The Newsletter of
The Cliometric Society
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Third World Congress of Cliometrics

by Douglas Puffert, University of Munich, and
David Greasley, University of Edinburgh

(Munich) One hundred cliometricians from around the world gathered July 10th through 13th to sample the local beer,[1] to hear invited lectures from Jeffrey Williamson (Harvard) and Nicholas F. R. Crafts (LSE), and to take part in 44 characteristically spirited discussions of papers. Papers were presented two at a time in 22 sessions, with sessions running in parallel pairs during each time slot. Because abstracts of these papers are available in the EH.Net web server **(http://www.eh.net/AEH)** and space is limited, your reporters will focus on highlights of the general discussion, lectures, and banquet.

The Sessions

Steve Broadberry (Warwick) opened the first session by considering how the United States and Germany overtook Britain's GDP per employee in the period since 1870, stressing the impact of shifting resources from agriculture and differentials in service sector productivity. Joel Mokyr (Northwestern) asked whether Victorian Britain had "failed", Erik Craft (Richmond) asked whether non-marketed agricultural output affects the results and Price Fishback (Arizona) and Leandro Prados de la Escosura (Carlos III°, Madrid) questioned the choice of benchmarks. Giovanni Federico (Pisa) wondered why the mining figures appear so volatile. Martin Shanahan (South Australia) and Margaret Correll (South Australia) presented the following paper, which re-examines wealth distribution in the United States between 1650 and 1950, concluding that the case for the Kuznets curve remains unproven. Robert Fogel (Chicago) raised the issue of comparability with Soltow's research on housing inequality, and Richard Steckel (Ohio State) asked whether regional differences in wealth inequality influenced the findings. Joerg Baten (Munich) questioned the importance of sample size in evaluating the direction of bias, and Michael Haines (Colgate) mentioned the distributional consequences of land value appreciation.

Session 2 dealt with price setting behavior in two quite different contexts. John Howard Brown (Georgia Southern) presented his paper on whether Standard Oil practiced price discrimination or rational contracting in sales of railroad lubricants, with discussion focusing on identification and specification of his estimated equation. The paper by Bishnupriya Gupta (St. Andrews), on the international tea cartel of the early 1930s, was discussed largely in terms of the relative age of tea plantations and how local industry concentration affected the ability of cartel members to cooperate effectively.

[1]Munich beer was discussed in the October 1996 issue of the *Newsletter*.

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What's Inside

was a general paper in economic history and I attended [J. H.] Clapham's lectures on the subject. But my main interests as an undergraduate were not in economic history, they were in medieval European history – largely because it was all fresh to me.

And what led you to move from medieval to economic history?

Accident. There were very few research students in my day. You could hardly take a topic which didn't appear promising and if one had a truant disposition and a number of intellectual interests, one started on a large number of topics. My first research subject was the influence of Dutch theologians on political thought in the 17th century and I remember going in 1935 to see G. N. Clark who was then, curiously enough, Professor of Economic History at Oxford – he gave me introductions to a large number of Dutch theologians. I moved from that into William Paley because Harold Laski thought it would be an interesting subject; he also suggested Arthur Young and I dabbled in Arthur Young. And Clapham suggested to me that really I ought to write the history of the Industrial Revolution in South Wales and I very much regret I didn't take his suggestion. I was at that time, I suspect, rather anxious to get away from South Wales. It was Postan who suggested that I should do 18th century landowners. I think he had just come back from looking at the archives in Northamptonshire and was very much impressed by their abundance. And I was very anxious to have a subject which wouldn't run out because of lack of sources. So I decided to do Northamptonshire and added Bedfordshire for safety. So my transition to economic history was very circuitous, with a lot of accidental twists and turns.

What was it like being a research student in the 1930s? Were you closely supervised, or allowed a free run of the subject?

I think by modern standards it was all very casual. I was never registered for a Doctorate; very few people in my situation were. I was loosely assigned to John Clapham and I used to go along to him once a fortnight. He used to sit at his desk opening his correspondence and I would talk and he would comment, and comment very shrewdly. Land ownership wasn't his subject but he found a lot of interesting things to say about it and he gave me good advice. He recommended taking a single family, doing it thoroughly and surrounding it with a penumbra of suggestions – another of his ideas which I wish I had acted on. He didn't profess to guide me to the sources on land ownership and he knew very little about the legal arrangements of the landowners, in which I became rapidly submerged. But he was a great man and I think that in so far as I was sustained in research it was by his example and his general personality and character, and by the fact that each fortnight he was very interested in what I said.

How were you influenced by having Clapham as a supervisor?

Clapham liked talking about problems. He liked historical anecdotes. He was a wonderful lecturer because, although there was no general scheme and a complete absence of analysis, there were some wonderful vignettes. So, at his lectures on England before 1066, you felt as though you were living in an Anglo-Saxon village. And he was as good on the Middle Ages as on the 19th century. And he brought 19th-century industrialists to life. He knew an enormous amount about them and it all flowed out. Why he wasn't interested in theoretical systematic explanation, I don't know. I think it was partly a judgment that this was what the subject needed at the time. It needed a large number of raw facts, not linked to any theory at all, just raw material. But I think that Usher's criticism of him was justified. He ought to have made more attempt to pull the bits together. But then again, his output was enormous and the range was enormous. He wrote from the horsing of the Danes to the current history of the Bank of England. And I think that he wouldn't have done it if he had been interested in analysis.

You published your first article on land ownership in 1940 and you are still pursuing issues in this area almost 60 years later. How have you maintained your interest and enthusiasm for so long?

Because of a liking for the English countryside – and vested academic interest. I have deserted the field at various times and in one period, immediately after the war, thought I was going to desert it forever. In 1939 I was asked by my old tutor who was editing the Cambridge History of the Empire to write a chapter on the economic history of Empire from 1853 to 1870 as a matter of urgency because the original contributor of the chapter had let them down. I think it is an indication of the extraordinarily casual and unprofessional nature of the research community before the war that someone who was deeply immersed in family settlements in the 18th century could be expected to turn aside and apply himself to a different period and a very different set of problems and with, even then, a very considerable set of literature. So I worked like hell for a year and eventually produced the chapter, which was published in 1940. And that gave me an interest in 19th-century history and in the sort of history which contemporary economists wrote. And then during the war I was for some years in the Board of Trade, immersed in economic questions, which raised a lot of sticky points and interests which diverted my research into more recent periods and into more strictly economic subjects.

One of your wartime duties put you in contact with Keynes, did it not?

Yes. I was one of the Secretariat at the talks on post-war financial reconstruction which I suppose took place in the early part of 1943, before Bretton Woods. The talks were attended by delegations from the Commonwealth countries and they discussed Keynes's plan for an I.M.F. – as well as plans for an International Trade Organisation, and the plans for international commodity agreements. I suppose they lasted about four months. Keynes was one of the dominating figures in the discussions and much the most magical of the protagonists. But the talks brought me into contact with someone who had much more influence over me – Dennis Robertson, an economist with very strong historical interests, whose first book had been on industrial fluctuations, and I suspect my later interest in the trade cycle of the 19th century arose in part from knowing him and getting to read his book.

Did this experience alter your approach to economic history?

I think so. I think that those three or four months, in which I listened to Keynes, Robbins and Robertson and T. E. Gregory and [James] Meade and Hubert Henderson and a splendid Canadian economic historian, W. A. Macintosh, were very influential. They were discussing in a sense an episode of history – their discussion of the plans for the post-war was overshadowed by their ideas about pre-war problems – all the plans were revised to prevent a repetition of the 1930s. That made one analyse the 1930s in a quite different way. I think it was a crucial episode for me. When I went back to Cambridge in 1946 and had rapidly to prepare lectures for second-year economists, what was meant to be a general course on world economic history since 1750 became two terms of comparative study of industrialisation followed by a third term on international and economic relations since 1850, using economic theory throughout, however primitive. And of course one mixed with economists. I spent the summer of 1947 on a working party of dons who were ex-civil servants giving advice on what attitude the British Government should adopt to moves towards a European Customs Union. There was very strong pressure from the Americans as part of the Marshall Aid package, and the Treasury and the Board of Trade appointed the group of us who used to meet at frequent intervals. That was another immensely formative episode.

You mentioned your interest in the 19th-century trade cycle earlier. Later you had a student, a very famous student, work on the topic, did you not?

Yes. Well, when I came to Oxford, I set such pupils as were amenable to suggestions, and some who were not, on two fields. One was trade cycles. I set someone onto the 1820s. W. A. Sinclair, the Australian economic historian, was set onto the period after 1886 and the recovery from that. John Wright [Trinity College, Oxford] did a later 19th century cycle, and J. R. T. Hughes did the 1850s. I think the argument was partly that there was a body of theory, partly that it at least provided safe subjects. That is to say they were limited in time and limited in sources and if you worked hard you finished your thesis. Jonathan Hughes worked harder than anyone I have ever met and he finished it in two years – a very remarkable achievement. The other set of research topics I thought were suitable, again for a similar reason, was English industry – so Roderick Floud did the machine tool industry and Ian Byatt, the electrical industry. And again, the reasoning was that no one had done these industries, they were relatively well defined and an industrious student couldn't fail to finish.

Do you feel that those dissertation topics were perhaps a little restrictive in terms of the possibilities for ingenuity and inference that they allowed?

Jonathan Hughes was very kind and said that that piece of detailed work occurred at the right time in his career. But looking back it was absurd to set a man of his range into such a Procrustean bed. But I think as a first piece of research the cycle is a useful field to work in. It gives you some of the tools of the trade. It gives you some sense of the complexity of the interconnections between the various sectors of the economy.

You have had strong connections with the US and

American academics. When did that begin?

My first visit to the United States was in 1952. I went as a member of a committee set up to examine the retention and choice of records for permanent preservation, and I went with a man called Paul Chambers, who eventually became Chairman of ICI, to the States to see how they did it. So I saw a lot of records but not much of American academics. My first academic visit was in 1954-5, when I took Alexander Gerschenkron's place at Harvard for the year. I was then at Berkeley in 1962-3.

You must have noticed a difference in the academic dialogue between those visits!

The difference between the two was enormous. When I went to Harvard the seminar was enormously stimulating. There was Henry Rosovsky. I suppose he was then a young research student. There was Goran Ohlin. There was Alfred Conrad, who sometimes used to come, and, of course, John Meyer used sometimes to come. Then there were a number of people from the entrepreneurial research centre – Hugh Aitken and, of the older generation, Arthur Cole. But I think there was nothing either in the theory or in the statistics in the seminar which I felt was beyond me. The most advanced statistics I remember being used were by Goran Ohlin, who did a thesis on population in the 18th century. Now he did employ what I think were very sophisticated statistical methods but I don't remember statistical methods being applied to other problems. I knew Conrad quite well, I was a good friend of his, I think we talked poetry! Now when I went to Berkeley I was taken to a conference of cliometricians – I don't remember where. And I saw a great deal of Fishlow in Berkeley and I remember thinking that he was a different sort of historian. I did sense an enormous divergence between what many of the Americans historians were doing and what I did.

Did you come into contact with Fogel at all?

I think I must have done because I've been looking at my papers and Fogel used to send me the early editions of some of his papers. And when I had a New Zealand research student who had been well equipped in statistical methods [Gary Hawke], I did explicitly put him on to Fogel's problem in relation to British railways, which was very rewarding.

Coming from Cambridge, what struck you most forcibly about this new breed of economic historian?

Looking back, before 1939, there were people who did the statistics of national production – stocks and things like that. I had a colleague in Cambridge who was a close friend of mine, J. W. F. Rowe, who in the 1920s was doing national production figures. But I never remember thinking that what he was doing had any relevance to what I was doing. There was a curious hiatus between the statisticians and the theorists and between the theorists and the historians. I think it is partly a matter of the English intellectual tradition – the tradition is that you do things on your own, that you're a handyman; particularly if you're an historian, you're a craftsman. You write a carefully crafted book and it's yours. It's partly, I think, a matter of the production of statisticians, and I suspect they were more abundant in the United States than they were here. So there wasn't the spillover into other subjects. It's also partly that economic history, more often than not, was not in economics departments. It is partly that the background to the development of the subject in England owes much to German legal studies and the German historical economists. [Alfred] Marshall set off a great tradition of applying theory to actual situations. But if you look at his history, there is very little theory in it. Indeed in some senses he was *avant garde*. When I read Temin's presidential address in the *Journal of Economic History* on culture, I immediately thought of Marshall. When Marshall wants to explain the recovery from the depression of 1873, he does it in terms of national character and the distinguishing ability of Englishmen to rise to the occasion – no nonsense about interest rates and production functions. So he believed in the influence of culture. And Clapham, who helped Marshall with his historical chapters, although a Professor of Economics, ostentatiously eschewed theoretical speculation. I once acquired a volume of Clapham's. It is Cassel's *The Nature and Necessity of Interest*, and it is full of underlinings and little notes in the margin and he was obviously fascinated by this highly theoretical subject. Nothing at all of this appears in his works. He wrote the classic article on 'Empty Economic Boxes', which foreshadowed Joan Robinson and imperfect competition, but none of it is in his history.

But you did get interested in the interplay between economic theorising and historical change. How did you get interested in the themes of American and British technology?

The accident was knowing a German refugee called

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Report on the NBER/DAE Meetings

by Jonathan Pritchett, Tulane University, and Rebecca Menes, UCLA

(Cambridge, Mass.) The Development of the American Economy Summer Institute met at the National Bureau of Economic Research July 15 through 17. Price Fishback and Shawn Kantor of the University of Arizona organized the 18 presentations into seven sessions, with papers by both DAE veterans and three economists new to the Summer Institute, Carolyn Moehling (Ohio State), Petra Todd (Pennsylvania) and Melissa Thomasson (Arizona).

Richard Sylla (NYU) opened the first session, Capital Markets, with a new approach to early US financial history. He and co-authors Jack Wilson (North Carolina State) and Robert Wright (Temple) have assembled a monthly data series from New York City, Philadelphia, and Boston bond markets on US bond prices from 1790 until the complete retirement of the Federal debt in the 1830s. The behavior of the price series for three kinds of Federal bonds suggests that financial markets developed swiftly in the early US, at least in the Northeast. Although bonds had been issued at three different interest rates, arbitrage between the series quickly eliminated the divergence in the returns. Evidence of parallel price movements in the three cities in times of financial distress and war also suggests that the markets, although geographically separated, were integrated as closely as contemporary information technology allowed.

Discussion centered on limitations of the data series ("no volume data and prices limited to monthly observations") and on analysis of the series around specific historical and financial events such as the financial crash of 1792 and the War of 1812. Sylla ended the presentation with an interesting unsolved mystery. When the US price series is compared to similar series for US Federal securities sold on the London secondary market, there is a suggestion that returns to English holders of American bonds were higher than returns to domestic bondholders during the last 20 years of the analysis. A lively discussion ensued, with several explanations for the survival of such a tempting opportunity for arbitrage between two apparently well-functioning markets.

Jeremy Atack (Vanderbilt) presented the results of recent work with Peter Rousseau (Vanderbilt) on business activity and the Boston stock market, 1835-1869. The data series were of annual high and low prices, dividends, par value, capitalization of firm and other financial data of manufacturing firms and banks traded in Boston. The authors test for a relationship between poor performance in the banking sector and poor performance in the manufacturing sector, theoretically mediated by rising interest rates and restricted credit during periods of financial distress. Granger causality tests appear to indicate such a relationship. Not surprisingly, the data series stimulated discussion of other issues in American financial history, as well as the econometric issues raised by the authors' analysis. Robert Margo (Vanderbilt) made several suggestions for the econometric analysis.

Eugene White (Rutgers) presented recent work with Cormac Ó Gráda (University College-Dublin) on the remarkably complete records of deposits and mortgages at the Emigrant Savings Bank of New York City. One of a number of philanthropic banks, the Emigrant was chartered in 1850 at the impetus of Bishop John Hughes to serve the large and growing Irish immigrant community. Depositors were overwhelmingly Irish or of Irish extraction (based on names), but borrowers were a varied group: individuals, houses of worship (churches and synagogues) and businesses from a range of New York City communities. The work presented is preliminary, and most of the discussion revolved around future plans. Several interesting aspects of the mortgages were discussed, including the fixed interest rate (7%) and the lack of closing dates on loans – apparently borrowers could choose when to pay back the principal.

The Economics of War session began with the paper by John Wallis (Maryland) on the US government's role in encouraging economic growth over the past two centuries. His paper traces three eras (with some overlap) in government provision of infrastructure: 1) a state-dominated system from 1790 to 1860, 2) a local-dominated system from 1840 to 1932, and 3) a federal system from 1916 to the present. In the first period, states subsidized or otherwise encouraged banks, turnpikes, canals and railroads. In the second half of the century, rising urbanization increased demand for local infrastructure, which local landowners were willing to fund because infrastructure led to increased land values. In the 20th century, suburbanization of population and employment broke the link between the beneficiaries and tax payers for many

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Hugh Rockoff (Rutgers) presented an amusing paper with a serious point concerning the command economy of the war years. The famous World War II salvage drives served multiple political and economic purposes, but they did not represent as large an improvement over the market as is generally believed. Scrap drives could raise morale on the home front by getting everyone involved, and, for some drives, such as the silk-stocking drive, this was probably the dominant motivation. More serious drives, for iron, steel and rubber scrap, could and did gather significant quantities of important raw materials. However, the amounts were only slightly greater those normally handled by the private scrap and salvage industry in peacetime. Rockoff asserts there is little evidence that a market-based mechanism could not have accommodated the demand for scrap created by the war effort. Instead, the need for scrap drives may have been generated largely by the inefficiencies of the wartime command economy. The drives may have made up in part for declines in the market due to fixed nominal (hence declining real) prices offered for scrap. The drives also were politically popular with producers of certain consumer goods (*e.g.*, soap and rubber), who faced potential rationing if supplies of raw materials were not sufficient to supply both the war and consumer markets. In sum, scrap drives did gather scrap, which was (mostly) used in the war effort, but there is no evidence that a market-based system would not have worked as well – or better.

Wednesday morning, the New Deal session began with a comparison of two regulations, the Agricultural Adjustment Act and the National Industrial Recovery Act. Both Acts were passed in 1933 with the express intent of allowing farmers and industrial producers to form cartels to limit production and raise prices, and both were found unconstitutional. Yet the provisions of the AAA were resurrected within two months of the court decision and are still with us, while the attempt at an American industrial policy was abandoned. Barbara Alexander (Wellesley) and Gary Libecap (Arizona) presented an explanation for the success of the development of government-supported agricultural cartels in the US in the 1930s and the failure of the industrial cartels. It is based on the homogeneity of the constituency for the agricultural policy – medium-sized commercial family farms – and the heterogeneity of the constituency for industrial policy – labor unions, large firms, and smaller firms. Farmers could, and did, present a unified political front, represented by the American Farm Bureau Federation. In contrast, even within a single industry, firms may have disagreed about the pricing policies and intra-industry coordination they desired; in general, the largest firms enjoyed large cost advantages over their smaller, more numerous competitors. The addition of labor, a politically powerful force, complicated the political difficulties.

Alexander and Libecap support their thesis with historical narrative, game theory, and a regression analysis of the relationship between industrial homogeneity and ability to take advantage of NIRA provisions. Participants accepted the thesis in general, but urged the authors not to ignore other political factors, such as the development of the New Deal coalition (farmers and labor but not big business) and the importance of the Senate's rural bias.

Price Fishback and Shawn Kantor followed with a micro-level analysis of the economic effects of New Deal expenditures on local economic development in the 1930s. Using county-level data, they examine the impact of Federal revenues on economic development as well as the factors determining the distribution of New Deal funds. Their preliminary results suggest that New Deal spending had a small positive effect on land values, and that the people distributing these funds considered economic relief and recovery as well as politics.

Many comments concerned the aggregation of New Deal spending. Sukkoo Kim (Washington U) suggested that federal spending should be disaggregated into transfer payments and productivity enhancing projects; Peter Temin (MIT) made a similar suggestion with regard to farm and non-farm spending. Margo pointed out potential problems in the authors' use of the county-level wealth estimates. Participants also suggested that the authors include additional political variables as instruments in the regression equation for New Deal spending.

The next session on big business started with an examination of American Tobacco, one of the major consolidations from the merger movement at the turn of the century. Karen Clay and Gillian Hamilton (both of Toronto) investigate whether American Tobacco's monopoly in cigarettes, snuff, and other tobacco products was technological or strategic in origin. They find that American

Tobacco's initial growth was due to scale economies in manufacturing. (Apparently, these scale economies did not extend to the manufacture of cigars, a segment of the market never monopolized by American Tobacco.) According to the authors, the cost advantages in the manufacture of cigarettes were dissipated by 1900. Subsequently, American Tobacco maintained and improved its market position by erecting barriers to entry: controlling distributors, engaging in brand proliferation, and advertising. Clay and Hamilton find evidence of strategic behavior in fliers advertising American Tobacco's products and in contracts the company made with its jobbers. The lively discussion included suggestions as to what actions constitute a restraint of trade.

Peter Temin then presented an update of William Miller's classic study of the American business elite. Miller's original study reported the background of business and political leaders a century ago. Temin compares their backgrounds to those of current business leaders, whom he defines as Chief Executive Officers of Fortune 500 firms. He finds little evidence of change in the demographic composition of the American business elite: most of today's business leaders are native-born, white males who were educated at elite private colleges. In contrast, he finds that political leadership has become more "democratic" during the past century.

In discussion, Charles Calomiris (Columbia) suggested that CEOs are the hired help of the rich rather than part of the elite. Joseph Ferrie (Northwestern) suggested that CEOs are agents of the stockholders, and politicians are agents of the electorate. Furthermore, he suggested that the democratization of the political elite reflects the democratization of the electorate during the last century. Menes suggested that religious affiliation may be endogenous – some business leaders changed their religions after achieving success.

The next paper was by Daniel Raff (Pennsylvania), on technology diffusion and productivity in the American automobile industry, 1904-1915. Much of the discussion centered on why Japanese cars are perceived as superior. Recent attention has been focused on the manufacturing process rather than the product itself. The popular press suggests that Toyota's advantage was the result of "just-in-time" production techniques, small inventories, and pull-through, rather than pushed, production. Alleged advantages also include flexible machine tools with low set-up costs, and a multi-skilled labor force, capable of moving quickly from task to task. Raff questions the importance of some of these production techniques. He also suggests that some of the techniques credited to the Japanese, such as flexible machine tools, were previously adopted by Ford.

Joseph Ferrie opened the Thursday morning session, on demographic changes in the 19th century, presenting a new project on internal migration in the US. Using linked Census samples, he reports that early results support one of the oldest hypotheses in the American economic history: the opportunity to move to the Western frontier was a viable option for urban, unskilled workers and thereby relieved downward pressure on wages in urban labor markets in the East. The experience of 5,000 males linked between 1850 and 1870 finds "substantial migration to the frontier by urban residents, particularly unskilled workers, and substantial gains in wealth for these migrants." These exciting results led to a lively discussion of Ferrie's methods, with some suggestions for different regressors, but with general support for the project and the method.

The next paper, "The Short and the Dead: Agricultural Surpluses, Mortality, and Stature in the Ante-bellum United States", was by Lee Craig (North Carolina State), Michael Haines (Colgate) and Thomas Weiss (Kansas). In the decades before the Civil War, Americans experienced rapid population growth, the adoption of the technologies of the first industrial revolution, and solid growth in *per capita* income. However, during the same decades, life expectancy and adult heights declined. Several reasons for this apparent paradox have been suggested, including what might be termed an industrial explanation and a Malthusian explanation. The industrial explanation entails a rise in disease and pollution and possible fall in quality of diet due to the increase in negative externalities that may have accompanied Smithian growth (increased urbanization, an enlarged transport network, and/or rising inequality). The Malthusian explanation links increased morbidity and mortality to population increase that reached lower margins of maximum sustainable agricultural output (given traditional agricultural techniques) in a significant number of agricultural regions.

To test these hypotheses, the authors link county death rates and county adult height (measured by the heights of Union Army recruits born in the county) to the urbanization and transport linkages in the county and to the agricultural output of the county. The regression analyses suggest that the impact of urbanization and rising travel were more important (negative) influences on life expectancy than any Malthusian crises in which population growth reached the limits of local food supplies. Agricultural outputs were not statistically linked to county crude death rates. However, there is evidence in the height data that high agricultural output was linked to higher adult height. Even controlling for urbanization and transport, agricultural output of a county has a small but statistically significant effect on the heights of adults born in the county. In brief, negative externalities appear to be a larger factor in the "paradox" of rising income and falling health statistics, but there is evidence for perhaps the very gentlest of Malthusian squeezes.

Robert Margo next presented two chapters of his forthcoming book, *Ante-bellum Economic Performance: The Labor Market Perspective*. The nominal wages reported in Chapter 3 are based on a hedonic regression analysis of wages paid to civilians at US Army forts. A series of wholesale prices for consumer goods (food, textiles, etc.) is used to calculate real wages from the nominal wages; in the main, results agree with previous estimates. Chapter 5 examines farm-nonfarm wage gaps. If there were such a gap during the ante-bellum years, there may have been an economic "free lunch" as labor shifted from farm to city after the war. Unfortunately for those who wish to ascribe income growth to labor reallocation, Margo finds no real wage gap in the ante-bellum years, although the nominal wage gap was relatively large (living costs were higher in cities). Perhaps even more controversial was his finding that there was no significant real wage gap between North and South before the Civil War.

In the next session, Claudia Goldin and Lawrence Katz (both of Harvard) took the conference back to high school with two papers: a review of their findings on the rise of US secondary schools using state and city level data, and a preview of a schooling and earnings analysis using individual 1915 Iowa State Census returns. America invented the modern high school, free terminal secondary education for the majority of citizens. Between 1910 and 1940, the rich agricultural states of the West and Midwest led the nation establishing high schools, followed by New England, the manufacturing heavy Mid-Atlantic states, and, far behind, the South. High incomes and low employment of adolescents both positively influenced the increase in number of high schools.

The spread of high schools was a combination of private decisions by students (and their families) to attend, and public decisions to build high schools. The state level analysis presents a reduced form of the outcome of the combination of private and public decisions. The individual level data from Iowa, including information on both schooling and earnings, promises to make possible a decomposition of the reduced form into public and private decisions.

Improvements in public education for black Americans in the first half of the 20th century and political and legal action to eliminate employment discrimination in the 1960s are sometimes posed as competing explanations for the decline in the wage gap between black and white workers in the US. "Public Action, Private Choice and Philanthropy: Understanding the Sources of Improvement in Black Schooling Quality in Georgia, 1911-1960", by John Donohue III (Stanford), James Heckman, Anne Layne-Farrar (both of Chicago), and presenter Petra Todd, strives to bring the two stories together. The authors analyze the various causes of improvements, absolute and relative, in the quantity and quality of education for black children in the state. The same political, legal and philanthropic motives behind the Civil Rights Act and subsequent legal activity supporting black Americans in the 1960s were also sources of much of the improvement in black education in Georgia before 1960. Between 1911 and 1930 black migration from rural to urban districts and the actions of philanthropic institutions, especially the Rosenwald Fund, kept improvement in the measurable quality of black schools equal with improvements in white schools. The authors find that the convergence of black and white schools in most quality measures between 1930 and 1960 appears to have been driven largely by white fears of Federal legal action to desegregate the school systems.

Jonathan Pritchett's presentation on school size and school district size for black and white school systems in Delaware in 1910 illuminated an aspect of racial discrimination in education unlikely to appear in the usual measures of school quality or cost – pupil walking distance to school. Before school buses, school boards balanced the tax-payer demand for cheap schools and parent demand for near-by schools when they established school size and location. Several one-room schools were more expensive and provided lower quality education than one larger, graded school, but with smaller schools the average student did not have to walk as far. Eager to keep children close to home, parents lobbied hard for smaller, more expensive schools. School board officials, were immune to political pressure from black parents, however, due to the disfranchisement of black citizens. Not surprisingly, the schools for black children were on average 50% larger

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22nd Annual Economic Policy Conference Report

by David C. Wheelock, Federal Reserve Bank of St. Louis

(St. Louis) Each October, the Federal Reserve Bank of St. Louis hosts a two-day conference on monetary and financial policy issues. This year's conference, on the 16th and 17th, entitled "Lessons from Financial History", included seven research papers in three sessions.

The first session focused on the role of banks. Eugene White (Rutgers) presented "A Microeconomic Study of Bank Lending in Late Nineteenth Century America: Were Banks Special?" He describes both modern and 19th-century theories of banking and illustrates the unique role of banks as lenders to borrowers of high or uncertain credit risk. Recent advances in information-processing technology have increased competition for banks from non-bank intermediaries and markets. White shows that banks faced similar competitive challenges in the 19th century, and that the declining market share of banks has been a long-term phenomenon.

Caroline Fohlin (Cal Tech) followed with "Bank Structure and Growth: Insights from British and German Bank Balance Sheets Before World War I." Previous researchers have argued that German bank underwriting and investment in securities at the turn of the century boosted the country's economic growth relative to the United States and Britain, where banks were substantially less involved in the securities business. Fohlin, however, casts doubt on this view by demonstrating that German banks held no more non-government securities than British banks, and typically held equities only when markets for new issues were insufficient.

The second session examined broader issues of institutional change and market development. In "The Bank of England's First Return to Gold and the Stock Market Crash of 1825", Larry Neal (Illinois) describes how Britain's return to the gold standard in 1821 triggered a subsequent monetary expansion and lending boom to the newly-emerging Latin American countries. But British investors failed to discriminate adequately among borrowers; when the crash came, the Bank of England was slow to respond. Neal argues that these events led to reforms which made information-gathering and analysis more efficient, and contributed to a more smoothly functioning financial system.

Next, Richard Sylla (NYU) presented his paper on US capital markets and the banking system, which describes the mutually-reinforcing evolution of American banks and financial markets during the years 1790-1840. Sylla argues that the nation's accelerating rate of economic growth was spurred by restructuring of federal and state government debt, which contributed to America's rapid financial development.

The final paper of the session, "Bank Capital and Portfolio Management: The 1930s Capital Crunch and Scramble to Shed Risk", by Charles Calomiris (Columbia) and Berry Wilson (Federal Communications Commission), presents an option-pricing model of how banks manage their portfolio choices in the face of changing capital costs and lending opportunities. The authors show that New York City banks responded to a "capital crunch" during the Great Depression by reducing their asset risk, rather than by investing in costly capital or allowing the default risk of their deposits to rise.

Session three addressed central bank provision of payments services. Bruce Smith (Texas) and Warren Weber (Minneapolis Fed) presented "Lessons from a *Laissez-Faire* Payments System: The Suffolk Banking System, 1825-1858", written with Arthur Rolnick (Minneapolis Fed). Economies of scale and scope enabled the Suffolk Bank of Boston to monopolize bank note clearing among New England banks. The authors argue that the experience suggests possible inefficiencies in *laissez-faire* payments systems.

Alton Gilbert (St. Louis Fed) then asked "Did the Fed's Founding Improve the Efficiency of the United States Payments System?" Gilbert explains how contemporaries perceived the problems of the pre-Fed payments system, and how their perceptions influenced Fed design. Gilbert shows that banks reduced their cash holdings about the time the Fed was established, suggesting that the Fed's presence enhanced payments system efficiency.

Concluding the conference, Joseph Mason (Office of the Comptroller of the Currency) described a new data set that he and Calomiris have compiled from reports of income and condition for all Federal Reserve member banks at 1929-1935 call dates. Mason explained how they have used these data and discussed possible directions for future research.

Fourth Hughes Prize Awarded

by Hugh Rockoff, Rutgers University

(New Brunswick, NJ) This year's winner of the Hughes Prize for Excellence in Teaching Economic History is Martha Olney, University of California at Berkeley. The award was announced at the President's Awards Banquet September 13 during the annual meeting of the Economic History Association.

Olney is a dedicated, inspiring, and innovative teacher who has made an indelible imprint on her students. With equal success she has taught large lecture classes and small upper division classes, and advised students on an individual basis. Her former students routinely describe her as "an incredible educator" and "one of the best on the entire campus," and many volunteer that she has changed their lives.

The EHA Teaching Committee, which chooses the winner of the Hughes Prize, also sponsored a Teaching Breakfast on Sunday morning. One of this year's highlights was Olney's description of her approach to teaching economic history. A written version of that talk appears below.

Reflections on Teaching

Martha L. Olney

As the 1997 recipient of the Jonathan Hughes Prize, I have been asked to pen this essay on teaching. I am honored to do so. And humbled. Shouldn't reflections on teaching be written by the senior members of our profession, those who have honed their skills over decades of practice? In my less-guarded moments, I can admit that at age 40 I have nearly 20 years of teaching experience under my belt. Two decades of practice ought to produce a fairly well-honed skill. But I hope that I have another 20 or 30 years of experience yet to gain, and that I will learn and grow as a teacher over those next few decades. So consider these comments "reflections along the way" rather than "reflections on a completed journey."

First some background. I have taught at UC Berkeley (Cal) both as a graduate T. A. and now as a professor, at College of San Mateo (CSM, a California junior college), and at University of Massachusetts, Amherst (UMass). My first teaching job was as a T. A. for Principles of Economics in Fall 1979 at Cal. At CSM I taught principles courses to 30-student classes, a mix of high school whiz kids, struggling twenty-somethings, returning students, housewives, recent military retirees, and retired folk. At UMass, I taught principles, intermediate macro, US economic history, math (!), and graduate courses in macro and economic history. Principles classes enrolled about 300 students. Twenty to 30 students were enrolled in the other classes. Many undergraduates at UMass were first-generation college students, the children of blue-collar and working class Massachusetts families. Most of the in-state students emerged from a culture that (wrongly!) said only the poor or the stupid go to UMass. Here at Cal, I teach mostly large lecture courses to undergraduates: principles (700 students), intermediate macro (400), money and banking (250), and US economic history (250-400). I have also taught undergraduate seminars in US economic history and senior thesis writing, plus the teaching development seminar for the department's graduate teaching assistants.

Students tell me two things that they like about my teaching: my organizational skills and the life lessons I teach. The first are easy to acquire. More on organization below. The second – life lessons – happen through some mysterious process that I'm not sure I understand. I suspect my teaching philosophy is the key.

It is difficult for me to talk about my teaching philosophy without talking about my personal theology. Not that I bring my faith explicitly into the classroom. Just that I think my faith informs who I am and how I behave. If you are reading this essay for "tips on how to be an award-winning teacher," please be clear: I am not saying that a prerequisite is being a person of faith. But in these "reflections along the way," faith is part of what makes Marty tick as a teacher.

I believe we are all loved children of God. I believe that there are no privileges or handicaps that come with birth into one family or one gender or one race or one sexual orientation. I believe the Golden Rule – do unto others as you would have them do unto you – is a useful guide for behavior. I believe that we are all striving to become the people our Creator created us to be.

All of which boils down to one word: respect. In my teaching, respect is my number one rule. I respect the students. Not because they are bright or rich or well-dressed or white or male or Asian or gay or Muslim or an economics major or the star quarterback or daughter of the President. I respect them because "students are people, too," people deserve respect, and because I treat them as I would want to be treated.

I recognize the many demands on their time and don't change exam times or deadlines at the last minute. I give a complete course outline in the syllabus so they can plan their semester. I realize they truly may have commitments barring them from coming to my office hours, and so I readily schedule appointments for mutually convenient times.

I assume their goal is to learn the material. When they come to me with graded exams, I respect their desire to learn what they did wrong so that they might improve the next time. I recognize that I am sometimes unclear, so when they ask for a second explanation I provide one without offense.

I want them to gain confidence in their intellectual abilities. At UMass, this was my over-arching goal. Too many students at UMass assumed their instincts were wrong. They did not trust their ability to reason, to think. I praised them when their logic served them well, and helped them see the flaws in their thinking when they got off-track. I cheered (literally!) when they got something right after struggling with it. I believe that acquiring an ability to think through problems – be they economic or political or social or personal problems – is critical to gaining self-confidence, and so I explicitly teach a method for criticizing arguments.

And I respect who they are. There is more to life than economics. "What should I major in? My parents want me to major in business." Follow your heart. Figure out what you love, what makes you groove, what turns you on, and major in that. Your parents will eventually come around. If you love business, then go for it. But if your love is art history, if your passion is politics, if you can't get enough of microbiology, follow your heart. Life is too short to not do what you love.

Somehow in all of this, I teach them life lessons. One student said what he really admired was how I "carried" myself. I suppose it's more than having grown up next door to an Army colonel who threatened to tie a broomstick to my back if I didn't stand up straight! After my father died in April 1995, a number of students commented that my return to the classroom one week later taught them about fulfilling obligations even when life interferes. Did they know I went back to my office after class and sobbed? I don't know. I think they knew I was struggling through a rough time, but that "through" was the key word. I have often had conservative Republican students seek me out as their mentor, knowing full well that I am certifiably liberal. Why? I think because I treat the students with respect. Together we think through the perplexing political and social problems of the day, we disagree, we figure out why, and we don't feel a need to belittle each other because of our disagreement.

Surprisingly, my teaching ratings following my brother's death in May 1996 were at their all-time high. Fall 1996 was hell personally. Why did the students respond so positively? I think I was more vulnerable, more human. My brother dropped dead at 50. One of the several things that killed him was the inability to find a permanent job after having been downsized out of his company four years earlier. To me, unemployment has always been about real people. But following Ken's death, my discussion of the problem of unemployment took on a whole new level of poignant reality. Students from previous classes were shocked when they heard of Ken's death: he had been "their example" of structural unemployment and the effect of corporate downsizing. This is real life we're dealing with when we teach economics. Students respond.

Bringing more vulnerability to my teaching does not mean eliminating all professional boundaries. The relationship between me and my students is about teaching. I believe they need not know much about my personal life in order for me to be an effective teacher. I use examples drawn from my life: my brother's unemployment, my mother's Social Security, my dad's business, my immigrant grandmother. But the various demands on my time, my emotional struggles, my home life – I see those as inappropriate to share with the students. My confidence in myself and in my teaching must come from internal sources. I cannot and should not depend upon my students for ego gratification. All the mutual respect in the world doesn't eliminate the power relationship between us.

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Cliometric Society Undergraduate Paper Prize Awarded

(New Brunswick, NJ) The third annual Cliometric Society Undergraduate Economic History Paper Prize was awarded to Eric Bickford, a student at UC-Berkeley, for his paper "White Flight: The Effect of Minority Presence on Post-World War II Suburbanization". Selection Committee chair Ann Carlos (Colorado at Boulder) announced the award at the EHA banquet September 13.

The Cliometric Society and the prize committee congratulate Bickford on his achievement. We are pleased to publish his complete paper in the *Newsletter* and in the EH.Net web server (**http://www.eh.net**). An abstract of the paper will be published in a forthcoming issue of *Explorations in Economic History.*

Bickford's paper was nominated by his professor Martha Olney, who writes:

It is my pleasure to write this brief introduction to Eric Bickford's prize-winning essay. Eric wrote this paper for Economics 114, an undergraduate seminar in US economic history during the Spring 1997 term. His conceptualization of the question and use of econometric technique impressed me. I am delighted with The Cliometric Society's selection of Eric's paper.

Economics 114 is a seminar course for undergraduate students who have taken one or more previous semesters of economic history lecture courses. My goal for the seminar was to teach research skills. I took what was for me a new approach to this task: watching Hollywood films. One week, we watched "It's a Wonderful Life." The next week we brainstormed a list of all of the film's implicit and explicit assertions about economic history. Our list filled several chalkboards. We sorted the list into broadly-defined categories. I left the room for five minutes while the students formed research groups of three to five members. Each group then came up with several researchable questions based on the assertions in the film. For instance, the Martini family was denied a mortgage by the commercial bank but received one from the Bailey Building and Loan. What was the extent of racial and ethnic discrimination in housing and in home mortgage lending in the interwar years? Over the next week, the group found answers to their questions and put together a four-to-seven page research report, which they presented in the next class meeting.

A 15-to-20 page term paper was a class requirement. Through the film groups, the students had learned the difference between researchable and non-researchable questions. They had learned how to find and use primary and secondary sources. They had gained confidence in their ability to research and write. I was uniformly pleased with the result: well-written and researched term papers on "doable" topics of interest in economic history. Eric Bickford's paper was one of the best.

White Flight: The Effect of Minority Presence on Post-World War II Suburbanization

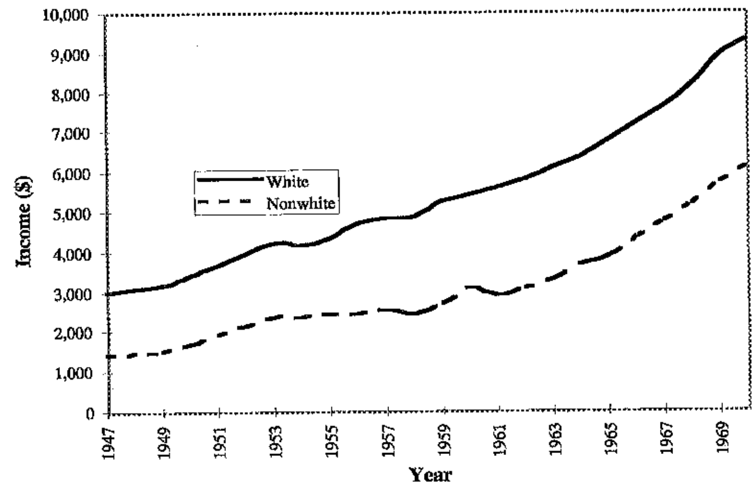
by Eric Bickford, University of California-Berkeley

Background

Immediately following World War II, the United States underwent an urban transformation. Kenneth T. Jackson, citing a Bureau of Labor Statistics survey for home building in the six largest metropolitan areas for 1946-47, notes that over 62% of all home construction occurred in suburban areas. US Census data show that for the period 1950-1960, central city populations in the largest 25 SMSAs increased by just over 3%, while total suburban populations increased by well over 60%.[1] While the total populations of the nation's largest central cities stagnated, the number of blacks in the central cities increased substantially, implying an urban depopulation by whites who migrated to the suburban fringes. The numbers for all the nation's metropolitan areas are somewhat less striking. However, even in this broadly based sample, the distribution of the population shifted: the suburbs remained largely white despite tremendous growth and the cities became increasingly nonwhite.

However, from this evidence alone one cannot necessarily conclude that the black/white - city/suburb dichotomy arose purely on racial grounds. Cities in the United States tend to follow a pattern of neighborhood succession in which wealthy residents abandon smaller, older units of housing in favor of larger, newly constructed units. As the existing housing stock in older neighborhoods depreciates with age, it becomes occupied by lower-income households; a neighborhood transformation takes place in which older, central neighborhoods decline while new affluent communities form on the urban fringes. Since blacks historically have had lower incomes than whites, as shown in Figure 1, the supposed post-war "white flight" could be attributed to simple neighborhood succession. White households, with their higher incomes, were able to locate in the new, more expensive housing in these new neighborhoods, while nonwhites, with lower incomes, increasingly occupied the older, less desirable areas near urban centers. These new neighborhoods simply happened to lie outside the political boundaries of the large central city, and the normal pattern of neighborhood succession was given a new name – suburbanization.

Figure 1 – Median Annual Wage or Salary Income for Families and Unrelated Individuals, 1947 to 1970



Source: US Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, Washington, 1975, Series G-354-355.

This reasoning is somewhat tempting. After World War II, housing became affordable to more families because of new mass production techniques, government financing, relatively low interest rates,[2] and high real wages. A family with the median white postwar annual income of $3000 could handily have afforded the no-down-payment mortgage for a $7,990 Cape Cod production house in William Levitt's famous Levittown subdivision.[3] The median nonwhite income, just over $1500, would be insufficient. Upon careful examination of the evidence, this reasoning fails. First, at least a small fraction of the nonwhite population would presumably have earned sufficient income to locate in the new suburbs. If the racial distribution of the population were due simply to differences in income, then higher income nonwhites, able to afford the newly produced housing, would have located in the new developments alongside whites. In the case of Levittown, NY, however, not a single one of the original 82,000 residents was black.[4]

Reynolds Farley finds a tendency toward racial segregation after adjusting for income and educational differences between races. Regardless of socioeconomic status, whites tend to live in the suburban ring, while blacks disproportionately inhabit the cities. Finding proportionally fewer college-educated blacks than grammar-school-educated whites in the suburbs, Farley concludes that the absence of blacks from the suburbs cannot be explained entirely by socioeconomic status.[5]

The Question

One can explain the apparent racial segregation in one of two basic ways: the "pull" hypothesis and the "push" hypothesis. The "pull" hypothesis asserts that, following World War II, a combination of federal housing programs, highway expenditures, and mass production techniques combined to make suburbanization possible. It assumes that there is an intrinsic lure to suburban residence and that when the above events occurred in postwar America, suburbanization resulted as thousands of households were "pulled" out from the city centers by the amenities of the suburbs. This hypothesis explains racial segregation as a function of the extent to which nonwhites were denied access to the means of suburban home purchase, through discrimination by government programs and "redlining" by lending institutions and realtors.

The opposite theory explaining the nature of postwar suburbanization, the "push" hypothesis, implies that the movement to the suburbs was substantially affected by changing social conditions in the central cities. Proponents of this hypothesis charge that white urbanites, in response to an increasing population of members of undesirable minority groups, chose to migrate outward to achieve a more homogeneous lifestyle in the suburbs. They argue that the postwar trend toward suburban development is substantially a result of the large urban influx of ethnic minorities, largely Southern blacks, who migrated to the cities seeking employment in wartime factories.

In this paper, I attempt to examine the "push" hypothesis of suburbanization. Specifically, to what extent did the growing nonwhite population of the central cities motivate middle-income whites to establish residence in the homogenous suburban ring?

Methodology

To answer this question, I first examine the rhetorical arguments which underlie the reasoning of the "push" hypothesis. Upon establishing it as a plausible explanation, I analyze population data for the largest metropolitan areas in the United States to confirm or deny a correlation between the "push" factors and the deconcentration of metropolitan population.

Arguments in Favor

There are three plausible "push" explanations for this suburban flight: the distribution of key government services, primarily public education, the role of status in neighborhood formation, and the nature of political behavior of various socioeconomic classes. First is the notion of shared institutions and governmental services, especially publicly funded schools. The Supreme Court's Brown *v.* Board of Education decision in 1954 desegregated school districts nationwide. Proponents of the public-school explanation argue that following the Brown decision, schoolchildren who once attended classes only with the homogenous population of their urban neighborhood enclaves were suddenly placed in classrooms alongside children from less desirable urban areas. By locating outside the jurisdiction of the central city and its corresponding school district, white families were able to circumvent the Brown decision by placing their children in districts with a majority white population. Using the separate political institutions of the newly forming suburban communities, whites were able to escape the rising tide of integration and establish their own schools, libraries, police, and parks.

There is evidence from the real estate industry to substantiate this claim. Anthony Downs, a senior real estate consultant testifying before the Senate Select Committee on Equal Educational Opportunity, argued that "for most middle-class Americans with school-age children, choosing a place to live is greatly influenced by the nature of the schools serving the various neighborhoods available to them" and that "middle-class Americans . . . typically judge the quality of any school mainly in the kind of families whose children are predominant in its classrooms."[6] By this reasoning, white families discriminate against nonwhite schools not necessarily out of blatant prejudice against nonwhites, but because they perceive schools enrolling unusually large numbers of nonwhites to be suspect or inferior and wish to place their children in more ethnically homogenous schools which they perceive to be superior. This phenomenon can be observed in the case of Park Forest, Illinois, which, prior to 1970, was a largely white, middle-class community south of Chicago. When a black housing development was built on a tract of land outside the boundaries of Park Forest and children from the development began attending local schools, white residents began selling their homes, fearing a drop in land values. As land values dropped, more blacks began to occupy the once-white community and a majority-white area was transformed in the span of a few years.[7]

The second explanation, that of status, depends upon the importance of the perception of status to the middle-class population. This reasoning assumes that the paranoid, status-conscious middle-class resident selects a neighborhood based on the social status it conveys to the rest of society. While the individual may not necessarily be a racist, he acknowledges the correlation between low status and membership in an ethnic minority group that is determined by the rest of the racist society. He chooses not to locate in a neighborhood of nonwhites because to do so would equate his own status with that of the existing low-status residents. This is explicitly expressed by postwar author Gerald Abrams who writes, "It is no longer the type of house but the type of neighborhood which reflects social standing . . . Fine looking homes in Chicago may be still as fine looking but are considered blighted when Negroes or other minorities live in the neighborhood."[8]

The third possibility is that of political behavior. This explanation divides individual assumptions about political institutions into two camps: the public-regarding and private-regarding. This theory, advanced by Banfield and Wilson in *City Politics*, suggests that the older WASP members of American society are descended from a Colonial tradition of a classless society and enlightened self-government by and for the people. These public-regarding individuals favor nonpartisan local government in which concerned citizens act altruistically for the public good. Conversely, "private minded" blacks and the newer immigrants from southern Europe have a fundamental distrust of the political process and view politics as a brokerage of private interests by various competing groups. According to Banfield and Wilson, private-regarding citizens identify strongly with members of their

Table 1. Explaining Urban Deconcentration, 1950-1960: "Push Hypothesis" Model – Sample 1-21
(Dependent Variable:
CON60, the rate of Urban Concentration, 1960)

	Equation (1)*	Equation (2)*
Constant	11.534	8.129
	(2.483)	(2.530)
NW50	-6.590	-6.720
	(-2.544)	(-2.670)
ΔNW(40-50)	0.223	0.207
	(3.996)	(4.190)
LOGMSA50	-1.086	-1.040
	(-2.049)	(-2.189)
LOGINC50	-0.000573	. . .
	(-0.674)	
LOGINC60	0.0000426	. . .
	(-0.0506)	
R^2	.634	.607
SE	.884	.861

NW50: % of City Population Nonwhite, 1950
ΔNW(40-50): Absolute Δ in Nonwhite City Population, 1940-1950
LOGMSA50: Total SMSA Population, 1950
LOGINC50: Median Family SMSA Income, 1950
LOGINC60: Median Family SMSA Income, 1960
*(t-statistics in parentheses)

Table 2. Explaining Urban Deconcentration, 1950-1960: Standardized Regression Coefficients – Sample 1-21
(Dependent Variable:
CON60, the Rate of Urban Concentration, 1960)

	Equation (1)	Equation (2)
NW50	-0.520	-0.530
ΔNW(40-50)	1.723	1.599
LOGMSA50	-0.857	-0.820
LOGINC50	-0.215	. . .
LOGINC60	-0.0158	. . .

own group and see the governmental process as a means for advancing their own interests, while public-regarding citizens identify with the community at large and see government as a means of achieving good for all.[9] The push hypothesis asserts that public-regarding white inhabitants of the central cities, dismayed over the machine politics and contentiousness of special interests which proliferated as a result of the expanding private-regarding (nonwhite) population, migrated to the suburbs to re-establish "good government". Prewitt and Eulau, examining the voting behavior of 80 San Francisco Bay area cities, find that low-status communities tend to have high voter turnouts and high rates of eviction of public officials, while high-status communities tend to have relatively low voter turnouts and high rates of incumbency.[10] This seems to confirm the suspicion of government by low-status, private-regarding individuals, and the relative comfort with government by high-status, public-regarding individuals.

Data Analysis

Thomas Guterbock, in "The Push Hypothesis", develops a measure of urban deconcentration in which he assumes that the geographic distribution of urban population is exponential: population density is greatest at the center and declines exponentially in all directions from the center out to the edge of the metropolitan area. He represents the density at a certain point in the city as $D_x = ae^{-bx}$, where x represents the distance from a city's center and a and b are constants. For a group of 19 Standard Metropolitan Statistical Areas (SMSAs), he calculates the parameters a and b based on available data on population size and land area. From these parameters a and b, he is able to estimate a density gradient for each SMSA, arrives at levels of concentration for the years 1950, 1960, and 1970 and computes a rate of relative deconcentration for each SMSA.[11]

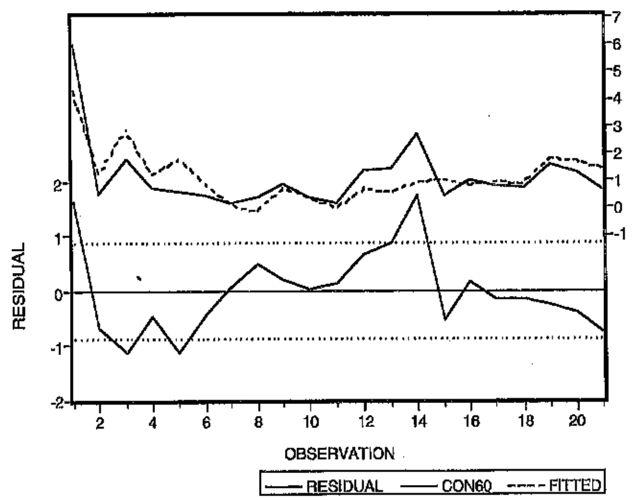
Guterbock's method suffers from two basic flaws. First, it is applicable only to monocentric, circle or pie-shaped SMSAs. This eliminates areas like San Francisco, which have complex geographical limitations that do not conform to his simple geometric definition. Second, it cannot account for areas with more than one central city, like New York-Newark, Los Angeles-Long Beach, or Dallas-Fort Worth. Third, it assumes that the push hypothesis refers primarily to density and distance, rather than political boundary. If one accepts that the decision to migrate to the suburbs is motivated by the promise of independent government, then political location, as well as geographic location, is also significant.

As a measure of urban concentration that incorporates both population density and "political density", I used the measure $(CITY/SUB)^2$, where CITY equals the central city population in a given year, and SUB equals the suburban fringe population in the same year, for a given SMSA. Although actual population densities (*i.e.*, population per square mile) vary significantly across SMSAs – *e. g.*, the city of New York is physically much denser than the city of Los Angeles – this ratio can still provide relative comparisons for political density for a given SMSA. The higher the density value, the greater the preference a given metropolitan population has for its central city, and presumably, the lower the central city's corresponding disutility. This term is squared to attempt to incorporate the concept of physical density. Taking

Guterbock's assumption that density increases exponentially as one moves toward the center of an urban area and that the central city or cities generally lie at the center of a metropolitan area with the suburbs forming an outer ring, the ratio $(CITY/SUB)^2$ will also crudely approximate an area's population density gradient. As the (CITY/SUB) ratio declines, the population becomes more geographically and politically fragmented.

Figure 2

Residual Plot—CON60



To study the effects of the push factors on urban concentration I calculated concentration ratios for the 25 largest SMSAs in 1960, excluding any SMSAs, such as Paterson, NJ, which are classified as part of a consolidated area within another, larger SMSA. Four of the SMSAs, Dallas, Houston, Atlanta, and Miami, were excluded because their central cities annexed heavily over the period, resulting in a high level of central city concentration which may more strongly represent local governmental fiat rather than individual residential choice.

As explanatory variables, I used the following. ΔNW(40-50), the absolute change in the number of nonwhite city residents for the years 1940-50, measures the push effect of the increasing numbers of nonwhites in the central city. NW50 represents the percentage of the city population that is composed of nonwhites in 1950. LOGMSA50, the natural logarithm of the total SMSA population for 1950, represents the effects of relative metropolitan size on concentration. INC50 and INC60 are the real median family incomes, deflated by the CPI for each SMSA as given by the Bureau of Labor Statistics for 1950 and 1960.

For Equation (1), shown in Table 1, I regress CON60 on the explanatory variables NW50, ΔNW(40-50), LOGMSA50, LOGINC50, and LOGINC60.

Only the variables NW50, ΔNW(40-50), and LOGMSA50 are statistically significant. As predicted by the push hypothesis, there is a negative correlation between both the relative size of the nonwhite central city population (NW50) and the level of concentration (CON60). More surprisingly, the correlation with the absolute increase in nonwhite population [ΔNW(40-50)] is positive. This implies the possibility of two simultaneous effects: while a high percentage of central-city nonwhites might correspond with a white exodus to the suburbs, a large sustained influx of nonwhites to the central cities helps to maintain relative density by populating the central cities. Put another way, a city in 1950 with a relatively high percentage but relatively low in-migration of nonwhites tends to have a lower concentration score, while a city with a low percentage but a large influx of nonwhites tends to have a higher concentration score.

To test this hypothesis, one might then calculate a measure of white concentration, similar to the one used above for the total population, and regress it on the above explanatory variables. This would likely produce a negative coefficients for the ΔNW(40-50) variables, but only as a matter of mathematical identity. Since the suburban population remained 95% white over the period, a correlation between nonwhite in-migration and white deconcentration would represent the inverse relation between white and nonwhite population which exists, by definition, within the central city.

Modifying the equation to eliminate the statistically insignificant income terms, LOGINC50 and LOGINC60, does not produce substantial changes, as seen in Equation (2). Figure 2, the residual plot of Equation (1), shows the predictive value of this model.

The relative importance of each of the terms is shown in Table 2, the plot of the standardized regression coefficients. Ultimately, NW50 accounts for less than 18% of the predicted variation within the model and less than 11% of the total variation in CON60, the $(CITY/SUB)^2$ population ratio for 1960.

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Munich Report *continued from page 1*

In session 3, Pedro Lains (Carlos III°, Madrid) opened with an investigation of Portugal's exports in the period 1850-1914, emphasizing the role of trade treaties. Alan Taylor (Northwestern) asked for elaboration of the model. Gianni Toniolo (Rome) and George Grantham (McGill) debated the role of political forces, while Broadberry suggested more attention be given to price effects. Larry Neal (Illinois) highlighted the apparently perverse export-reducing effects of the departure from gold in 1891, and John Komlos (Munich) asked whether low education levels had hindered exports. In the second paper, Giovanni Federico and Antonio Tena (Carlos III°, Madrid) assessed whether Italy was a protectionist country, suggesting that only the 1880s and 1890s witnessed high protection. Taylor and Vera Zamagni (Bologna) requested that more attention be given to individual industries. Knick Harley (LSE) argued that "new growth" theory perspectives be incorporated, since tariffs may affect returns to scale in some industries. Neal and Broadberry raised the issue of whether the dynamic effects of constraining agricultural imports were detrimental. Prados queried whether the use of lire prices would understate protection.

The fourth session offered a twin-set on responses to information asymmetries in French financial markets. Philip T. Hoffman (Cal Tech) and Gilles Postel-Vinay (INRA-EHESS, Paris) presented their research with Jean-Laurent Rosenthal (UCLA) on the behavior of Parisian notaries in making markets for loans in 1751. Questions covered numerous institutional details, probing exchanges of information among notaries, the ability of customers to use loyalty or switching to encourage good service, and the representativeness of the data. The authors observed that the value of notarizing for the enforceability of contracts made it likely that virtually all significant contracts, except for short-term mercantile credit, were in their data source. In the second paper, Pierre-Cyrille Hautcoeur (Rutgers) examined the impact of high agency costs on the financing of French firms from 1890 to 1939. In response to questions, he noted that information asymmetry changed little over this period and that there is no evidence that security exchanges required financial information for listed securities.

Session 5 treated mid-19th-century US monetary history. First, Trevor Dick (Lethbridge and UC-Berkeley) and John Floyd (Toronto) presented their finding that capital flows played a major role in the balance of payments. To Haines's question about how previous studies had missed this fact, they answered that previous models had addressed capital only by tacking on interest-rate differentials, whereas they have employed an asset portfolio model. Jeff Williamson remarked that their story allowed little role for major shifts in monetary institutions over the period. Floyd responded that capital mobility was all that mattered for their mechanism, and that the authors were surprised that the US switch from gold to silver had little effect. Michael Haupert (Wisconsin-LaCrosse), in the second paper, offered an explanation for the level of bank note issue under the free banking regime of 1838-62. Although note issue was highly profitable at the margin, deposit banking was even more profitable, and notes were issued at all largely to show stockholders that their banks' reputations were strong. Several comments suggested that the levels of both note issue and deposits were determined primarily by demand (supply being inelastic), and Haupert generally agreed.

In session 6, Peter Solar (Brussels) spoke first, reporting findings on the birth and death of flax, hemp, and jute spinning firms in Ireland and Belgium in the period 1820-1914, and highlighting the continuity of firms. Timothy Hatton (Essex) recommended using survival analysis to investigate the success of particular firms. James Foreman-Peck (Oxford) and John C. Brown (Clark) suggested more focus on sites and centers of production rather than firms. Greg Clark (UC-Davis) and Federico asked for explanations of variations in firm formation in the third quarter of the 19th century. In the next presentation, James Foreman-Peck and Elisa Boccaletti (Oxford) provided an assessment of French entrepreneurship, arguing that the Second Empire had possessed a dynamic business sector. Neal and Mokyr raised the issue of making inferences from a limited sample, and Zamagni suggested that the case of cotton may not be typical. Grantham and Lou Cain (Loyola) recommended that international comparisons should be pursued.

Session 7 offered discussions of the US Great Depression and the New Deal. Several participants questioned the argument by Adam Klug (Ben-Gurion), in his paper with Eugene White (Rutgers), that railroad shippers' forecasts offer the best available indicator of contemporary business expectations during the Depression. Some participants suggested that the forecasts could have been upper-bound estimates – or strategic behavior – used to assure a sufficient supply of railroad cars, while Taylor and Fishback suggested that prediction errors might be larger in major depressions. Klug responded, in part, that there were procedures to reduce overestimates by surveyed shippers, and that estimates were most accurate for perishable agricultural products, for which the cost of underestimates would be greatest. Broadberry asked for a model of forecasters' behavior, and Klug indicated that he is working at estimating the shippers' implicit forecasting models. In the second paper, Price Fishback and Shawn Kantor (Arizona) presented their preliminary results on determinants and effects of county-level New Deal spending. Sam Williamson (Miami) and Cain asked how New Deal and overall government spending could be differentiated. Andrew Seltzer (Melbourne) and Mokyr requested more focus on political variables, particularly relating to Congress. Hatton inquired whether Federal, state, or local government was being modeled, and Klug wondered if county-level data captures spending for smoothing the cycle.

"And now, it's Paulaner time":
Greg Clark, Mike Haines, Joel Mokyr, Cormac Ó Gráda, and Gunnar Persson

Opening session 8, Loren Brandt (Toronto) presented his research with Arthur J. Hosios (Toronto) on credit markets in early 20th-century rural China. Jeff Williamson asked whether sample data offer enough detail to identify household types for a logit analysis, and Brandt said yes. Joe Ferrie (Northwestern) inquired whether long-term credit relationships could be observed. Brandt noted that in fact all previous contracts were summarized in the one year's observations. In response to a query from Yasukichi Yasuba (Osaka Gakuin), Brandt pointed out that loans were made in both directions between rich and poor. In the next paper, Satomi Kurosu (International Research Center for Japanese Studies) discussed marriage and fertility in 19th-century Japan. Haines wondered whether low reported fertility was due to infanticide or to the common Asian pattern of early marriage and early cessation of child-bearing. Komlos suggested that nutrition could have played a role; Kurosu agreed but added that hard physical labor was a factor as well. Maristella Botticini (Northwestern), Jaime Reis (European University Institute) and others discussed whether richer or poorer people were likely to marry later and suggested more research on cross-sectional variations.

Session 9 opened with Gregory Clark's argument that the enclosure of English common fields in the late 18th century yielded an average return of only 1 to 2% above the risk-free cost of capital, substantially lower than previous estimates. When some participants suggested that this was too low a risk premium, given the wide variance in returns, Clark responded that landowners in general faced similar risks. Clark concluded that land tenure institutions adapted to changing conditions – enclosure occurred when it became worthwhile – and thus had little hindering effect on English *per capita* income. Partly as a result of this, he said, "the English are a people without an economic history before the industrial revolution... In some ways that makes the industrial revolution more serious." Still in session 9, George Grantham applied multiple-equilibrium macroeconomic models to pre-industrial Europe, arguing that long-term expansions and contractions over more than 2,000 years can be attributed largely to endogenous positive feedbacks, magnifying the effects of relatively small shocks. Mokyr suggested that Grantham's story neglects the impact of exogenous and cumulative technological change. Grantham replied that much technological change occurs by a Smithian process of perception due to specialization, a factor in his heuristic model. Nevertheless, Grantham acknowledged that "synthetic" inventions, which he likened to Mokyr's "macroinventions", shift the production possibilities of society; thus we need to discriminate among exogenous technological change, exogenous demographic change, and endogenous processes as sources of secular change. When another participant asked which of many multiple-equilibrium models should be used, Grantham said that these models are all of the same family, and that he seeks to demonstrate that the structural conditions of these models were present in history.

Andrew Seltzer commenced session 10 by considering the internal labor market at the Union Bank of Australia, concluding that senior positions had been reserved for insiders. Haines wanted more information on wage and promotion decisions within the internal market. The issues of comparisons with other white-collar workers in Australia, and with banks elsewhere were raised by Fishback and Greasley. John Howard Brown asked whether bias arose from a lack of data on leavers, and Neal wondered how performance of the bank had been influenced by its internal labor market. In the absence of Irina Garskova (Moscow), Ray Rees (Munich) stepped in to present a paper, written with N.V. Long (Montreal) and Ulrich Woitek (Munich), which develops a model for explaining the age-height profile of slaves. Haines requested an extension to capture gender differences in slave heights. Hatton suggested that more attention be paid to empirical comparison of slave and non-slave heights. Komlos argued the model indeed offered a plausible explanation of the inflexion point in Richard Steckel's data.

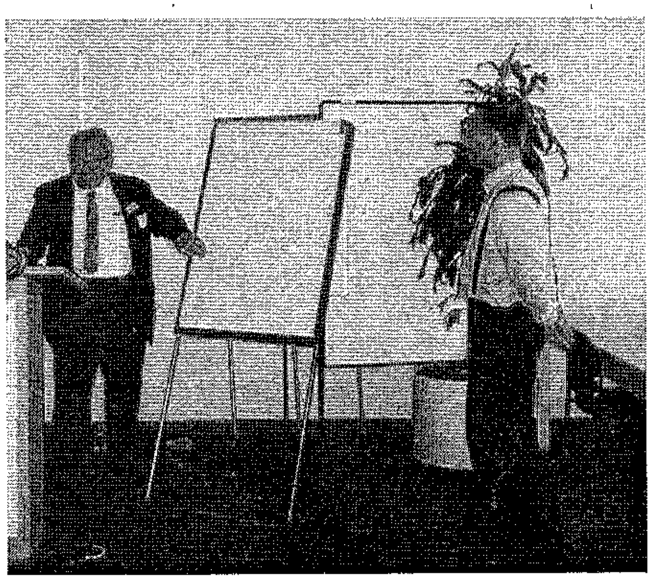
Marc Flandreau and Jacques le Cacheux (both of OFCE, Paris) opened session 11 by considering central bank independence and market discipline under the gold standard, 1880-1913, highlighting rising debt and exchange instability to the late 1890s, and greater stability thereafter. Michael Bordo (Rutgers), argued that high debt might not have caused instability, and that the importance of central bank independence has been overstated. Klug wanted more attention paid to the literature on creditworthiness. Nathan Sussman (Jerusalem) questioned the use of convergence criteria in relation to exchange stability, and Broadberry recommended the weighting of individual European countries to illustrate aggregate stability. In the next paper, Carsten Hefeker (Basel) considered the political economy of fiscal federalism in the German Reich, arguing that distributional conflicts between the states and the center almost inevitably led to default. Hautcoeur and Neal suggested separate treatment of the issues of pre-1914 distributional conflict and of later hyper-inflation and default. Albrecht Ritschl (Barcelona) suggested the German tradition of fiscal sharing had provided a stable tax base.

In session 12, Joel Mokyr and Cormac Ó Gráda (University College Dublin) explained their use of detailed, but highly censored, data from the 1851 Irish Census to estimate specific causes of death during the Irish famine. Part of their conclusion was that about half of excess mortality during the period was due to the famine directly, while half was due to social disruption, largely a matter of loss of "sanitary capital" at the household level. Discussion focused on how well their method deals with various sources of bias, including variations in the extent of under-reporting, emigration, and misidentification of disease. The authors acknowledged the impossibility of precise estimates while arguing that they were able to set useful bounds. Robert McGuire (Akron) argued, in his paper with Philip Coelho (Ball State), that an important, neglected cost of American slavery was the introduction of tropical parasitic diseases from Africa, particularly hookworm and malaria. Fogel asked why southern whites were taller than northern whites if these diseases had been widespread, to which McGuire responded that the shortest southern whites were from malarial regions. Several participants addressed the effects of these diseases on productivity, asking that the authors put this part of their argument in a testable form.

Michael Bordo and Ronald MacDonald (Strathclyde) led session 13, using a target zone framework to assess the gold standard by considering interest rate convergence, arguing that the system was highly credible for France, Germany, and the UK in the period 1880-1914. Marc Weidenmier (Illinois) queried whether changes in banking risks might have impinged on interest rates, and Ritschl requested extensions to capture the effects of shocks on bank confidence. Turning attention to issues of estimation, Marina Sorrentino (LSE) asked whether the ordering of variables was important, Taylor recommended a structure for incorporating all countries, rather than the pairwise approach, and Broadberry wondered whether varying central bank concern with depreciation raised issues of asymmetry. In their paper, Yona Rubinstein (Jerusalem) and Nathan Sussman investigate Gibson's Paradox, using evidence from British consols, 1790-1850, finding that it was not confined to the wartime era, but was even stronger following specie payment resumption. Bordo raised the issue of the exogeneity of money, and its implications for investigating money, price, and interest rate relations. Klug questioned the exogeneity of the regime switches, Mokyr was concerned about the omission of wheat prices when looking at the interest rate-price nexus, and Broadberry wanted more attention paid to the dynamic modeling.

Session 14 treated labor markets in pre-World War I Germany and colonial India. Using Munich health insurance records, John C. Brown finds that the probability of leaving a given job had increased with employment duration, contrary to standard labor-market models.

Discussion focused on the representativeness of the data and secondarily on institutional features of the labor market. Brown argued that the data are biased in favor of more stable employment. Next, William Collins (Harvard) presented his research showing the lack of labor migration and wage convergence in late 19th-century India. Clark urged Collins to develop a fuller model of wage convergence and to consider the role of variable labor efficiency, and Collins noted that he is doing so. More of the discussion, however, related to the role of social factors not addressed in the paper. Collins noted that, because castes were geographically concentrated, moving from one place in rural India to another generally resulted in the migrant becoming an outcaste. Such migration as there was often took place in groups. Initially two men might be sent to investigate an employment opportunity, and if it proved promising a larger group would follow. There was a substantial response to new industrial or plantation labor opportunities. Myung Soo Cha (Yeungnam) suggested that a comparison of the Indian experience to colonial Korea (which had more migration – to Japan – but also low wage convergence) might be more relevant than Collins's comparison to migration within the Atlantic economy.



Robert Fogel comments on Joerg Baten's paper.

Session 15 began with an argument by Lee Alston (Illinois), Gary Libecap (Arizona) and Bernardo Mueller (Brasilia) that the level of violent land conflict on Brazil's Amazon frontier (compared to the earlier US western frontier) can be explained by uncertainty in the establishment and enforcement of property rights. One issue of discussion was rational strategic behavior by Brazilian squatters. The authors said that squatters have increasingly used "invasions" and resulting conflict to stimulate reallocation of property rights in their favor. In recent years squatters have become the best organized political movement in Brazil, and they are expected to be a major factor in coming elections. Broadberry suggested that the study's regression does not capture the regime shift implied by this change in behavior, and Mueller answered that the same model specification applies nevertheless. Fishback and others asked for further detail on the nature and incidence of violence. Continuing session 15, Catalina Vizcarra (Illinois) and Richard Sicotte (Carlos III°, Madrid) examined the effectiveness of the royal Spanish tobacco monopoly in colonial Peru, arguing that high revenues during the "factory system" period of 1780-90 were due primarily to low net incentives for smuggling. Broadberry probed whether scale economies in production could be an alternative explanation, while Jeff Williamson suggested that effective pricing could be part of the story. In response to the authors' assertion that Spanish policy was motivated by revenues rather than development, Lains asked, "Why would anyone think Spain wanted to develop colonial economies?"

J. W. Drukker (Delft) and Vincent Tassenaar (Groningen) opened session 16, reporting declining heights for Dutchmen during early industrialization as urban circumstances worsened. Haines asked that greater attention be paid to rural-urban transmission mechanisms. Mokyr queried the existence of Dutch *per capita* growth, and the existence of the Komlos paradox, observing that the connection between food intake and height is unclear. Clark and Steckel asked about the abrupt height discontinuities, with Fogel suggesting that these might be due to changes in the system of measurement. Joerg Baten's paper followed, with a discussion of nutritional inequality in Bavaria, using the coefficients of height variation (CV) to highlight widening inequality during early industrialization. Steckel wanted more interpretation of the CV measure, for example, its connection to incomes. Fogel thought that the CV may not reflect inequality, and that control variables were needed. Haines asked for discussion of migration's effects on the CV, and Mokyr wanted explanations for the disparate fortunes of the skilled and unskilled.

In session 17, Albrecht Ritschl used archival foreign exchange data to reject several prevalent theses regarding Nazi Germany's foreign trade policies. Discussion participants generally agreed that Germany did not rationally prepare for an extended war by moving toward autarky. Although Ritschl said that data do not bear out the thesis that Germany intentionally reoriented its trade relations toward eastern Europe in general, he did acknowledge Poland as an exception. Tarik Yousef (Harvard) then presented his argument that Egypt could have drawn a large portion of needed government revenues from an optimal export tariff on cotton during the inter-war period, and he explained the lack of such a tariff as the result of established political interests. Most of the ensuing discussion related to technical aspects of estimating world demand for Egyptian cotton. Klug commended the author as a pioneer in the economic history of the Middle East, the "last frontier" for the discipline.

All for me? Larry Neal prepares to partake . . .

Jari Ojala (Jyvaskyla) was first in session 18, assessing the profitability of Finland's sail shippers, finding stagnation and decline in the 19th century. Guillaume Daudin (LSE) suggested that carrying and trading profits be distinguished, and Lains thought a comparison with the more successful Norwegian industry would be useful. Hoffman wondered how openness to competition had influenced profits, while Haupert and Clark asked for elaboration of the links between productivity and technology. In the following paper, Maristella Botticini provided an economic rationale for the 15th-century Tuscan dowry system, using data from 209 marriage contracts. Haines suggested that more attention should be paid to links between family circumstances and sizes of dowries. Gunnar Persson (Copenhagen) wanted further elaboration on how dowries had provided for intergenerational transfers, and Steckel on specific benefits to daughters. Zamagni raised the issue of possible tensions between the econometric results and interpretation of contemporary views.

Myung Soo Cha opened session 19, investigating the integration of rice and wheat markets, arguing rice markets were more effectively integrated between the world wars, but wheat before 1914 and after 1945. Fishback asked why Cha had tried to eliminate the effects of shocks, since, he argued, their comparative effects were central. Federico wanted more explicit tests of market efficiency, and Neal recommended that transaction costs be included. Michael Pammer (Linz) thought that the effect of World War I would introduce monetary shocks, not market integration. Ulrich Woitek followed with a paper on pre-industrial Bavarian grain markets, which, he argued, on the basis of monthly price fluctuations for wheat, rye, barley, and oats, had been highly integrated. Grantham was sceptical of the emphasis on transport improvements, suggesting that organizational cost (*e.g.*, storage) may also have been important. Clark wondered whether the finding for barley indicated the existence of segmented grain markets. Persson recommended graphical exposition, for example to show seasonality, and to assess whether storage was efficient.

Session 20 addressed the economics of migration. First, Joseph Ferrie presented initial results from his census manuscript sample research on US westward migration during 1850-70. Discussion focused on different ways to interpret the Turner thesis that the frontier had functioned as a safety valve and how Ferrie might address these issues in continuing work. For example, Steckel suggested treating miners differently from other migrants, Mokyr suggested looking at the propensity of sons of large families to move, and Sam Williamson suggested a look at westward migration to settled places as well as to the outer frontier. Responding to Yasuba's question on whether frontier migrants had come from the eastern seaboard or further west, Ferrie noted that 18% of the sample's Ohio residents had moved west of the frontier by 1860. Then Simone Wegge (Lake Forest) presented her findings that, in the mid-19th-century, Germans from villages following primogeniture rules of inheritance had been more likely to emigrate than those in villages with partible inheritance. Ritschl noted that this result contradicts the received wisdom in Germany, and he and Baten probed whether she had studied a representative region and controlled for co-varying factors, but Baten concluded that Wegge's region, Hesse-Cassel, was probably a good test case.

Session 21 offered two studies of securities markets prior to 1914. First, Larry Neal presented his research with Lance Davis (Cal Tech) (who had had to depart that morning) on the effects of market rules on efficiency and capital mobilization in the securities exchanges of London, New York, and Paris. Neal characterized these rules as a means of reducing transaction costs and limiting strategic behavior. Mokyr commented that securities exchanges offer a good case study of the evolution of institutions, including the question of whether differences in initial conditions lead to subsequent divergence or convergence. In this case, as Neal had noted, there was convergence. Clark said that the case may provide an opportunity to demonstrate the unimportance of institutions, based on the cost to the economy of different sets of rules. Richard Sylla (NYU) responded that the introduction of ticker tapes and continuous trading by the New York exchange show that institutions do matter. Robert Fogel noted that the recent flourishing of East Asian securities exchanges offers new opportunities for comparative research. Second, Michael Pammer offered his analysis of Austrian investment in Hungarian securities. Klug suggested a statistical test for the role of Jews in securities financing, given Jewish kinship networks between Vienna and Budapest. Pammer said he had tested for this, finding no effect, but that he thought that the Jewish role might have been larger in companies with unlimited liability. Scott Eddie (Toronto) said that much of the business community in Budapest was German speaking.

In session 22, Juan Roses (European University Institute) provided a growth accounting for Catalan industrialization, arguing that the dominant factor was residual productivity growth rather than accumulation. Max Schultz (LSE) remarked on the apparent richness of the data, and asked for elaboration on sources. Grantham thought that the fluctuations in total factor productivity (TFP) seemed too sharp to reflect changes in technology. Foreman-Peck wondered whether explanations of continental industrializing which stress British technology transfer might be undercut by the TFP findings. Yasuba suggested the TFP results could be checked using price duals. John Treble (Bangor) concluded the session with a discussion of Durham miners' productivity, for which he found no effort response to wage rate changes. Fishback queried how teamwork and "putting" affected the possibility of identifying an individual worker's productivity response to wage changes. Greasley raised the issue of the use short-run data to explain the secular decline in coalmining productivity. Clark wondered whether piece rate changes would be known in advance, and if this would have implications for miners' productivity.

Invited Lectures

In the first invited lecture at the Congress, Jeff Williamson argued that the standard understanding of the demographic transition is wrong, because it neglects the age structure of the population, particularly the size of the economically active population relative to young and elderly dependents. Williamson found that age structure alone accounts for half of the late-20th-century East Asian economic "miracle", *i.e.*, half of growth in excess of the long-term rate of 2% per year *per capita*. Looking further back in history, Williamson noted that major migrations consisted largely of young adults. As a result, age structure alone explains about 90% of differences between the Old and New Worlds in growth of GDP *per capita*.

In the second invited lecture, Nick Crafts argued that real GDP *per capita* is a poor proxy for living standards and explored how several proposed adjustments or alternative measures might affect our evaluation of economic performance in different eras. He noted, for example, that by the standard of the United Nations Human Development Index, Britain in 1870 compares well with relatively less developed countries today. Addressing the classic standard of living debate, Crafts argued that the decline in human height during British industrialization should be taken as a symptom that all is not well with traditional measures of economic performance, not as a replacement for such measures. Crafts noted that recently proposed living-standard indexes show general improvement during British industrialization, although results are somewhat sensitive to the weighting of different factors. Comparisons between East Asian countries and Europe prove highly sensitive to the weighting of different factors.

Banquet and Other Events

The conference banquet took place Saturday evening in a restaurant of the Augustiner brewery. In brief remarks, Lance Davis, one of the two organizers of the first Cliometrics Conferences, reminisced about the early meetings at Purdue University in the 1960s. Gianni Toniolo traced the origins of cliometrics to European economists such as Smith, Marx, members of the German Historical School, Heckscher, and Hicks, while crediting Americans with reinvigorating the tradition within Europe in recent decades. Bob Fogel paid tribute to Lennart Jorberg (Lund), recently deceased Swedish economist and member of the Swedish Academy, for his support of cliometrics over more than 30 years. Lou Cain, as master of ceremonies, introduced a new anthropometric statistic: the CAIN ratio, defined as the distance from chin to apex of the head, divided by the distance between the eye (I) and nose. After crediting various scholars present with impressively high CAIN ratios, at least when compared to *Time* magazine's impression of the Roswell alien, he reported the highest value of all for Cleo Laine. Thus, by proxy, our beloved muse Clio reigns supreme.

Congress meeting facilities and two lunches were generously provided by the Bayerische Motoren Werke Aktiengesellschaft (Bavarian Motor Works Co.). Congress participants were treated both to a reception, hosted by the Bavarian State Government at the former royal residence in Munich, and to a city tour, hosted by the Munich tourist office. After the formal program ended, 17 participants and accompanying persons journeyed out to the country monastery Kloster Andechs to sample its famous beer and hear a performance of Vivaldi's *Four Seasons*.

Hughes Prize *continued from page 14*

I see it as my responsibility to respect my students by maintaining proper professional boundaries.

If you've read through this entire essay searching for specific tips for your teaching, this may be the paragraph for you. Think about the teachers you have had. See in your mind's eye the best of the best. What made him or her the best? For me, it is Bill Medigovich, one of my high school math teachers. Medigovich was best because he cared about me as a person, because he brought extraordinary enthusiasm to every math topic, and because he was very organized and prepared. Do you remember any of the material you learned from that person? I bet the answer is "no, not really." The best teachers aren't those who fill our minds with facts we never forget. The best teachers are those who inspire us to be the people we were created to be. Bill Medigovich taught me to believe in myself. He taught me to find joy in learning and in math. He made more difference in my life than dozens of other teachers combined. I have modeled my teaching after the best teacher I know.

Organization takes time but pays off. I plan out the entire semester. The syllabus tells what material I will cover each day. I know when the exams will be and so do the students. Each lecture is organized and easy to follow. I arrive 10 minutes before class is due to begin. An outline goes on the board and into their notes. I learn a few names while I'm waiting for class to start. I relearn the same names the next day. They laugh at my poor memory, but clearly appreciate my efforts. I cajole them into arriving on time and quieting down. Each lecture covers about 12 pages of paper. Having written the lecture out in the previous 24-48 hours, I barely refer to my notes but present a clear lecture. I make lots of eye contact. I smile a lot. I exude enormous levels of energy. I jump and move and use my hands and slide and point and encourage participation. They smile and snooze and take copious notes and somehow learn lessons of life in the process. I say "bless you" when they sneeze. I have fun. I teach the students, not the material.

We each bring our own personalities and life experiences to our teaching. We develop skills. We talk with each other about our teaching, gain insight, solicit feedback. We are engaged in an activity that has tremendous social value and which entails tremendous responsibility. We are helping to create the future. Respect ourselves, respect our students, respect each other. And have fun. There is unbridled joy in the mystery that is teaching.

NBER Report *continued from page 10*

and the average student had to walk an extra 2/10 of a mile each way to reach the assigned school.

Discussion revolved around the two-stage least squares procedure used to estimate the optimal choice of school district size (from the point of view of the school board) for black and white children and around the implications of the findings. Distance traveled per student is estimated based on student enrollment, not attendance. Margo pointed out that, in black schools, larger enrollment may not have translated into higher attendance. Money was saved not only by increasing class and school size but also by discouraging attendance. If longer travel distances reduced attendance, cost to black students of larger districts was not just extra time spent walking, but also decreased educational attainment and future earnings.

In the Friday morning session Household Decisions, Dora Costa (MIT) considered the impact of widowhood on the living arrangements of older women. Since the 1940s, increasing numbers of older widows have been living alone. The improving financial status of these women may account for the change in their living arrangements. Among the elderly, women have higher rates of widowhood than men. Differences in life expectancy account for much of the difference in widowhood rates between men and women today, where, in the past, age differences between spouses were relatively more important. Today, as in the past, older widows are more likely to live in poverty than older married women. The primary program providing support for the elderly between 1940 and 1950 was Social Security Old Age Assistance. Because pension benefits varied by states, Costa is able to identify the impact of Old Age Assistance on the living arrangements of widows. Her evidence suggests that Old Age Assistance has contributed to the increasing numbers of older nonmarried women who are living alone. Costa concludes, however, that even if Social Security benefits were sharply reduced, the fraction of older nonmarried women living alone would not return to the lower levels found in the 1940s.

During discussion, Kantor asked, if husbands were concerned about the welfare of their wives, why were so many older widows impoverished? Wallis asked about the provision of old-age assistance by counties, rather than by states.

The next presentation, by Carolyn Moehling, pointed out that most studies of intrahousehold resource allocation are concerned with the relative contribution of spouses, rather than children, to household earnings. During the first decades of the 20th century, many older children were employed outside the household as wage-earners. Moehling examines how the share of household income earned by children affected the allocation of income within the household. Using data collected by the Department of Labor in its 1917-19 "Cost of Living Survey", she finds that children's contributions to household income affected the distribution of resources.

Much of the discussion concerned questions of econometric methodology. Some participants suggested the variables in the regression equations may have been endogenous, such as working children's food consumption or fathers' wages. Others questioned the interpretation of the regression results as evidence of intrahousehold bargaining.

Sessions ended with a paper by Melissa Thomasson, "Tax Policy, Group Insurance and Health Insurance Demand: New Evidence on How the 1954 Tax Subsidy Encouraged Market Expansion." Beginning in 1954, employer contributions to the health insurance plans of their employees have been excluded from employee taxable income. Thomasson examines the initial effect of the tax subsidy on the expanding market for health insurance. Using data from both before and after the tax change, she finds the tax subsidy generated a 2% increase in the probability that an individual would buy health insurance. In addition, the change in the tax law made it easier for individuals to buy group health insurance. Because of the problem of adverse selection, employment-based health insurance was cheaper than individual health plans.

Thomasson argues that the tax subsidy not only lowered the price of insurance, but also encouraged the growth of group insurance by stimulating institutional change in the health insurance market. Much of the discussion concerned the growing importance of exemptions during the 1940s because of income-tax withholding, and several suggestions were made for the econometric analysis of the data.

Habakkuk Interview *continued from page 6*

Erwin Rothbarth who had been trained at the LSE and was evacuated with the LSE to Cambridge in 1939. We were introduced by an old college friend, and he was a highly intelligent, highly analytical person with a passion for history and a passion for speculating and I would provide facts and he would tell me what they meant. A sort of *histoire raisonée*. We would have talks about the Black Death and its effect on the ratio of the population to the land, and he would develop theories and he would ask me if these were plausible hypotheses. Among the things we discussed was the ratio of land to labour in America. And he published a paper on this, although posthumously. Now Rothbarth was a very brilliant man and a very admirable man too. He helped Pigou and Keynes with their statistics but then volunteered for the armed services – I think on the grounds that he had a special obligation as a German and a Jew to do so. He was killed at Arnhem – a great tragedy because he really was extraordinarily clever and a very nice person. He laid a lot of seeds, intellectual seeds, and when I came back after the war and had to do lectures on comparative industrialisation, the four lectures on America were grouped around the ideas of labour scarcity. I developed the ideas further at Harvard [in 1954-5], where I met a lot of economists so that, when I was asked to lecture at Columbia in 1958, this topic seemed the most appropriate. Labour scarcity hadn't figured very largely in my lectures in 1946, but by 1958 they were a major preoccupation. So, I spent a semester at Columbia, developing the ideas into formal lectures. They were published as the book in 1962.

That book unleashed what has been one of the longest running intellectual debates in economic history. It has been going for 35 years and there are still people thinking about it and discussing it. What's your reaction to this longevity?

Well, I kept up with the literature on labour scarcity after a fashion until 1978, and I delivered some lectures at Princeton, the Jane Lectures, which were revised thoughts, which have never been published because after that I was overwhelmed. I supposed that if culture as an explanation really takes over, I shall end up by thinking that it is all due to the fact that the Americans were democrats, egalitarians, enterprising people living in an enterprising culture. We needn't go further than that!

You're not an adherent to the idea that culture was shaped by labour scarcity and factor availability!

Oh, I am. I am just overwhelmed by recent evidence to the contrary. At least recent evidence by historians I respect, who have taken a different view.

The technology book was in some ways a major departure for you. Indeed, I suspect that a reason that the labour scarcity debate has had such stamina is because it raises some important theoretical questions, rather than just being limited to historical issues.

Yes. I think there were some terrible muddles there. I realized that when I first read Peter Temins's first critique. And there obviously was a body of theory which was considerably more complicated than I had supposed. And I think if I were to do the subject justice, I would have had to devote the rest of my life to it, which I didn't really want to do! Indeed, in the end, I decided not to publish the Princeton lectures because there was so much literature appearing. Partly I wasn't really competent to assess it, and partly it went against the grain and the prejudices of an historian. I think that an historian never loses the sense of the individual and the circumstance, and generalises over large categories with a great deal of diffidence. I find I quite like doing parochial work, the enclosure movement in Little Biggleswade, and I quite enjoy large speculations of the kind which started off with labour scarcity. What I am disconcerted by is the middle ground which involves the hard work, when you take a body of evidence and apply a theory and use the theory to get from the known to the unknown. And that seems to me to involve so many assumptions on the way that it is difficult to take any other person's work without going through the pain and labour of the whole procedure and it is difficult to do it oneself unless one has a firmer grasp of econometrics than I have. So it is partly a lack of technical training, and it is partly a certain emotional resistance to the emphasis on system, which I think is involved in most forms of cliometrics in the strict sense. I think that the mere presentation of statistics, like the article on the first 2,000 steamships, doesn't really count as cliometrics, although I suspect that it was in relation to that exercise that the word was devised. Now that seems to me something one could do without any interest in theory, just an interest in quantitative history. But I think that is quite different from the exercises in American slavery which do involve a simplified version of economic theory and data, known data, to get to the unknowns and which interpret the links by reference to the theoretical model. That is something which I think doesn't come easy to an historian and wouldn't come easy to an historian even if he were a better master of the tricks of the trade than I am. But until '78

I did try to be. And I do find Fogel and Engerman enormously fruitful, partly because the prose is lucid, even though some of the calculations are beyond me and also because they do throw out problems. And also because they make one realise the extent to which when one writes qualitatively, one is implying quantitative estimates which one couldn't in fact defend.

More recently, you have returned, as it were, to your academic starting point – the land. Perhaps you would like to tell us what you are currently working on.

I have finished landowners for the moment. I am interested in the rate of interest and the price of land in the Middle Ages. This is a subject which I was interested in as an undergraduate, after a fashion, and I have taken it up again. I think it is the sort of subject where a person with my rather primitive theoretical apparatus can ask questions which a well-trained medieval historian wouldn't necessarily ask but which would be commonplace to any student of asset markets at any later period. And the hope is to say something which is interesting in new ways and which will raise new problems which will enable people to interpret evidence in a different way.

And is there is one particular overriding problem which you have been trying to straighten out?

Yes. What set off the problem was what set off my problem in 1935 – that land was valued at 10 years' purchase in 13th-century England and 20 years' purchase by the mid-15th century. And one might suppose that so marked a change in the valuation of the major asset must have deep causes and possibly deep consequences. Or at any rate, that an attempt to explain why it happened would shed interesting light on the main economic variables. This is a major change and I hope to compare it with similar transactions on the Continent. There was a very large body of literature produced by the German historical economists and by the German historical lawyers in the 1880s and 1890s on the return on the sale of rent charges. They were interested in the problems of usury in the canonist doctrine and they were not interested in the economics of it, but the raw material lends itself to asking economic questions.

You have not yet reached a resolution to this particular paradox?

I have, but it is so complicated that it is not suitable for verbal explanation!

Let me ask you one question about your 1994 study of land ownership. Am I right in thinking that you consider this your *magnum opus*, the most important of your books?

Yes, most certainly.

Would you like to explain what you think its significance is?

No, I'd like to defend it! My candid friends say, why the hell are there so many examples and why are there so few statistical tables? And this is a charge I can't rebut. The formal defence is that landed families differ enormously in their history. And it takes a great deal of time to get the history of even one family – its financial history, its legal history, its demographic history – straight. When you have studied a number of families in detail, you get hunches. It's a process of subconscious inference. You get a hunch as to how things happen. You don't feel that you know enough about a sufficiently large number of families to use them as units in a table, so the writing becomes essentially descriptive, with illustration. And I don't think it is possible at this stage to do anything else. But it must be said that my choice of evidence is partly dictated by a dislike of statistical tables. There is information available about land ownership in the 18th century which does lend itself to statistical tables. There is all the land tax evidence. And I did look at land tax evidence when I started in the 1930s. That can be put into tabular form and deductions drawn and indeed a splendid Canadian historian, [Donald] Ginter, has done just that. He has produced an enormous work. And he has done it no doubt partly because he had research assistance in the way someone working in England certainly before 1940 would not have. But partly also because he is not deterred by statistical tables. I went and looked at the archives of the individual family rather than the aggregate statistics and that is partly a matter of my training as an historian, but it is partly a cultural matter.

Particularism of this sort surely doesn't preclude your coming to general conclusions.

Oh, no, I've drawn general conclusions. What it does preclude is a really satisfactory demonstration that they are the right conclusions. This is not merely a matter of historical training. It is true of English economics and particularly of Cambridge economics in the period when it influenced me. The most influential economists were not interested in large-scale empirical research on the

National Bureau model. And allegedly Keynes was not sympathetic to Tinbergen's econometric work. They concerned themselves with a relatively limited range of established facts, and devoted a considerable amount of intellectural effort to discovering and analysing why on this basis the system worked as it did. The intellectual habits which my Labour Scarcity essays betray are much more those of Cambridge economics than they are of Cambridge history.

The Cambridge applied tradition has always been more macro-economic rather than micro-economic and I suppose that the cliometric revolution when it began was essentially micro-economic rather than macro-economic in orientation, though it did later change. You earlier mentioned Dennis Robertson's study on industrial fluctuations, and that was a fairly broad piece of applied work and so too was Pigou's book on industrial fluctuations. So there is a case that some Cambridge economists were prepared to take a broader brush, but once again from a macro-economic or trade-cycle perspective, rather than a micro-economic.

And also there is an absence of the formal statement of propositions and then the statistical proof of their truth. Robertson's book is enormously detailed and extremely useful to an historian. But the conclusions are suggested by theory rather than formally demonstrated. His methods of argument were literary and qualitative, not theoretical and quantitative.

So this aspect of the 'new economic history' was not especially new to you?

No, I think that what is new is not the use of statistics and not even the use of theory. I think the real intellectual revolution was this association of very high-powered models being used to deduce the unknown from the known, or to measure the unknown, and I don't know what the origin of this is. But these ideas are certainly the main change in my lifetime. And I was 50 when it really hit me and it hit me too late to do anything about it.

Ah, now that raises an interesting question. If you had been born 25 years later, do you think that your career might have unfolded differently?

No, because I think that three years doing history at Cambridge shapes one so. Even when Postan, who was a great systematizer and enormously stimulating, was lecturing, the approach was fundamentally narrative – in terms of the development of institutions or of constitutional change, or in terms of political episodes. And that does produce an emotional reluctance to exert the effort necessary to acquire the tools to do cliometrics in the Fogel-Engerman tradition, or even to understand them fully. My impression is that I followed the cliometricians reasonably well for the first 10 years and then lacked the appetite and the capacity to continue. I certainly recognised while reading them that they had altered the world, dramatically. And they had the enormous advantage in having as their sort of leader, Fogel, who writes in a very attractive way and does his best to be intelligible to people like myself who don't practice, and manages to reach out. Just look at *Time on the Cross* – the enormous impact this had on general American history.

I think too that my choice of original research subject confirmed my disabilities, because it is not a subject which lends itself very easily to models based upon rational choice and because it is bound up with social prestige. It's hard to escape the conclusion that land is the most uneconomic of all commodities, at least when it is in the hands of an aristocracy. So it is the furthest removed from rational calculation. If I had gone to do banking, I think I might have felt differently. At the end of 1938 I applied for a Henry Fellowship to go to Harvard to study railways. My instinct was right and the project was killed by John Clapham, who being a very honest man said, 'I am one of the electors, I am not going to support you. I think that this is infirmity of purpose. I think you ought to sit in one place and finish your work on Northampton landowners. That is what you were elected to do and it is what you ought to do'. And, of course, he was perfectly right. It was frivolous of me. But it was the right instinct. And my thinking would no doubt have been very different today if it had happened. But there it is.

Indeed. Well, thank you very much for the interview.

Thank you.

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White Flight *continued from page 19*

Conclusions

The model tends to support the assertions of the push hypothesis. While the effect of nonwhite population is relatively minor, it is nonetheless present. The reasoning behind the push hypothesis is compelling, but it clearly does not fully explain postwar migratory patterns, nor does it address the assertions made by the pull hypothesis. Indeed, it is entirely possible that nonwhite concentration is simply a result of large-scale white suburbanization, not the cause. Even if we interpret the model as causal, the effects of the push factors are relatively small.

Additionally, the model is incapable of controlling completely for metropolitan growth. Because of the definition of SMSA population (City + Suburb = SMSA), it is impossible to include the 1960 SMSA population as an explanatory variable, thereby controlling for urban growth. It may be that many areas deconcentrated simply because the central cities failed to annex while large numbers of people flowed into the large SMSAs.

These flaws aside, there is merit to the push hypothesis at some level. While the effect may or may not be huge, it likely did exist. In the face of increasing fragmentation between neighboring municipalities, it provides a framework for understanding the current conflict between cities and suburbs as a function of the underlying reasons for which they exist in the first place. By accepting the idea that elements of racism have played a part in community formation, we understand that we must first address those lingering elements. Only then can communities trust each other sufficiently to cooperate and tackle the more mundane regional planning problems of transportation, air quality, taxation, and public services.

Endnotes

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[2]Ibid., p. 241.

[3]Ibid., p. 236.

[4]Ibid., p. 241.

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[12]The total population and concentration scores were calculated from metropolitan population data in U. S. Bureau of the Census 1960, *Census of Population*, Vol I, Table 33. The nonwhite city population figures for 1940 and 1950 are located in U. S. Bureau of the Census, 1940, Vol II, part 1, Table 86, and 1950, Vol II, Part 1, table 50, respectively. The income data for 1950 and 1960 are taken from U. S. Bureau of the Census, *Census of Population*, Vol I, part 1, table 148 and are deflated by the CPI's for all prices given for the respective cities in U. S. Bureau of the Census, *Statistical Abstract of the United States*, Washington, 1974, No. 667.

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