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The Cliometric Society

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Summary of Papers and Discussion from the 33rd Annual Cliometrics Conference

Northwestern University
May 14-16, 1993

by
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Barbara Sands, University of Arizona,
with assistance from Anthony Kwasnica, University of Arizona

(Evanston, IL) The 1993 edition of the annual Cliometrics Conference was held at the Omni Orrington Hotel. Conference hosts were Joel Mokyr and Joseph Ferrie of Northwestern and Louis Cain of both Northwestern and Loyola-Chicago. Participants came from as far away as Paris, France, and as close as Evanston. Paper discussions were informative with remarkably few heated exchanges. Discussions are summarized in the order the papers were given; abstracts are included as an insert.

Stephan Klasen (Harvard) presented empirical evidence on female relative to male mortality in late 18th and early 19th century Germany. In general, Klasen found that excess female mortality (EFM) was concentrated among adult women, but was also present among children of agriculturalists during the late 18th century. He briefly discussed possible determinants of EFM including overall fertility and mortality levels, structures of production, and shifting relative prices. Comments on Klasen's paper focused on three areas: 1) determining the norm for female mortality, 2) empirical and econometric problems, and 3) explanations of EFM. Donald Paterson (UBC) inquired about whether female mortality was significantly different from that for males, and, if so, why. Did the care-giving activities of women really increase their exposure to fatal diseases? Motomura questioned the empirical support for Klasen's suggested story of high EFM in the late 18th century and a subsequent decline; data presented in the paper indicate a more complicated story. Donald McCloskey (Iowa) fired the first of several salvos concerning econometrics by offering his "Comment S" on the frequent confusion of economic with statistical significance. Timothy Guinnane (Princeton, heading to Yale) suggested Klasen might have confused birth parity with total numbers of births in his econometric work. John Lyons (Miami) worried that the regions Klasen analyzed were too large and masked important variations within. Paul Hohenberg (RPI) suggested that falling per capita incomes during this time might have affected EFM, observing that recent research supports no (demographically) significant link between poor nutrition and high death rates. Mokyr questioned the

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What's Inside

Executive Director's Notes

Clionet Gopher

The Society is now running a 'Gopher' server available to Internet users. As I explained in the February *Newsletter*, a gopher is a computer server offering files for perusal or downloading. Our gopher's address is **clionet.cas. muohio. edu** If you have a 'gopher client' on your machine, just follow the directions for connecting to another server, then put in our address. You can acquire the gopher soft-ware *via* Internet from boombox.micro. umn.edu by anonymous ftp. Once in boombox, change directories to /pub/gopher. They have versions for many configurations, including NeXT, OS2, Windows, PC, and Turbogopher for Macintosh. Alternatively, you can access the Clionet server *via* Telnet to our address above and type 'gopher' to log in. Once you reach our server, reviewing files is very simple: use the up and down arrow keys to select a file and a few key strokes to open it. Clionet contains a growing set of files we think will be of value to economic historians.

At present the Society's files include abstracts of papers for the ASSA sessions we sponsored for 1990-1993, a list of papers presented at annual Cliometrics Conferences from 1960 onwards, and The History of Cliometrics. Past issues of the *Newsletters* will be added in coming months. Perhaps our most useful file is a directory of postal and e-mail addresses (where available) of members of The Cliometric Society, the Economic History Association and the Business History Conference. Our goal is to make this directory the most comprehensive list of economic historians around the world.

A few years ago Robert Whaples initiated a syllabus exchange for the Economic History Association; the full set of syllabi he collected for 46 courses is now available in our server. With one command, you can mail copies of as many files as you wish to your own machine. (I found this quite helpful when I revised my own syllabus.) The task of expanding and updating the syllabus exchange has been handed on to the Society by Robert Whaples, and we hope our members will contribute additional materials. Please e-mail them to us or send a (3 1/2") disk to the Society's office. Finally, we have just begun a third service on our gopher, a depository of data files, either of summary information or the smaller data sets themselves. The first entry contains the list of variables of the 36 state BLS data sets collected by the Historical Labor Statistics Project of the University of California.

'ECONHIST' listserv

The Society has also initiated a listserv bulletin board 'For those interested in teaching and research in Economic History'. E-mail messages sent to Econhist are forwarded to all other registered users. We shall shortly send an invitation to join to Society members whose e-mail addresses are known. If you do not hear from us and wish to register, send us a note and we will reply with instructions for signing on. Econhist will be operated as an experiment through early 1994 and we encourage members to try out the bulletin board, to tell us of its usefulness, and to make suggestions for improvement, with a view to expanding the service.

All this is just the beginning!

I am pleased to announce that the National Science Foundation has given tentative approval for funding our proposal to create a 'full scale' server to collect and organize information on economic historical aspects of Global Change. We will establish the service in stages, with a goal of managing this server like commercial servers such as CompuServe. We hope our server will ultimately provide a membership registry database, bulletin boards, 'real time' electronic conferencing, research 'rooms', global change databases, and electronic libraries. Look for more information in future *Newsletters*.

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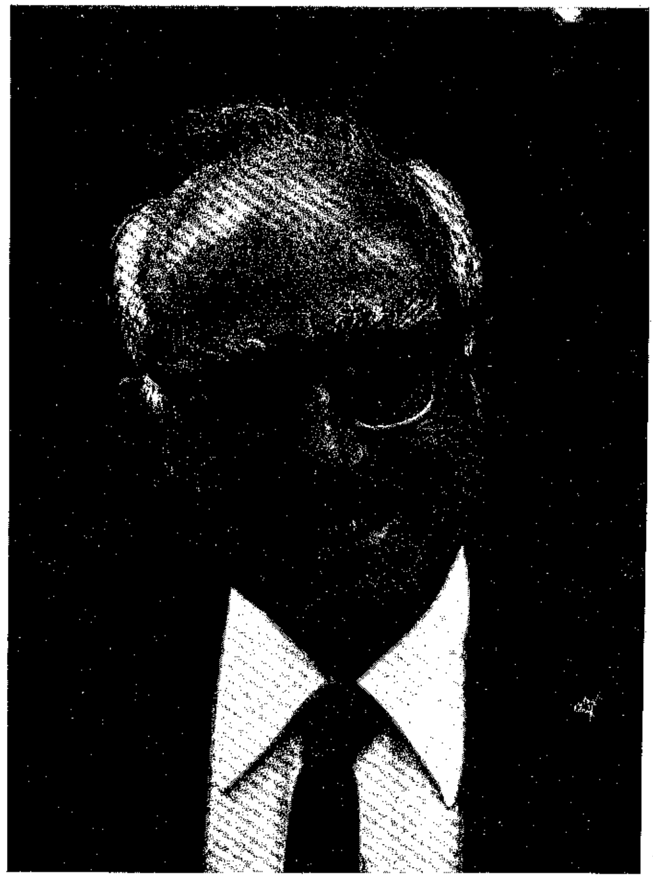
AN INTERVIEW WITH MOSES ABRAMOVITZ



Moses Abramovitz is Coe Professor of American Economic History, Emeritus, at Stanford University. This interview was conducted by Alex Field of Santa Clara University on December 9 and December 16, 1992, in the offices of the ***Journal of Economic Literature*** *at Stanford. Alex writes:*

Moses Abramovitz is one of a select group of scholars whose pathbreaking empirical work has vastly expanded our understanding of the dimensions and determinants of economic growth and fluctuations in the industrializing and industrialized countries of the 19th and 20th centuries. He is best known for his work on inventories, which identified the critical role of fluctuations in inventory investment in accounting for short term cycles in output, for his studies of long swings of growth in the 19th and early 20th centuries, particularly their monetary and balance of payments components, for his work separating the relative contributions of technical change and capital accumulation to economic growth in the 19th and 20th centuries, and, most recently, for his research on catch-up and convergence: the closing of the productivity gap between the United States and its competitors in Western Europe and Japan.

Professor Abramovitz was educated at Harvard and Columbia Universities and has had a long association with the National Bureau of Economic Research. After working as an economist for the Federal Government during and immediately after the second world war, he came to Stanford in 1948 where he taught until his retirement in 1977. He was Managing Editor of the ***Journal of Economic Literature*** *from 1980 to 1985. Past president of the American Economic Association, the Western Economic Association, and the Economic History Association, he remains an Associate Editor of the* ***JEL*** *and an active scholar.*

Having first met Moe when I joined the Stanford department as an assistant professor in 1974, and having worked with him as an Associate Editor of the ***JEL*** *since 1981, I was offered an opportunity in this interview to explore and place in perspective his many intellectual contributions, as well as some of the formative influences on his work.*

How did you first get interested in economics? Was it in college?

Yes. During my first year at Harvard I intended to concentrate in history and literature. Two courses that I took during my first year, however, proved to be quite uninteresting. In English literature of the 17th and 18th centuries the professor talked only about the authors' styles and those of the earlier writers from whom they derived; the substance of the literature, what was being said, was of little or no concern. And the course in history was the great survey course, which all the undergraduates at that time took. I thought it fairly shallow. But I took a very good beginning course in economics, and I had a very good instructor. His reading list offered us the opportunity to read a short book by H.D. Henderson in the old *Cambridge Economic Handbook* Series called simply *Supply and Demand*. That proved to be a formative, almost aesthetic experience. It provided in utterly lucid terms a summary of the neoclassical theory of that time. It showed you how from people's tastes, the state of technology, and people's feelings about the relative costs of different kinds of jobs one could derive the prices of finished goods, the relative quantities of output of different kinds of goods, and factor prices, and the whole thing hung together. It seemed a lovely structure. It fascinated me and I could see its practical applications, so I decided to concentrate in economics. This was in my freshman year. When I came back in the Fall for my sophomore year I was assigned a tutor, Edward S. Mason, then a young Assistant Professor. When I paid him my first visit in the witching month of September 1929 I asked him, "Well, Professor, when is the stock market going to break?" He answered without hesitation, "Almost immediately." When I came back for my second meeting two weeks later it had happened. And then I learned something about economists. I said, "Well, Professor, you must have made a mint of money." He laughed, "Are you crazy? I've never owned a share of stock in my life."

I had another very good tutor later on, Douglas Vincent Brown, the man who later taught labor economics at MIT for many years. And there was a talented group of economics students in Dunster House where I lived. We saw each other often and talked at great length; all of us became economics professors or writers. Paul Sweezy was there, Spencer Pollard, John Perry Miller and still others. We were a self-reinforcing support group.

Besides Professor Mason and Professor Brown were there others at Harvard who particularly impressed you as an undergraduate?

Frank Taussig was surely one. He taught price theory to both undergraduates and graduates. I took both courses, and I owe my knowledge of the classical writers and even more of Alfred Marshall to him. But the man who impressed me the most as an undergraduate was Schumpeter, who was a visiting professor in 1931. I came to know Schumpeter much better later when I returned to Harvard in 1936.

How did the graduate training you received at Columbia compare with what you received as an undergraduate at Harvard?

If I hadn't been to Harvard, Columbia would have been a poor preparation. But I had a good preparation at Harvard. I have mentioned Taussig's courses. This was important because Columbia, at the time, offered no course in price theory for graduate students. It was regarded by the faculty as theology and they refused to teach it. The result was that graduate students, who knew better, organized themselves into study groups that were led by Milton Friedman and me because we were among the few students with some prior theoretical training. The neglect of price theory went on at Columbia for years.

Was this antipathy to micro theory due to a tradition at Columbia of greater concern for the aggregative, more macro aspects of the economy?

Yes. Also there was Columbia's institutional flavor. As between Harvard, Chicago and Columbia, which were the leading schools at the time, Columbia was the one with an institutional flavor and an empirical bent.

Whom did you work with ultimately?

I worked with J.M. Clark mostly, so far as anybody could work with that brilliant but reclusive man. He rarely answered a question orally. Instead he went away to think it over and responded by letter. Perhaps that was better.

Before we go further, I wanted to ask you about your government service during the war as an economist for the War Production Board and the OSS, and later as an advisor to the US delegation to the Allied Commission on Reparations. Were there ways that government service enriched your understanding of economics or suggested certain problems?

I'm not sure whether it was government service in itself or simply the subjects with which I was concerned during the war, which later became important for me. At the War Production Board I was an assistant to Simon Kuznets. We were attempting to put together estimates of the production capabilities of the US economy as a basis for major decisions about such matters as the size of the armaments program that was feasible, given the requirements for civilian consumption. We made estimates of potential GNP at full employment for past years and tried to extend them, having regard to the growth of the labor force, the numbers of men who were being pulled out to serve in the armed forces, the number of women coming into the labor force to replace them, and the amount of capital accumulation, adding an allowance, as well as we could, for the growth of productivity. We were, in effect, formulating a forward-looking picture of what our aggregate economic capabilities might be and what that implied for the armaments production program and for the allocation of resources between the armed forces and civilian production. There were two great controversies to which all this applied. The first one, however, was largely resolved by the time I came to Washington in early 1942. That had to do with the country's production capabilities when our first great armaments program was being planned in 1940 and 1941. When the early plans were being put together the only clear evidence of the country's capacity to produce went back to 1929 because the intervening years were those of the Great Depression. Absurd estimates were proposed that put our production capacity at perhaps ten percent more than we had done in 1929. But the group around

Kuznets and, in particular, Robert Nathan, became convinced that our economic capabilities were vastly greater, making allowance for the growth in the labor force and for the increase in productivity and, of course, for the recovery in the intensity of use of the resources we had. Well, Nathan and a few others like Richard Gilbert managed to sell the view that we could have a huge armaments production program, and I think that may have been the most important strategic decision made in Washington during the War. When the contracts for numbers of airplanes, larger than anybody had ever conceived of before, were awarded, and similarly for tanks and for ships and so on, it turned out that we did have the capacity to produce them along with a flow of civilian goods which was at least as plentiful as anybody had expected. And, in the end, the Axis was overwhelmed by Allied men and materiel. When this production success became apparent, however, the ambition of the military knew no bounds, and they formulated armaments programs which, in the opinion of our group at the War Production Board, could never be handled. If contracts on that scale had been awarded, competition among producers for the limited supplies of labor and crude materials would have caused great misallocations of resources. That was the second fight about war production. It was a very difficult one indeed because the Army strongly resisted having to cut their programs. But we were largely successful.

But to answer your question: in what way was this experience formative? After a year at the War Production Board I was drafted into the Army and assigned to the OSS (Office of Strategic Services). There I was put in charge of the section on German industrial intelligence. The problem here was the same: What were Germany's economic capabilities; what was bombing doing to Germany's economic capabilities? So I worked on Germany for a couple of years and that is how I came to work with the Reparations Commission. These experiences did more than make me appreciate the importance of GNP for the outcome of the war, a war that was won by GNP. The question that lingered in my mind was this: How did the different countries--the United States, France, Germany, Britain, Japan, the USSR--come to have the economic capabilities which in fact they did have? In short I was thinking about economic development. That was the formative influence of my wartime experience.

But your dissertation was on inventories?

Why, no. My dissertation was a theoretical essay. It was called "Price Theory for a Changing Economy". The question that I posed was: If supply curves and demand curves are in process of change, what effect does this have on the allocation of resources and on the way prices are set? Obviously the old static theory had a kind of answer to this question, but I was asking whether there was something more. And it was in the course of answering that question that I came to consider inventories as one of the ways in which the economy responded to prospects of change. So I suppose it was in part because of that, that I was eventually recruited by Mitchell to come to the Bureau to work on inventory cycles.

The thesis itself was largely theoretical.

It was completely theoretical. My Harvard training had after all made a lasting impression.

So it was only after you joined the Bureau that you began to do empirical work?

That's right, it was only after I joined the Bureau in 1938. I worked there for four years before I went to Washington. You should know, first, about the state of business cycles research at the Bureau. When the Bureau started in 1919 or 1920, their first project was national income. That produced Wilford I. King's book. The second project was business cycles. That was Mitchell's undertaking to extend the work--limited work, as he saw it--that he had been able to report in his classic 1913 volume. His conception of the project was that he should repeat what he had done for the 1913 volume, only in a more elaborate form, with some more extensive data, covering a longer time period. Mitchell's 1927 book, *Business Cycles: The Problem and its Setting,* was an elaboration of Part One of the 1913 volume. Then he went on to study the cycles in production, prices, construction, marketing, inventories, credit and banking, the money supply, and so on. Working essentially single handedly at first, later with much help from Kuznets and later still with much help from Arthur Burns, he had completed almost the entire cycle of these studies. It was a remarkable achievement. But when Mitchell and Burns came to review these chapters they decided they were not adequate. They would have to be redone, and it was hopeless to think that Mitchell could do the job by himself. So they decided to enlarge the staff by adding research associates to each of whom one of these chapters could be assigned. These associates would do still more extensive empirical work and perhaps contribute a deeper understanding. Finally

Mitchell and Burns would put the whole thing together. That was the state of affairs when I came in. There was a "chapter" on production of 850 pages. There was a "chapter" on prices of 450 pages. There was a "chapter" on construction of 350 pages. I was given a short chapter on inventories; it was only 60 pages long.

Who else was on the research staff at the Bureau at the time?

Well if you are talking about the major members, there were, of course, Mitchell, Kuznets, and Burns. Leo Wolman worked on unions, wages and labor markets. Fred Mills was working on prices. There was Fred Macaulay who studied the securities markets and who published that very good book on stock prices, bond yields, and interest rates. Sol Fabricant was doing his path-breaking work on production and productivity. Milton Friedman was there for a while working with Kuznets on incomes from professional practice. He largely took over that work from Kuznets and made it his own. Alan Wallis, Geoff Moore, Ruth Mack and Thor Hultgren were other younger people in the business cycles program. So we were a goodly group.

The genesis of your book on inventories was essentially an assignment to complete work which Mitchell had started...

Yes. But you should know that the Bureau's collection of data on inventories was quite small, and it referred to the most diverse and often insignificant commodities. That is what Mitchell's chapter was based upon. The collection was in itself altogether inadequate to provide any useful picture of aggregate inventories and inventory investment. Fortunately, Kuznets was then completing his book on national product from the expenditure side. In that connection, he had made crude estimates of aggregate inventory investment that were one component of total investment and product.

So I spent some months finding out how he had arrived at his numbers. He had a wonderful set of workbooks. You could trace every figure. I went through his work sheets carefully asking him lots of questions, and that's how I really became acquainted with Simon. His inventory figures were another example of Kuznets' ability to use the most imperfect kinds of materials and proxies to make valuable estimates. His judgment about the construction and use of these materials was so good that the results turned out in the end to be quite consistent with the measures that the Department of Commerce later made on the basis of surveys of manufacturing, wholesaling, retailing and other sectors. Kuznets' data yielded very striking results. They suggested that some 85 percent of the change in aggregate output from the peaks to the troughs of short recessions consisted of changes in the volume of inventory investment, and similarly, but not quite so strikingly, in the longer expansion phases of business cycles. So in the minor fluctuations, which were the larger part of the cycles identified in the Bureau's chronology of business cycles, it was not investment in durable equipment or structures that was the major proximate source of the fluctuations in output, it was fluctuations in the rate of inventory investment. This finding was the most important thing reported in the book. It was a result yielded by Kuznets' estimates and, indeed, one he had anticipated in an earlier paper. Then I used Mitchell's miscellaneous collection of time series to formulate views about the very different behavior of finished goods, goods in process, and purchased materials inventories.

What notable advances have occurred in our understanding of inventory behavior since you wrote?

There have been very important advances. The theoretical model, which relates inventory investment to business cycles, was given an enormous push forward by Lloyd Metzler and Ragnar Nurkse. Before they wrote, J. M. Clark and Kuznets had proposed speculative hypotheses about inventory investment based on the acceleration principle. But in their models, the movements of sales were taken as given and inventory investment was a response. I got no further in my book. Metzler and Nurkse worked out models in which income and sales responded to inventory investment and vice versa, and they showed how cycles of output could be generated from this interaction. That was one great forward movement.

Beyond that, there have been very considerable advances in our notions about what constitutes rational inventory policy and, therefore, what expectations we can hold about the way inventories would behave during cyclical fluctuations. So really a great deal has been done since my book.

Let's move on. It seems that after the war your interests moved beyond cyclical fluctuations to issues of long-run growth.

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Conference Report: The Use of Census Manuscripts for Historical Research

University of Guelph
March 4-7, 1993

by

Tony Ward, Brock University and
Michael Huberman, Trent University

(Guelph, ON) More than 50 historians, economists, sociologists and demographers from North America, along with several from Europe, assembled in Guelph to present and discuss a variety of ways to use information contained in manuscript censuses and similar sources. The conference was developed and organised by Kris Inwood, Economics, and Richard Reid, History, of the University of Guelph, and was sponsored and supported by the University, the Canadian Committee on History and Computing, and the Social Science and Humanities Research Council of Canada. By the time of our arrival, the conference had grown from an initially expected one-day affair to a four-day program with 28 papers, over half of which had been distributed to participants prior to the conference.

José Igartua (Québec à Montréal) welcomed everyone Thursday morning (*en Français*), and chaired the opening session on "Canadian Industry and Social Structure, 1850-1900". The first presentation was by Chad Gaffield (Ottawa) on the interrelationships between industrialisation and family reproduction in Hawkesbury, a lumber-milling town in the Ottawa Valley, 1861-1881. He focused on the social infrastructure of Hawkesbury – its gender, ethnicity, literacy and occupational patterns – based on wills, employment records, diaries, etc., as well as census manuscripts. Gaffield argued that much of the literature on Canadian industrialisation inappropriately focuses on cities, and that census manuscripts show rural areas and small towns in fact led the transition. Indeed, by 1840 Hawkesbury was one of the largest industrial establishments in the world. Gaffield encountered problems pervading many studies subsequently presented at the conference: unrecorded multiple occupations, linguistic bias of Anglophone enumerators in Francophone areas, the evolution of family names over time, and misstatements of increasing age over the two decades studied.

Eric Sager and Peter Baskerville (both of Victoria) next discussed industrialisation and urban employment at the turn of the 20th century. They compared a sample of wage-earners in six Canadian cities from the 1901 census with data from the 1881 and 1891 censuses, and constructed estimates of the size of the work force. They note that the structure of the questions, and enumerators' instructions, led to bias in the generated information. In 1901, for example, enumerators were told to question only wage and salary earners, excluding employers and the self-employed, thus providing insufficient information for comparison with more modern figures. Although enumerators' instructions were vague about whether to record weekly, monthly or annual earnings data, the authors were able to identify the chronically unemployed, and found that only 79% of reported wage-earners worked twelve months of the year. Comments from the floor focused on the meaning of 'work' and 'the work force' during this period of transition, and therefore the meaning of contemporary censuses. Occupational pluralism was very common, but was explicitly not recorded, an omission which might be rectified by comparing census data with local directories.

After the first of a regular series of breaks accompanied by ample refreshments, Ben Forster (Western Ontario) presented an analysis of the Ontario furniture industry, 1860-1880. The main issue was the impact of machinery use on wages, skills, productivity and profitability. Primary data were from the 1871 manuscript census, supplemented with data from newspapers, etc. Concentrating on two large firms, Forster examined whether the introduction of machinery led to 'de-skilling' of craftsmen. Initial results of the analysis suggest that, at this stage of industrialisation, machinery enhanced labour skills. However where the labour force contained women and children, there was some evidence of 'de-skilling'. Forster also argued that locational factors such as access to raw materials, a skilled work force, and large markets were important in the determination of firm size. Regrettably, the manufacturing census failed to identify multi-plant firms or vertical integration. Questions on the paper related primarily to problems with the original manuscript record and its transcription.

Andrew Boyd and David MacMillan (both of Toronto) next presented their work on industry and economic elites in the Huron Tract of Ontario from 1850 to 1900, which analyses social and economic changes in Grey, Bruce and other counties as industrialisation progressed, using a large database from the household and industrial manuscript censuses for 1871. They focused on how to use computer software to organise, categorise and link individual records, addressing questions about entrepreneurship, employment and wealth creation. Their approach allows for greater flexibility in categorising and grouping observations. Recent increases in computing power now make it far easier to investigate categories other than the current widely-used but imperfect occupational classification schemes. Participants again worried about data reliability – it is not at all clear how enumerators classified workers' occupations, making it difficult to analyse the transition from craft to proto-industrial activity. Enumerators also did not record distinctions between fixed and floating capital, nor the extent of offsetting liabilities.

After lunch, Gillian Hamilton (Toronto) chaired a session on "Urban North America *circa* 1800", which began with a paper by Mary Schweitzer (Villanova) on the spatial distribution of economic activity, and of social and ethnic interactions, in Philadelphia in 1790. The main thrust was analysis of population and rent densities and gradients in the city. As well as census manuscripts, Schweitzer used early city directories and municipal tax records to assemble a detailed cross-sectional database containing information on location, economic activity, and property type and value. Procedures used by census and municipal enumerators were not fully compatible, causing problems in linking properties by location and people by name, especially blacks, who often did not use a family name. The study showed steep rent and population gradients for 1790 Philadelphia, and readily identifiable social, economic and occupational clustering. Discussion raised the possibility of using Geographical Information Systems to analyse and display this type of information.

Joanne Burgess (Québec à Montréal) next presented a study on reconstituting a craft population *via* linkage among Census manuscripts, parish registers and notarial records. She examined the artisan population of Montréal as it evolved during the early stages of industrial capitalism, 1871-1831. Since there was no nominal census in this period, Burgess used property records generated by the seigneurial system, notarial records of apprenticeships, marriages and property transactions, jury lists, and parish registers to develop her data. Whilst the parish registers generated some useful information, many problems arose from inaccurate or missing occupations, omission of the unmarried, and variant spellings. Despite such data problems, she can analyse one sector of the population satisfactorily – those connected with the leather trades. She constructed a base population group for the period 1815 to 1831, and then traced it backwards and forwards using linked supplementary information. The population statistics so constructed enabled Burgess to analyse both the dynamics of population growth and changes in the structure of the leather trades.

In a shift of perspective, Bruce Curtis (Wilfred Laurier) discussed the social and political influences that gave rise to the organisation of the 1861 census in Canada. He argued that as governments grow in scope and power, they need to generate and control statistical information, both to justify and to coordinate state revenues. Knowledge gives the state power to increase its revenues and to delimit people's lives, and a census therefore is intended to further a specific political agenda. In this light, data obtained from census manuscripts should be used with great care, since interpretations might be imposed on the data that are not compatible with the way the information was originally collected. Curtis discussed organisational problems and biases of the 1861 census: no funds were allocated until about eight weeks before the midwinter census date; many census officers had not received their schedules by the final date; the hurriedly-printed forms contained logical and typographical errors, and commissioners had to apply their own (disparate) interpretations of the information requested. Whilst generally agreeing with Curtis' thesis, participants nonetheless argued that this did not imply deliberate deception, and that use of information derived from census documents is not inherently illegitimate.

The last paper of the day came from Kenneth Pryke (Windsor) on an overview of the 1871 Canadian Census for the Dead. Pryke's interest is in the changing conditions in cities as population density increased and social conditions evolved. Pryke first noted some problems common to information collected retrospectively: a tendency for faulty recollection to yield data grouped at focal points such as decadal years or the most recent month, and significant under-counting in all returns (about 25 to 35%). Most important, the state of 19th-century medical knowledge makes interpretation of true causes of death problematic. Mortality was significantly higher in Montreal than elsewhere, apparently due primarily to cultural factors rather than population density. There were systematic patterns to deaths from both air- and water-borne diseases, but it is hard to correlate these with population density. The incidence was localised, and may have been due to some factor such as transmission by shipping.

Richard Reid invited participants to a reception at his home during the evening. Those attending had to defy a blizzard, which, like 97% of Ontario spring storms, was deemed to be most unusual.

On Friday morning, David DeBrou (Saskatchewan) presided over a session on "Women's Work in Western Mining Communities". Elizabeth Herr (Colorado) discussed how excess product demand and corresponding shortages of labour affected women's labour force participation in Colorado in 1880. She used a sample from eight counties with distinctive economic characteristics to compare women's participation rates. The relevant census manuscripts suffer from the standard problems of under-enumeration of women's work. Perhaps induced by a cult of domesticity, many women reported that they 'kept house' even when they spent much of their time on income-generating activities. Unless there was clear evidence to the contrary, enumerators appear to have assumed that women 'just kept house'. Some of the bias can be corrected through the inclusion of women who were probably working in family enterprises, such as family farms and small stores. The reported participation rate was about 43% for unmarried women, 5% for married women; Herr's proposed correction increases this to 29% for married women, with greater effects in farming and ranching communities. Discussion focused on the difficulties of coping with biases inherent in all data sources, and on a more precise correction from linking census records with such sources as tax registers.

Jeremy Mouat (Athabasca) next presented his work on the small mining town of Rossland, BC, which grew from nothing in the 1890s, was by 1901 a booming but transient community, and went rapidly into decline shortly thereafter. Mouat focuses on the lives of women and the less privileged, rather than the white middle class. Using information from newspapers, and union and corporate records, as well as the census manuscripts, he examined both ethnicity and women's work. Racism in many forms flourished in the community – particularly against Chinese and Italians. Other 'undesirables' were the 'lawless Americans' – who 'kept this community trembling in fear of disaster.' The social and economic hierarchy of this ephemeral society followed standard patterns: British on top, Chinese at the bottom, and other groups at varying intermediate degrees of privilege. Gender also was a source of discrimination, with both social and explicit legal barriers restricting women's employment. Only about 20% of women had formal occupations, and Mouat had to derive information on those who did from sources such as newspapers. Some discussants doubted the reliability and representativeness of newspaper reports. Defenders felt that anecdotal evidence is important to understanding the context of economic and social life, even though its statistical significance cannot be tested.

In the second morning session, chaired by Jamie Snell (Guelph), two members of the Cambridge Group for the History of Population and Social Structure contributed papers on "Women's Work in England *circa* 1900". Eilidh Garrett spoke first on changes in women's work in England at the turn of the 20th century. Although census manuscripts in the UK are subject to a 100 year confidentiality rule, the Cambridge Group has been granted access to anonymous records for censuses up to 1921 to allow study of demographic, social and economic change during the decades before and during World War I. In this project Garrett is investigating changes in women's participation rates and the nature of their work, with data from 13 clustered samples from different areas of England and Wales. Preliminary results show that there was a transition in women's work from service occupations to office jobs, but without any significant increase in women's labour force participation rates over the first two decades of the 20th century. Spinsterhood increased up to the 1911 census, but was lower in 1921, suggesting that wartime mortality did not disrupt marriage patterns. As in other studies, Garrett experienced many problems with how to define women's work using reports which conceal behind stereotypes as much information as they reveal.

The second presentation from the Cambridge Group was by Jennifer Wood, who examined women's work in Stoke-on-Trent (Staffordshire) between 1891 and 1921, with emphasis on interrelationships between fertility and both family and women's employment patterns. Her sample includes a mix of occupations, although the principal source of work in the area was in the potteries. Between 1891 and 1921, women's participation in paid employment increased, women at work had lower fertility than those at home, and pottery workers had lower fertility than all other groups aside from textile workers. An issue is whether their lower fertility (and higher than average infant mortality) was due to exposure to toxic chemicals in the workplace.

Extensive discussion ensued, questioning the effects of World War I and the following flu epidemic. Deciding on an appropriate definition of work, particularly for women's occupations, had been problematic for both Garrett and Wood, who were further frustrated by the impossibility of tracing individuals through 1911 and 1921. Garrett stressed the importance of getting past the proximate effects of fertility to understand underlying attitudinal changes. It was suggested to Wood that the epidemiology literature might help establish links between changing production technology and fertility.

After lunch, Dianne Betts (Southern Methodist) chaired a session on the "Econometric Analysis of Industrial Data". The first paper, by Elizabeth Field-Hendrey (Queens College, CUNY), examined "The Permissibility of Aggregating Male and Female Labour in Manufacturing Production Functions". Most studies of 19th-century US manufacturing have aggregated male and female labour into a single, implicitly homogeneous, factor. Her paper explored the validity of this assumption, using observations for firms employing both male and female labour from the census manuscripts for 1860. There was significant occupational segregation, which itself indicates that male and female labour were not perfect substitutes. Using a Generalised Translog production function, she concluded that male and female labour should not be aggregated in manufacturing production functions, owing to their substantial degree of complementarity. Data problems included absence from the 1860 census of firms producing less than $525 in output, and of proprietors and managers. The distinction between artisan shops and industrial establishments is also difficult to determine, as is the method used to value the capital stock. Discussants wondered about the roles of men in firms also employing women, who were frequently less skilled than men. To some extent men had supervisory roles, but most of the difference was probably due to job skills, Field-Hendrey responded.

Kris Inwood's paper was on economies of scale without mechanical power in Ontario. His proposition is that many firms achieved both productivity growth and scale increases through factors other than the application of mechanical power, for example *via* labour specialisation or organisational improvement. This study applies his own machine-readable data set comprising all 45,000 individual firms in the 1871 industrial census, the only manuscripts to have survived. In this instance, Canadian data are more precise than the American, containing information on small firms and proprietors, industrial categories in fine detail, and distinguishing between adult and child labour. A fundamental problem is to identify *industrial* activities, distinguishing them from domestic production for home use. Inwood excluded firms with power, and industries with insufficient observations to illustrate variation in scale, reducing the sample with the desired characteristics to (only) about 28,000 firms. By the time of the conference Inwood had found that population density was an important (positive) determinant of location, although the exclusion of small firms weakens this effect, and that there was little evidence of economies of scale. Discussants suggested that Inwood test explicitly for constant returns to scale, and examine the effect of disaggregating labour input into men, women and children. Information on wages would have been useful, but was not recorded in the Canadian census.

The final session on Friday, chaired by Jim Irwin (Central Michigan), looked at the "Census Enumeration of Blacks". Michael Wayne (Toronto) opened with a paper on enumeration of Blacks in the 1861 Canada West census. Wayne is studying the assimilation of blacks into Canadian society, and its effects on their culture and ideology, building on a literature about the movement of blacks to Canada before the Civil War. The official report of the 1861 census said there were 11,223 blacks in the province, but this has always been questioned; alternative estimates range from 15,000 to 75,000. Wayne worked through the entire 1861 manuscripts, entering the information in dBase III+, which is adept at the necessary cross-checking and verification. Households and families were poorly identified in the enumeration, but generally can be reconstructed. Recording of this information was generally better in cities than in the countryside. Enumerators were required to record 'Negroes' and 'mulattos', but often this was not done carefully. Wayne shows that the official summary report was inaccurate, with over 17,000 blacks actually being enumerated. An under-enumeration of perhaps 20% is likely, which would suggest a total of 20,000 to 21,000, with yet more blacks 'passing' as white.

Richard Reid (Guelph) then presented a paper on under-enumeration of blacks in the 1870 US Census, part of a project following the lives of black Union soldiers in North Carolina through Reconstruction. The 1870 census is probably the least accurate American census, with the

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Conference Report: Global Change, Resource Shocks, and Economic History
April 2-4, 1993

by
Bob Barde, University of California, Berkeley,
Jan Martuscelli Kent, University of California, Riverside, and
Rob Oxoby, University of California, Davis

(*Pasadena, CA*) In celebration of the 125th anniversary of The University of California, the All-UC Group in Economic History hosted its conference at the Ritz-Carleton Huntington Hotel in Pasadena. This year's objective was to communicate and encourage research on the economic history of global change, reflecting topics now being funded through the programs on "Human Dimensions of Global Change" and "Economics of Global Change", recently established by the National Science Foundation. Sixty scholars participated, most based at UC campuses, but nearly half came from other institutions in Asia, South America, and Europe, as well as the United States. Support was provided by the NSF; the Institute of Business and Economic Research and the Provost for Research, UC Berkeley; the Institute of Government Affairs and the Agricultural History Center, UC Davis; the Metallurgical and Mining Society of America; the California Institute of Technology; and the Latin American Center, UC San Diego. The program was organized by Susan Carter (UC Riverside), Jean-Laurent Rosenthal (UCLA), and Richard Sutch (UC Berkeley). Program and participants reflected both the global theme and multi-disciplinary approaches, served to solidify international ties formed by previous All-UC initiatives with scholars in Russia and Spain, and (it is hoped) sparked new links for future joint activities.

The first session, "Land, Labor, and Money", began with a paper by Gary Libecap (Arizona, visiting at Cal Tech), written with Lee Alston (Illinois) and Robert Schneider (World Bank) on settlement, property rights, and land use in the Brazilian Amazon. They focused on Amazon frontier settlement, state land tenure policies and the role of government, and compared Brazilian with US land settlement history. Particularly applicable is the claim and title procedure used to transfer land and mineral rights from public to private ownership on the Comstock Lode in Nevada between 1858 and 1873. Data from this experience were used to formulate a model of settlement, land claims, ownership and land use by squatters, loggers, farmers, ranchers and others, which might then be applied to settlement of the Amazon. Brazilian data were derived from surveys conducted in three settler sites in the state of Pará, Brazil. Discussant Mary Yeager (UCLA) reviewed the current historiography of the frontier, and related the paper's focus to global issues of state organization, economic development and sustainability. She urged that their examination of contestability issues and resource exploitation also consider the effects of frontier land settlement on indigenous peoples and on the sustainability of natural and social environments as development proceeds. Yeager also suggested that the model control for implicit Western perceptions of social organization.

Peter Lindert (UC Davis) reported on his current work on land use in China, which proposes employing quantitative soil histories to study soil degradation. He has organized six data sets on soil quality and agricultural productivity in China for the 1930s, the 1950s, and the 1980s, which will be used to measure and represent trends in soil degradation.

Why did Japan industrialize successfully when other countries failed? What microeconomic factors underlie the macroeconomic variables of high rates of saving, literacy, and productivity growth? What was the corporate structure of a century ago, and what criteria were used in selecting the governance structures that eventually led to economic development? These questions were addressed by Jennifer Frankl (UC Berkeley) in her paper on the role of the *zaibatsu* in Japan's technological development. One successful governance structure was the family-owned conglomerate (*zaibatsu*). She examined how three different *zaibatsu*, Mitsui, Mitsubishi, and Suzuki, successfully acquired and adopted foreign technology in three industries (cotton spinning and weaving, steam turbines, and ammonia synthesis) in the late 19th and early 20th centuries. Key to *zaibatsu* success was their ability to capture the full returns from investing in new technologies *via* combining ownership of an overseas trading network (*sogo shosha*) to acquire a technology, ownership of raw materials and manufacturing facilities to apply the technology, and ownership of a bank to finance the project. Where it was too costly for a single firm to import a new technology, a *zaibatsu* could do so and ensure its profitability. Daniel Klein (UC Irvine), the discussant, suggested that Frankl compare her explanation to other explanations of *zaibatsu* success and discuss why a centralized market nexus would not do what the *zaibatsu* apparently did.

José Morilla Critz (Alcalá, Madrid) gave the last paper of the session, reporting on economic growth and regional economic disequilibria in 20th-century Spain. The issue of regional backwardness or progress has been fundamental to Spain's political economy and has dominated Spanish economic history in the past three decades. Morilla's paper traced the evolution of geographic economic disequilibria in 20th-century Spain, and attempted to establish a relationship (if any) between overall economic growth of the country and the trends affecting the degree and nature of disequilibria. The paper presented two broad conclusions: 1) there has been a close relationship between the rhythm of economic growth and the degree of economic disequilibrium among the regions, although population mobility has tended to equalize income across regions, and 2) regional disequilibrium is a constant, but the pattern of disequilibrium has varied over time. Discussant Brian A'Hearn (UC Berkeley), who is working on economic growth and disequilibrium in 20th-century Italy, noted that Morilla's work confirmed his own findings that "jobs and capital want to stay in one place." A'Hearn suggested more emphasis be given to identifying the appropriate use of regional GDP *growth rates* as distinct from regional GDP *levels*. He also suggested possible reasons why Spain's record of regional income convergence was rather different from that in other countries.

Jan deVries (UC Berkeley) opened the second session on "Continental Connections" with his paper "Connecting Europe and Asia: A Preliminary Consideration of the Factors Driving Long-Distance Trade". Past research has tended to focus on trade of a single European nation with Asia or, more recently, on European trade in a single Asian market. Establishing a framework for the trade as a whole, deVries argues, may make it possible to discern systemic trends, cycles, and shocks, and to discuss the Asian trade as part of European and Asian economic histories combined. For the period 1501-1800 deVries charted the total number of sailings (including the share returning), fleet and shipping tonnage, numbers of European sailors and amounts of specie invested in the trade with Asia, the development of Asia-based European fleets, fluctuations in profitability, and the changing share of Asian in total European trade. An important, although tentative, conclusion is that the driving force of long-run stable trade growth might be based on Asian rather than European economic expansion. The discussant, Mancur Olson (Maryland), suggested the great importance of Asian trade to the *Europeans* was evidenced by slower Asian growth rates, high cost in European lives, and that Europeans had little to offer but metals. Evidence from European trade with the New World, Olson suggested, would support the view that intra-European trade grew faster than the Asian trade because transport costs to Asia were so high. He referred to a "law of economic history" that rapid domestic growth is not visible in inter-country trade unless both trading partners sustain rapid growth, a pattern observable in the trade of contemporary developing economies. DeVries said he takes this point seriously, but that he wishes to continue focusing on the Asian dynamism that drove trade.

Lance Davis (Cal Tech) presented "Capital Flows Between the Continents in the 19th Century", a paper written with Robert Gallman (North Carolina). Their interest in market evolution has led them to compare the investment histories of US firms in the American and British financial markets, finding that British savers were more experienced, confident buyers than US savers, and that restrictive conditions in the US market reflected security signals for savers. Edwin Perkins (UCLA) proposed expanding the analysis to include connections between Southern US cotton brokers and their London financiers. If integration is found at the international as opposed to the domestic level, then the source of less successful investments by Americans might be entrepreneurial failure or bad marketing by the US firms, a point suggested by Perkins' current research on Merrill of Merrill Lynch, who, before the 1840s or 1850s, apparently took no advantage of many readily-available opportunities.

Tim Hatton (Essex) presented a paper, written with Jeffrey Williamson (Harvard), on rising mass emigration from Europe to the New World, one of the most important features of the evolving 19th-century world economy. They use new data on relative real wages to model the primary forces underlying European emigration. A main challenge was to explain why emigration rates were not always highest for the poorest countries, whose populations clearly would have gained most from the move, and why emigration rates often rose from low levels as successful development took place *at home*. There was considerable persistence in emigration rates linked both to recent emigrant flows and to the growing stock of previous emigrants. Emigration responded systematically to real wage gaps between home and abroad, whose effects were offset or reinforced by direct demographic influences and, more weakly, by the changing level of industrialization. Their results also explain why emigration rates were often lower for poorer countries and why emigration rates typically increased as home development took place. Ken Sokoloff (UCLA) pondered applying their life-cycle analysis to migration from Mexico to the US and suggested they consider, for example, mobility and growth within as well as between countries.

Session three of the conference was devoted to papers and presentations on resources for teaching and research on global change and economic history. Ane M. Quade (California State, Sacramento) first presented material from her paper on cognitive development in economic education, addressing the problem of teaching students to think, not only to think like economists, particularly by incorporating a global approach to economics and economic history into the curriculum. Stressing a need for diversity in theory and perspective in the classroom, she argued that students can be encouraged to learn how to identify and resolve theoretical and real world anomalies by reading interdisciplinary journal materials, engaging in teacher-monitored writing assignments and participating in discussant panels. Eleanor Swent (UC Berkeley) then discussed oral history materials available at Berkeley's Bancroft Library: "autobiographical interviews with persons who have contributed significantly to the development of California and the West." Materials cover western mining history, land-use planning, agricultural parks and the environment, business history, sanitary engineering, California water resources, the Sierra Club, and forestry. Alan Olmstead (UC Davis) commented that he has benefited from these valuable materials in his own research.


After dinner on Saturday, Mancur Olson gave the keynote address, 'Beyond the Measuring Rod of Money'.

Samuel Williamson (Miami) presented a survey of electronic networks, discussing the resources they offer to historians and economists, and conducted a demonstration of some of their services. [See Williamson's article "E-mail, TelNet, Gophers, Servers–the List Keeps Growing", *The Newsletter of the Cliometric Society* 8:1 (February 1993), 9-10.]. Susan Carter continued with a review of current offerings of the UC Historical Labor Statistics Project: 36 data sets from a variety of state Bureau of Labor Statistics surveys, 1874-1920, now available on diskette, including cross-sectional surveys of workers and firms, with micro-level information on working conditions, living standards, family demography, and household economy. Richard Sutch closed the session with a history of the NSF Task Force on the Economics of Global Change and the origin of the related NSF Global Research Program, an important source of extra-mural funding for economic historians.†

Miguel Jaramillo (Pacifico, Lima) began the session on "Human Dimensions of Institutional Change" with a paper on Lima's indigenous labor force at the beginning of the 17th century. Jaramillo disputes a widely-held view that coercive institutions such as debt-peonage were required to retain people in centers of economic development in early colonial South America. Census data show that in 17th-century Lima, 90% of the population were immigrants, providing ample labor for nascent mining ventures nearby. Migrants were no doubt pushed from previous locations by the land expropriation of the *conquistadores*, but rapid economic growth and high wages in the Lima region constituted a strong pull factor. The data show a high correlation between skill and income, suggesting the indigenous population was

†For a list of oral history materials, write the Regional Oral History Office, The Bancroft Library, UC Berkeley, Berkeley, CA 94720 USA. A list of data sets in labor history is available through the Clionet gopher; see the article on page 2 of this issue. For a brochure on the UC's archive of computerized social science information, write UC DATA, 2538 Channing Way, Berkeley, California 94720 USA. Topics suitable for NSF funding are outlined in the Report of the Task Force, and were discussed in the *Newsletter* 6:2 (July 1991), p. 2.

responsive to market incentives and not solely to coercion. Susan Carter stressed that European diseases and Spanish taxation of the indigenous people may have impelled many to relocate in major centers, and that they were rather adaptable, citing the high percentage of migrating orphans who were fluent in Spanish. Finally she suggested examination of travel costs, information flows, and of migration differentials by sex. General discussion revolved primarily around the issues of labor-contracting institutions and the effect of migration on families and Lima's social structure, and the possibly limited extent of a money economy.

"Trade, Institutions, and Law: The Experience of Mexican California", was then presented by Karen Clay (Stanford), who examined organization of mercantile activity in Mexican California in the absence of legal contract enforcement. [This paper was later presented at the Cliometrics Conference, and an abstract is included in the insert for this issue.] Discussion was led by Dave Bannack (UC Davis), who pointed out that the institutions Clay examines were effective for only a decade before the Gold Rush, and were based on an infrastructure provided by the missions, themselves centers of trade, training, and captive labor forces. Also, since the retailers were expatriates, they could remain differentiated from local communities, a status which probably facilitated contract enforcement and debt collection.

"Wealth Distribution in the Antebellum North" was presented by Krishna Pendakur (UC Berkeley), who observed that while the Eastern region was wealthiest, it also had greater wealth inequality, especially for those over 65, and that wealth of natives was more equally distributed than for other groups in that region. Greater inequality later in life may have been due to life cycle patterns of wealth transition; greater wealth inequality for non-natives of a region may have been derived from leaving families behind, thus defaulting on parents' expectations and abandoning claims on their wealth. Gary Richardson (UC Berkeley), the discussant, contrasted the homogeneous population of the West, mainly on small family farms, with the heterogeneous East containing a large migrant population and a higher share of industry. An old age security theory may explain the patterns of inequality: as land became more scarce, people had fewer children and inequality increased as parents saved to smooth consumption later in life; alternatively, a modified life-cycle transition theory may apply: as children became less valuable as security, parents turned towards banks. In both theories fertility and savings rates are correlated, which may explain the observed differences. In general discussion, participants stressed the possible biases of using data limited to household heads: those working and maintaining the land may not be the enumerated heads of household; likewise, the head and the wealth-holder may not be the same.

The closing paper of the session was by Michael Monteon (UC San Diego) on "Exchange Rates and Political Survival in Chile During the 1930s". The common Latin American response to the collapse of trade in the 1930s, import-substitution, led to political and institutional upheaval, especially in Chile, one of the hardest-hit countries. Monteon examined the Chilean use of exchange-rate control to save existing liberal economic policy and how this developed into a pattern of government intervention assigning priorities and rights to various sectors of the economy. Exchange rate control then became an entrenched tool to enhance government revenue by tapping repatriation of funds from foreign interests; that is, the more subtle threat of imposed repatriation at low exchange rates, rather than direct taxation, was used to encourage development of bilateral trade, particularly with Germany and Japan. Roger Ransom noted that the contingency fostering exchange rate controls (saving the economy during the Depression) was replaced by the need to finance the government, and that controlling the exchange rate became even more important as Chile's foreign debt increased. This combination allowed exchange rate control to continue even in light of its apparent inefficiency, becoming institutionally embedded to the point that the cost of eliminating controls outweighed the benefits. Finally, exchange rate controls allowed the Chilean government to put off many difficult and politically unpopular decisions at a time of political and economic turmoil. Further discussion stressed Chile's industrial institutions at the time, most notably cartel control of many commodities and the collaboration of US mine owners with the US government prior to discussions with Chilean officials.

The session on "Mineral Resources and Technology" began with "The Making of America's Industrial Resource Base" by Paul David and presenter Gavin Wright (Stanford). They argue that the increasing resource intensity of US industry after 1850 was due not simply to an abundant resource endowment but to high resource exploitation and extraction rates. That is, the generous resource base supporting industrial development was created endogenously rather than given geologically, with

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Clio Conference Report *(continued from page 1)*

verity of women actually receiving worse nutrition than men. After all, didn't the woman control the kitchen?

Dora Costa (Chicago, heading to MIT) presented her paper analyzing American farmers' use of land and children as old age security assets in the mid-19th and early 20th centuries. She argued that improvements in the functioning of land markets enabled farmers to sell parcels of land as they aged rather than hope for support from their children. In her sample of Civil War veterans linked to the 1900 and 1910 censuses, farmers retired at rates matching those of nonfarmers. Several participants addressed how markets functioned. Jeffrey Williamson (Harvard) asked how we could know that capital gains on land, and not child default, drove changes in strategies and behavior. McCloskey asked whether land markets functioned better in 1900 than in 1850, a concern also raised by Christopher Hanes (Penn) and others. Costa's measure of land markets' functioning was the ease with which land could be sold; much improvement occurred before the Civil War. McCloskey then suggested she use a portfolio model of children and land. Thomas Weiss (Kansas) suggested studying land acquisition patterns, while Randall Kroszner (Chicago) asked about capital markets and savings behavior. Others discussed whether children and land were substitutes, Jacob Metzer (Hebrew; Woodrow Wilson Center) suggesting they may have been complements. Anand Swamy (Maryland) and David Weir (l'INRA-HEDM) countered that good markets could supply labor other than that of children, and Paterson mentioned two further sources of labor: neighbors and family members, such as J. S. Mill who had returned home at harvest. Hanes found it odd that substitution effects showed up only after the fourth child. Susan Carter (UC-Riverside) responded that Becker and Lewis argued that many children can substitute for land whereas a few cannot. Costa noted that when children were young, mortality risk might affect a family's strategy. Cain and Larry Neal (Illinois) wanted consideration of other factors affecting childbearing, such as religion, contraception, and ethnicity. Samuel Williamson (Miami) and Kwasnica voiced their concerns about potential biases from using a sample restricted to Union Army veterans.

Susan Carter began her paper with the question "Does intra-household resource allocation offset or merely reproduce gender- and age-based inequalities in the larger economy?" Using a data set produced from a survey of 943 employed male and female youths in New Jersey in 1903, she focused on trying to determine whether household heads in turn-of-the-century America behaved altruistically or strategically towards their children. Comments focused on Carter's data, statistical work, and suggested causal stories. Martin Eisenberg (Knox) worried about differences in reported occupations within the sample and their effect on the statistical results. Jeff Williamson wondered about the representativeness of the sample, and suggested the strongest empirical finding was that young women gave most of their earnings to the family while the young men did not. Neal suggested ethnicity differences should be taken explicitly into account. Daniel Raff (Harvard) questioned whether retained earnings really represented consumption (and not savings) as Carter argued. Weir asked that she include family size as a variable in her empirical work, suggesting that the altruistic retention rate was determined by size of household and might itself influence the decision to work outside the home. Rick Sullivan (Holy Cross) questioned the apparent distinction between altruistic and strategic behavior, particularly in the face of all-or-nothing consumption goods like education. John Nye (Washington U.) also was skeptical about the defined differences between altruistic and strategic behavior, pointing out that a household's decision to assume an apprenticeship might be seen more productively as an endogenous choice rather than an exogenous event. Swamy worried that the different requirements of different jobs might confound Carter's defined behaviors.

Karen Clay (Stanford, heading to Toronto) started Saturday's proceedings with her paper analyzing trading coalitions in Mexican California. She used a game theory (GT) model and qualitative evidence to argue that three interlinked types of institutions substituted for a formal legal system and enabled trade to grow by enforcing credit, agency, and other obligations. She began by addressing questions Mokyr had raised at earlier presentations and suggested that ostracism of cheaters paralleled *The Scarlet Letter*. Several participants discussed the relation of the various coalitions to the legal system. Their existence apart from, yet within, Mexican law reminded Hohenberg of Zorro. Klasen suggested that rotating *alcaldes* in the coalition had strong incentives to enforce the law. Jeremy Atack (Illinois) wondered why the distinction between law and custom mattered, and Clay defined 'law' as involving state enforcement. Film-fan Hohenberg found coalition members' observance of commitments – even at great expense – analogous to *Die Meistersinger*. Carolyn Tuttle (Lake Forest) responded that long-run gains from keeping commitments might exceed short run gains from defecting. McCloskey opened another can of worms, questioning whether "Hobbesian" game theory helped one analyze merchants of a common culture. Guinnane and Nye questioned GT's value-added for Clay's argument. Mokyr defended GT and called for a paper explaining its value for economic history. Clay maintained that GT provides a useful structure that makes behavioral assumptions and strategic interactions explicit. Culture can help identify more likely solutions. Daniel Shiman (SUNY-Oswego) and Neal wanted more evidence. Shiman argued that cultural screening tested whether potential members would behave as members would, hearkening to *A Stranger Among Us*. Annamaria Simonazzi (Rome and Notre Dame) suggested using trust relations as a framework for analysis and Ferrie asked whether coalitions of outside merchants, perhaps from Oahu or Boston, could be observed trading in California. Clay replied that while such a coalition may have existed, it did not act cooperatively.

Those attending a Clio Conference for the first time: (l. to r.) David Surdam, Anthony Kwasnica, Randall Kroszner, Tony Ward, Mike Scherer, Annamaria Simonazzi, Stephan Klasen, Karen Clay, Daniel Shiman, Sukkoo Kim. Not shown: Dora Costa, Anand Swamy.

Anand Swamy's work centered around capital market imperfections in rural India and their differential effects on poor and rich farmers. His household-level data were drawn from various villages in the Punjab in the 1930s and report production costs for sugar cane and cotton crops. Questions from the group focused mainly on the econometrics and his conclusions concerning causality. Clay wondered whether increasing returns to scale in farming (and thus the competitive advantage of large farms) might bias the empirical findings regarding poor (and presumably smaller) farmers' disadvantage in capital markets. Sands and Carmel Chiswick (Illinois-Chicago) both wondered about land acreage serving as the proxy for wealth. Neal suggested sugar cane and cotton might have unique characteristics and that other crops should be investigated as well. Jeff Williamson requested background information on variance in economic fortunes across time better to understand this problem. He and quite a few others expressed their doubts regarding the value of last year's profits as proxy for wealth. Christopher Udry (Northwestern) suggested that it is currently considered acceptable for changes in current cash flows (*i.e.* last year's profits) to represent changes in wealth. There were many questions regarding empirical evidence for imperfect capital markets, and a long discussion ensued about what measure might best be employed. McCloskey voiced skepticism regarding Swamy's apparent linkage of commercialization with imperfect capital markets in causal arguments. Costa suggested that even if formal credit markets were to improve, we still might not observe small farmers increasing their borrowing.

Loren Brandt (Toronto) presented a paper, written with Arthur Hosios (Toronto), analyzing incentives, credit and rural labor contracts in northeast China in the 1930s. They used a model of wages and a sample of 1,049 contracts from northeast China in 1935 to argue that wage profiles reflected credit arrangements and worker-incentive effects. Several people asked about employer-employee relations. Tony Ward (Brock) asked if those relations were semi-feudal and whether they involved linked contracts. Brandt replied that tenures averaged three months and that formal interlinking was rare. Swamy was struck by the tenure brevity, thinking that an employer would want trusted employees. Udry suggested that respondents were more willing to report employing than being employed. Responding to Neal's question about reported agreements, Brandt said that the data provided information *ex post* rather than *ex ante*, and that women had not been employed using the long-term contract arrangement. Chiswick asked if families hired out rather than individuals; Brandt doubted it. Jeff Williamson asked if there was evidence of continued bargaining; there appeared to be some. Sands compared the paper to Swamy's and suggested that Brandt explicitly look at interlinkages of the credit, labor and land markets, including the use of land as collateral. Swamy noted that some long-term

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MWM desires SWF to run counterfactual experiments

The Mullah's pilgrimage to the Middle East had apparently borne fruit, and he was informed early on at the annual Spring rites of the clioms that the society would once again recognize the best pearl of wisdom to be thrown up during the discussions. He was also delighted to see that the scholar from the desert sands whose bits of wisdom inaugurated the award was in attendance. She and a minor league player from that area were sent as the official wildcat representatives because the first team was apparently too embarrassed to attend after their not so fabulous five was ousted from the roundball tournament by a group of amateurs. Her presence, as well as that of the great hawkeye rhetorician and he who has studied the potato at great depth, bode well.

The Mullah reminded himself and his fellow-listeners that the best proverb must be pithy, and must contain wisdom for all times and places. If it is also germane to cliometric methodology so much the better. A lack of universality explains why some excellent sayings are not among those chosen for the award. This year for example, the saying "I don't mean to insult you, but that is the same question that the garrulous gourmet from the Bay area asked" was recognized as pertinent to the society's deliberations and an obvious truth, but it was uncertain that future generations would agree. Likewise, the allegation that "trade in Mexican California was a zorro-sum game" tied in nicely with the broad theme of the meeting, but has little to say about export-led economic growth.

A number of methodological aphorisms were put forth: "Where the optimum is depends on a lot of parameters." "As a rule, I never care about the constant term." "Anecdotal evidence is not proof at a cliometrics conference." Or that proffered by she who keeps labor statistics by the riverside – "The nature of the results will not be substantially changed, whatever they are." Many of the clioms were dismayed to discover that the latter bit of wisdom was not among the finalists. The Mullah, having once learned the costs of taking the clioms' opinions into account, gave their pleas short shrift.

So, with little discussion among the senior members of the tribe who had failed to recognize the appeal of "there is more than chewing gum between Canada and the United States," the Mullah pared the entries to three. The scholar who brought the oil can that fits beneath the seats of the silver birds screamed out in feverous pitch that "you should never give back arguments with thermometer symptoms." A truly catchy idea, but it was never made clear whether it was true for all thermometers or only rectal ones. And it had the potential for leading the society astray; on the one hand shifting the focus of research to the wrong end of things and on the other raising methodological discussions about thermometry.

1993-94 Can Holder Sam Williamson with previous winners: (l. to r.) Larry Neal, Jeff Williamson, S. Williamson, Peter Lindert, Joel Mokyr, and Tom Weiss. Absent: previous recipients Don McCloskey, Roger Ransom, Richard Sutch.

The hawkeye rhetorician lived up to his expectations as a purveyor of pith with "instead of doing game theory, you might as well do sociology." This has all the hallmarks of a winner -- brilliant, incisive, and seemingly universally true. Some of the Mullah's best friends, however, are sociologists. While he looks forward to bringing this issue to their attention, he thought it premature to ensconce it in the great book of wisdom to which all scholars should turn.

The winning phrase was uttered by a newcomer: "Single people do worse, because they die before they have a chance to get married." This certainly seems true, although its exact meaning is ambiguous. On the face of it, it would seem to carry on nicely the Republican themes about family values. If Dan Quayle had had the benefit of such speechwriting ability, would the 1992 election have turned out differently? Would it now be worthwhile for the society to engage in great counterfactual debate? Had Bush been reelected would the price of haircuts in southern California be significantly less than $200? On closer reading, however, it can be seen that the phrase does not necessarily stress family values. Single people can do better so long as they have the chance to get married; they need not actually marry. Thus, as with so many of these aphorisms, new meaning emerges as one ponders more deeply, albeit wisdom increases in smaller and smaller amounts.

The Mullah missed the lively banter evoked by the rule of two hands, and hopes that it will be reinstated at future meetings. After all, most of those in attendance are economists and feel constrained by one-handed arguments.

He was pleased to see that again this year the announcement of the oil can winner was presented musically — or nearly so. The falsetto rendition of the revised score of "I Did It My Way" in no way diminished the crowd's enthusiasm for the choice of he who has been organizing these meetings longer than anyone else. With that vast experience, can we expect next year's announcement to be made in the grandiose style of an Andrew Lloyd Webber musical, or perhaps more fittingly for Tucson, a Las Vegas Revue?

Much pressure will be on the wildcats next year as well. With the meetings to be held in their home town, they had better realize they can run, but they can't hide!

Submitted humbly by
The faithful and obsequious servant
of the Mullah

Call For Papers

Changes In Long-Term Competitiveness: An Historical Perspective

The journal *Structural Change and Economic Dynamics* has asked me to act as guest editor for a special issue on the above subject. The editors intend that the papers selected would add a long-term perspective to current debates on the restructuring of the world economy and the changing relative position of countries and regions.

Within this framework we are willing to consider papers in a wide range of areas, with the proviso that they should consider (empirically or theoretically) issues relating to the long-term. Contributions outside the traditional domain of economic growth (*e.g.*, from historians) will be encouraged. Possible topics might include (but not be limited to):

- Methodologies for analysing long-term competitiveness
- Validity of "competitiveness" statistics
- Interactions between economic restructuring and political/social/institutional factors
- Governments *vs.* markets as influences on changing competitiveness
- National or regional determinants of competitiveness
- Temporal patterns of restructuring
- Multinationalisation and globalisation as long-term influences
- Trends in major sectors/industries
- Management factors and determinants
- Static *vs.* dynamic comparative advantage
- Role of science, education, etc.

You are invited to submit a paper for possible publication in this special issue. It would be helpful if you could give me an indication of intent to submit a paper as soon as possible after 30th June 1993, and in any case supply an abstract by 31st August 1993. Deadline for final submissions is 1st January 1994, and the issue should appear in September 1994. Usual refereeing procedures will be followed. Selected papers ready by August 1993 may be invited to the annual meetings of the journal, to be held at Lake Garda, Italy, in mid-September.

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Clio Conference Report *(continued from page 16)*

labor contracts in India also had a credit component, suggesting that default rates might help clarify the analysis. Udry observed that extension of credit through the labor market (and the imputed interest rates) ought to reflect seasonal fluctuations. Jeff Williamson asked for a discussion of the historical issues addressed by the paper. Brandt responded that in earlier work he has argued for a view of the early 20th-century Chinese economy as dynamic and growing. The present paper, he believes, suggests that perspective for the rural northeast; he plans further investigation of this topic for additional places and periods.

The afternoon session on Saturday was held in memory of Jonathan R. T. Hughes, containing three papers linked to important themes in his book *The Governmental Habit.* Sam Williamson introduced the session with a brief overview of Hughes' contributions to economic history and of his formative influence on Cliometrics.

The first paper of the session was by Werner Troesken (Pittsburgh), analyzing the political economy of the Sherman Antitrust Act. In his book, Hughes was uncertain about just why the Act had passed. Troesken argued that because many farmers had already participated in farmers' alliances to buy and sell goods, they then faced relatively low costs in organizing to support antitrust laws after a jute-bag price increase. By the same token, some trusts were quite influential in key preliminary votes and probably succeeded in weakening the Act. Klasen opened by questioning Troesken's interpretation of lobbying pressure in Congress and suggested more extensive examination of the historical literature. Eisenberg wanted a model of legislators' actions in which re-election mattered more than simple vote maximization. He argued that petitions entered into the record reflected representatives' positions already taken. Shiman suggested using the percentage of the farm vote as an indicator of support. Michael Scherer (Harvard) argued that the votes analyzed were ambiguous indicators of true positions because strategic concerns mattered. Troesken said that he would look at other preliminary votes. Kenneth Sokoloff (UCLA) recommended that Troesken estimate how much effect lobbying pressure had, while Simonazzi inquired about the role of the Judiciary Committee. David Augustin (Northwestern) argued that Troesken should separate the various economic interests within each industry, such as producers and refiners. Cain suggested looking at the passage of state antitrust laws and whether there were parallels in the ICC's formation subsequent to creation of several state regulatory commissions. Troesken said he is working on state antitrust laws. Mokyr then asked why the antitrust movement developed in the US but not in Europe. Hohenberg responded that, in Germany, foreign firms were viewed as sufficient competition for cartels that promoted stability and investment, and Rebecca Stein (Northwestern) commented that the presence of farmer alliances might differentiate the situation from that in Europe.

Next, Michael Scherer presented a paper, written with William Comanor (UC-Santa Barbara), on the effects of the Early Sherman Act on industrial performance in the American economy. They used two cases, the breakup of Standard Oil and the non-breakup of US Steel, to discuss the effects of the American government's trust-busting activities in the earlier part of this century. Their conclusion was provocatively stated – corporations that were forced to divest seem to have flourished while corporations that just avoided breakup seem to have struggled. Since their interpretations depended on two large industrial cases, most of the floor discussion focused on the facts of the two cases. Tuttle, looking at the paper's tables, wondered why labor productivity fell after Standard Oil's divestiture. Scherer responded that this may have been a real phenomenon, or it may have been a fiction produced by bad data; he suspected the latter. Nye thought the recovery of steel production in the 1980s came from fringe firms rather than from the entire American industry, but Scherer noted that gains were pretty much across the board. Costa asked if US Steel had a monopoly because it was more productive, like Standard Oil. Scherer replied that he wasn't certain either firm had been the low cost producer. For example, US Steel's only edge was in acquiring Carnegie Steel, and Standard Oil's advantage came from its low railroad rates. Neal inquired about the role of judges in these cases; was there a biased filter in the legal system? Scherer commented most judges were able to sort out divestiture cases, but some could not. Several members of the audience suggested the authors should emphasize more strongly their counter-Schumpeterian/Galbraithian findings. Mokyr suggested the paper showed the falsity of Schumpeter's contention that a competitive industrial structure is statically efficient, while monopoly is dynamically efficient. Klasen defended Schumpeter and Galbraith by stating they looked at perfect competition *vs.* simple monopoly, not oligopolies such as the oil and steel cases. Scherer said that Galbraith had at least talked about oligopoly. Peter Lindert (California-Davis) agreed with Mokyr and suggested more attention should be paid to the three "unbusted" industrial failures. Sokoloff cited David Landes' argument that industrial structure was irrelevant in determining technical change. Scherer countered he is still a Schumpeterian who believes technical innovation is endogenous to firms.

Peter Lindert concluded the Saturday sessions with a cross-national analysis of patterns of social spending in the 20th century. He seeks to explain the monotonic increase in income shares of social spending, a phenomenon discussed by Hughes in *The Governmental Habit.* Among industrial countries, democratic institutions and demographic factors apparently affected the level and composition of social spending, with the most important variables changing over time. The level of social spending has not been correlated with the rate of economic growth. He suggested that countries with high social spending have found relatively efficient ways to deliver those services. Hanes suggested that the additional social spending was skewed toward the kinds of public goods Adam Smith (Glasgow) would favor, and Motomura added that modern democratic governments might have wanted to maximize economic growth and thus raised efficient social spending to achieve it. On the skeptical

Hard at work: Cliometric intensity at the Orrington.

side, Nye posited that all governments try to maximize revenue; the real changes are in technologies of administration and in composition of spending. Guinnane suggested looking for mandated private spending. Paterson postulated that Fabian socialism and benefit guarantees might have pushed entitlement costs out of government control. Metzer asked Lindert to clarify what he meant by cutting the deadweight loss in social spending, while Mokyr was not sure Lindert's examples really supported his point. Jeff Williamson suggested measuring macroeconomic instability to predict spending on the safety net, and urged Lindert to link his paper to the new growth literature. Lindert agreed that trend and cycle could be separated, at least for 1960-81. Positional goods might matter, said Klasen, and pay-as-you-go pension schemes might be sensitive to the age distribution. Carter suggested that the connection between the elderly population and educational spending weakened after 1960 as people became more mobile. Lindert responded that spending per elderly person increased as the population aged only until the 1970s. Chiswick asked about the distribution of religion in the countries surveyed. Lindert responded that 25% of the population was neither Protestant nor Catholic. Costa argued that heterogeneity might hurt social spending.

Rick Sullivan began the Sunday morning session by presenting estimates of the value of patent rights in Great Britain and Ireland during the period 1852-1876. He examined the role of research and development, changes in GNP, and the possibility of technological exhaustion and compared the value of patent rights in his period with those in the latter half of the 20th century. Most of the questions focused on econometric issues. Mokyr noted that the estimation of the value of the patent rights is based on a log normal distribution. Scherer thought there were insufficient data to estimate the tail of this distribution. Both urged Sullivan to perform some tests to determine the appropriateness of his specification; Scherer commented there are German data that could be used to do this. Sullivan replied that he used the log normal to maintain comparability with other studies. Hanes asked why a riskless discount rate was used, Nye adding that the value of patents must be related to the value of invention at some constant rate. Sullivan disagreed by pointing out that some highly valuable inventions might have had limits on the value of their patents. Jeff Williamson asked whether the model had been corrected for autocorrelation and wondered whether the changes in the mean value of patent rights were "big" or "small"? Both Hohenberg and Sokoloff commented that not much credence should be placed in mean values; rather, patent values should be disaggregated by sector. Ferrie added that if some inventions also earned returns outside Britain this would change their mean value. Sokoloff, who recently has completed a study of great inventions (forthcoming in *JEH*), thought it would be interesting to estimate the level of research and development in order to get some notion of the expected level of invention. Lindert suggested that firms might have been able to "sleep" on valuable patents, and that actual exercise of such ability would probably change the social implications of Sullivan's work.

Sukkoo Kim (UCLA, bound for Washington U.) presented his paper on the importance of economies of scale and externalities in the regional specialization of American manufacturing. He calculated Krugman's specialization index across nine US census regions and found that regional specialization climbed from 1860 to the early 20th century, leveled off, and then declined after mid-century. His results suggested that factor endowments and mobility, along with changes in firm organization, were important explanations of this pattern. Hanes asked whether census regions were the appropriate level of analysis, suggesting the US was a national economy. Sokoloff protested, and Swamy noted that migration was not a good measure of goods market integration. Klasen suggested that the single market might hold after 1945. Paterson asked what was occurring within the regions. Scherer thought that different types of industries should be distinguished, given the possibility that some industries reached diseconomies of scale. Cain asked whether industries and job descriptions remained constant enough over his time period. Simonazzi suggested considering vertically-related firms in the same sector, and Kim responded that he is indeed studying specific industries. Lindert asked whether he could disentangle the importance of immobile factors from the growing demand for skill-intensive products. Hohenberg and others commented that externalities might still be important. For example, those regions catching up might benefit from externalities, which would cause national decentralization. Motomura, Clay, and Shiman asked about possible effects of federal and state government policy changes after World War II. Kim did not believe any of these weakened his basic point. Mokyr argued that although it is hard to find quantitative data on technological spillovers from concentration, they are still important. Scherer noted others' efforts to measure spillovers and suggested that internal economies of scale could increase regional agglomeration.

Christopher Hanes opened the concluding presentation with a discussion of predictive differences between Keynesian and real business cycle (RBC) models with respect to the cyclical behavior of real wages. He then presented a model to show what sticky-wage (Keynesian) and RBC assumptions imply for the cyclical behavior of wages relative to the prices of two consumption bundles – one going through different finishing stages and one remaining at a single, unchanging stage of production. He

Rapporteurs Kwasnica, Motomura, and Sands

used data for the periods 1870-1907, 1923-1941, 1947-1990, and 1976-1990 and concluded that his results are very close to those predicted by the sticky-wage model. Most questions were directed towards the time series data that Hanes had constructed. Nye asked about the reliability of the data in the various time periods. Hanes replied that prewar prices were as good as postwar prices, the late 19th-century data were "not so hot", but he was more worried about noise in the CPI. Raff, Scherer, and Carter expressed concerns about compositional changes affecting Hanes' industrial employment series. Hanes pointed out that he was using data from manufacturing industries: this is where business cycles are most apparent and his fixed wage index took care of most of the objections. All three participated in a give-and-take discussion regarding the data's representativeness.

Sam Williamson wondered if there were regional problems underlying the data set – most people would say that labor markets were not fully integrated, so how can Hanes use one wage series? Hanes replied that both his price and wage series came from the Northeast. Jeff Williamson asked about the price series – whether Hanes should use the CPI or PPI? Hanes suggested the CPI as the right price: since the CPI was more rigid in business cycles, in downturns PPI would fall faster than the CPI, making the CPI/PPI ratio countercyclical. An oddity occurred during the 1970s when the CPI/PPI moved procyclically because of the oil price shocks. Other questions from the floor concerned the role of labor supply shocks in explaining real wage behavior. Sokoloff, citing an old argument between Paul Douglas and Albert Rees, asked about the effect of migration waves on US real wages. Hanes replied that he saw migration more as a response to US business cycles rather than as a cause. Cain seconded Sokoloff's suggestion that labor supply shocks were important, but Hanes replied that, according to his research, labor supply shocks cannot explain real wage movements. Nye suggested that Hanes' model assumed all supply shocks would hit all firms evenly, and, particularly in the case of technological shocks, this did not seem a tenable assumption. Mokyr suggested that in recessions employers might have down-graded skilled workers, *i.e.* changed their wage and/or occupational status. Hanes replied that, if wage cuts occurred simultaneously with occupational downgrading, this would present no problems; however, if wages stayed the same and occupations were downgraded, this would prove troublesome.

The Sunday morning session ended in a flurry of jovial hand-shaking and hasty preparations for departure to O'Hare. Some participants were bemused by the unusual quietude of discussion, which perhaps had resulted from imposition of (and adherence to) a strict rule of queuing for comments. Restoration of the two-handed signal for urgent intervention "on this point" might restore some of the "fireworks" common to past conferences. Absent this minor reservation, all expressed delight with the smoothly-running sequence of excellent sessions and the fine organization of the hosts – whereupon the participants from Arizona were beset by queries about the crucial elements of next year's conference in Tucson – sight-seeing opportunities and the quality of the city's facilities for dining.

Announcement

With Volume 16, the editorship of *Research In Economic History*, a research annual, will pass from the able team of Roger Ransom (Editor), Susan Carter, and Richard Sutch to a new team of Alexander Field (Editor), Gregory Clark, and William Sundstrom. Their statement of policy follows:

Editorial Policy: *Research in Economic History* welcomes original contributions in all areas of economic history including (but not limited to) empirical analyses of labor and product markets; studies of technological, legal, and institutional aspects of economic development; and articles in financial, monetary or macroeconomic history. We can publish papers which are longer than those typically appearing in the *Journal of Economic History*, *Explorations in Economic History*, or *The Economic History Review*. This makes *REH* particularly appropriate for presentation and analysis of new empirical sources.

Submissions will be read by the editors and at least one outside referee. Our policy is to give authors an initial evaluation of submissions within two months. Please send three printed copies of each submission to Alexander J. Field, Editor, *Research in Economic History*, Department of Economics, Santa Clara University, Santa Clara, CA 95053 USA.

A style sheet and further information on submissions is available on request.

Alexander J. Field, Editor
Gregory Clark, Associate Editor
William Sundstrom, Associate Editor

Abramovitz Interview *(continued from page 6)*

That's right. I explained that to you when you asked about my experience in Washington. That's where the seed was really planted. My interests did shift to long term growth. I could not get to the new subject, however, until the inventory book was completed, and that stretched until almost 1950. Then when I proposed to Arthur Burns that I begin working on issues having to do with long-term growth, I met considerable opposition. Burns had been against involving the Bureau in work on long term growth for some time past. He had been reluctant to have the Bureau support Simon Kuznets' proposals to work on long term growth and to extend his estimates of national product back into the mid-19th century. He distrusted the data. So Burns was hardly enthusiastic about my proposal. That was apart from the fact that he wanted me to continue to work on business cycles. We compromised. The compromise, which was interesting for me and which engaged me for some years, was to work on long swings. And I still think that the work on long swings is a useful way in which to study the manner in which the longer-term movements of the economy assume a cyclical shape and generate alternating periods of fast and slow growth, as well as major depressions. I worked on those subjects off and on for most of the 1950s. But I really got into the work on long-term growth itself when I came to Stanford. Bernard Haley asked me to prepare a survey article on economic growth for the *Survey of Contemporary Economics*. That was my first growth project. My second was the result of an invitation to contribute a paper to the AEA meetings of 1955 in which the economic growth of the United States since the mid-19th century was being reviewed. That was the article that was published as "Resource and Output Trends Since 1870". And that was how I got into the early work on growth accounting and encountered the big "Residual".

You continued in this same vein with some of the work you did with Paul David.

That was later, of course. We began in the middle sixties and we continued working into the early seventies. The general objective was to gain a better understanding of American postwar growth in the light of the longer term record. Our plan was to compare economic growth in the 19th and 20th centuries, emphasizing the postwar experience. The framework of the comparison was, at least initially, the growth accounting framework. We built growth accounts for the 20th century based upon the very considerable number of publications that existed by that time, particularly those of Kendrick, Denison, and later Jorgenson. Paul put together the data for the 19th century. This rested partly on his own original studies of output and inputs before 1840 and partly on the work of Gallman and his collaborators on output, investment, and capital accumulation in the later part of the century.

That led us to a great puzzle, one that I tried to talk about to the Economic History Association last September. You may remember what I said. The growth accounting results for the 19th century were on their face completely at variance with the growth accounts for the 20th century. In the 19th century capital accumulation was a far more important source than total factor productivity, whereas the reverse was true in the 20th century. Indeed, total factor productivity growth appeared to have been so slow in the 19th century that one had to ask oneself whether one can possibly believe that technological progress was as unimportant as the figures suggested. Our answer was that--no--it was really very important but it took a form that created a great demand for capital. Technological progress in the 19th century was strongly physical capital-using. It raised the rate of return to investments in physical capital much more than to labor and therefore supported a rapid accumulation of capital relative to labor. In the growth accounts, therefore, capital accumulation per worker was very large, and little was left over to represent technological progress.

This taught us something important about the limitations of growth accounting. The various sources of growth are interdependent and interactive: technological progress supports capital accumulation and, in various ways, capital accumulation supports technological progress. The supposed measures of the proximate sources of growth that appear in standard growth accounts or in regression analyses that are based upon the same growth model are, therefore, inaccurate. It is much more difficult to work out the relative contributions of the different sources of growth than the growth accounts suggest. The major "sources" are jointly responsible for growth.

Let's return to long swings. You also wrote about the monetary side of long swings; would you elaborate on what particularly interested you about the international aspects of infrastructural and other lending?

The notion I had starts from some simple considerations. Kuznets had focused on the physical manifestations of long swings: immigration, population growth, construction, aggregate output. Presumably there were also monetary requirements. When you look at the figures you find that the long swings in real output growth are matched by long swings in nominal output growth which are of greater amplitude and by long swings in the growth rate of the money supply. Those three curves run in a beautifully parallel relationship to one another. If one takes the price fluctuations as given, then it's clear that long swings in real output imply similar swings in money supply or velocity. The fluctuations in the growth rate of prices mean that nominal income fluctuates with a greater amplitude than physical output, and this demands concomitant large movements in money supply or velocity. It turns out that the velocity movements were relatively small and virtually the whole of the fluctuation in the difference between real output and nominal output takes the form of variation in the growth rate of the money supply.

Operating under a fixed exchange rate, as we did for so long, the question is how that came about. My hypothesis is that it happened because of swings in what I call the exogenous components of the balance of payments. "Exogenous" here should not be taken to mean truly independent of the whole process of adjustment and response. I use the term in the same preliminary way that Keynes does when he treats investment as "exogenous". I identified exports and capital imports as the exogenous components of the balance of payments. I argued that an increase in the sum of exports plus capital imports leads to growth of high-powered money and this to an increase in money supply and nominal income, which results partly in a rise of output and partly in prices. I treated imports, on the other hand, as a lagged response to the swings in nominal income. The hypothesis is at least consistent with experience, in that a graph of the growth rate of exports plus capital imports displays long swings essentially parallel with those of the money stock and nominal income for the century between the mid-1820s and the early 1930s.

In any event, the long swings in nominal income growth involved some kind of alternating degree of enthusiasm for lending, some changing impetus from the European side which swelled the surpluses on capital account during an expansionary phase, financing some of those huge peaks of railroad construction?

Not necessarily. I traced it primarily to the fact that the long swings in output were essentially based on long swings in railroad building and earlier in canals. I tried to explain the characteristics of railroads that produced long swings in railroad construction and how that bore on capital imports. The point is that when railroads became profitable after a period of depression, they could market their bonds. Their bonds were very attractive to the British and to other foreign investors as well. The general underlying idea was that there was a long term need for additional railway capacity. The pace at which this could be satisfied, however, depended upon the ability of railway companies to obtain finance. Railway bonds were treated by the market somewhat like equities. Their value rose and fell together with stock market prices in general and for railroads in particular.

Reflecting the probability that they would indeed be serviced.

Exactly. So that's the sort of direction the argument took. Now you realize that Simon Kuznets, who really brought long swings to the fore, had a quite different sort of theory. He had no money in his theory. Indeed he treated the long swings as divorced from ordinary business cycle considerations. It was fluctuations in productivity growth and accompanying movements in immigration, labor force growth, and in the allocation of investment that generated fluctuations in the growth rate of productivity and total output without any contribution from a fluctuation in the intensity of use of existing equipment. My own view was quite different. I regarded serious depression as the culminating (or originating) episode in each swing, and I attributed the spurt in productivity growth that was characteristic of the opening phase of long-swing expansion to a rise in the intensity of use of employed resources.

What about the long swings in immigration?

My notion about the immigration waves is that a long term condition drew people to the United States. It lay in the different levels of wages in Europe and America. But the timing of immigration depended upon the levels of unemployment. There's lots of evidence that there was very good communication between older immigrants who were already established in America and intending immigrants in Europe. The letters from here would say, "Now is not a good time, wait until next year, employment is difficult", and then later letters would say "Come ahead, we can get you a job when you get here." So when the unemployment situation improved in the course of a period of long-swing expansion, when we had come out of a period of persistent stagnation or depression, then we had a big flow of immigrants. Indeed, the waves of immigration lagged behind the growth rate of total output, just as the employment rate does. Kuznets had a different view. It turned on the rate of growth of real consumption per head, which he attributed to shifts in the allocation of investment between equipment and structures, that is in housing and railroad building.

Your story would certainly be consistent with the story that Brinley Thomas tells about the alternations in the pace of domestic capital accumulation in the UK and overseas.

It is consistent in that it takes account of the origins of waves of capital imports and their significance for the growth rate of the money supply.

Your *Economica* article was entitled "The Passing of the Kuznets Cycle"...

Yes. That had to do with a number of developments after World War I that together changed the process from which present-day intermediate-term fluctuations in growth rates arise. One was immigration restriction. Immigration declined sharply during the war but the old mechanism didn't start up again in the 1920s because of the new restrictive legislation.

The old waves of immigration that rose and fell with employment conditions gave way to a more stable rate governed by the immigration quotas. That, in turn, changed the mechanism of residential building cycles. Whereas in the past, residential construction had followed the waves in immigration and population growth, it now became more responsive to changes in the ratio of households to population, the relation of which to long swings in output growth was far more complex. Then, beginning in the Thirties, came the expansion of the Federal budget with its built-in stabilizers. In the Fifties, the dollar-exchange standard replaced the gold-based, fixed-rate régime that had ruled for so long before the Depression, and that shift altered the old balance-of-payments money-supply mechanism. And a more sophisticated Federal Reserve policy and deposit insurance did away with the recurrent monetary crises that had been such prominent concomitants of the older long swings. The moral of my article was not that long swings were a thing of the past but that their notable features and the mechanism that had produced them had changed in decisive ways.

I should like to understand how you got interested in comparative growth.

That arose from the same project in which Paul David and I were engaged with respect to the United States. When John Kennedy came into office he became disturbed by the fact that the dollar was under pressure in the foreign exchange markets and that we were losing gold. He attributed this, correctly in my view, to the fact that our growth rate, as he would have put it, or our productivity growth rate, as I would have put it, although high by our own historical standards, was now much lower than productivity growth rates in western Europe and Japan. He wanted studies made of the causes of these difficulties, and the SSRC at the behest of the Ford Foundation undertook to organize them. Denison conducted one of these projects and reported his results in *Why Growth Rates Differ.* Another was entrusted to Simon Kuznets. He planned a comparative study in which the central question would be: How can we understand the postwar experience of the advanced countries in the light of their longer term experience? It was decided that there should be studies of five European countries in addition to the United States and Japan. I happened to be in Paris when these plans were being formulated, and Kuznets asked me to find the European collaborators. I undertook to do that and, in addition, Kuznets and I agreed that when the seven national studies were completed, the two of us would prepare a synthesis. So I came to feel that I had a measure of responsibility for developing a general view of the postwar growth boom in the industrialized countries. As often happens, the national studies came along very slowly and some of them never were completed. Kuznets was drawn off to other work. I was left with a sense of lack of completion. I finally did something about it at the Tokyo meeting of the International Economic Association in 1977. The program included a section devoted to growth for which I was invited to present a paper. I was able to undertake the job because several years earlier I had finally hit on a view of what had been happening which enabled me to talk in general about the experience of the industrialized countries. I'd been casting around for a long time for some sort of hypothesis that would have a wide application, that would bring out what was common in the postwar experience of the industrialized countries and serve to explain them.

My hypothesis was that the great growth boom of the Fifties and Sixties was a boom of catch-up and convergence, the central feature of which was a transfer of technology from the leading country, the USA, to those that were behind, with an effect on their growth roughly inversely proportional to their initial productivity levels.

This would account for America's relatively slow growth and for the systematic pattern of relative advance among the "followers".

The notion of rapid growth based on technological borrowing and catch-up was not unfamiliar. But its cogency as a generalization was borne in on me only when I came to look closely at the early Denison and Maddison estimates of productivity levels and growth rates. In these, the productivity levels had been rendered comparable across countries by purchasing power parity conversion rates derived from the pioneering work of Milton Gilbert and Irving Kravis. When I lined up the countries by their productivity levels, two things stood out. One applied to the Fifties and Sixties. In this period, the variance of the productivity levels declined rapidly, and there was a close inverse association between countries' initial productivity levels and their subsequent growth rates. The second, which the longer Maddison series revealed, was that in earlier decades, the inverse association was much weaker and the rate of convergence of levels much slower. The failure of less developed countries to catch up was also in my mind.

I had to think of an hypothesis which was consistent with both observations. I argued that catch-up and convergence were a general tendency but that their operation and strength depended on the satisfaction of other conditions. As stated in 1977, these were of two sorts. One was a set of conditions that I summarized under the heading of "Social Capability". These constrained the *potential* for rapid growth that was otherwise created by the possibility of technological modernization. The other sort had to do with what I called the "factors supporting the realization of potential". The macroeconomic conditions of a period are one such factor, but there are others. The catch-up and convergence boom of the post-war period arose from a favorable conjuncture of strong potential, adequate social capability, and macroeconomic and other conditions supporting rapid realization. With some refinements and supplements, these are the central ideas that have concerned me ever since.

A final question on the future and current state of economic history.

Alex, that's a deep question. I'm afraid I don't have any clear thoughts. My notion is that we now have built into economic history–indeed into economics in general, but economic history in particular–a respect both for work that is close to formal theory and work that is distant from formal theory but still informed by it. It's surely true that both kinds of work are useful provided they're pursued seriously. I do think both are being pursued seriously and that economic history, therefore, is in a flourishing condition. I think it's going to continue to be valuable, particularly because economists at large have become more interested in questions of economic development and in international comparisons. For both purposes historical work is essential. I think there has already been a revival of interest in economic history within economics departments and I look forward to that continuing.

Works by Moses Abramovitz discussed in the interview:

"Price theory for a changing economy." Ph.D. dissertation, Columbia University, 1939.

Inventories and business cycles, with special reference to manufacturers' inventories. New York: National Bureau of Economic Research, 1950.

"Reinterpreting economic growth: Parables and realities" (with Paul A. David), *American Economic Review; Papers and Proceedings* 63:2 (May 1973), 428-40.

Thinking about growth and other essays on economic growth and welfare. Cambridge: Cambridge University Press, 1989. Includes the following papers:
- Chapter 2: "Economics of growth," reprinted from B.F. Haley (ed.), *A Survey of contemporary economics,* Irwin, 1952.
- Chapter 3: "Resource and output trends in the United States since 1870," reprinted from *American Economic Review* (May 1956).
- Chapter 9: "The passing of the Kuznets cycle," reprinted from *Economica*, new series 35:140 (November 1968).
- Chapter 6: "Rapid growth potential and its realization: The experience of the capitalist economics in the postwar period," reprinted from E. Malinvaud (ed.), *Economic growth and resources*, vol. 1, *The major issues*, Proceedings of the Fifth World Congress of the IEA, Tokyo; London and New York, 1979.
- Chapter 7: "Catching up, forging ahead, and falling behind," reprinted from *The Journal of Economic History* (June 1986).

"The search for the sources of growth: Areas of ignorance, old and new," *Journal of Economic History* 53:2 (June 1993), 217-43. (Presidential Address, Economic History Association, September 1992)

Census Conference *(continued from page 10)*
poor, young or itinerant significantly under-recorded, particularly in the South. Previous analyses of the censuses of this period have concluded that under-reporting varied between 10 and 20%. Reid linked pension records for black Union soldiers, recorded in 1890, with the 1870 census manuscripts. He encountered many problems making the links: names differed for reasons of literacy or spelling, or because blacks deliberately changed their names after emancipation. Many had had no family name while slaves, and some had used one or more aliases when enlisting in the Army. A final usable sample consisted of 575 men, of whom 55% could be traced to the census, with another 10% probably linked, suggesting that the under-count of blacks in the 1870 census may have been as high as 35 percent.

Discussion of the papers centred on the extent to which record linkage could be automated through database management software. The use of such programs is primarily to remove the bulk of straightforward cases, leaving to individual consideration the (hopefully small number of) more difficult instances. Reid noted that there were two different sources of under-counting, one a general tendency to miss about 10% of people and the other to overlook such specific groups as the poor or itinerant – perhaps another 10 percent.

At dinner on Friday Robert Swierenga (Kent State) regaled the group with a history of the use of census archival material. Canadians, with so few census manuscripts surviving, felt envious of those working with the voluminous material available for the US.

The Saturday morning session on "Wealth" was chaired by Phyllis Wagg (Dalhousie) and began with a paper by Paul Lachance (Ottawa) on wealth holdings of New Orleans residents in 1860. Using tax records and the population and wealth characteristics recorded in the census, Lachance calculated the value of property under-reported by the census, and the difference these adjustments make for estimates of the level and distribution of wealth in the city. Participants welcomed Lachance's creative approach, but the demographic comparability of the two data sets remained open to question. A further complication was whether or not owners and occupiers of property could be distinguished. Lachance answered in the affirmative.

The paper presented by Gordon Darroch (York), written with Lee Soltow (Ohio U.), covered wealth accumulation in Canada to 1871, based on a 'film-manuscript sample' of over 5,000 adult males and of female heads of household in Ontario. In the past century home ownership has been the primary source of wealth accumulation, and in 1871 nearly half the men in the province owned their homes, with ownership strongly related to age. Religious divisions were not found to be important, but Francophone Catholics did not fare as well as others. As usual, questions centred on sample reliability, choice of year, and whether a combination of time-series and cross-sectional analyses would be appropriate.

In the final paper of the morning's session, Livio Di Matteo (Lakehead) reported his study of wealth in the Niagara region of Ontario in 1892 using data constructed from 344 probate records linked to the census. He carefully examined the strengths and weaknesses of the data; using independent sources to provide cross checks, he concluded his data were of high quality. There was a definite relationship between urbanisation and wealth holding, but analysis of the motives for holding wealth proved more problematic. His attempt to distinguish between life-cycle and bequest motives using a cross-sectional study raised a number of questions from the audience. It was also suggested that since death was unexpected it was difficult to model.

After lunch Eugene Sigel (Massachusetts-Amherst) chaired a session on censuses of agricultural communities. Sebastián Coll Martín (Cantabria, Spain) was mainly concerned with the reliability of agricultural production statistics in early 19th-century Spanish censuses. Using independent estimates to estimate total factor productivity, Coll found that the censuses significantly under-reported productivity, especially of land. Participants questioned, however, whether some procedures underlying his estimates – for example, back-casting from data for the 1920s – were appropriate.

Jon Moen (Mississippi) examined the composition of the rural southern US labour force using a new sample drawn from the 1860 census manuscript schedules. He contrasted his own sampling technique linking agricultural and population schedules with those of Parker and Gallman, Bateman and Foust, and Ransom and Sutch. For his sample, he found that, in accord with Fogel and Engerman, slaves were not commonly supervised by white overseers. These findings, and others, sparked a discussion about whether it was possible to distinguish between overseers and farm managers from census data, and whether there was as much mobility and wealth accumulation on the part of overseers as Moen believed.

Robert McKenzie (Washington), in the final paper of the session, asked whether post-bellum change can be described accurately using the US agricultural schedules for 1860 and 1880. He argued that the schedules are not appropriate for studying structural change (for example, in patterns of tenancy) because they omit a 'farmless farm population', that is, individuals who reported a farm occupation on the population census but who were absent from the corresponding agricultural schedule. Commentators questioned the accuracy of the schedules, and the reliability of independent estimates of the farmless.

Michael Huberman (Trent) was chair of the last session on Saturday, on industry-specific linkage to the British census. Valerie Burton (Memorial) examined seafaring communities in and around British ports during the 19th century. The censuses missed the large component of these communities who were at sea on the date of the census. Further, absence of seamen makes it difficult to analyse the true structure of the family groups found in the ports, since many 'households' recorded on enumeration day were temporary arrangements. By the 1890s a quinquennial census of seafarers was made based on crew agreements, revealing that the decennial census omitted two-thirds to three-quarters of foreign-going seamen. Burton used seafarer's work contracts for two ports for 1851, contrasting the contracts' longitudinal implications with inferences from the cross-sectional decennial census. Comments centred on the issues of trying to reconstruct family relationships when large proportions were absent. The use of marriage registers might help, although it is likely that not all relationships were formalised. The evolution and transmission of work skills in such communities was also discussed.

John Treble (Wales, Bangor) gave the final paper on Saturday, entitled "A Victorian Household Panel Study". Treble, a labour economist, has been investigating the relationship between effort and earnings – a difficult area for empirical research because of paucity of data. Treble has found detailed data on miners' income from one coal mine in County Durham for the 1890s, where earnings were entirely output determined. Workers were regularly reassigned in pairs at random within the mine ('cavilling'), thereby controlling for the effects of quality variation in the coal seam and of working conditions, and were subject to a 'County Averaging Scheme' which reduced earnings fluctuations. Some output variation was due to the geological conditions of individual seams, which Treble can estimate from maps of the workings. The manuscript census yields information on miners' personal and household characteristics, which can then be linked to the usable data on earnings. Estimation of the effort-response function has not yet been carried out. Among the comments was an observation that this was a period of disruption, with very high labour turnover and working-to-rule by the miners.

The Sunday morning session on human capital was chaired by Tony Ward. In the first paper David Galenson (Chicago) compared and analysed rates of school attendance of the children of immigrant and native-born Americans in Chicago. Drawing on a sample from the census of 1860, Galenson found that the direct effects of nativity on school attendance were inconsistent and unimportant. Immigrants were not alienated from the mainstream, nor did they place a lower value on formal education. The audience wanted to know whether the results held for both boys and girls and about the relation between religion and school attendance, as opposed to nativity. A more general problem surfaced in the discussion: the underlying model may not be appropriate if the choice between school and the labour market had not been dichotomous.

William Phillips (South Carolina) presented some preliminary findings on invention in the South, based on data for 25,000 patents issued from 1831 to 1890. In his presentation Phillips restricted his remarks to his attempt to link censuses with patent records for South Carolina. He found that most patent holders were concentrated in Charleston, many were foreign born, and most had a well-specified occupation. Participants compared these results with findings by Khan and Sokoloff, and asked whether Phillips' inventors could be accurately described as 'tinkerers'.

In the final session on occupational mobility, also chaired by Ward, Joseph Ferrie (Northwestern) presented his findings on 2,000 male immigrants to the USA whose appearance in ship lists had been linked to the census of 1850 or 1860 or both. He examined the influence on occupational mobility of duration in the US, place of origin, and previous occupation by estimating hazard functions. Results show that the most successful immigrants were literate and had settled in places with rapidly-growing populations. Questions were raised about the effects on settlement decisions of information flows among migrants, and how well immigrants fared in areas where other migrants were not already present.

John Lyons (Miami) was also concerned with geographic and occupational mobility in his study of the growth of the factory sector and the decline of handloom weaving in early 19th-century Lancashire. His data consist of a linked sample of about 1,500 migrants and non-migrants from the 1841 and 1851 censuses. Contrary to the conventional wisdom, which holds that labour markets were fluid in the period, Lyons found small changes in the income structure of employment and little mobility across the middle class–working class divide. Participants were concerned about sample size for some subgroups, but in general did not find the results surprising. Lyons was encouraged to situate his findings on mobility internal to the factory sector in the recent literature on labour market development in Lancashire.

In closing, Ward expressed appreciation to Reid and Inwood for organising a stimulating and successful conference, a sentiment echoed by those remaining to the end. Whilst the full measure of its success will be revealed in its influence on research and teaching by participants and their colleagues, the conference provided extensive opportunity for cross-disciplinary communication. Despite a wide variety in subject matter, approach, technique, and disciplinary perspective in the participants' papers, the conference revealed both underlying commonalities in research issues and the empirical problems encountered, as well as the ingenuity with which one can extract information from conjoined or even single data sources.

Second 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 August 15, 1993. Please send abstracts to:

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All UC Report *(continued from page 14)*

the high degree of mineral extraction spurred on by an accommodating legal environment and investment in public knowledge and mining education. Because of its heavy commitment to mining and exploration, the US was quite sensitive to global shocks. Jean-Laurent Rosenthal suggested employing a comparative perspective: first, an impetus for exploration and exploitation may have been common to European as well as US industrial growth; second, regional variation of industrial development in the US may account for varying degrees of regional resource exploration and not the reverse. Joel Mokyr (Northwestern) pointed to improvement in regional transport facilities, and Mary Yeager contrasted technical change in US mining with the Brazilian case. Peter Lindert observed that other countries had legislative environments similar to the US and, despite different regional or institutional circumstances, may have had similar patterns of mineral search.

Deidre Busacca (UC Davis) presented the next paper, on "Economic Development and Underdevelopment of the Colorado Plateau: The Impact of Uranium", providing an overview of uranium mining during the 1950s and incorporating a slide show of her various trips to the region. The mining of uranium and its use in the burgeoning nuclear arms industry of the US gave the Colorado high country an economic boost in all facets of life: infrastructure building by mining corporations, entry of women into the labor force, growth of spin-off industries, and expansion of tourism. In the 1970s, long after the mineral boom had subsided, most mining sites were razed and the land restored by the Department of Energy for environmental and health reasons. This work continues, and a piece of history from the uranium boom is being lost. A discussant argued that the case for saving some mine sites, and thus a physical record of regional history, was a normative one. Further questions were raised about the balance between benefits of saving mines, many in remote locations, as historical sites, and the costs of razing and cleaning the sites for public health purposes. Richard Sutch doubted the uranium boom was efficient; the free market may have "run amok" in an industry with only one customer, the Federal Government. Finally, Wright urged a broader historical perspective on development and use of exploration technologies.

Joel Mokyr presented his work on the political economy of technological progress in the context of "Cardwell's Law", which states that "no nation has been (technologically) creative for more than an historically short period." Mokyr asks whether this Law can be defeated through national interactions or institutional diversity. Since technological creativity exists in all societies, it is institutional frameworks which constrain people and enforce Cardwell's Law. A game-theoretical model suggests that interaction between two separate economies can defeat Cardwell's Law; competitive forces and gains from trade might perhaps prevent loss of technological leadership. Institutional diversity may also defeat the Law since institutional integration tends to temper the autonomy of a given economy. His results do not depend on political structure, suggesting that democratic regimes may not be necessary for defeating Cardwell's Law. Zorina Khan (Northeastern) pointed out the many difficulties in distinguishing among the various factors affecting technological creativity. Declining leadership may be due to lack of standardization, externalities of competing technologies, or other types of market failure, and the Law may hold while an incomplete model may indicate its defeat. Likewise, technological innovation may be concentrated in the service sector, where measurement problems are manifold, and we might be unaware that Cardwell's Law has been overcome.

The final paper of the conference "The Future Through Yesterday: Long-Term Forecasting from the Perspective of the 19th Century", was presented by Stephen DeCanio (UC Santa Barbara), who attempts to determine what economic history and economic forecasting might be able to say about long-term issues such as the greenhouse effect by testing the long-range predictions of such 19th-century futurists as H. G. Wells and Jules Verne. Both authors, although they predicted the power and prevalence of new technologies in future societies, did not get the "look and feel" of actual technological change. Wells did not envision the fragmentation and the pace of technological progress, nor did he mention damage to the environment deriving from some forms of "progress". Verne foresaw many types of social and political conflict which have arisen since he wrote: imperialistic oppression and conflict with indigenous peoples, as well as the continuing conflict between political and personal morality which many governing bodies face. It is important for historians to look at literature historically, because it is a source for the thoughts and beliefs of people within a given epoch. In closing, DeCanio observed that economics is too imprecise for long range forecasting and wondered whether the place of economics should be in the social sciences or the humanities. Commentators objected that much science-fictional prediction has not come true, and that looking at only a few writers whose forecasts have been realized in some form will not capture the true accuracy of the genre. However, others argued that the technological predictions were less important than the social predictions of science fiction. A final objection raised in the discussion was to the paper's failure to analyze the predictions of economic writers of the 19th century; Marx, for example, had made many predictions based on a keen insight into human relations and their development.

For Sale

A complete run of the *Economic History Review* from 1927 to 1972. Old Series (1927-1948) is in fair condition. New Series is in good condition.

A complete run of the *Economic Journal's* supplement, *Economic History*, from 1926 to 1940, in fair condition.

Explorations in Entrepreneurial History, Volumes 4, 5, 8, 9, and 10.

I will accept the best offers I receive as of March 31, 1994.

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Call for Papers and Participation

California and the Pacific Rim: Past, Present, and Future

A special conference sponsored by the John Muir Center for Regional Studies
University of the Pacific
47th Annual California History Institute
April 29 - May 1, 1994

This conference is designed to encourage a better understanding of the nature and significance of the Pacific Rim's international economy and its relationship with California, past, present, and future. Goals are to develop a multidisciplinary overview of the Pacific Rim and the California connection that will better prepare policy-makers for decisions vital to California and the nation, and to balance the general perspective on American relationships abroad.

Two days of academic sessions, followed by a regional field trip, are open to presenters and participants from all relevant disciplines, including agriculture, anthropology, communication, ecology, economics, ethnic studies, geography, geology, history, linguistics, oceanography, and other fields. Academics, business leaders, students and the general public are welcome to participate as presenters or as registrants.

Submit proposals for papers and sessions on any aspect of the theme, along with a brief resume, to:

CHI 94 Program Committee
c/o Chair Professor Dennis O. Flynn
Economics Department
University of the Pacific
Stockton, CA 95211
Phone: 209-946-2258 Fax: 209-946-2596
Deadline: November 15, 1993

For general conference information and registration details, send your name and address to:
The John Muir Center for Regional Studies, University of the Pacific, Stockton, CA 95211

1993 Cliometrics Conference Abstracts

Excess Female Mortality During Early German Development: An Analysis of Village Genealogies

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In 1990, 90 million women are "missing" in some Asian and African countries as a result of cumulative discrimination in the distribution of resources within households of these countries. This excess female mortality among girls and adult women is concentrated in rural areas and is high in places where agricultural change has been particularly rapid.

From the late 18th to the mid-19th centuries, Europe also underwent rapid agricultural change accompanied by a period of excess female mortality among children and adult women. Although the problem in Europe was much less severe, the same sources of today's deficit of women may have been active two centuries ago.

To test this hypothesis and to understand more fully the historical episode of excess female mortality in Europe, vital statistics from 80 villages in Germany from the 17th through 19th centuries are considered and analyzed. Life table analysis shows that excess female mortality emerged in the late 18th century and rose until the mid-19th century, declining thereafter. The paper presents cross-section, time series, and panel data regressions to trace economic and social correlates of this occurrence. While there is considerable regional and temporal variation, the problem appears to be worst for children of agriculturalists, for married women and for areas most dependent on agriculture.

These findings then are linked to the literature about agricultural change in Europe as well as to current research on household economics and women's deprivation in developing countries.

Providing for Old Age: Land, Retirement, and Fertility Among American Farmers

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I examine retirement and provision for old age among farmers using a sample of Union Army recruits. Unlike previous researchers, I find that farmers were more, not less, likely to retire than non-farmers. Farmers were able to retire because land ownership provided them with a secure retirement asset. My findings bear on explanations of the secular decline in fertility. I show that children and land were substitutes. Therefore, as land markets grew and land became a liquid and divisible asset, the value of children relative to land as old-age security assets declined.

"Rotten Kids" or Manipulative Parents? New Evidence on Household Decision-Making in Turn-of-the-Century America

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This paper is a preliminary exploration of intra-household resource allocation in turn-of-the-century America. Were parents altruistic or did their perceptions of children's future bargaining power influence their treatment of their young teenagers? Our analysis of survey evidence collected by the New Jersey Bureau of Labor in 1903 is inconclusive. Youths' predicted wage and apprenticeship status meant improved resources within the home. On the other hand, parents appear to have made educational loans to their children and to have favored daughters over sons. More work on this important topic seems indicated.

Trade, Institutions, and Law: The Experience of Mexican California

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In Mexican California between the mid-1830s and American annexation in 1846, trade based on credit flourished in the absence of law. The paper investigates historically and theoretically the institutions that permitted exchange and the impact of changes in the environment on these institutions. Historically, it illuminates the role of trade and the institutions that supported trade in California's transition from a preindustrial economy to a market-based economy. Use of a game-theoretic model enables detailed analysis of these institutions and facilitates comparison with institutions in other economies.

Trade on the California coast was organized as follows: wholesalers sold goods to merchants, who then sold these goods to their retail customers or to other merchants. The high transactions costs attendant to organizing barter created an opportunity for efficiency gains by selling goods on credit. Commercial uncertainty, particularly supply and demand risk in each town, and the high opportunity costs to travel for the merchants resulted in gains to buying and selling among merchants and transacting business for one another. However, a commitment problem characterized all of these transactions, because individuals handled goods that they did not own and could act opportunistically. Hence, efficiency gains at each level of trade could not be captured without law or another supporting institution.

The hypothesis advanced in the paper is that three interlinked types of institutions, wholesaler-merchant institutions, the merchant coalition and the local coalitions, governed agency relations on the California coast. Coalitions organized agency relations among a small number of individuals,

merchants or residents of a town, who were linked by an information network. Within each coalition, the information network allowed the hiring of individuals based on their past behavior, which mitigated the commitment problem. Barriers to entry and exit sustained these networks.
The approach of the paper is to construct a non-cooperative repeated game that explicitly models the agent's commitment problem. Evidence from merchants' correspondence on punishment and strategies followed allows the realized equilibrium to be identified from among the set of possible equilibria. The model is then used to explain facts from the historical record unrelated to players' strategies such as patterns of social interaction, use of the legal system, the development of intermediation, and institutional response to exogenous change such as the Gold Rush.
Two insights emerge from this research regarding coalitions: coalitions are not resilient to change and they arise in response to inadequacies in the legal system. Coalitions require stability in order to operate; hence, they must effectively limit entry into and exit from the coalition and operate without abuse from the government. Changes that provide more profitable opportunities for members outside the coalition may also lead to the breakup of coalitions. Coalitions overcome inadequacies in the legal system such as lack of enforcement of legal norms, inefficient legal norms, or inability of the legal system to verify events. Thus, the likely response of coalitions to changes in the legal system depends on whether the changes can credibly remedy the fundamental weakness in the legal system that the coalition addresses. These insights highlight the limitation of coalitions as substitutes for a legal system and the importance of an effective and impartial legal system in the functioning of capitalist societies.

Constraints and Inequality: Evidence from Rural Punjab, *c.* 1920-40

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Does the commercialization of agriculture lead to a process of differentiation within the peasantry? Do all sections reap the gains from trade equally? Typically, historians have addressed these questions by looking at time-series evidence: trends in wages, inequality in land ownership, etc. I use a household-level data set to address these age-old questions, in the context of the Indian province of Punjab, in a different way, by focusing on imperfections in the capital market.
It is often claimed in the literature on Indian economic history that the commercialization of the agrarian economy during colonial rule led to an increase in inequality. Implicit in the work of many writers is the view that factor market imperfections, especially in the capital market, worked against the poor peasant. There is, however, little econometric work seeking to understand how capital markets worked and whether their failure contributed to increasing inequality. This paper takes a step in this direction. I use a household-level data set which provides detailed information on farm-level resource use. My strategy in the paper is to see if patterns of resource-use reflect capital market imperfections. In particular, I explore the question of whether the failure to access capital reduces the profits from farming for poorer farmers.
I find that the anecdotal evidence is consistent with the predictions of theories of credit rationing. The fact that loans were given against collateral suggests that poorer households, which would not have had assets to provide as collateral, may have been rationed. The econometrics in the paper focus on resource-use on the farm. I argue that if poorer households are rationed in the capital market we should observe certain patterns in the data. First, production expenditures should be positively correlated with the wealth of the household. Wealthier households should cultivate on a larger scale, because they have greater access to capital. Second, for households which do not hire in labor and are rationed in the capital market, as their wealth increases the shadow price of capital decreases, thus lowering the opportunity cost of purchased inputs, leading to a decline in labor intensity of cultivation. Finally, I argue that, because of their greater access to capital, wealthier households should earn higher economic profits. All of these predictions find support in the data. Despite some concerns regarding measurement issues, I conclude that, in sum, the evidence supports the view that the earning capacity of poor peasants in colonial Punjab was undermined by imperfections in the capital market.

Incentives, Credit and Payment Patterns: An Investigation of Rural Labor Contracts from China in the 1930s

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Drawing on a detailed household level data set covering over 1,000 households in 21 geographically and economically independent villages, in this paper we investigate the determinants of compensation profiles among rural labor contracts from China in the 1930s. Each contract in our sample (583) began and ended in 1935 and had a duration of one to 12 months. Altogether, nearly 90% of the labor hired in these villages was covered by these longer-term (as opposed to daily spot market) contracts.
We are primarily interested in the timing of the payments made to the worker over the course of these contracts. In 162/583 of the contracts, a single payment was made to the worker at the beginning of the contract; in 79/583 cases, a single payment was made at the end; and in the remainder of the contracts, several payments, usually of unequal amounts, were paid over the duration of the contract.
A number of alternative models have been proposed to explain multi-period contractual payment patterns. However, for the time horizons and technologically primitive rural setting we consider, neither the human capital, insurance, nor self-selection explanations seem to be especially persuasive. Moreover, none of these models can account for negatively sloped wage profiles, *i.e.* payments loaded at the front of the contract. This leaves job-related incentive problems.
When employers do not engage in malfeasant behavior, an upward-sloping wage profile that postpones payments until the end of the contract will discourage shirking by workers if monitoring of their behavior removes employers' commitment to not terminate them, *i.e.* a worker who shirks and is caught and fired will lose the wage premium at the end of the contract. Likewise, when workers do not shirk, a downward-sloping wage profile will discourage employers from engaging in malfeasant behavior, *e.g.* arbitrarily changing working conditions, to the extent that such behavior removes workers' commitment to not quit.
While job-related incentive problems can rationalize the range of contractual wage profiles we observe, the following alternative explanation needs to be considered: In the absence of malfeasant behavior, if one party to a contract is constrained in the credit market, the wage profile can be designed both to pay for labor services and to provide a consumption or production loan to the constrained agent. That is, a downward sloping wage profile becomes a device for providing a short-term loan from an employer to a worker, and an upward-sloping profile is, correspondingly, a means of providing a loan from a worker to an employer.
Labor market transactions are viewed here to be tied sales in which workers and employers exchange labor services and contractual payment patterns. Holding labor quality and contract length fixed, freely mobile workers and employers from heterogeneous households choose to participate on opposite sides of a continuum of markets for different wage-profile slopes; these choices are determined by preferences concerning contractual wage profiles based on either household credit problems or anticipated incentive problems.
In this paper, job-related incentive and imperfect credit market models are developed which provide distinct testable implications. To test these propositions, we estimate a full structural hedonic model that consists of: (i) compensating wage difference function that expresses the wage as a

function of worker attributes, *i.e.* skill, age, and literacy, implicit credit, duration, tenure of relationship, and village factor endowment (25 variables in all); (ii) demand and supply equations for implicit credit which model demand and supply as functions of employer/employee's attributes, *i.e.* assets, family size, net indebtedness, and key reputation variables. Estimating these kinds of models poses a host of difficult identification issues that are carefully discussed in the paper.

The empirical work provides strong support for the role of credit market constraints in shaping wage payment profiles among Chinese rural labor contracts in the 1930s. The econometric work also confirms that incentive effects of the payment patterns are important; it suggests that shirking by workers is the dominant concern, but does not rule out conclusively malfeasant behavior by employers. To place the credit exchanged through labor contracts in context, the total value of implicit credit exceeds the lending in the informal credit market by all households in these same 21 villages. Having thus identified credit exchange through labor contracts as a motive for individual contract choices and quantitatively important in the aggregate, we now intend to use our cross-sectional sample of 21 independent villages to provide an explanation for why some villages rely more on one form of contract than the other.

Through this analysis, a much better picture emerges of the workings of China's rural labor markets, and its links with credit markets. Combined with our ongoing work on credit constraints and renegotiation in land rental contracts (nearly a quarter of all fixed rental contracts are negotiated downwards), this paper offers new insights into the economic organization of rural China and basic questions of growth and distribution.

The Political Economy of the Sherman Antitrust Act: New Empirical Evidence

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The paper reconsiders three questions associated with the passage of the Sherman Antitrust Act. First, the lobbying efforts of agrarian protest groups for antitrust legislation are reconciled with a rational choice paradigm: farm groups overcame the free-rider problem by offering an excludable good; and if they extrapolated their experience with trusts directly related with agriculture to other industries, it was not unreasonable for them to believe that trusts tended to lead to higher prices. Second, the paper presents qualitative evidence–*e.g.* Senator Sherman's personal correspondence with small oil refiners–that suggests that both trusts and their smaller competitors had representation in the Senate. Third, four different probit models are used to analyze key votes in the Sherman Act's legislative history. For example, the Senate's vote to send Sherman's original bill to the Judiciary Committee is considered; the Judiciary Committee re-drafted Sherman's bill so that it was much less stringent. The analysis of this vote, and others, shows that trusts and small business were more effective in the Senate than in the House while the opposite may be true for agriculture. The results also show that in the Senate, smaller sugar refiners, along with the cotton-oil trust, the meat-packing trust and the Standard Oil trust, were especially influential.

Rewriting History: The Early Sherman Act Monopolization Cases

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In a precedent-setting 1911 antitrust decision, the Standard Oil Company of New Jersey was divested into 34 pieces. In a 1920 decision that set further precedents, the Supreme Court concluded that the United States Steel Corporation was not guilty of monopolization. In this paper we ask the counter-factual questions, what if Standard Oil had not been fragmented? And what if U.S. Steel had been broken up, as sought by the government? We trace the qualitative evolution of the Standard fragments following the 1911 divestiture decision and the corresponding history of U. S. Steel. We then undertake quantitative analyses of the survivors' market share trends and the comparative productivity growth, average plant sizes, price-cost margins, and net exports of the petroleum refining, blast furnace, and steel industries from 1899 through 1939. At first, the post-divestiture performance of the Standard fragments did not change much relative to pre-divestiture patterns and the parallel behavior of iron and steel producers. Exports were the main exception. They declined sharply in petroleum, although it is difficult to disentangle the causal influence of lessened monopoly power, and hence less incentive for dumping, as compared to the rapid growth of oil supplies abroad. Decentralization may also have permitted more rapid technological innovation. The evidence on the adoption of thermal cracking is particularly compelling. Competitive contact among the Standard survivors grew only slowly. However, by the 1930s, the Standard refining company fragments had interpenetrated each others' markets extensively and competition intensified. The steep decline in Standard survivors' market shares, paralleling that of U.S. Steel in earlier decades, came to a halt. The end result appears to have been numerous petroleum refining companies "fit" to compete vigorously in world markets. In the steel industry, on the other hand, competition continued to be mild, sustaining traditions established at the 1907-11 Gary dinners. A bureaucratic, lethargic U.S. Steel held a pricing umbrella over its rivals and steadily lost market share. Steel companies were ill-prepared in terms of technology, wages, and prices for rising import competition during the 1960s and 1970s and repeatedly sought government protection until massive changes were compelled by recession and new competition in the 1980s. We conclude that the 1911 breakup of Standard Oil had few deleterious short-run consequences and, by shaping a more competitive environment, a decidedly positive long-run effect. Although it is, as always, difficult to be certain, well-conceived divestiture of United States Steel in 1920 could plausibly have led to an industry more competitive internationally during the period following World War II.

What Limits Social Spending?

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The net national deadweight costs of the higher-budget welfare states are low enough to be drowned out in the noise of the data. The underlying reason is that policy makers have long known that a high-budget program of social redistribution, which creates natural enemies, could survive politically only if its deadweight costs are kept low. These costs do not differ greatly among nations.

If costs do not explain why some nations kept their social programs to a minimum while others expanded theirs beyond precedent, the explanation must lie in differences in nations' political will to move up the same program-cost curve faced by all industrial nations. The forces behind the will to expand social programs (and the taxes that pay for them) have changed over the last hundred years. Before World War II the dawn of modern social spending programs was shaped in part by the extension of political voice to women and to lower-income groups, and by religious differences. Democracy and religion no longer explain international differences in the social spending of industrial countries, however.

More durable are the influences of the income level, the age distribution, and social affinity. The continued rise of incomes continues to exert gentle upward pressure on the share of GDP spent by government on social programs. Changes in the age distribution have now begun to check the rise

of pension (and overall social) spending, except in seven countries whose "gray power" had still not peaked as of 1981. These and other determinants of social spending are estimated for 19 countries, 1960-1981.

Estimates of the Value of Patent Rights in Great Britain and Ireland, 1852-1876

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Parameters of the distribution of the value of patent rights in Britain and Ireland for the 1852 to 1876 period are estimated, and compared with similar estimates by Schankerman and Pakes (*Economic Journal*, 1986) for the post-1950 period. Market variables of the earlier period have a low ability, but of the later period a high ability, to explain fluctuations in the value of patent rights. Aggregate patent rights have more lower-valued patents in the earlier period. In the earlier period there was not, but in the later period there was, a trade-off between quantity and "quality" of patents. Aggregate value of patent rights increased relative to gross fixed capital formation, from 1870 to 1970, by approximately 100%. A greater amount of R&D effort helps to explain many of the differences found between the two periods.

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

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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 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. In order 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 to 1987. As U.S. regions integrated economically between 1860 and 1890, regional manufacturing structure became more specialized at both the two- and three-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 continously 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, the 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 early 20th centuries, their significance has diminished considerably over time. The evidence suggests that there are limits to such economies and that basing long-run policies on their realization may prove inadvisable.

Changes in the Cyclical Behavior of Real Wage Rates, 1870-1990

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In Keynesian sticky-wage models, firms choose output and employment levels subject to nominal labor costs that underadjust to aggregate shocks. With technology and the capital stock more or less fixed over business-cycle durations, the marginal product of labor should diminish with increases in employment and the marginal cost of output should rise relative to the marginal cost of labor. Thus John Maynard Keynes at first supposed that his business-cycle theory implied countercyclical real wages. But Keynes and others soon noted that this follows only if product markets are perfectly competitive. Otherwise prices exceed marginal cost. If the difference is smaller in booms than in recessions, that is, if markups are generally countercyclical, a sticky-wage model can generate acyclical or even procyclical real wages. Nonetheless, the failure of most empirical studies to show a clear countercyclical pattern is cited as evidence against sticky-wage models, often in favor of "real business cycle" or RBC models in which both product and labor markets are assumed to be perfectly competitive and clearing. Perhaps the old-time Keynesian religion is emotionally unsatisfying because it appears to sidestep falsifiable implications for real wage behavior. When output levels rise, the behavior of a particular good's price relative to wage rates would depend on whether the increase in marginal cost in the good's various stages of production overwhelms the decrease in markups in its stages of sale. Wages are more likely to be procyclical relative to the price of a more-finished good because countercyclical markups make up a large proportion of its final price. The cyclical behavior of wage rates relative to the price of any *bundle* of goods, including the consumption bundle, would depend on the characteristics of the particular goods in the bundle.

RBC models make a much more definite prediction about real consumption wages, since they assume that all variations in employment and output reflect shifts in aggregate labor supply or demand. Anything that would cause households to increase labor supply, *i.e.* consume less leisure at a given real wage, would cause them to reduce consumption of goods and services as well. Since consumption is in fact strongly procyclical, labor-supply shocks must be overwhelmingly dominated by shifts in labor demand. Real consumption wages must be procyclical.

I point out a testable implication of sticky-wage, countercyclical-markup models for *changes over history* in the cyclical behavior of real wages: real consumption wages should have become more procyclical (or less countercyclical) as the goods in the consumption bundle have become more finished. At the same time, wages need not have become more procyclical relative to the price of an *unchanging* bundle of goods.

Historical records allow comparison between the interwar (1923-1941) and postwar (1947-1990) periods and between the late 19th century (1870-1880, 1890-1907) and 1976-1990. Results are just as predicted by the Keynesian story: real consumption wages are more procyclical in each of the later periods. Though real consumption wages are procyclical today, they were countercyclical in the late 19th century. But wages do not appear to have become more procyclical relative to the price of a fixed bundle. These patterns are hard to account for under RBC assumptions.