointed out a second issue ignored by conventional approaches to exchange rate determination. Assuming that different currencies are perfect substitutes for one another, a "seigniorage incentive problem" can occur, whereby individual colonies or states could levy an inflation tax on their neighbors by issuing large quantities of paper money which would then circulate in surrounding colonies or states. Fixed exchange rates alone could not solve this problem; states had to be prevented from issuing their own money.

Sylla commended the relevance of the paper for issues now facing the members of the European Community as they attempt to form a monetary union. He then went on to suggest an alternative hypothesis about why state currencies were banned. He argued that the prohibition on state money issue was similar to the "commerce clause" of the Constitution, wherein the Federal government held the right to regulate interstate commerce. Sylla also pointed out that assuming perfect substitutability among currencies did not make much sense. If this had been the case, why did different currencies trade at such varying exchange rates?

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What's Inside

Executive Director's Notes

By Laws

In your December renewal letter you received the proposed By-Laws of The Cliometric Society. I am pleased to report that ballots returned to the Society's office as of this date unanimously approve their adoption. Part of the By-laws allows any member to propose agenda items for the Trustees meetings. The Trustees normally meet in May at the Cliometrics Conference and in September at the Economic History meetings. Please feel free to send any item to the Society's office and it will be discussed at the next meeting.

Also note that any member's name can be placed on the ballot for Trustee by petition. Petitions are due at the Society's office by November 15.

Trustees

Mary MacKinnon and Lance Davis have been elected Trustees for 1993-1996. They replace Susan B. Carter and Gary Libecap, whom we thank for their services to the Society for the last four years.

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Announcement

As part of the celebration commemorating the 125th Anniversary of the University of California, the Multicampus Group in Economic History will hold a Conference on

"Global Change, Resource Shocks, and Economic History"

The Conference will focus on how economic systems have responded to large-scale changes (such as the massive reallocation of labor and capital across the globe in the 19th century) and "resource shocks" such as the discovery or exhaustion of economically important mineral deposits. We are particularly interested in papers that present research on how such changes have affected the evolution of economic institutions and on the capacity of new institutions to buffer the shock to the system. Topics to be considered include, but are not limited to, "Global Change and Economic History," "The Importance of the Mineral Industry to Economic Development," "Economic History of the Extractive Industries," "The History of the Exploitation of Renewable Resources" (*e.g.*, timber, fisheries, soil exhaustion), "The Impact of Large-Scale International Capital and Labor Flows in the 19th and 20th Centuries," and "Climate Changes in Economic History."

The Conference will be co-sponsored by a gift from the American Mining and Metallurgical Society, whose members are interested in stimulating research on the importance of mining and metallurgy in economic history.

Conference dates are April 2, 3, and 4, 1993,
in Pasadena, California.

For additional information, please contact:

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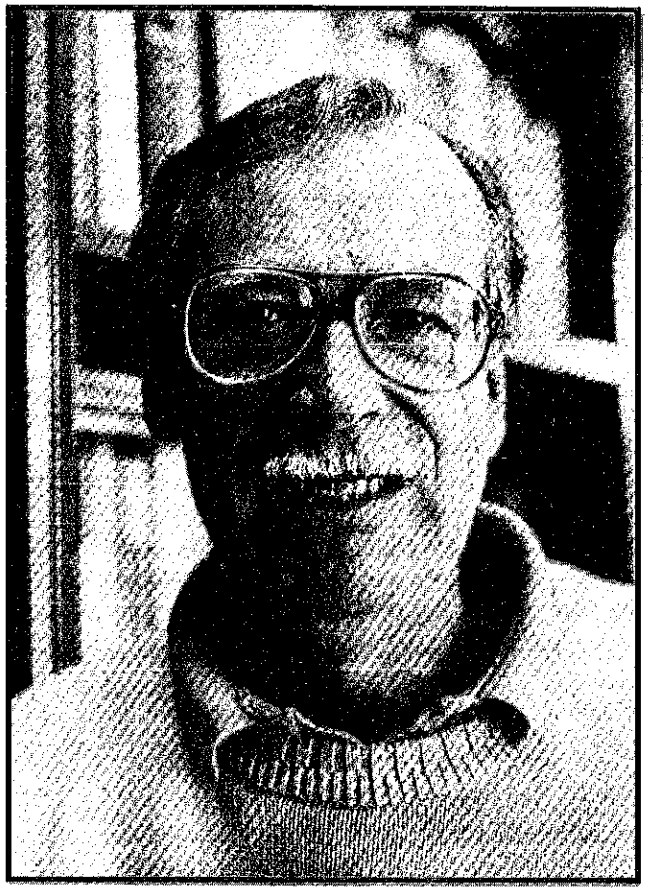
AN INTERVIEW WITH RICHARD A. EASTERLIN

Editors' Note:
Richard Easterlin has been Professor of Economics at the University of Southern California since 1982, following nearly three decades on the faculty of the University of Pennsylvania. The interview took place last fall at USC and was conducted by Ken Sokoloff (UCLA). [A few additional responses were elicited by the Editors in January 1993, and have been inserted into the record of the original conversation.] Himself a denizen of Los Angeles since 1980, Ken tells us:

With three of the last four Cole Prizes having been awarded to members of its community, Southern California definitely has come of age as a center for economic history. Lance Davis and Richard Easterlin preside over the local economic history group, and the two close friends are enthusiastic boosters of life in this part of the world. Both are deeply serious about scholarship, and apply high standards to their own work, as well as to that of others. They differ dramatically, however, in personal style. Lance is intense and social, empathetically drawing from strangers the intimate details of their data and their love lives. With Lance, what you see is what you get. Dick maintains more distance, makes every word count, and exercises quiet charm and dry wit. There is always an air of mystery about him. I have long admired Dick for the originality of his ideas and the fundamental importance of the issues he tackles. Despite his interests having shifted to other fields in recent years, he still matches Bob Fogel, another student of Simon Kuznets, in having the greatest number of individual items on the reading list for my undergraduate course in American economic history. It was thus an especially great pleasure when I was asked to interview Dick for the Clio Newsletter.



As a scholar who began his career studying the process of economic growth with Simon Kuznets, what observations would you make about the recent slow-down in U.S. economic growth? Are we in the midst of a cycle, or has there been a change in the secular trend?

Well, I think I'm more optimistic than many currently are. I think you want to distinguish the secular forces at work from the cyclical. The moving force behind long term economic growth is productivity growth. Behind that is technological change, and behind technological change is basically the advancement of scientific knowledge–primarily natural sciences knowledge but also certain types of business knowledge like organizational techniques. So if we go back to the level of basic science, I don't see that our potential has leveled off. I see basic science as continuing to expand and by implication our technological potential is continuing to expand over the long term. Looking 30 years down the road, I expect we'll see an economy in which productivity expands at rates commensurate with the long-term rate we have observed in historical experience.

Turning to the question of swings, we know that in the past there have been long-term swings in productivity growth connected with aggregate demand movements. I think we are still experiencing something of the same sort. In the past two decades we have obviously had a substantial retreat from the post-World War II policy of stimulating long-term growth of aggregate demand *via* monetary and fiscal policy, and connected with this we have had a decline of productivity growth. At the same time, there have been major changes of an adverse nature in the international economy. The OPEC changes, the shift from fixed to fluctuating exchange rates, reduced international cooperation–all of these have combined to produce a more adverse environment for economic growth. But I don't anticipate that the bulk of these developments will persist.

What government policies, if any, do you think would be desirable?

I am sympathetic to policies of the traditional sort to maintain economic stability and a high growth of aggregate demand–that is, a combination of monetary and fiscal policy that will promote high employment. I think international economic cooperation in the state of the world today is an essential prerequisite of rapid economic growth. And I think a desirable emphasis is on policies that promote investment and education as opposed to consumption, coupled with a reduction in military spending.

There has been increasing concern recently about a shift in the distribution of income in the U.S. and to a lesser extent in other industrial countries, perhaps driven by changes in international environment or technology. Do you have any strong feelings about these recent developments–whether they are permanent or transitory, or whether there might be policy changes capable of offsetting these trends?

Again, I think this is a temporary rather than a lasting phenomenon. Most of the work I am familiar with suggests that it's connected with technological developments, probably things like computerization that have raised the relative demand for more educated workers and increased the wage differential by level of education. The international argument has to do with the adverse impact of international competition on the manufacturing sector, and the evidence, at least for the U.S., doesn't seem so persuasive for that hypothesis.

The movement in wage differentials by level of education is ultimately a function both of supply and demand developments, and I think experience suggests that the supply of young persons responds to earnings differentials, or at least to awareness of job opportunities if not earnings differentials *per se*. (The exact mechanism is debatable.) And so I anticipate that supply-side changes are going to operate to reduce the wage differential substantially, as has occurred in the past when we have had disproportionately big influxes of college graduates. So I don't see that development as a long-term one.

The other element in the picture, it seems to me, has to do with the relative supply of young persons in the labor force. Young persons are typically low-income people, and the age distribution is going to be changing in a way that will have a favorable impact on the income distribution. We are moving into a period where the baby bust cohorts are coming into the labor market and that's going to reverse the labor market conditions that existed for the baby boomers. We're going to find shortages of younger, less experienced workers and that's also going to contribute to lessening relative wage differences, in this case among age groups.

If I read your hypothesis correctly, you also predict rising fertility and perhaps fewer two income families from the baby bust generation. Do you think that's happening or are there other factors?

Well, fertility is definitely moving upward, from about '86, '87 to about '90, '91. Certainly the recession has set it back, but my basic reason for expecting those developments to materialize or resume is that the size of the young cohorts reaching the family-forming age is continuing to decline and will decline through this decade. So, assuming we get this economy going again, my expectation is that in the next few years we should see resumption of these developments. I don't anticipate a decline in two worker families but a leveling off in the phenomenon among younger adults, rather than the continued uptrend that has been occurring.

You and Eileen Crimmins have recently been working on value formation among American youth. In particular, what accounted for the rise in materialism and corresponding decline in social consciousness during the 1970s and 1980s? My understanding is that your explanation is focused on unmet economic expectations on the parts of their parents due, both to cohort size considerations and to a slowdown in economic growth. Since economic growth has continued to be slow and a variety of factors seems to be driving a widening of income distribution, how confident are you of a return to less emphasis on material success? Do you think the changes in values have an independent effect on growth?

As far as income distribution is concerned, as I've just indicated, I don't see that as a persistent secular problem. It's a serious problem, but not a persistent secular one. Again I want to distinguish in my answer between long-term secular trends in relation to values and shorter-term movements. I think over the longer term values do exert an independent effect on economic growth, and that's the sort of thing I talked about to some extent in my presidential address, because I think education can influence aspirations. It has the effect of developing rational attitudes and producing a set of values more commensurate with the attainment of long-term economic growth. I think the evidence from studies done by sociologists like Inkeles is consistent with the notion that there is an independent impact of values on economic growth.

With regard to fluctuations in values, our work, based on data since the 1960s, has stressed that there is some evidence of swings which occur between more purely materialistic goals of making money and public interest values–the importance of helping others, racial integration, greater equality, environmental concerns, and so on. It's very clear that from, say, around 1973 to about the late 1980s there was a substantial shift toward private materialism and away from the public goals, as far as youth were concerned, and I think also for the adult population. It's less clear how much this has persisted in the last few years, although obviously there has not been a substantial swing back. Our explanation for the value shift is that basically it's a reflection of the slowdown in economic growth, which has left a shortfall between aspirations and the realization of desires. The result has been to make adults more concerned about making a living and passing that sort of emphasis on to their offspring. With the resumption of a more normal rate of productivity growth, I feel that there will be a swing back towards public concerns. So I see education and its impact on values as having a substantial independent long-term secular effect on economic growth, but I see the swing back and forth as being induced by changes in the state of the economy as productivity advances and slows.

What are your feelings about the outlook for fertility and the stability of the family in our society, or industrialized societies, more generally?

Well, the theme of what I say always seems to be secular versus cyclical. From the secular point of view, I don't see evidence of a decline in the nuclear family as a value. When you look at the concerns and goals that young people express in surveys running back over the past two decades or so, the persistence of the notion of forming a nuclear family, staying with one spouse, and of having at least two children, prevails in 80% to 90% of the population. These are the people who will be in the family formation ages over the next 30 years. So I think the notion that the family is in decline is not really supported by the evidence. Turning to the cyclical aspect, it's very clear, however, that the ability of young people to realize their aspirations has been adversely affected as the baby boom generation has come of age. The baby boom generation has been under severe labor market pressure by virtue of its size as well as from adverse changes in aggregate demand conditions. As a result, baby boomers made a lot of adaptations to economic circumstances by postponing marriage, postponing childbearing, and increasing mother's labor force participation while they have pre-school children. But the boomers are going to be succeeded now by the oncoming baby-bust generation and I think there will be a reversal of these conditions and a return to less problematic family circumstances.

Sam Preston, a former colleague of yours, has made a great deal of increasing numbers of births outside marriage in European societies as well as in the U.S. Would your analysis of European cases be similar to that for the U.S.?

Yes, if you look at the demographic history of the developed European countries–the leading ones in the northwestern and central sectors of Europe (the ones that went through the demographic transition by the 1930s)– the pattern is very similar to the U.S. They had very low fertility in the 1930s, then they had a post-World War II baby boom, and since around 1960 they have had a baby bust. So in all these countries there was a young adult generation after World War II that was relatively small in size, followed by a young adult generation that was relatively large, and now a new generation that will be relatively small. So I see this phenomenon as occurring fairly commonly across the spectrum of countries, though with individual variations. Since the mid-1980s fertility has moved upward in the U.S. and these European countries. For some the increase is negligible but for others it is very sizable. And the U.S. is one with a sizable increase. The Census Bureau projections published in 1989 based on data through 1987 were already below actual fertility in 1989. In 1990 and 1991, even though 1991 was a recession year, the actual number of births in the U.S. was above the high projection of the Census, and was 12% above the middle projection, the one that everyone adopts for long term projections. So I think recent evidence is favorable to the hypothesis that fertility will go up.

Given the pattern of fertility across income class and the widening distribution of income we discussed, do you think the evidence is consistent with the hypothesis?

Well, we don't have the evidence yet on recent fertility by income class. We do know that fertility seems to be edging up among people in their 20s. Now whether that's true of those at the lower end of the income distribution, we just don't know. Heretofore swings in fertility have been widely diffused among all income classes, education groups, and racial and ethnic groups. It would be unusual if any new sizable upswing were not participated in by every group. But of course the recent growth in income inequality may mean less participation by the low income group–we'll see.

Probably because of slow economic growth, there is increasing concern about immigration into the U.S. Would you favor any change in immigration policy?

No, I think the existing policies are satisfactory. If you compare rates of immigration–that is, immigration in relation to the size of the adult labor force–we are considerably below the rates of immigration that we had back before World War I. I don't see present rates as a serious negative in the picture. I think the concern arises when the economy slows down and, as a result, all sorts of anti-immigration and anti-free trade types of attitudes are fomented.

Some observers would claim that despite their high material welfare, the populations of western countries have become an increasingly surly lot, with many segments of those populations preoccupied with their relative rather than absolute position. Is this your take on the current situation? If you were writing an addendum to "Does Economic Growth Improve the Human Lot?" what would you say?

As I suggested earlier, what we are observing is, I believe, a phenomenon linked to the slowing of economic growth and the disappointed aspirations this has produced in the population at large. If I were writing an addendum to that article, I don't think I'd change anything. I think the basic idea is still correct. Put simply, it is an extension of the idea that you have to deflate the money value of national product to get real national product. If in addition you want to evaluate happiness, then you have to deflate real national product by aspirations, which are themselves a function of real national product. So aspirations are going up commensurately with real national product, leaving happiness unchanged over the long run.

Has relative happiness declined for the people of less-developed countries as they have become more aware of conditions in the developed countries ?

No. I think that material aspirations that people form are a function of the living conditions they experience in their own country as they age. Indians, in India, are exposed through numerous movies to the consumption levels that prevail in the United States. But they don't identify with what they observe on the screen. And so when they are asked about what they need to make themselves perfectly happy, it's not a Mercedes–it's a transistor radio or something that is a realistic element of their own experience.

Some of your recent work has dealt with long-term projected population change in industrialized countries. Is the prospect as dismal as some suggest? I gather, probably not.

Correct. I think this is a good example of how important it is to look at historical experience. The arguments about the dire implications of population change in developed countries have to do with the adverse effect of dependency on productivity growth as population ages. Also, to some extent, with slowing growth of aggregate demand connected with slower population growth. As I have already indicated, I think that projections based upon persistent low fertility are highly dubious. But even if one accepts projections of low fertility, they compare prospective dependency 60 or 70 years down the road with conditions at the present time or, at best, with conditions in the last couple of decades rather than with past periods of comparable length, let alone longer periods. If you take a longer historical perspective, say over the past century, the kinds of dependency burdens we are looking at are not out of line with historical experience in any of the countries. Moreover, there is a caricature of the aged that has presented them as low-educated or illiterate people. But the aged who are now coming along are typically people who are much better educated than the aged used to be. They are people who in many cases have completed secondary school or higher and compared with the past the differentials in education from the young are small. One issue of some relevance is whether the tax burden will be disproportionately great as the population ages. OECD projections, however, suggest that very modest rates of growth of real wages would result in no increase in the tax burden. By modest, I mean 0.5% per year.

How about environmental stress? Does that concern you?

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Conference: "A Century of European Industrial Policy"

Worcester College, 18-19 December 1992

by James Foreman-Peck (St. Antony's College, Oxford)

(Oxford, England) What policies have European states pursued to support industrial development and why? What, if anything, have state industrial policies contributed towards European economic development over the last century or so? These were the key questions addressed by this conference, sponsored by the European Historial Economics Association. In Oxford, the home of Lewis Carroll, words may be chosen to mean what we wish, and here they were. Opinions differed greatly as to what industrial policy meant. Alan Milward (LSE) maintained a distinction should be drawn between industrial and industrialisation policy. Jozef van Brabant (United Nations) believed industrial policy must be linked to an industrial strategy. Mark Casson (Reading) defined industrial policy as being intended to improve the coordination of the economy. Michael Utton (Reading) reminded the conference of the very different inheritance of U.S. monopoly legislation over the last century. Only since 1945 and even more recently has Europe taken an interest in competition and merger policies, raising the question of whether these should be subsumed by industrial policy. Inevitably analogies were drawn in the discussion with the very explicit policy pursued by the Japanese M.I.T.I. (Andrea Boltho, Oxford; Akiro Kudo, Tokyo).

The two world wars are clear markers for periodisation of European industrial policy and performance, on occasion supported by the world economic crisis of 1929-32. In Eastern Europe 1989 was no less radical a break. National security emerged as a key industrial policy goal for 19th-century late industrialisers (Renato Giannetti, Florence and Giovanni Federico, Pisa; Antonio Cubel, Valencia), as it has in the 20th century (if a broad definition of industrial policy is accepted). Regulatory or policy capture (Stephen Broadberry and Nicholas Crafts, Warwick; Pedro Fraile, Carlos III° Madrid) was a common theme but one not easy to prove when challenged. The most effective policy instruments identified were trade controls (Valentin Manulyov, Penza; Arend Jan Boekestyn, Utrecht), the legislative stance on railways (Rainer Fremdling, Groningen) and property rights (van Brabant). A clear distinction between national faiths in competition, dislikes of size and desires to catch up, emerged from the papers. Far less apparent was the impact of national industrial policies. In the 20th century a very few national firms were bearers of advanced technology (John Cantwell, Reading) but how their competitiveness and survival were influenced by national policy was not established. Industries subject to industrial policy considered by the conference included electricity generation (Jürgen Müller, DIW Berlin; Francesca Antolin Fargas, Barcelona), steel (Fraile), textiles (João Confraria, Catholic U. of Lisbon), railways (Fremdling) and "infra-structure" (Foreman-Peck).

An extended report on this conference is scheduled to appear in the forthcoming Newsletter of the EHEA.

Second Call for Papers

Eleventh International Economic History Congress
Milan, 1994

The Cliometric Society will sponsor a three and one-half hour "C" session at the September, 1994, Milan Congress of the International Economic History Association.

Anyone wishing to present a paper at this session should send three copies of a three-to-five page proposal to:

The Cliometric Society
Department of Economics
Miami University
Oxford, OH 45056 USA

Relevant Dates: May 1, 1993: New deadline for submitting paper proposals
June 1, 1993: Notification of acceptance of papers

Economic History at the IEA Moscow Meetings

by Paul A. David (Stanford)

(Moscow) Recent work in economic history was unusually well represented at the International Economic Association's Tenth Congress, held in Moscow during August 24-28, 1992. In addition to a morning session on business history, organized by Les Hannah (LSE) and Peter Temin (MIT), the I.E.A. program featured a full day session on the theme "Institutional Evolution and Regime Transitions in Historical Perspective", organized and chaired by Paul A. David. After introducing the international cast of contributors–P. Akhanchi, L. Borodkin, I. Garskova, A. Greif, R. Hayami, A. Kussmaul, C. Leonard, D. Sedik, C. Taft Morris, and G. Toniolo–David launched the session with some brief remarks about (you guessed it) *path-dependence* and its relevance for current economic policy analysis.

As far as any of the Association's officers could recall, this was the first occasion on which the I.E.A. had devoted a session to economic history–as distinct from inviting individual papers on historical subject matter. Happily, the experiment was acknowledged by all concerned to have been a great success. The economic historians attracted an audience of about 45 to both the morning and the afternoon sessions, easily outdrawing the day-long program organized by Amartya Sen (Harvard) on the topic of "Ethics and Economics"–which was reportedly the day's next most popular venue. Undoubtedly, the standing room only turnout was attributable in large part to the choice of a theme that proved to be of intense topical interest, not only to numerous economists from Russia and the other former Soviet Republics, but also to economists from western Europe. The comments from the audience during and after the lively discussion periods attested to their appreciation also of the papers' high quality.

The same appraisal apparently led the I.E.A.'s President and head of the editorial committee for the Tenth Congress Proceedings, Anthony B. Atkinson (Cambridge), to decide to publish brief versions of almost all the contributions to the session in Volume I of the proceedings, including therein a section entitled "System Transition in Historical Perspective", which is to contain the following papers:

Avner Greif (Stanford): "Trading Institutions and the Commercial Revolution in Medieval Europe"

Paul A. David (Stanford): "The Evolution of Intellectual Property Institutions and the Panda's Thumb"

Ann Kussmaul (St. Hilda's, Oxford): "The Gradual Coming of the First Industrial Revolution"

Cynthia Taft Morris (Smith): "Insights from Early Capitalism for Eastern Europe Today"

Leonid Borodkin (Moscow State) and Carol Leonard (Russian Research Center, Harvard): "The Russian Commune and the Mobility of Labor during Industrialization, 1885-1913"

Irina Garskova (Moscow State) and Parvin Akhanchi (Azerbaijan Institute of History): "Discrimination in the Labor Market in the Baku Oil Industry, 1880-1913"

David Sedik (Economic Research Service, U.S. Department of Agriculture): "Systemic Differences and Stabilization Policies in Germany and the USSR, 1923 to 1928".

Volume I of the Proceedings, entitled *System Transformation: Eastern and Western Assessments*, edited by Abel Aganbegyan, Oleg Bogomolov and Michael Kaser, is scheduled for publication by Macmillan, London, in the fall of 1993. Students of recent economic history will doubtless wish to buy it for the lead chapter–the Closing Address to the Congress–delivered by Yegor Gaidar.

E-mail, TelNet, Gophers, Servers-the List Keeps Growing

by Samuel H. Williamson (Editor)

Even as the world of electronic communication gets more sophisticated, it becomes easier to use and more valuable to academic researchers. Knowing how to use the Internet computer network system connecting universities and government agencies around the world is increasingly useful. Some of us make extensive use of it every day, while others have felt that the bother is not worth while. You may feel that it's similar to having a second phone–you are not sure you want to have to answer it all the time.

This article is another primer on this world, suggesting you will find computer networks to be an important "tool" in your work. I discuss below the electronic equivalents of the communication tools of our trade: the mail, telephones, libraries, and conferences.

Telephones and Mail

We use the phone to call or FAX someone and request they send us a paper. Often they reply the same way, mailing the paper or, if in a hurry, sending it by FAX.

E-mail is an electronic substitute for phone, FAX, or mail. You can type a note within the computer mail system you use, or you can send a file that you have created and saved, *e.g.*, the requested paper. Depending on the system at the receiving end, you can send only the text, or even complicated tables and equations.[1] For additional discussion of file transfers, see the February 1992 *Newsletter*.

The Library

The sources for much of our research are libraries and archives. We search the card catalogue, then wander the stacks to locate the data set or the volume we need.

The Internet system links us to the electronic equivalent of the card catalogue, and pulls materials from the "stacks" for us to read as well. Many libraries now record their collections on electronic card catalogs. My University has a very good system for its library, accessible by Internet. Our system is also connected to many other universities in Ohio, so I can search several libraries simultaneously for a source. Other states have similar systems.[2]

Hundreds of networks are accessible on the Internet system. One of the better software applications to navigate this system is Internet Gopher, created at the University of Minnesota.[3] Just recently, I became a client of gopher servers. As a client, I am given a directory of servers all over the world. I simply use the arrow keys or the mouse to highlight where I want to go, then press "enter". This allows me to travel around the world, browsing through material stored in servers at hundreds of other sites, mostly universities. In the last few minutes, I checked an address in the campus directory at Australian National University, looked at a guide to restaurants in Evanston, skimmed the titles of almost 200 books related to economic history in the Library of Congress listings of new books of the last 14 months, and checked the movies showing in Iowa City this week. The gopher client software is free. Contact your computer department and see if it is available on your system. The gopher server is exceptionally easy for the client to use. The direct benefits to you of course will depend on the material available in the server.

My intent is to set up a Cliometric Society gopher server

1 FTP is another method of transferring files. Instead of sending the file, you allow others direct access to your personal machine. They can add and take out files as if your computer were an extension of theirs.

2 Most of us have heard that all the information in the largest libraries in the world can be converted to a digital record and stored in a relatively small space (which is getting smaller all the time.) Some day soon we will be able to access the text of volumes in libraries as well as the index.

3 Minnesota's mascot is the Golden Gopher, so when their computer department developed this software, it is not surprising that they named it "gopher". The name is very appropriate, however, because with a gopher you can "dig" into a world of computer directories that, to me, is quite mind boggling. The University of Minnesota's definition of a gopher is: "The Internet Gopher client/server provides a distributed information delivery system around which a world/campus-wide information system (CWIS) can readily be constructed. While providing a delivery vehicle for local information, Gopher facilitates access to other Gopher and information servers throughout the world."

in the near future. It will provide two things quite quickly: an electronic directory of members and a place for storing papers and data. Its capacity can be expanded when we can afford it. I am asking for membership directories of other economic history associations as well, to create as useful a directory as I can.

Conferences

For intense interaction with like-minded researchers, we go to conferences. There we present our work to those attending our session, or we participate in panel discussions of a particular issue. There are electronic equivalents to conferences called "bulletin boards." These are servers which facilitate "passive" and "real time" conferencing.

At this point, it is helpful to explain the types of servers. Some servers are created for the user to browse and receive information only, for example, electronic catalogues of libraries. Another kind of server allows the user both to receive and add information of his/her own.

The most common of the latter is Listserve, an electronic version of the simple bulletin board. A note is posted and everyone else can see it. Thousands of listservers exist, on every subject imaginable. You register to use the server, and then when any member sends a note to the "bulletin board," it is sent to all the registered members on a predetermined schedule. However, the listserver manager rarely reviews postings for its registered receivers and it may become the electronic equivalent of junk mail or auto-dial phone advertisements.

More complicated versions of interactive servers are more useful, although they require more sophisticated software and more maintenance. These servers allow for "conferencing," where participants log on, choose the topic they wish to participate in, read the discussion that has taken place, and then add comments if they want. Passive conferencing is a selective bulletin board, where you leave notes to others who log on later. With passive conferencing, users can join the conversation if they want; or can review others' discussion, adding a point; and then "leave the room" and go to check another bulletin board. With real time conferencing, participants are logged on at the same time, so there is an immediate give and take conversation in progress. Keeping a record of the conversation is an option, so interested people can follow the conversation up to the point they log on and may join the discussion or review the session at a later date. Many commercial servers announce regularly scheduled real time conferences on a specific topic, with someone moderating the discussion.

Clionet

At present, I am seeking funding to set up a server that allows for these forms of conferencing, as well as the gopher services outlined above. When it is in place, economic historians will be able to gain access to data sets and membership files, and will have the ability to interact with colleagues anywhere in the world.

I envision a system in the future where the *Clionet* system run by the Society will have links with gopher servers set up by several of you. When a user is logged into *Clionet*, he/she will have the choice of looking at everything we have filed here and, in addition, will have the same access to papers, data sets, etc., on economic history servers throughout the world.

I need your help. If you find the ideas presented here interesting and potentially useful, please send me a message sharing your opinions. The more support I can document, the easier it will be to get funding. I especially would like to hear from people who, possibly because of their remote location or lack of colleagues with similar interests, would benefit from this method of accessing resources and exchanging ideas and research materials with others.

I would also like to hear from you if you have set up a server, or are considering it; if you would like assistance setting up a server to be linked with ours; if you know of a funding source which might help finance maintenance of the network; or if you have any suggestions. Please contact me at:

shwillia@miamiu.acs.muohio.edu.

ASSA *(continued from page 1)*

Michael Haupert (Wisconsin-LaCrosse) then presented a paper on note issue during the free banking era, co-authored with Howard Bodenhorn (St. Lawrence). The authors aver that free banks in the antebellum period issued fewer banknotes than they could have, given their assets. Why did this happen? A recent line of research, centered on the National Banking Era, examines the actual costs of issuing notes and finds that these costs were higher than previously thought, thus lowering the apparent profitability of additional note issue. Haupert and Bodenhorn applied this methodology to the free banking era, and found that profits from note issue were reduced, but were still much greater than zero. They then argued that three other factors must be examined in order to solve this paradox. First, there was a risk that the circulation privilege for banknotes might have been revoked. Second, banks wanted to minimize their exposure to long-term interest rate risk. Third, and most important, banks had an alternative method of increasing loans: creating deposits. Bodenhorn and Haupert found that profits earned from attracting deposits were much higher than those to be earned from issuing notes. According to the authors, "When viewed in this light, the conundrum is no more."

Charles Calomiris (Illinois) was the formal discussant. He distributed a seven page handout summarizing his remarks. Calomiris did not find the arguments in favor of charter-revocation risk or interest-rate risk to be plausible. He supported these doubts with evidence from his own research on free banking. Calomiris also expressed reservations regarding the profitability of deposit creation *vis-a-vis* note issue. First, free banks issued fewer notes than other types of banks; if deposits really had been more profitable, then the "under-issuance" should have held across different types of banks. Second, banks cannot change the quantity of deposits at will; customers must do this. When the calculations are adjusted for these factors, profit rates on deposit creation fall.

Annamaria Simonazzi (Rome) presented the final paper of the session, on central bank independence in post-war U.S. experience. She analyzed the monetary policies pursued by the U.S. government from 1942 to 1951, in particular, the pegging of short-term interest rates from 1942 to 1947 and the ceiling imposed on long-term interest rates from 1942 to 1951. Both these policies implied that the Federal Reserve gave up its ability to pursue an independent monetary policy. Recent research has concluded that this was a "credible" monetary policy, since it reflected a government commitment to maintain low rates of money growth and inflation after World War II. Simonazzi argued that this view is flawed for two reasons. First, it misses the fact that low inflation was not the goal of the monetary authorities; rather, interest rates were pegged in order to stabilize financial markets. Second, monetary policy was not credible because of the money supply rule resulting from the peg. Simonazzi points out that no one after the war could agree on how money affected output, and thus the money supply rule was not relevant to macroeconomic policy. Instead, monetary policy was credible because it reduced agents' uncertainty regarding the future. Bondholders after the war could make decisions knowing that short-term interest rates would be pegged and that a floor price was in place for long-term bonds.

Johnston was discussant for this paper. (He did not have a handout.) While he found many of Simonazzi's ideas interesting and plausible, he thought she did not support her hypotheses with enough empirical evidence. In particular, is there any direct evidence indicating the government wanted to promote financial market stability? Or, are there any data on private bank portfolios showing that banks shifted their portfolios in response to changes in the pegging policy? Second, Johnston noted that, while Simonazzi proposed an alternative view of "credibility," she provided no way of testing her hypothesis against current theory. He suggested that some sort of cross-country test could be used to disentangle the hypotheses–a test similar to those employed in the debate over the steepness of the Phillips curve.

The second session, "Money, Exchange Rates, and Capital Markets in the Long Run," started bright and early Wednesday with Pierre Siklos's (Wilfred Laurier) presentation on the velocity of money for five countries over the last century or so. Using tests of cointegration, Siklos and his co-authors, Michael Bordo (Rutgers) and Lars Jonung (Stockholm), found that a single cross-country relationship existed for velocity in the long run. They interpreted this as evidence of common institutional developments in the monetary-financial systems of these countries. Institutional variables, such as proxies for urbanization and the structure of financial assets, helped explain money demand when added to an equation with more conventional variables such as income and interest rates.

In his comments, James Lothian (Fordham) questioned whether the institutional variables used in the study might not be acting as proxies for more conventional determinants of velocity. He also suggested structuring the empirical test to see whether the institutional variables were necessary to obtain the co-integration of velocity across countries.

The second paper, co-authored by Lothian and Mark Taylor (CEPR, London), explored the behavior of real exchange rates in a data set spanning 200 years. Recent studies examining shorter data series have been unable to reject the hypothesis that real exchange rates were a random walk, contradicting the view that they would return to purchasing power parity (PPP) over time. Using a much larger data set, Lothian and Taylor found a long-run tendency for reversion to PPP in the real dollar-sterling and franc-sterling exchange rates. Lothian noted the low power of the unit-root tests often cited in the literature when sample sizes are small. He also showed that their model was better able to forecast exchange rates than a random-walk model for the period of the currency float after 1973. Thus there appears to be little reason for rejecting long-run PPP, even for the period of the float.

Larry Neal (Illinois) recommended that Lothian and Taylor make use of their information about changes in monetary regimes to see if exchange-rate behavior exhibited breaks when monetary regimes shifted. He also expressed some nervousness about the validity of the price indices used by the authors to generate their real exchange rates, noting that the authors used fixed-weight indices for France and the U.S. in spite of the long periods covered. Lothian responded that he suspected errors in the variables would create a bias against the results they obtained.

The final paper of the session was Alan Taylor's (Harvard) survey of Argentine economic growth (or retardation) since the turn of the century. The unifying theme of Taylor's story was the slow pace of capital accumulation after the country's Belle Époque in this century's first decade. After losing access to British capital during World War I, Argentina fell victim to its own high dependency rate and a corresponding inability to generate savings internally. Later, government interventions in the capital market steered scarce capital away from productive investment and toward the purchase of consumption goods and raw materials.

The discussant, Adam Klug (Princeton), took issue with Taylor's approach, suggesting that its exclusive reliance on a simplified version of neoclassical growth theory overlooked the importance of institutions and property rights, as well as the role of human capital accumulation. Klug also argued that Taylor had failed to identify how effective the protection of Argentina's capital-goods market had been during the postwar period, and challenged him to show how his explanation was consistent with the successful growth of countries, such as Korea, with high effective protection of capital goods. Finally, Klug questioned Taylor's use of Canada and Australia as benchmarks, although comments from the floor suggested that a fuller comparison among these countries, including an examination of differences in property rights, might be enlightening.

"Employed and Unemployed on Three Continents" was the topic of the third session. Carl Mosk (Victoria) began with a paper exploring the development of segmented labor markets in pre-war Japan. A casual female labor force, supplied elastically from the agricultural sector, came to dominate such light industries as textiles. On the other hand, an internal labor-market system resting on cultural traditions inherited from the samurai became important first for white-collar men and later for male blue-collar jobs in heavy industry.

Roger Ransom (California-Riverside), although pleading ignorance of the details of Japanese economic history, urged Mosk to draw more comparisons between Japan and the United States. He noted, for instance, the similarity between use of female labor in Japanese and New England textiles, and wondered about the role of the military in shaping internal labor markets and bureaucracies in Japan.

The session continued with John Brown's (Clark) paper on job tenures in turn-of-the-century German cotton mills. From an analysis of worker records, Brown argued that, while job tenure tended to be short in the firms, Germans made efforts to increase worker attachment by introducing tenure-based benefits such as pension plans and other elements of welfare capitalism. These were more widespread and significant than was true for U.S. firms at the time. Brown suggested that German employers hoped to increase tenure and reap the benefits of increased firm-specific human capital. He estimated a duration model which showed that workers originating from areas around the mills had lower turnover, as did those who had accumulated longer job tenures.

In his comments, Robert Whaples (Wake Forest) noted that although tenure-based benefits were more important in Germany than the United States, it was not necessarily the case that U.S. employers tried less hard to reduce turnover. To obtain a measure of the returns to staying with a firm, one would have to examine the wage-tenure profile. Whaples also wondered whether it was really possible to identify the occupations with more firm-specific skills independently of our knowledge of their typical job tenures. Susan Carter (California-Riverside) suggested that Brown might pay more attention to the supply side, such as the nature of the local workforce, as a determinant of job attachment.

The final presentation of the session was by Robert Margo (Vanderbilt), who discussed two papers on U.S. labor markets during the Great Depression–one a survey of recent work on the topic, the other a piece co-authored with his Vanderbilt colleague, T. Aldrich Finegan, on female labor-force participation in 1940. Discussing the survey article first, Margo noted that recent work has revealed the importance of taking a disaggregated approach to analyzing the behavior of both workers and employers during the Depression. The Margo-Finegan paper attempted to resolve an old debate about the relative importance of "added" and "discouraged" worker effects on the labor-force participation of married women, using the public-use sample of the 1940 census. The added-worker effect occurs when married women enter the labor force as a result of husbands being unemployed; the discouraged-worker effect occurs when workers in general leave the labor force because of the difficulty of finding work. Margo and Finegan found evidence of both effects in 1940; the added worker effect showed up only when a distinction was made between an unemployed husband and a husband working on a WPA job. Women whose husbands were employed by the WPA were much less likely to be in the labor force than other women, a phenomenon the authors attributed to WPA qualifications which set limits on total family income.

In his comments, Sundstrom focused on the second paper, advocating a more explicit model of the husband's decision to apply for (and ability to obtain) a WPA job. He also questioned Margo and Finegan's conclusion that there was a clear "treatment effect," such that some women left the labor force so their husbands could qualify for the WPA. Richard Sutch (California-Berkeley) wondered how many of the added workers were self-employed. He noted that in constructing his unemployment rate series for the 1930s, Stanley Lebergott had excluded added workers on the grounds that they were "in distress" and not regular labor-force participants. Sutch suggested that conventional distinctions between employed and unemployed–as well as "in" or "out" of the labor force–were ill-suited to describe the real world and might best be abandoned in favor of a larger, more realistic set of categories.

Later that evening a cheerful, if rather sleepy, crew of 25-30 Clios gathered in the "Gambits" lounge at the Marriott for beer and further discussion, some of them soaked to the ankles after fording the small river that had formed on the street separating the Marriott from the Hilton. A few rounds later, Sam Williamson passed the hat for drink donations, and the tidy sum of $4.50 was collected to add to the Society's coffers.

The fourth and final session, "Tinkerers and Takeovers: Historical Patterns of Technological and Organizational Innovation," was held Thursday morning. The room was packed, despite the time conflict with another session cryptically entitled "The End of (Economic) History." The first paper, co-authored by Kenneth Sokoloff (UCLA) and B. Zorina Khan (Northeastern), extended their earlier work on geographical and temporal variation in patenting activity in antebellum America by comparing the subset of patents issued to "great" inventors to all patents issued. The use of patents as a proxy for innovative activity has been criticized in the past, in part on the grounds that only a small percentage of patents were of much economic value. Sokoloff and Khan argued that patents issued to "important" inventors represented a sample of economically significant patents. Patenting activity was found to exhibit quite similar patterns for the two groups of inventors, with a concentration of activity in areas with well developed markets and during periods of more rapid economic growth. These findings confirm a "Smithian" interpretation that the development of markets and the demand for innovation tended to drive the innovation process itself, especially before the 1850s.

Joel Mokyr (Northwestern) began his comments by noting that the search for a prime driving force behind technological change was unlikely to turn up a "smoking gun or dripping knife." While no murder occurred, the paper was nonetheless severely thrashed by Mokyr, who

took strong issue with the authors' implicit model of innovation (namely, that innovators were rational entrepreneurs responding in a fairly conventional way to market demand incentives) and noted several difficulties with such an approach. Sokoloff and Khan's evidence, argued Mokyr, does not allow one to distinguish between their demand-driven story and a less neoclassical, more evolutionary process of technical change. Sokoloff responded that their perspective was essentially Schumpeterian and that their evidence at least indicated the importance of demand in steering inventive efforts in certain directions.

Richard Sullivan's (Holy Cross) paper also made use of patent data--specifically, English patents between 1711 and 1850. His goal was to examine the machine intensity of innovation during the period, using the percentage of patents issued for production machines as a proxy for machine intensity. Sullivan reported that by this metric, machine intensity varied significantly across sectors, being especially great in textile manufacturing, but that it did not increase much over the period.

Alexander Field (Santa Clara) noted that Sullivan's work appeared to confirm the conventional wisdom that textiles were a leading sector for mechanical innovation during the industrial revolution, and Jean-Laurent Rosenthal (UCLA) suggested the paper might have an important bearing on the recent debate over whether British economic growth during the period was balanced or unbalanced across sectors and industries.

The final paper of the Clio sessions was by John Leeth (Bentley) and J. Rody Borg (Jacksonville), who studied the effects of mergers on acquiring-firm shareholder wealth in the United States between 1905 and 1930. Leeth motivated the paper by noting that some studies using recent data have found that mergers do not benefit the shareholders of the acquiring firms, a phenomenon sometimes attributed to features of government regulation. Mergers and the stock market were less regulated during Leeth and Borg's period, so their findings could shed some light on the hypothesized role of regulation. When both pre- and post-merger abnormal returns are combined, acquiring-firm shareholders benefited from mergers before 1919, but heavy post-merger losses eliminated such benefits for shareholders in the 1919-1930 period. Leeth argued that the evidence was consistent with the view that the merger wave of the 1920s created price bubbles that burst after mergers, leaving shareholders no better off in the long run.

In his comments, Malcolm Burns (Kansas) praised the paper but suggested a few improvements. He noted that in some of the economics and finance literature there has been a "creeping disenchantment" with the capital-asset pricing model employed by Leeth and Borg; the skepticism was worthy of a brief discussion. He also recommended an attempt to examine what happened to the combined value of the acquiring and acquired firms before and after the mergers, to see if there was evidence relevant to the question of efficiency gains to mergers.

As the satisfied cliometricians filed out and prepared to make their way to Orange County's John Wayne Airport, no doubt many were thinking of their own favorite lines from the Duke himself. We leave you with the personal favorite of one of Your Humble Reporters: "Cow ain't nuthin' but a whole lotta trouble wrapped up in a leather bag. Horse ain't much better."

Easterlin Interview *(continued from page 6)*

Well, I think environmental issues are serious in certain areas–much more serious in third world areas than in developed areas that are more attentive to them and can afford to be more attentive to them. But the critical environmental problems have to do not with the fact that there are more people, but that there are people able to afford goods like automobiles. It is modern technology that creates the environmental problem, not population growth.

To my knowledge Simon Kuznets did not outline any systematic patterns of how political development progressed with economic growth. Should his followers be working on such patterns? If so, what issues should they focus on?

Well, Kuznets did try to look a little bit at political conditions, but his major work was on economic growth. The data that he used were purely cross-sectional and fairly rudimentary. I think it was the lack of time series evidence that was responsible for his not pushing that line of inquiry. In contrast, the availability of so much demographic data led to that being the line of research that he pursued more intensively himself. Certainly the linkage between political and economic change is a key matter. I think that one way to look at it more intensively is to focus on the role of developments in universal education and how those are connected with political change in a society. And there has been a recent growth in political science research of a quantitative nature that's generated a lot of historical evidence, as well as in quantitative research on education. However, exploring links between economic growth and political change *via* education may be asking too much of economic historians.

Speaking again of Kuznets, how did you become an economic historian?

It was Kuznets' influence in a couple of ways. Partly, he introduced me to economic history and economic development, and Bob Gallman and I were classmates, in Kuznets' courses.

You just went to Penn and found Kuznets–you hadn't gone *because* he was there?

No, no. But, clearly, he was a towering intellect and all the students were somewhat in awe of him. I think I was interested...most people who end up in economic history start with some sort of predilection toward the study of history and so, you know, Kuznets' approach fit in. Also, for part of my thesis, he had me read a lot of the earlier literature of the British economic historians, the German historical school and more, so that, I think, served as additional fuel to stimulate my interest.

He certainly has had a big influence–not just in his own work, but on the economic historians–you, and Gallman, and Schmookler, and wasn't Lance his student, too? Was there a seminar at Penn?

No. Kuznets–all he did was give two courses. One was in statistics and one was a course he called "Economic Development." A lot of that course was really traditional economic history, going back and reading people like Usher. I thought of his proposals for the study of economic growth when he was trying to get the National Bureau to develop a program in this area, which involved the systematic measurement of national income over long periods of time and a variety of things, and I think that was of considerable appeal to somebody who was coming from economics as I was, because then you could...it was a little bit like he was doing was what the German historical school aspired to do, namely, comparative economic history, but he had put his finger on the quantitative technique that provided for a much more systematic study than the German historical school had envisioned–well, had worked out.

So he was one of the fathers of cliometrics, then?

Well, I would say, definitely, from the point of view of quite a few of us who were involved.

What in your view have been the most important advances in knowledge in economic history? The most serious shortcomings?

By far, the most significant advance has been in the area of measurement. Thanks to the work of Kuznets, and many others whom he inspired, we now have quantitative records about long-term economic growth in many countries, developed and less developed. These records encompass not only overall rates of growth, but allocation of resources, distribution of income, and international relationships. Compared with what was available when I studied economic history in graduate school, a vast void has been filled, increasing our knowledge of the facts of economic growth as well as our ability to test hypotheses and generalizations.

The shortcomings stem, it seems to me, from a tension in the field of economic history that has prevailed throughout my career, a tension of the following sort. On the one hand, there is a set of questions that were traditionally the focus of concern in economic history, questions set very largely by people who came from the discipline of history. Then starting with the emergence of economic development as a field in economics after World War II, a new set of questions arose, put by economists like Simon Kuznets. These had to do with the sources and measurement of technological change, and its role in raising productivity growth, with population growth, with capital accumulation, with education, in short, with an application of the economist's production function framework.

Unfortunately, much of economic history since its takeover by economists has continued to focus on the first rather than second type of question. Take the subject of technology, one dear to your own heart. If we go back to Schmookler's work with U.S. patent data, almost nobody did anything further with those data for the next two or three decades. And yet here is something that would really provide insight into the way technology spread and grew in our society–quantitative insight.

I think education is another example. The number of economic historians who have done serious quantitative research on education is very limited. To my mind some of the fundamental questions of economic growth have to do with technology and education. So from my point of view, many important issues in economic history have shown relatively little progress. One might, perhaps, point to recent work on institutional change as a hopeful sign–certainly I feel that way. But even those interested in institutions tend not to look at education, a most fundamental institution. In addition, one could hope for more attention to quantification in the study of institutions.

The development approach to economic history leads to a concern with the worldwide experience of economic change. This contrasts again with the traditional emphasis in economic history which has been on the national experience of a handful of Western countries, plus Japan and Russia. We need to know more about how development in Western countries had an effect on the developing ones–a few people like Lance Davis and Bob Hanson have done important work in this respect. But the history of international trade, migration, and capital flows is viewed by most economic historians as at the margin of the subject. We also need to know much more about historical experience of developing countries. Indeed, they currently provide a laboratory to observe the process of economic growth and most economic historians are not taking advantage of this opportunity.

To sum up, I feel we have made enormous progress on the facts of economic growth. When it comes to improving understanding of economic growth, however, there needs to be more attention to questions of technological change and institutional change, including education, as well as to international relations and the experience of less developed areas.

Technological change was already identified in the 1960s as a major contributor to modern economic growth. Have cliometricians failed to take on the challenge of explaining technological change?

Well, my answer is in two parts. The first is, when you say technological change was identified as an important factor in economic growth, I think the primary basis for that would be a reference to Solow's work, which essentially said that the main source of growth was an upward shift in the production function; he called that technological change without identifying it as technological change substantively, and a lot of work subsequent to Solow's, such as that by Dennison, and to some extent by Griliches and Jorgensen, moved in the direction of diminishing the role of technological change and replacing it with things like education and economies of scale. So, I don't think that in the substantive sense of major inventions and patenting and all those types of things that there had been acceptance of the substantive importance of technological change. And, I think the cliometricians were not particularly attuned to that. Since we're talking about cliometrics, my first participation was in the second meeting as a reviewer of Doug North's new book (the name of which I now forget because he's written lots since then, much to the benefit of the profession). The thesis of that book was essentially the staple thesis of Harold Innis, that international trade was the great genesis of economic growth. In my review at the meetings and in my review published in the *Journal of Economic History*, I argued that I thought that was wrong, that the critical basis for understanding American economic growth was the transfer of technology from Great Britain and the development indigenously of technology in the United States and its interplay with the high level of education, compared to other areas, in the United States. To judge from the small amount of subsequent work on such topics, I don't think that line of reasoning had any widespread acceptance among the cliometricians. The tendency was to focus on shorter term factors rather than substantive technology.

How did Rostow and Habakkuk influence cliometricians' approach to growth?

Well, you know, it's a little hard to go back and recreate the circumstances of the time. Let's take Rostow first. Rostow basically never argued about the importance of the transfer of technology. His emphasis in the take-off was a rise in the savings rate. So I think his argument was more consistent with a traditional emphasis like Harrod-Domar models, rather than looking at concrete technological change. Habakkuk, on the other hand, was certainly much more attuned to the importance of technological change, and interested in the issue of transfer of technology and the role that factor prices played in that. His work was, in my view, moving in the right direction. But very little was done among the cliometricians to pursue that; maybe Paul David was an early exception when he, as I recall, did an analysis of the introduction of the reaper. Among the American cliometricians, I felt that there was very little attention to technological change, you know, in terms of trying to do a systematic study of it quantitatively. I remember I thought another interesting line of work was by a Swedish geographer, Torsten Hägerstrand, who was studying the diffusion of technology, quantitatively. I set up a session at one of the International Economic History meetings, and what he had to say seemed to fall on deaf ears. So I was discouraged with the kinds of things cliometricians tended to focus on, *vis-a-vis* the central role of technological growth and transfer.

Do you think that's changing now?

Yes, moderately, but not a great deal. I mean, I certainly think that your own work is on exactly the kind of thing that I'm interested in, and Nate Rosenberg was of course the exception. But Nate, as you know, never did things quantitatively, and I think that made his work not very amenable to cliometric approaches. Although there has been greater attention to technological change, for Clio, I still feel it's pretty minimal. Let me add that Ed Mansfield's work in economics is another example of relevant analysis of technological change, and Dick Nelson's work and to some extent, Griliches' work. I just attended a conference in Nate's honor a couple of months ago at Stanford, and the papers there represent a major advance over the state of research on technology of 20 or 30 years ago. But I didn't see the economic historians terribly well represented there.

Given the record of economic historians, are you surprised at the importance of economic history within economics departments–within the discipline of economics?

It's a little hard to say. I would like to think that if economic historians had paid more attention to these concerns–if, for example, productivity change and its substantive interplay with technology and scientific development had been a more central concern of the discipline–that economics departments would feel that economic historians have a lot more to say that is relevant to today's circumstances. But it's also true that many economists tend to have a black box view of technological change and are much more preoccupied with short-term issues.

Would you say there is more of a short-term orientation in the younger generation of scholars in economics and economic history compared to your generation?

No, I wouldn't say that. I hesitate to generalize about how one generation compares with the next. But it seems to me there are younger economic historians who are working on more important problems, and I'm not saying this because you are my interviewer. But there is valuable new work going on, on technological change, on education, on international economic relations. So I would hesitate to generalize about the merits of research in the current generation versus the last. I'd like to see more of the current generation working on the problems I've talked about and with a broader perspective. But there weren't a lot of people that ever did that.

We may have passed sort of a local peak with respect to attention to technique over the substance, or to technical aspects as opposed to the substance–where do you think the discipline stands in this regard? Have we gone too far, at least at this time, in emphasizing

technical purity?

I guess what concerns me is the lack of attention to data versus technique. Much of the discipline of demography involves techniques for evaluating data, for improving comparability and continuity, and so on. I see no counterpart to this in economics or economic history regarding economic data.

I'm also concerned about the extent to which technique serves to establish the questions that one researches. I need hardly point out that time series analysis is in disfavor. The kinds of econometric techniques that predominate are oriented primarily toward the short-term issues on which one can get a substantial body of relevant data. The techniques are valuable for that purpose, but a lot of the analysis of historical experience involves the use of fragmentary data for which econometric techniques are not well suited–using complementary types of series to piece together the clues that you get, to see whether you can find a consistent pattern. One reason I was persuaded that long swings in economic growth were a real phenomenon was that when you look at series on production, wage rates, the composition of output, occupational change, and so on, you get a very consistent pattern. In terms of economic analysis, this pattern told a plausible story about what was going on. It seems to me that today there are often historical studies where one gets a one-shot body of cross-sectional data and runs regressions on it, without much insight regarding historical change. The preoccupation with econometrics tends to predispose work in that direction. I do think that technique is becoming more a matter of routine, and there is more interest in substantive concerns. But it still remains the case that sometimes technique prevails, and important questions are left un-researched.

One last question which is, I suppose, of personal interest. How would you say that the life and experience of someone in the university, of an academic, has changed over the years?

Well, it's very much a generational phenomenon. My generation was a small cohort and had the advantage of being in academia when it was expanding very rapidly during the 1950s and 1960s. There was a scarcity of faculty, research money was readily available, and there was no serious competition. That situation has clearly turned around both on the supply and demand sides. I see today's young faculty as being under much more pressure than our generation to get funding, and to publish. I don't feel that it's a very desirable thing. It forces people to work on the current fad and on what will lead to quick publication. I don't see any solution to that until people get their tenure and by then, they may be so committed to a certain line of work that they're not going to turn to more basic research. I wish I had a solution to this problem, but I don't know what it is.

At the annual meeting of the Economic History Association in Boston this past September, the Trustees of that organization created the Jonathan R. T. Hughes Prize for Excellence in the Teaching of Economic History. A committee of Lou Cain, David Mitch, and Sam Williamson was appointed by Moe Abramovitz, President of EHA, to raise an endowment for the prize. The Committee solicits your support.

Contributions should be sent to:

The Hughes Prize Committee
Department of Economics
Northwestern University
2003 Sheridan Road
Evanston, Illinois 60208 USA

Checks should be made payable to the
"Economic History Association."

Call for Papers

Nineteenth Conference on the Application of Quantitative Methods in Canadian Economic History

McGill University, April 8-9, 1994

The 19th Conference on the Application of Quantitative Methods in Canadian Economic History will be held April 8 and 9, 1994, at McGill University in Montreal.

This conference is being held to honour the contributions of M.C. Urquhart to Canadian Economic History. Given the breadth of Mac's interests, the organizers expect that papers on a wide range of topics, using a variety of quantitative and theoretical methods, will be forthcoming. It is hoped that a volume will be published, drawing on a selection of the papers presented.

Interested scholars who wish to contribute a paper should submit a brief abstract of the proposed paper and a *curriculum vitae*, which is required for conference grant applications.

The deadline for submission of abstracts is July 1, 1993. Please send abstracts to:

Mary MacKinnon,
Department of Economics
McGill University
855 Sherbrooke St. W.
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FAX: 514-398-4938 EMAIL: MARY@FACL.lan.McGill.ca

CALL FOR PAPERS
1994 ASSA MEETINGS

The Cliometric Society will sponsor sessions at the ASSA meetings in Boston, MA, January 3-5, 1994. In addition to our usual sessions, we will have a joint session with the American Real Estate and Urban Economic Association entitled *Urban Cliometrics*. Authors of papers suitable for this session are especially encouraged to participate. The Selection Committee for these sessions will be co-chaired by John C. Brown (Clark University) and Martha Olney (University of Massachusetts). Please share this announcement with colleagues and students who may wish to submit their work.

Members interested in presenting papers should send two copies of a two to five page proposal to John Brown. A requirement for selection is the ability of the authors to send complete 3,000 word paper summaries to The Cliometric Society office by the deadline date. At least one author must be a member of The Cliometric Society. Summaries of all papers will be published as an insert in the October *Newsletter*.

John C. Brown
Department of Economics
Clark University
Worcester, MA 01610 USA
Telephone: 508-793-7238

Relevant Dates:

Two copies of paper proposals to ASSA Session Chair	May 25
Authors notified of acceptance of papers	June 21
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THE ALL-UNIVERSITY OF CALIFORNIA CONFERENCE IN ECONOMIC HISTORY

November 13-15, 1992

The All-University of California Group in Economic History held a conference of sixty scholars November 13, 14, and 15, 1992, at the Marina Marriott in Berkeley. Conference organizers were Jan DeVries (U.C. Berkeley), Peter Lindert (U.C. Davis), Martha Olney (University of Massachusetts, Amherst, visiting U.C. Berkeley), and Christina Romer (U.C. Berkeley). This was one of the Group's open-theme "Hundred Flowers" conferences. The paper abstracts follow, in the order of conference presentation.

The Rise and Fall of the Dutch Public Debt, 1590-1811

Jan de Vries (University of California, Berkeley)

The conference paper consists of two parts. One is a draft of a chapter from a book-in-progress on the economic history of the Dutch Republic. This chapter, with the title above, tours over two centuries of evolving financial institutions and fiscal change. The nation marched back from the verge of default, around 1713, with a long policy of government debt retirement that promoted the rise of the famous 18th-century Dutch foreign lending until the government was plunged into war and debt again in 1780.

The second part of the conference paper and presentation summarizes issues raised by other chapters of the book-in-progress. It ponders these questions: (1) Why did the Dutch saving rate seem so high, especially in the 18th century? (2) Did a regressive tax structure and a large public debt act to redistribute income to the rich? (Yes.) (3) Why did the Republic rely so heavily on credit instead of taxes (except for 1714-1780)? (4) Were there inadequate alternative domestic investment opportunities for the holders of public debt?

A Solution to the Industrial Revolution's Interest Rate Paradox: Public Debt, Private Investment, and Crowding-Out

Gary Richardson (University of California, Berkeley)

Did government borrowing crowd out private investment during the Industrial Revolution? In his 1984 article, Jeffrey Williamson assumed it did, and Robert Black and Claire Gilmore recently published regression results supporting his hypothesis (1990). They showed that changes in Britain's government budget deficit coincided with changes in nominal interest rates, and this, they argued, proved that the neoclassical model was right. On the other hand, Carol Heim and Philip Mirowski critiqued Williamson's hypothesis (1991, 1987). They demonstrated that changes in the government's budget deficit were not correlated with changes in real interest rates, and this, they argued, proved government borrowing did not crowd-out private investment and proved that the neoclassical economic model was wrong.

This paper argues that neither Heim and Mirowski's results nor Black and Gilmore's results proved anything. Their regressions could not discriminate between their maintained hypotheses and more plausible alternative hypotheses. This is because they assumed that the neoclassical economic model predicted a simple, positive relationship between government borrowing and interest rates. It does this, but only under unrealistic conditions of extreme *ceteris paribus*. In reality, particularly during the turmoil of the wars of the late 18th and early 19th centuries, most macroeconomic variables frequently fluctuated, and their fluctuations influenced the apparent relationship between government borrowing and interest rates. The present paper elucidates the historical effects of macroeconomic variables such as government monetary policy, international capital flows, recessions, foreign trade, and war's effect on businessmen's desire to invest. Taking them into consideration, I analyze the relationship between government borrowing and interest rates using a simultaneous equations model. Due to the lack of available time-series data and statistical instruments, it is impossible to prove whether or not government borrowing significantly crowded-out private investment; therefore, that the debate over crowding-out can only be resolved using Jeffrey Williamson's method of the early 1980's: assumption. The validity of the neoclassical economic model cannot be proven or disproven using incorrect statistical tests on an incomplete data set.

Family Organization and Economic Innovation in Europe and China, c. 1780-1900

Jack Goldstone (University of California, Davis, and Center for Advanced Study, Stanford)

Demographers have suggested that the failure of China to achieve indigenous industrialization, and its slowness to adopt Western factory organization, can be explained by focusing on the household organization of classical Chinese society. In China, female marriage was young and nearly universal; by contrast, in Europe female marriage occurred later, and a fair proportion of women remained unmarried through their fertile years. This difference in nuptiality may indeed be the cause of differences in the adoption of factory production. However, I point to a different causal mechanism than is usually considered.

One very specific element of classical Chinese culture may have discouraged factory-type industrialization: the division of labor which relegated female labor almost exclusively to the family household site from an early age. This division was related to early marriage, in which young women entered their husbands' homes in adolescence, and spent the rest of their adult lives working under close supervision of their mothers-in-law in household work units. Women were barred, under this practice, from seeking wage-labor work outside the home once they married. Moreover, early and near-universal marriage meant that there was no pool of unmarried single young adult women to provide a market-based female labor force.

This restriction on the mobility of female labor—unknown in Western Europe—rendered impossible the deployment of female textile workers to sites of central supervision and centralized power sourcesi.e., factories. Thus, while labor in general may have been plentiful, there were cultural restrictions on how it could be deployed. Unable to shift low-cost female labor out of household production, potential entrepreneurs seeking to develop factory spinning would have been unable to compete with home handicraft producers.

A number of observations about the early deployment of factories in China support this view. State-supported and private entrepreneurs who sought to start factories in the interior failed, due to an inability to recruit labor. When mills succeeded, it was in Shanghai, and primarily under the direction of foreign investors, who had few compunctions about employing women in factories, and who could take advantage of the breakdown of state-enforced Confucian family norms in the port city. Chinese-owned mills, as in Tianjin, started with almost all-male labor forces, but eventually brought in women to compete with the foreign-owned mills. Chinese-owned factories had an earlier, and more successful, start in silk production in southern China, where the dominance of extended lineage groups over nuclear families meant that Confucian norms of working in family groups under family supervision could be made consistent with work-groups large enough to constitute small factories for silk-reeling. All these observations suggest that the fall of the Chinese Empire, and the decay of Confucian family labor restrictions, was necessary to free the labor market enough for factory cotton spinning to overcome home handicraft production.

Cultural Beliefs and the Organization of Society: A Historical and Theoretical Reflection on Collectivist and Individualist Societies

Avner Greif (Stanford University)

It has long been conjectured that cultural variations account for inter-society differences in institutional structures which influence economic performance. Yet, in the absence of an appropriate theoretical framework economists and economic historians pay little attention to the relations between culture and institutional structure. This limits the ability to address a question that seems to be at the heart of developmental failures: Why do societies fail to adopt the institutional structure of more economically successful ones?

This paper integrates game theoretical and sociological concepts to conduct a comparative historical analysis of the relations between culture and institutional structure. It utilizes a game theoretical model to examine the cultural factors which have led two late medieval societiesthe Maghribi traders from the Muslim world and the Genoese traders from the Latin worldto evolve along distinct trajectories of institutional structure. Interestingly, these distinct institutional structures resemble those found by social psychologists to differentiate contemporary developing and developed economies. Hence, this paper indicates the theoretical and historical importance of culture, and in particular cultural beliefs, in determining institutional structures, in leading to path dependence of institutional frameworks, and in forestalling inter-society successful adoption of institutions.

The Advantage of Printing Money: Seigniorage, Common Currencies, and the Latin Union

Marc Flandreau (University of California, Berkeley;. D.E.L.T.A., Paris)

It is often argued that the management of a common currency by sovereign states may result in excessive inflation. This paper examines this argument in the light of the Latin Monetary Union experience. Quite surprisingly, the Latin Union did not prove inflationary. The solution of this puzzle lies in the importance of national central bank independence as a means to rule out the "inflationary bias" of monetary unions.

The Crash of '29 and Volatility

George Bittlingmayer (University of California, Davis)

The October 1929 crash and 1930-32 stock decline occurred at the same time as a major shift in antitrust policy and a three-year struggle over antitrust revision that culminated in the NRA. Hoover's shift in policy put in jeopardy the mergers and trade association agreements carried out during the 1920s with the approval of the FTC and the Departments of Justice and Commerce. The lax policies of the 1920s explain the boom and merger wave, the attorney general's Oct. 25, 1929 announcement of strict enforcement explains the crash, and ensuing developments provide a rationale for part of the subsequent stock decline. Formal statistics here focus on the short-run effects of the fluctuating debate on stock prices. Each of 322 *New York Times* news articles on antitrust for 1929-32 is linked with an average 1% decline of the Dow over two to three weeks and a decline of normalized Dow returns equal to one-third of the local standard deviation. The results hold for 1929 as well as 1930-32. Each event also increased volatility by 20 to 40%, a stronger implication since not all antitrust developments were negative or immediately transparent in their consequences. The newspaper dates, while crude proxies, are plausibly exogenous, and the results point to business policy as an important cause of stock volatility.

'Excess Volatility' on the London Stock Market, 1870-1990

J. Bradford DeLong (Harvard University) and Richard S. Grossman (Wesleyan University)

Robert Shiller (1981, 1989) and many others have found "excess volatility" in long-run U.S. stock market indices. We study "excess volatility" in long-run British stock markets indices, building on Bulkley and Tonks' (1989) investigation of the post-World War I British stock market. We extend their sample by using pre-WWI data, and find that the British stock market does exhibit excess volatility if the pre-WWI period is included in the sample.

We also find that British price/dividend ratios before World War I were low relative to those of other nations or to post-WWI Britain, suggesting that pre-war investors were extraordinarily suspicious of those equities quoted on the market. This suspicion of equities may have caused the British stock market to perform poorly as a social capital allocation mechanism before World War I, and may have played a role in British industrial decline.

Agrarian Discontent in the Populist Era: Farm Surplus and Mortgage Payments in Nebraska

Dennis Halcoussis (California State University, Northridge)

In order to compare farmers' economic welfare before, during, and after the Populist movement, a measure of farmers' cash flow is calculated for 77 Nebraska counties, from 1884-1905. The results indicate a decrease in farmers' economic welfare during the Populist movement, with a subsequent recovery which continued after the decline of the movement.

Farmers complained about mortgage payments and wanted an expansionary monetary policy to offset deflation. The real value of mortgage payments, had there been no change in the nominal price level, is calculated; these payments are used to recalculate farmers' cash flow. The increase in farmers' mortgage payments from deflation had a negligible effect on farmers' economic welfare.

The Evolution of Irrigation Institutions in California: The Rise of the Irrigation District, 1910-1930

Edward McDevitt (California State University, Northridge)

This paper addresses the phenomenon of the sudden decline in private irrigation enterprises and the corresponding rise in public irrigation enterprises in early 20th-century California. The existing literature contains explanations of this phenomenon that sees public irrigation enterprises either as solutions to market failure or as non-efficiency enhancing, redistribution mechanisms. An alternative explanation is proposed. This explanation does not rely on the usual market failure arguments nor on rent-seeking theories, but rather it explains the rise of the irrigation district as a result of a combination of agricultural, regulatory, and legal changes during the early years of this century. Using newly collected primary data from irrigation districts and various newspapers, as well as data from state and federal sources, these alternative hypotheses will be tested.

Some Economic Consequences of Railway Development in Brazil: Direct Social Savings on Freight Services, 1870-1913

William Summerhill (Stanford University)

This paper briefly addresses some aspects of the economic impact of railways in Brazil. Freight social savings are estimated for five benchmark years in order to measure direct benefits between 1870 and 1913. A backward pre-rail mode of freight transport meant that direct gains from low cost rail transport services were large. While railways were most frequently constructed to foster the expansion of agricultural exports, it turns out that forward linkages were also strong in the domestic-use sector. However, backward linkages were virtually non-existent, and railway construction and operation failed to spark the growth of domestic iron, steel, and coal industries.

Industrialization in Colonial India: The Case of Jute Manufacturing Industry

Tara Sethia (Cal Poly, Pomona)

The rise of jute manufacturing industry in colonial India presents a case where a colony outstripped an imperial metropolis, Dundee—once known as the "Juteopolis." This paper examines the rise of jute manufacturing industry in Calcutta and its relation with Dundee between 1855, when the first jute spinning mill was installed in Calcutta, and 1914-1918, when Calcutta completely superseded the Dundee industry by replacing it as the leading jute manufacturing center of the world. This analysis highlights the interplay of political and economic forces, which operated in the context of changing pattern of global trade and economy, and which were instrumental in the growth of the Calcutta industry. The paper illustrates the significant role of colonial capital compared to metropolitan capital in the rise of the jute manufacturing industry in Calcutta. The paper also explores the ways in which this case study of the rise of Indian jute industry illuminates our understanding of industrialization in colonial India in the context of the various models of development and underdevelopment.

Expansion of Markets and the Geographic Distribution of Economic Activities: Trends in U.S. Regional Manufacturing Structure, 1860-1987

Sukkoo Kim (University of California, Los Angeles)

In recent years much theoretical attention has been paid to the phenomena of external economies and increasing returns. The endogenous growth models of Romer and Lucas, based on external economies in technology and human capital respectively, have been advanced to explain why there has been no convergence in growth rates across nations as predicted by the neoclassical Solow growth model. The "new international trade" theories of Helpman and Krugman, based on imperfect competition and increasing returns at the firm level, have been advanced to explain the volume of trade, the composition of trade, and the volume and the role of intra-firm trade. These new theories, unlike their neoclassical counterparts, suggest that an activist policy can in principle dominate over a *laissez faire* approach. In these models, a country's protectionist policies may improve the welfare of its citizens and an active industrial policy may even affect a country's long run growth rate. Given the potential welfare consequences of carrying out policies based on these new theories, it is important to examine whether the homage paid to external economies and increasing returns is empirically justified.

The paper investigates the empirical dimension of externalities and increasing returns by examining the long-run experience of the American economy. If external economies are important, then there should be increasing regional concentration or specialization of industrial production over time as transportation costs fall. To test this hypothesis, indices of regional specialization and of localization are used to establish the record of U.S. regional manufacturing structure from 1860-1987. As U.S. regions integrated economically between 1860 and 1890, regional manufacturing structure became more specialized at both the 2 and 3 digit levels of industry aggregation. It continued to specialize through the turn of the century and plateaued around the early 1930s. Since then it has despecialized continuously and substantially through 1987, to the point where U.S. manufacturing is less regionally specialized today than it was in 1860. Moreover, if agricultural and service sectors are included, convergence in employment structure is even more dramatic. While external economies or other sources of increasing returns might have been important in accounting for regional concentration during the late 19th and the early 20th centuries, their significance has diminished considerably over time. Evidence suggests there are limits to such economies and basing long-run policies on their realization may prove inadvisable.

Food for Thought: Rhetoric and Reality in Modern Mortality History

Sheila Ryan Johansson (Stanford University)

This paper scrutinizes the rhetorical devices used to support a nutrition-based theory of mortality decline in the 18th and 19th centuries, and presents evidence to support a competing explanation.

While several authors have conjectured that improved nutrition deserves most of the credit for raising life expectancy before the 20th century revolution in medical knowledge, Thomas McKeown's statement of this position stands out, both in prominence and in its abuse of rhetorical devices. McKeown systematically misused logic, quantification, and the principles of fair selection of scientific evidence, both in his *The Rise of Modern Population* and in subsequent writings. The misrepresentation suggests a subtext, in which McKeown sought to raise appreciation for the health benefits of private economic growth and private incomes as opposed to collective investments in public health programs. Thus half of this paper is devoted to a critique of McKeown's rhetoric.

The other half presents evidence in favor of a disease-environment interpretation of the decline of mortality in the 18th and 19th centuries. There is evidence that much of the lengthening of life occurred in the 18th century, before the classes less able to buy food experienced any clear gain in income. Perhaps even more telling are the spatial and class patterns in mortality in any one early period of time. Before the end of the 19th century, higher-income areas (cities, mainly) had higher mortality than lower-income areas. This consistent fact favors disease-environment explanations of early mortality patterns, not explanations based on nutritional intake.

The Rise of Social Spending, 1880-1930

Peter H. Lindert (University of California, Davis)

A closer look at the dawn of social spending before 1930 reinterprets the timing, sources, and effects of its rise. New data and tests suggest that income growth played less of a role in shaping the rise of social transfers than did democracy, demography and religion. Even in that early half-century the aging of the adult population was a leading force raising government transfers, especially pensions, and cutting support for schooling.