The Newsletter of

The Cliometric Society

Fall 2001 Vol. 16 No. 3

American League Park, NY c1910

Report on the Economic History Association Conference

By Pamela Nickless, UNC-Asheville, and William Craighead, Virginia

(Philadelphia) The Economic History Conference met in Philadelphia, October 26-28, 2001. We arrived in the birthplace of liberty eager to renew old acquaintances and welcome newcomers to the profession. The attendees expressed their thanks to the local arrangements committee of Daniel Raff, Farley Grubb, Lynn Hollen Lees, Joseph Mason, Cathy Matson, Scott Redenius, and Robert Wright for all the trouble of rearranging our postponed conference. The program committee (Ann Carlos, John James, Angela Redish, and Hugh Rockoff) also labored long and hard to recreate the program. Of course, none of it would have been possible without meetings coordinator Marty Olney and her able assistant, Elizabeth Cascio. President Dick Sylla and executive director Tom Weiss also deserve our thanks. for helping to create a very fine and timely conference.

The stage was set for the conference theme, "Finance and Economic Modernization," at the plenary session on "The Federal Reserve and the Financial Markets" with **Henry Kaufman** (Henry Kaufman & Co). Kaufman began his address with the observation that the recent terrorist attacks have thrown into sharp relief the need for more education in economic history. He lamented the lack of historical perspective, not only of what war is about but also on the state of the economy. A decade-long, euphoric boom has left us with little understanding of bad economic times. Kaufman proceeded to recount in a lively fashion the policy of the Federal Reserve and the relationship of that policy to financial markets. He reminded us that in the 1960s there were few Fed watchers, and the markets did not hang on William McChesney Martin's every word. (The only Billy Martin who was a household word back then played for the New York Yankees). He expressed three concerns about current central bank policies: the problem of securitization and the ability to define money and, hence, benchmarks for monetary policy, the impact of the wealth effect, and the problem of defining lender of last resort responsibilities. He warned that even the Fed must ask if their policies contribute to financial concentration. In the question and answer period, Anna Schwarz (NBER) asked Kaufman if he thought that the monetary aggregates, which are now rising at unprecedented rates, foreshadowed an inflationary surge. Kaufman

(Continued on page 17)

What's Inside

Executive Director's Notes

Greetings Gentle Members:

As required by the bylaws of the Cliometric Society, I hereby submit the annual report on the Cliometric Society. We currently have 428 members in our database, roughly 75% who renew on an annual basis. The rest renew on a basis not easily identified by the bean counters here at the home office. Still, herein you will find the Society's budget, as approved by the trustees, and once again we should be within epsilon of being able to pay our bills.

The Fall Newsletter reminds us that the next big event on the Society's calendar is the annual meetings of the Allied Social Science Associations in Atlanta, Georgia, January 4-6, 2002. Kyle Kauffman and Price Fishback have put together an excellent program, and I hope you can attend one or more of the sessions.

This year, the North Carolina State University Department of Economics will be hosting the annual Cliometric Society Reception. The reception will be held in the NCSU suite in the Hilton Hotel from 8:00pm-11:00pm on Saturday, January 5. The suite number will be announced at the sessions, and it will be posted on the announcement boards in the Hilton. I look forward to seeing you there.

Lee A. Craig
Executive Director

2001 Calendar Year Budget Report

Budgeted Income	
Dues – Regular	$7,200
Dues – Students	125
EEH Subcriptions	13,795
EHES Dues Collected by Clio	4,095
Donations	1,850
Interest and Other Income	250
Credits Pending	N.A.
Total Income	$27,315
Budgeted Expenses	
Salaries	$3,000
Newsletter	4,000
Academic Press (includes debits pending)	13,795
Clio Member Dues Remitted to EHES	2,925
IEHA Dues	175
Director's Travel and Trustees Meeting	1,000
Bank and Credit Card Fees	450
ASSA Meetings	1,000
Establishing the North Carolina State Office	N.A.
Office Expenses	500
Other Expenses	N.A.
EH.Net Dues	500
Total Expenses	27,345
Surplus	-$30

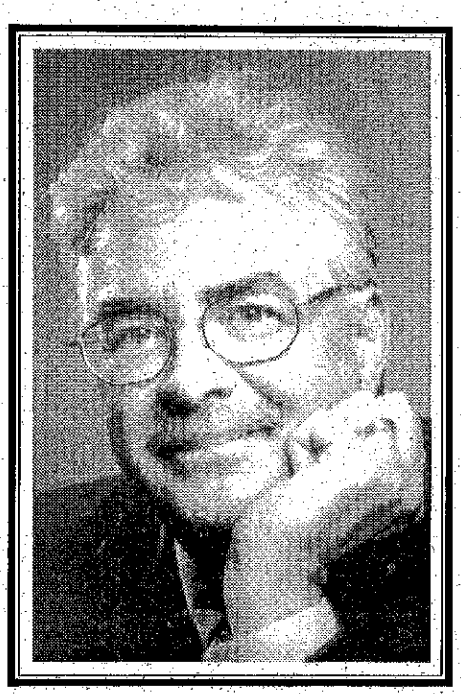
THE CLIOMETRIC SOCIETY
Department of Economics
Box 8110
North Carolina State University
Raleigh, NC 27695-8110
USA
E-mail: csociety@eh.net

The Newsletter of the Cliometric Society
Is published three times per year.

Michael Haupert, Editor
Jean Bonde, Managing Editor
Mary Beth Combs, Associate Editor
Pamela Nickless, Associate Editor

An Interview with Richard Sutch

Richard Sutch is currently Distinguished Professor of Economics at the University of California – Riverside. He is also a Research Associate at the National Bureau of Economic Research and the US representative to the Executive Committee of the International Economic History Association. After receiving his PhD from MIT in 1967, he served as Professor of Economics at the University of California – Berkeley from 1968 to 1998. In 1986 he was visiting professor at Cal Tech and in 1989 was elected President of the Economic History Association – the youngest scholar to have been so honored. His seminal work, ***One Kind of Freedom*** *(written with Roger Ransom), has the unusual distinction of having been the subject of two academic conferences held more than 20 years apart: at Duke in 1978 and at Lehigh in 1999. He also managed to survive intact the searing experience of chairing my dissertation. This interview took place via e-mail and telephone in October 2001 and was conducted by Tony O'Brien (Lehigh University).*



How did you first become interested in economics? Were you an economics major as an undergraduate?

I grew up in Richland, Washington, headquarters of the Hanford Atomic Energy Works, where plutonium was manufactured. I was a science buff and founder of a high school amateur rocket club that had quite a bit of success and post-sputnik publicity. The Richland Rocket Society successfully fired the first two-stage rocket built and launched by an amateur group. With that background, no close decision was required to choose physics as my major when I moved to Seattle and enrolled at the University of Washington.

As a sophomore I was looking for an elective and, with the advice of a friend, took the introduction to economics. Economics was a delightful discovery. Physics (as taught at Washington) was easy, a trifle boring, and disconnected from the vital issues of the day, except for its intimate and uncomfortable relationship with the terror of nuclear bombs and ballistic missiles. Economics was about people (not particles) and about significant issues like poverty, unemployment, and economic growth as opposed to manufactured problems concerning the movement of weights tied to springs. Economics also relied on theories appealingly grounded in intuition and common sense instead of the inference of unseen forces, quantum discontinuities, and masses conjured without reference to common experience and shockingly unintuitive. Economics appealed to me, although it was challenging (I got a "C"). It appeared to be a science that employed the same sort of detective work, careful measurement, and interpretive

theorizing that had made rocket science exciting. *And*, it promised to save the world. Also, it was 1961. Kennedy had taken office. His cabinet was full of PhD economists; his campaign and his first year in office were dominated by the promise of the "new economics" (that meant discretionary Keynesian macroeconomic policy, structural labor policy, and a glimmering of attention to the problems of poverty and racism). Economics was hot! I switched majors before the term was over.

You attended Douglass North's seminar in economic history at the University of Washington and out of that seminar came your paper on the profitability of slavery. Having a professional paper published as an undergraduate is a great rarity. Can you talk about how that happened?

At UW I enrolled in summer school classes and during several quarters I took overloads. As a consequence, I fulfilled all of my graduation requirements by the end of the first quarter of my fourth year. I had been admitted to MIT for PhD work and was very impatient to begin, but they didn't want me to come immediately in January. I asked Charles Kindleberger, the MIT graduate advisor that year, what to do with my time until the fall. He warned against theory courses. MIT would have me take them over again when I got to Cambridge so "everyone in the class would have a shared background." He suggested graduate courses not offered at MIT. When I asked which fields those might be, he replied "economic history." Since Walt Rostow was on leave advising Lyndon Johnson, I enrolled in Douglass North's seminar.

The other students had topics they were exploring for dissertation work, so I, who had no such ambition, asked Doug what I might present. He suggested I review a recent paper on the profitability of slavery that had appeared in the *Southern Economic Journal*. After I made my presentation, he encouraged me to write it up (as a substitute for a senior thesis I avoided by premature graduation). When I finished, he insisted I send it to the *SEJ*. The editor was Robert Gallman, who responded immediately in an extensive letter with suggestions for revision and a request for an expansion! So I spent the summer of 1964 rewriting the paper.

As a graduate student at MIT, I believe you attended Alexander Gerschenkron's economic history seminar at Harvard.

MIT and Harvard had a cross-registration agreement that allowed a student at one university to take courses at the other. North and Gallman had stimulated my interest in the "New (at the time) Economic History." Out of the blue, Robert Fogel had also written to me a letter of congratulations and encouragement to continue with the field when my *SEJ* article appeared. MIT had no economic history seminar, so I simply appeared at the Gerschenkron seminar unannounced at its first evening meeting. Professor Gerschenkron was far from delighted at my presumption, but he allowed me to stay for the meeting. Everyone else had read Neff's book on the *Coal Industry*, which was discussed with great intensity. Without having read the book and knowing nothing about coal in England, I remained silent. At one point, however, Donald McCloskey made a remark that referred to Okum's Razor. It was a concept as foreign to me as the style of this strangely aggressive seminar, which was like verbal mud wrestling while sitting in suits and ties. My only seminar experience had been with North's, where the polite discussion with friendly questions and helpful responses hadn't prepared me for the Harvard style. Seeing my opportunity to both contribute and learn, I

asked, "What is Okum's Razor?" Gerschenkron asked McCloskey to indulge my "innocent request." I gather without his intervention, McCloskey would have naturally assumed my question was not innocent. Don rose and quoted, no doubt accurately, in Latin what Okum had written. No translation or explanation was forthcoming. I kept my silence for the rest of the meeting.

At the end of the evening, Alex asked me to stay, translated Okum, and quizzed me about my motives and background. He warned me against continued attendance on the assertion I was likely to be unprepared and misinformed. I said I would like to give it a try. In that case, he said, I should make a presentation the following week. I was to review Albert Fishlow's forthcoming book on railroads for which he supplied the page proofs on the spot. When I accepted, I was unaware that Fishlow was a student of Gerschenkron, that the book was Albert's dissertation, and that the thesis had won the David A. Wells Prize. Whatever I said the next week must have worked. Alex admitted me to the seminar and thereafter treated me very warmly; but as I was soon to learn, he also used me as a foil (armed with MIT theory and an allegiance to quantitative methods) to inspire more rigor and precision from his Harvard students.

Did the seminar have a formative influence on your later work?

Three things stand out. First, I gained a deep respect for the conscious and explicit effort to use economic theory and quantitative methods to shed light on history. This was accompanied by a suspicion of the use of history to establish the superiority of one economic ideology over its rivals: economics should be in the service of history, not history in the service of economics. Second, I discovered the value of keeping historical analysis simple and clear. The seminar was a dialogue with the objective of mutual persuasion. If communication and persuasion are the goals, then the best theories to use, or at least to begin with, are the elementary principles of economics. Begin with simple descriptive statistics and exploratory data analysis before moving to elaborate econometrics (only when necessary). Dazzle with ideas, not footwork. Third, I learned good writing, clarity, and honesty are essential ingredients in writing economic history. No written paper, no book, could have been as seductive, captivating, and impressive as participation in that seminar. Thus, extra effort would be required to transfer good seminar ideas and careful research to the printed page.

Although you attended the Gerschenkron seminar, you ended up writing a macroeconomic theory dissertation with Franco Modigliani. Had you given thought to writing in the area of economic history?

Working with Franco Modigliani was my intention from the beginning. Franco had visited the University of Washington the summer between my third and fourth years. I attended his seminar on Monetarism versus Keynesianism (rules versus discretion) and worked with him as an unpaid research assistant. He offered me a paid position when I went to MIT, and he and I set out almost immediately after my arrival to imagine a project that would construct a multi-equation, econometrically estimated structural model of the macroeconomy. This ultimately became the famous MIT-FRB model.

A number of other MIT graduate students in my class (including Robert Hall and Robert Gordon) worked on the project. We wrote a

computer program to run regressions. Bob Hall wrote the matrix inversion routines; I wrote the input-output interface. We called it TSP (Time Series Processor), which is still around today in a highly improved and expanded form under both that name and, for the PC version, as EViews. We formulated and estimated numerous structural equations that were later embedded and linked together in the full model. My contributions were on the price-wage mechanism and, in what became my dissertation, the term structure of interest rates. These were exciting times as the mainframe computer, still relatively new to economics, made the required computations feasible. With this power in our control, we were making new and important empirical discoveries every day.

I don't recall considering a dissertation in economic history. Peter Temin arrived to join the MIT faculty during my second year, and by then I was already hard at work on the macroeconomic model. I worked with Peter when he was writing his paper on the Habakkuk controversy, and I included a historical chapter on the movement of interest rates during the Great Depression in my dissertation. But I still considered economic history a hobby I pursued in the evenings at Harvard rather than a part of my graduate training in economics.

You received a job offer from Berkeley before you had even chosen a dissertation topic, much less actually finished your thesis. Can you talk a little about what the academic job market was like during those years?

"Those years" were 1966-1968. The first of the baby boom cohorts were approaching college age. The Kennedy-Johnson economic boom helped make colleges more affluent. Students and their parents found college more affordable. Consequently, the demand to expand college faculties was great. Yet the supply of freshly minted PhDs to meet this demand was limited by the intake of students into economics programs in years when the economy had been weak and college teaching did not appear particularly attractive as a profession.

Interviews began when I was in my second year. Recruiters from the major departments came to MIT. That year I recall being interviewed by Berkeley, Princeton, and Columbia. Alan Enthoven from McNamara's Defense Department dropped by to chat about quantitative analysis, large-scale model building, and computer forecasting. In my third year, seven of us at MIT were surprised by a joint offer to join the department at SUNY Buffalo. We were offered 50% more than the going rate (that came to $14,000 per year), with the additional pitch that we could keep our MIT team together and could jointly "run the department." Perhaps as a response to this audacious offer, a feeding frenzy began. I don't recall having filled out a vita or writing a letter of application. I received offers from Princeton, Chicago, and Berkeley. I rejected feelers from Harvard (the story was that you couldn't get tenure there), Columbia (I forget why), my alma mater, and several other departments, as well as the Federal Reserve Board of Governors and Morgan Guarantee Trust. I was by no means the star of the department (they were Bob Hall, Joe Stiglitz, George Akerlof, Robert Gordon, and several others). I hadn't even begun my thesis. I accepted the Berkeley offer, although I asked to start in January 1968 rather than in the fall of 1967, so I might finish (and begin!) my dissertation. When I arrived in California in December 1967, I was surprised to find six monthly paychecks for July through December waiting for me. I had thought I was asking for a postponement of the appointment. Berkeley assumed I was demanding the first

half-year on leave with pay. Whereas in the world of real estate it is "location, location, location," I guess we might say in the academic job market it is "timing, timing, timing." I was lucky to come into it when I did.

You were hired at Berkeley as a macroeconomist. Was there an expectation on your part or their part that you would also be doing economic history, or did that come later?

The switch to economic history came a bit later. When I arrived Albert Fishlow was on leave, and Roger Ransom, visiting from the University of Virginia, had been hired to cover his courses. Roger's responsibilities included the graduate seminar in economic history. I was asked to sit in, both to encourage enrollment and to help provide a bridge between Roger and the future for any student who decided to write in the field. I had known Roger from the University of Washington, where he had been a graduate student. I even took a course in economic development from him during one summer.

Did you and Roger stay in touch in graduate school? Was it coincidental that you both ended up in the UC system (with Roger at UC-Riverside)?

Although I knew Roger and took a class from him at the UW, we were not close friends at the time. We didn't communicate again until I arrived in Berkeley. There were only a few students in his seminar, and the sessions often turned into a discussion between the two of us. I think that was how our real friendship began – when we discovered our mutual interests and complementarities. Meanwhile, the Riverside campus was starting an economics department, and they recruited Roger. I'm pretty sure that they could have and would have done that even if Roger had not visited Berkeley. I don't think I had anything much to do with his decision to accept the Riverside offer.

Can you discuss the origins of the *One Kind of Freedom* project?

Roger and I discussed the story behind *One Kind of Freedom* in the Preface to the *Second Edition* that was just published by Cambridge University Press. The Southern Economic History Project (as we called it) had its origins in the seminar that Roger led and I attended. The job market was different in those days as was the grants market. Jim Blackman of the National Science Foundation visited Berkeley and offered a grant if we could come up with a proposal. The first version was rejected as poorly motivated and not ambitious enough, so we rewrote it. In 1969 we were funded with the first of several grants that financed the project to its completion in 1977.

During the time you were working on the *One Kind of Freedom* project, you became involved in the controversy over *Time on the Cross*. Looking again at the *Reckoning with Slavery* volume, it's amazing the amount of time and energy that you and your coauthors put into minutely examining the details of *Time on the Cross*. Can you discuss what there was about that book that brought about such a reaction?

When *Time on the Cross* was scheduled for publication, I was invited to attend a conference at the University of Rochester in October 1974. It was to be a critical first look at the book. I had written a paper a few years before on the breeding of slaves for sale that was challenged by Fogel and Engerman in the course of their argument. When their manuscript arrived, I naturally scrutinized their criticism of my work (which was only a small part of their overall argument and

occupied only a few pages of *Time on the Cross*). I was so surprised at the errors I found, I began to look at other pieces of their argument. It was shocking. I found every aspect I looked at flawed and riddled with one-sided errors, misstatements, and misinterpretations of the work of others. Frankly, I was horrified, not so much by the errors (we are all capable of error), but by the accompanying claim that scientific history, econometric history, and the New Economic History had falsified years of diligent and voluminous scholarship by "old style" (and presumably obsolete) historians. This was not a multidisciplinary dialogue; this was an assault. The errors, I knew, would render the controversial findings of *Time on the Cross* dubious. With the findings in doubt, this arrogant boast, however, would expose the entire cliometric enterprise to ridicule. The only way to defend the New Economic History was to demonstrate the power of our tools to easily, quickly, and decisively catch and correct our own mistakes.

It took me two months to write the paper I presented in Rochester. When it was published it was over 100 pages and took up most of the October 1975 issue of *Explorations in Economic History*. I focused only upon the issues surrounding the care and feeding of slaves. When I arrived in Rochester, I discovered that my reaction was not unique. David and Temin and Gavin Wright tackled the issues of the productivity of slavery. Others brought papers that dismantled other parts of the Fogel/Engerman argument. The conference was more of a rout than a critique.

Fogel and Engerman's opinion of the whole controversy today is that, although they were wrong about many of the details, they had the big picture right. What are your own thoughts on this?

I wasn't aware that Bob and Stan make that claim, although I haven't read the lectures Bob recently gave on the slavery debates at LSU. If the ten points they listed in the opening pages define the big picture of *Time on the Cross*, only those concerning the profitability and viability of slavery have stood the test of time. And those were the contributions of Conrad and Meyer, Yasuba, and my own undergraduate paper that we discussed earlier. The points concerning the relative efficiency of slavery rest upon a definition of efficiency that is not widely accepted and that is insufficiently nuanced for my taste. On the welfare of slaves (the topic I took on), Fogel and Engerman have largely conceded the ground, either explicitly or by silence. Wait a minute. Perhaps, I misinterpret. Perhaps those ten points are the many details that Fogel and Engerman admit they got wrong. Perhaps the big picture of *Time on the Cross* was the power and the promise of cliometrics. On that issue, of course, they were right. But the power of the New Economic History was proven, not by the claims and arguments of *Time on the Cross*, but by the book's cliometric critics. The promise was proved, as Fogel and Engerman I am sure would agree, by the research that came after the controversy. That research was on topics that went beyond slavery to touch every other major story that is part of our economic history.

Do you think that the *Time on the Cross* episode to some extent soured conventional historians on the cliometric approach?

I think it did lasting harm despite the most earnest efforts of many of us to offer a corrective, to explicate the cliometric approach, and to join our colleagues in history departments in a two-way dialogue.

Do you think that the reception of *One Kind of Freedom* might have been

damaged as a result? That is, would it have been more widely read and cited by conventional historians if it had not been preceded by the *Time on the Cross* controversy?

I suspect not. *One Kind of Freedom* was widely read and cited by conventional historians and was frequently praised in reviews by historians. In the *Second Edition*, Roger and I include a list of over 170 references to the post-publication literature. A healthy majority of those citations are from historians. A retrospective review by historian Peter Coclanis published last year in *Reviews in American History* cites its continuing influence on historians qua historians. Coclanis explicitly makes the opposite point: the *Time on the Cross* controversy, he suggests, focused historians attentions on *One Kind of Freedom* as a methodological and ideological counterweight. Again, timing seems important, and Roger and I feel we were lucky to see our book arrive in the midst of methodological controversy.

After *One Kind of Freedom*, you and Roger moved away from African-American economic history. There is a lot of interest today in the links between what happened in the rural South in the late 19th century and what happened in the urban North in the 20th century. I know that you and Roger were motivated, in part, to undertake the project because of its implications for the civil rights struggle of the 1960s. Did you give any consideration of trying to tell that part of the story yourselves?

Initially we planned to research that era as well. However, we quickly decided to leave that story to others. We were not disappointed. Excellent cliometric work by Warren Whatley, Robert Margo, Gavin Wright, Bill Sundstrom, and many others have made significant contributions to that topic.

When did you begin attending the Clio conferences?

I first attended in 1970. I think that was the first Clio conference held in Madison. Roger and I gave a paper entitled "Tenancy, Farm Size, Self-Sufficiency and Racism: Four Problems in the Economic History of Southern Agriculture, 1865-1880." It represented the first results based on our sample of southern farms, which we intended to expand into four separate papers for eventual publication. There was a lively discussion at the time, much of which centered on the interconnectivity of the four issues. I think it was there that Roger and I convinced ourselves that we needed to write a book uniting those four topics. So we abandoned the plan for a series of papers. Thereafter, I attended the Clio meetings very regularly for about 20 years. In 1985 I became one of the founding trustees of the Cliometric Society, and in the following year I was "canned" with the Clio Award.

How did you come to start the projects on the history of saving and the history of retirement? Were the implications for current macro policy an important motivation?

In 1984, I was invited to apply for a Guggenheim Fellowship. I had been teaching the graduate course in macroeconomics since coming to Berkeley and had, of course, been following the debates on macro policy that accompanied Reagan's presidency. One of those debates concerned the interest elasticity of savings. Reagan had based part of his supply-side policy on the argument that high real interest rates would stimulate saving, generate a flow of loanable funds, and thus

stimulate the economy. Of course, I had been taught by Modigliani that savings was likely to be interest inelastic. So I proposed to the Guggenheim Foundation that I look at the history of saving using the life-cycle model of savings as an organizing principle. With the fellowship in 1984-85 and as a visitor at Cal Tech the following year, Roger and I got started on the history of savings project. The life-cycle model of savings is predicated on the idea that people save during their peak earning years to build assets that they can draw upon later in life when their income is lower, either because they have retired or because of the reductions in productivity that accompany old age. If that model is correct, then the history of saving must parallel the history of retirement. So the retirement project was a natural outgrowth of the savings project. Susan Carter, Sam Williamson, Roger and I have collaborated on a number of papers on the history of savings, the history of retirement, and economic growth. Guess what? Modigliani was right and Reagan was wrong.

Was the cliometrics revolution of the 1960s really a revolution?

Yes. I say that because, as a direct consequence, economic history became a serious field once again in economics departments. The field has been well supported for 35 years by the National Science Foundation. Cliometrics is largely responsible for moving economic history out of traditional history departments. And, as validation, the neologism "cliometrics" is in the dictionary.

To what extent were the high expectations of that period fulfilled?

From the point of view of creating lasting contributions to scholarship or from the point of view of sustaining an active dialogue with economists, historians, and other social sciences, we have done more and lasted longer than most research programs in economics. Cliometrics has remained a progressive field, while input-output analysis, large-scale macroeconomic modeling, monetarism, and growth theory have not. Those were the other research agendas that were new and hot at MIT when I was in graduate school.

Do you think that economic history has lost ground within the profession in recent years? There seem to be fewer graduate programs with economic history requirements. There are also some departments, including some prominent ones, that don't seem to feel they need an economic historian.

You are certainly right to lament the absence of economic historians in some prominent places. But this is not new; it is something that has long troubled me. In part, I blame the economic historians at the prominent departments (myself included when I was at Berkeley) for not taking a greater leadership role. There are a few exceptions (McCloskey, North, Fogel). Some good people have argued our case to the profession, but more could have been done. I take heart in two trends. It seems to me that departments that have economic historians and know that they need them have risen in quality and respect (Stanford, MIT, Harvard, Berkeley). Those that have not taken economic history seriously enough have not fared as well (Yale, Princeton, Brown).

What motivated your move from Berkeley to Riverside?

When I moved in 1998, I had taught at Berkeley for 30 years. Except for the year I visited Cal Tech, I had never taught

anywhere else, so the novelty was fascinating. While I was very pleased with Berkeley and my career there, I was eager to teach a more diverse student body, particularly after the Regents abolished affirmative action admissions. Because of its fortunate history and location, UC-Riverside has not only a strong minority presence (non-Hispanic whites are less than 25% of the freshman class) but also an extraordinarily diverse mix of minorities (Asian Americans, Latinos, blacks, and foreign-born students are all well represented). I figure if I can find successful ways of doing well for these students, then I will be giving something back to California for the opportunity the university gave me when I was starting my career as an economic historian. The second unique feature of the UCR environment is its projected rapid growth. The campus will double in size in the next ten years. That means that new and experimental programs can be implemented without diverting resources from existing programs, and there will be strong incentives for successful models to be adapted for use in other courses on that campus. At Berkeley, it was a zero-sum game. If you want to experiment with new strategies for teaching, UCR is very attractive.

A few years back, Susan Carter and I got the idea of organizing a project to produce a new edition of *Historical Statistics of the United States*. The old edition, you know, was published back in 1975 and is very out of date. None of the cliometric research undertaken during the last three decades is represented in the old edition. We started the project at Berkeley and then moved it to Napa County. When the opportunity came to move to Riverside, we were offered the opportunity to move the project as well. UCR has proved to be a good home for the project as well as for Susan and me.

How is the Historical Statistics project coming along? When will we see the new edition?

We are in the last stage now; the tables will be shipped off to Cambridge University Press in a few months. The Press will need to copy edit, set, and print it, of course, and that is no small task. It will be three volumes and 2,700 pages of data and will be made available in both print and electronic format. We are hoping to have it in hand in about a year. We think you will like it. More than that, we are hopeful it will stimulate a whole new round of cliometric research on the American economy.

References:

Carter, Susan B and Richard Sutch, "Myth of the Industrial Scrap Heap: A Revisionist View of Turn-of-the-Century American Retirement," *Journal of Economic History* 56 (March 1996), 5-38.

Carter, Susan B., Scott Sigmund Gartner, Michael R. Haines, Alan L. Olmstead, Richard Sutch, and Gavin Wright. *Historical Statistics of the United States*, Millennial Edition. New York: Cambridge University Press, forthcoming 2002.

David, Paul A., Herbert G. Gutman, Peter Temin, Richard Sutch, and Gavin Wright. *Reckoning with Slavery: A Critical Study in the Quantitative History of American Negro Slavery*. New York: Oxford University Press, 1976.

Fogel, Robert and Stanley Engerman. *Time on the Cross: The Economics of American Negro Slavery*. 2 vols. Boston: Little Brown, 1974.

Modigliani, Franco and Richard Sutch, "Innovations in Interest Rate Policy," *American Economic Review* 56 (May 1966), 178-197.

Ransom, Roger L. and Richard Sutch

"The Decline of Retirement in the Years before Social Security: U.S. Retirement Patterns, 1870-1940," in *Issues in Contemporary Retirement* (Edward Lazear and Rita Ricardo-Campbell, eds.). Stanford: Hoover Institution Press, 1988, 3-37.

"The Impact of Aging on the Employment of Men in American Working-Class Communities at the End of the Nineteenth Century," in *Aging in the Past: Demography, Society, and Old Age* (David I. Kertzer and Peter Laslett, eds.). Berkeley: University of California Press, 1995, 303-327.

"The Labor of Older Americans: Retirement of Men On and Off the Job, 1870-1937," *Journal of Economic History* 46 (March 1986), 1-30.

"The Life-Cycle Transition: A Preliminary Report on Wealth-Holding in America," in *Income and Wealth Distribution in Historical Perspective*. 2 vols. Utrecht: Rijksuniversiteit te Utrecht, 1986.

One Kind of Freedom: The Economic Consequences of Emancipation. New York: Cambridge University Press, 1977.

"One Kind of Freedom: Reconsidered (and Turbo Charged)," *Explorations in Economic History* 38 (January 2001), 1-39.

"Tenancy, Farm Size, Self-Sufficiency and Racism: Four Problems in the Economic History of Southern Agriculture, 1865-1880," *Southern Economic History Project Working Paper Series*, Number 8. Berkeley: Institute of Business and Economic Research, University of California – Berkeley, April 1970.

"Tontine Insurance and the Armstrong Investigation: A Case of Stifled Innovation, 1868-1905," *Journal of Economic History* 47 (June 1987), 379-390.

"The Trend in the Rate of Labor Force Participation of Older Men, 1870-1930," *Journal of Economic History* 49 (March 1989) 170-183.

Ransom, Roger L., Richard Sutch, and Samuel H. Williamson

"Inventing Pensions: The Origins of the Company-Provided Pension in the United States, 1900-1940," in *Societal Impact on Aging: Historical Perspectives* (K. Warner Schaie and W. Andrew Achenbaum, eds.). New York: Springer Publishing, 1993 1-44.

"Retirement: Past and Present," in *Retirement and Public Policy: Proceedings of the Second Conference of the National Academy of Social Insurance* (Alicia H. Munnell, ed.). Dubuque: Kendall Hunt Publishing, 1991, 23-57.

Sutch, Richard

"All Things Reconsidered: The Life-Cycle Perspective and the Third Task of Economic History," *Journal of Economic History* 51 (June 1991), 1-18.

"The Breeding of Slaves for Sale and the Westward Expansion of Slavery, 1850-1860," in *Race and Slavery in the Western Hemisphere: Quantitative Studies* (Stanley L. Engerman and Eugene D. Genovese, eds.). Princeton: Princeton University Press, 1975, 173-210.

Expectations, Risk, and the Term Structure of Interest Rates, PhD Dissertation. Massachusetts Institute of Technology, August 1968.

"The Profitability of Antebellum Slavery – Revisited," *Southern Economic Journal* 31 (April 1965), 365-377.

"The Treatment Received by American Slaves: A Critical Review of the Evidence Presented in Time on the Cross," *Explorations in Economic History* 12 (October 1975), 335-435.

Berkeley Campus 1907

BOOK PREVIEW

A Perilous Progress: Economists and Public Purpose in Twentieth-Century America

By Michael A. Bernstein

Note: The following preview is from ***A Perilous Progress: Economists and Public Purpose in Twentieth-Century America*** *to be published in November 2001 by Princeton University Press.*

On a muggy, partly cloudy day in May of 1930, a Swarthmore College professor took up the considerable task of writing to the President of the United States. From an office near Philadelphia's Main Line, Clair Wilcox posted his letter, along with a petition endorsed by over 1000 members of the American Economic Association (AEA). It "urg[ed] that any measure which provide[d] for a general upward revision of tariff rates be denied passage by Congress, or if passed, be vetoed[.]" Three days later a full copy of the entreaty sent to the White House appeared in the New York Times. Herbert Hoover neither privately nor publicly acknowledged receipt of Wilcox's communication, although he did, over the weeks and months that followed, sometimes reply to individuals who chose to write him about pending tariff legislation. The President stood silent when, over a month later, the Congress passed a bill – one that implemented a huge across-the-board increase in import duties, crafted by Senator Reed Smoot (Republican of Utah) and Representative Willis Hawley (Republican of Oregon).

To this day, the Smoot-Hawley Act remains the most restrictive and punitive international trade legislation ever conceived by Washington. It was, and still is, widely regarded as one of the singularly disastrous moments in the history of national economic policymaking, its impacts generally understood to have made the Great Depression much worse. It stood as a powerful example of the pitfalls of mercantilism; in the hands of free-trade advocates, its painful lessons have often been invoked in debates over the regulation of global commerce. Moreover, given the ultimate indifference with which the opinions of a remarkable cross-section of the membership of the AEA were treated in the weeks leading up to the vote (the signatories of Wilcox's petition alone had represented well over a third of the Association's membership), the circumstances surrounding the implementation of the Smoot-Hawley tariff gave American economists of the time great cause for worry. Their views, their expertise, their measured assessments were completely and utterly ignored. If this episode was taken to be an example of a profession's stature and influence, it was a tenuous, even perilous progress indeed.

When he signed the tariff bill into law, Herbert Hoover had far more on his mind than the sensibilities of professional economists. However, it was obvious to Clair Wilcox and his colleagues that theirs was a guild not yet recognized as a reliable and worthy participant in public affairs, since the President felt compelled to cleave to political pressures at odds with the prevailing wisdom of their discipline. For close to a half century, American economists had struggled both to establish the rigor of the processes by which individuals were admitted to their ranks and to demonstrate the usefulness of the skills represented by

their credentials and institutional affiliations. Yet events in the early summer of 1930 were only part of a larger list of particulars concerning how meager were the returns of their earlier investments. It was for American economists in the interwar years of the 20th century to wonder if their expertise might ever claim an unambiguous mission on behalf of a public purpose.

Subsequent decades brought experiences that would do much to resolve the frustrations and anxieties of this earlier generation, but they would do so in ways far more complicated and ironic than anyone could anticipate. The very determination with which a community of professionals cultivated a visibility virtually unique among their peers in other academic fields would also in time subvert both the means by which they secured their influence and the political and bureaucratic contexts within which it could be most effectively displayed. By century's end, American economists would ultimately work toward a retreat from the public realm and a scholastic introversion of their own research protocols and techniques. How that paradoxical outcome was fashioned and why its elaboration had ramifications far beyond the cloistered realm of a cadre of experts are the burden of the story that follows.

A history of a social scientific community, in a particular national setting at a particular time, is an enterprise requiring some justification. To be sure, the role of professions and experts in the United States has emerged as an important theme in the work of political, social, and intellectual historians. In the minds of these scholars, professionalisation has been a significant, perhaps even crucial, part of the process by which a national culture has been forged and a new political elite sustained in modern America. During the Progressive Era (at the turn of the century), an extremely volatile political climate nurtured, on the one hand, certain anti-capitalist and reactionary factions. These factions found themselves rather quickly marginalized in ways that caused them to be easily repressed or swiftly co-opted. On the other, this political climate encouraged burgeoning groups of reformers with closely held convictions concerning the virtues of meritocracy – individuals who sought to establish their ability and right to reorder social and political affairs for the benefit of all. Of these latter Progressives, professional communities were a striking example. Decades of inspired scholarship that has sought to understand the rise of these modern experts has also afforded great insight regarding that most peculiarly American political and social phenomenon: the ascent of an upper middle class.

Yet even beyond the contexts and preoccupations of the academy, there would appear to be good reason to pursue a multifaceted historical investigation of the world of expertise in general and especially of economic experts. Not the least remarkable aspect of the emergence of economics as a preeminent academic discipline in late 20th-century America has been the displacement of almost all other social scientists in significant and influential political and bureaucratic positions. Indeed, it has often been asserted that this development is symptomatic of a contemporary impatience with what have come to be regarded as the wasteful and inefficient practices of well-meaning, yet poorly trained meliorists. The systematic, hardheaded, thrifty realism of economists has taken on a special appeal in this context and has animated much policy debate in the present day. Paralleling this peculiar constellation of attitudes has been the influence economists have enjoyed within academic life itself. As a discipline, the field

has tended more and more to dominate the formulation of curriculums and research agendas in all of the social and policy sciences and, in some cases, even in the humanities. In this specific sense, economists may have eclipsed other professionals (such as attorneys and physicians) who deploy substantial amounts of authority but who have not been in a position to frame directly the schooling of entire generations. All the more reason to study this expert community which occupies such a unique place in the social and political life of contemporary America.

It is also interesting that the history of a discipline, which over time evolved into the rigorous examination of rational decision-making under the constraints of resource availability and uncertainty, may also provide particular insights into some of the essential characteristics of modern American culture. Arguably the preeminent capitalist society in the world today, the United States finds itself predominantly peopled by those disposed toward calculation, acquisition, and accumulation in ways far more accepted than elsewhere. In a nation in which "the pursuit of wealth, stripped of its religious and ethical meaning . . . become[s] associated with purely mundane passions, which often actually give it the character of sport," the agendas and methodologies of contemporary economists speak directly to the manner in which Americans go about their daily affairs, understand and order the world around them, and apprehend those different than themselves. Rethinking the history of the American economics profession is consequently very much tied up with rethinking the history of modern American culture itself.

Economic practitioners have also sought to have an enormous impact on political affairs and public debate in 20th-century America. The expertise they have displayed since the turn of the century has brought the attention of government officials and elected representatives to rest increasingly on questions of policy means rather than ends. Similar to the movement of the profession itself, contemporary policy debate (framed within the vocabulary and conceptual categories of mainstream economic theory) has assumed a decidedly desiccated aspect. Ends, objectives, and goals fall from view while means, techniques, and trade-offs become the sole object of debate. Assessments of the historical processes that have shaped the American economics profession also hold the promise that they might unpack a vast array of unspoken and frequently unconscious beliefs, languages, and presumptions that have come to mold and define, and in certain contexts actually deform, contemporary public discourse. Reckoning with the history made by American economists and their discipline is clearly no esoteric venture.

Neither is a determination to focus that accounting within the context of the 20th century. One of the singularly most important sets of forces shaping the evolution of both the discipline and its community of practitioners is found in the growing involvement of economists with the work of the federal government. As the past 80 years have witnessed the rise of the United States to unprecedented levels of economic, military, and political power, reconsiderations of the sources and consequences of that dominion have captured the imagination of many scholars. Consequently, this narrative is part of an ongoing and wide-ranging conversation about the historical meanings and contemporary impacts of national power both at home and abroad. This is all to say that grappling with the recent history of American economics necessitates coming to

terms with the history of the "American century" – an era not simply of national sway in world affairs but also of growing levels and complexities of state functions overall. While they often celebrate the virtues of market systems that function independent of state intervention, American economists have themselves ironically been the creatures of a history powerfully and dramatically configured by statism.

Having made these points, it would be a mistake if I left the reader with the impression that what follows is part of an effort to exhort economists and their fellow travelers to be aware. Far less is the narrative constructed as some kind of j'accuse. The objective here is neither to reform economics nor to presume to tell a community of scholars how to conduct their business. While years of advanced study have left me with an enduring respect for and fascination with economics, my intentions in this work are directed at a much wider audience. It is for this reason that I have been determined to create a narrative that will engage a wide array of interests and sensibilities rather than a technical discussion. In short, what follows is a history – one that seeks to contribute to a debate among scholars and interested readers that goes beyond the specific confines of a particular field of study.

Needless to say, a historian's work is defined as much by the evidence and information left out of the text as by that which is included. Having made a variety of decisions concerning the use of evidence and the coverage of particular issues, I am obliged to explain them. First, as to evidence, the book opens with a close analysis of classic texts relevant to the understanding of early articulation of modern economic theory at the turn of the 20th century. Coming to terms with the establishment of what I call a "new agenda" for economists is an essential part of the professional and political history that follows. Five subsequent chapters, surveying the history of the profession from 1885 through the Vietnam War era, are based on archival documents. Finally, a seventh chapter and an epilogue utilize contemporary publications and journalistic sources to reframe current assessments of economics and its role in policy debate in ways that take into account the historical findings presented earlier.

Second, as to the choice of topics and themes, I have confined my discussion to a focus on economics and professional economists and their involvement in national policymaking. Except for a few digressions, the primary objects of concern have not been the applied domains of the field. Even more to the point, my primary goal has been to discuss prevailing trends in the discipline and the profession and to avoid very much consideration of alternatives. Those readers anxious to learn about the history of schools of thought outside the mainstream of debate among professional American economists in the 20th century won't find it here. Other than certain ancillary notes, I have concentrated on what I take to be the core of the discipline rather than its critics.

Finally, there is the question of context. This is a study resolutely focused on American economics. Far from a chauvinistic choice, this perspective stems from both an effort to make the current study more accessible and a conviction that it is the evolution of the discipline within the US that has had a far greater influence on the professional trajectory of the field elsewhere than the other way around.

I should also confess that the discussion here carries within it no testable hypotheses or counterfactual arguments. Economists, in

particular, may attempt to render my core theses in "strong" formats that lend themselves to unambiguous assessments of proof. Aside from different methodologies, the avoidance here of such inferential claims stems from the objectives of the work. Far from an effort to argue that the historical forces examined here constituted the sole agents of change under review, this work offers a particular frame of reference for understanding the modern history of the American economics profession that up until now has either been ignored or under-appreciated. The result is a history written very much from the top down and configured in ways that defy the construction of simple mechanisms of causality.

Retrospection is the very nature of historical enterprise. However, it always runs the risk of assuming inevitability when the outcome is actually the product of a complicated array of contingencies, fortune, and choice. Whatever determination the historian may distill from past evidence must always be tempered with the realization that history is a process, not a preordained script. If that awareness leaves one with an appreciation of the fragility of historical outcomes, all the better. Even more to the point, it renders the understanding of necessity in an altogether different light. What seems necessary in history, when closely examined, appears more social and political and more conditioned than anything else. American economists themselves encountered that reality in their own efforts to understand society with reference to mechanisms they sought to cast in evermore rigorous ways. Necessity may seem comprehensible in ways that professional elites find straightforward, even comforting. Yet when inadequately appreciated as itself a historically specified perception, it becomes simply a thoughtless justification of the idea that the world we know is neither changing nor changeable.

EHA Conference *(Continued from page 1)*

responded that he is somewhat concerned, but credit expansion has slowed and "money matters but credit counts!"

"Crises in History" complemented the plenary session in its focus on real shocks and financial markets. This session featured the papers "Shock and Aftershock: The 1906 San Francisco Earthquake and the Panic of 1907" by **Kerry Odell** (Scripps) and **Marc Weidenmeir** (Claremont McKenna), "International Capital and the Brazilian Encilhamento, 1889-1893: An Early Example of Contagion among Emerging Capital Markets" by **Gail Triner** (Rutgers), and "Financing Spanish Expansion: Medieval Economic Ethics in Indian America" by **Marie Duggan** (Keene). Odell and Weidenmeir present a case study of how real shocks can lead to financial shocks and how local shocks can lead to world shocks. Explanations of the Panic of 1907 have typically focused on a liquidity crisis in New York, which is partly attributable to a sharp increase in rates by the Bank of England in December 1906. They suggest that the Bank of England's motive may have been to counteract the export of specie by British insurers. These insurers carried roughly half the insurance for the San Francisco financial district, which had been devastated by an earthquake and fire. Triner examines the linkage between Argentina's 1890 payments suspension, which is linked to the failure of the Barings bank and the collapse of Brazil's financial expansion in 1891. The countries were not linked by trade or economic policy, but contagion could have occurred through investor credit constraints or changes in investor expectations. An increase in the

correlation of Brazilian and Argentine interest rates after the Baring crisis provided evidence for the existence of contagion. Duggan considers the development of early California, which was largely managed by mendicant friars who had taken a vow of poverty. She uses data from colonial account books to show how the friars developed relationships with the natives by making gifts. The friars' medieval economic ethics were successful in this case, because their emphasis on creating allegiances helped turn the natives into the labor force needed for the development of the colony.

Two concurrent sessions completed the day on Friday. At "Medieval Affairs," chaired by Akira Motomura (Stonehill College), **Meir Kohn** (Dartmouth College) presented a new spin on sharecropping and "putting out" arrangements in "Finance Perspectives on the Organization of the Preindustrial Economy." He argues that these arrangements have generally been viewed as employment arrangements, but that a useful shift of perspective might be to view the farmer as an independent businessperson asking how to finance enormous fixed capital costs. This leads to an emphasis on the role of the land as security and the contract as a form of secured loan. He illustrated his model with intriguing anecdotes from Tuscany during the Black Death. **John Munro** (University of Toronto) emphasizes the importance of negotiability for secondary markets in "The Origins of the Modern Financial Revolution: Responses to Impediments from Church and State in Western Europe, 1200-1600." He contends that the legal foundation for modern negotiability is an English law case (Burton v. Davy) from 1436, which established bearer rights for bills of exchange.

Two papers were presented at the session on monetary history and the gold standard, chaired by Anna Schwartz . **Christopher Meissner** (Cambridge) presented "A New World Order: The Emergence of the Classical Gold Standard, 1870-1913," and **Michael Bordo** (Rutgers) and **Pierre-Cyrille Hautcoeur** (Orleans) presented "The Cost and Benefits of France Adopting British-Style Stabilization Policies after World War One." Meissner notes that monetary regimes appear to affect real variables and that there are similarities with currency unions being adopted today. He uses a duration model to explain the timing of the adoption of the gold standard by various countries. The results show that countries adopted the gold standard sooner if a high share of their trade was with other gold standard countries and that higher income countries tended to join the gold standard sooner. Hautcoeur and Bordo consider the possible costs and benefits to France if it had followed a strategy similar to Britain in the interwar period and restored its currency to the prewar parity. Their stabilization scenario involves pegging the franc to the pound and assuming that France would then have the same interest rates as Britain. In their counterfactual, beginning stabilization in 1919 would not have been successful because of France's large debt overhang. The inflation from 1919-24 was important for reducing the real value of French debt, however, France could have successfully stabilized in 1924 instead of 1926 thus avoiding the 1924-26 inflation.

Friday concluded with a reception at the Library Company of Philadelphia whose Program in Early American Economy and Society may be of interest to Clioms. (Contact Cathy Matson, PEAES at cmatson@udel.edu)

After a morning of business meetings, poster sessions, and an outstanding dissertation session, the conference continued with two concurrent sessions. The session on the

Great Depression began with "New Deal Policies and the Persistence of the Great Depression" by **Lee Ohanian** (UCLA), which was presented by discussant Chris Hanes (Mississippi). Ohanian considers the Great Depression from a real business cycle point of view. In particular, the effects of New Deal policies are quantified in a dynamic general equilibrium framework. The policies, which encouraged cartelization and unionization, acted to increase both prices and wages. Although the National Recovery Act was declared unconstitutional, Ohanian argues that these policies were continued through other means. The gap between actual and market-clearing friction created an "insider-outsider" friction with agents willing to wait in line for the high paying jobs. In his model, this friction was able to account for a significant portion of the output and employment declines. **Charles Calomiris** (Columbia) and **Joe Mason** (Drexel) investigate whether banking distress itself exacerbated the Great Depression in "Did Banking Distress Deepen the Great Depression." Using data disaggregated by state in a panel vector autoregression, they find support for the hypothesis that the reduction in output was in part due to a contraction in loan supply arising out of banking distress. "Liquidity Provision by Market Makers and Financial Crisis: The Case of the Great Depression" by **J. Peter Ferderer** and **Kyle Richey** (both Macalester) was partly inspired by the 1998 Russia/LTCM crisis, which saw a drying up of liquidity in secondary markets. They analyze bid-ask spreads in the government securities market during the Great Depression. Their results show that spikes in the spread are correlated with the price of bonds and that a particularly large spike is associated with the third banking crisis. This evidence suggests that the drying up of secondary market liquidity contributed to the deflation of asset prices.

A merged session on "Colonial Experiences" began with "A Market Economy in the Early Roman Empire" by **Peter Temin** (MIT). After a brief discussion of the problems of evidence and the models used to date to classify primitive economies, Temin poses the question, "How do I feed Rome?." He answers that this city of nearly a million inhabitants was supplied by markets, and he presents evidence in support of that claim. While not all of the markets in the Roman economy were tied together, he argues that they still functioned as a comprehensive Mediterranean market. "Signing with John Company: Contract Enforcement in Overseas Enterprises" by **Santhi Hejeebu** (Iowa) made up for Temin's lack of comprehensive data with a rich data source. She argues that long-distance trade depends on the enforcement of long-distance contracts and that the European trading companies of the 18th century were aware of the considerable problems of overseeing their agents. She looks at contract enforcement in the East India Company and discovers a complex and interesting story of death, risk, and fortune. It came as news to many in the audience that agents were paid very little but gained the right to trade port-to-port for their own profit. Enforcement of contracts relied on Chancery courts, and her work indicates that nepotism may not have been inefficient as trust was of crucial importance. **Siddharth Chandra** turned our attention to "The Opium Regie in the Netherlands Indies: Colonial Cash Cow or Drug Policy Triumph?." The Opium Regie was a government opium monopoly from 1914 to 1940 whose purpose was to help decrease the opium problem and make the Netherlands a lot of money. Using a rational addiction framework, Chandra contends that the inflation of 1919-22 and the Great Depression drove the fall in opium consumption, events the government's opium policy did not influence.

The next three concurrent sessions were all well attended, but the competitive edge went to "The Big Picture." **Jeffrey Williamson** (Harvard) and **Kevin O'Rourke** (Trinity) opened with "After Columbus: Explaining the European Overseas Trade Boom, 1500-1800." Williamson presents evidence that the conventional wisdom that the increase in world trade from 1500-1800 was due to declining trade costs is not accurate, because commodity prices did not converge. The alternative explanation focuses on the relative prices of traded and non-traded goods. In the 16th century, the relative price of traded goods fell, indicating an increase in supply. During the rest of the period considered, the trade boom could largely be explained through an increase in demand for importable luxury goods in Europe, evidenced by an increase in the relative price. **Alan Taylor** (UC-Davis), **Antoni Esteveordal** (IDB), and **Brian Frantz** (USAID) continued with "World Trade and the Gold Standard, 1870-1939." They use a gravity model to analyze the relative importance of shipping costs, tariffs, and a common currency (i.e. the gold standard) in the growth of trade and its decline during the interwar period. The results of a counterfactual examination suggest that trade would have been much higher in the interwar period had countries remained on a common monetary standard, but keeping tariffs at 1913 levels would not have made a significant difference. In "The Secret History of the Industrial Revolution," **Greg Clark** (UC-Davis) asserts that there was not a great turning point in Britain in 1770 as others have suggested. No "industrial revolution" can be located in the national income data, and the most important change in the trend does not occur until approximately 1860. A break in the total factor productivity trend around 1810 can largely be attributed to Britain's cotton exports. Instead, he claims that the economic history of the time should be viewed as "a story of continuity."

"The Role of Equity Markets" began with a study by **Eugene White** (Rutgers), **Larry Neal** (Illinois), and **Lance Davis** (CalTech). They investigate the long-term evolution of the NYSE's microstructure using the price of seats over 120 years to examine the evolution of market rules. They find that the largest changes in seat prices were not a function of the fundamentals of seat value. Large price movements seem to have been determined by changing rules and regulations. In his study of "Financial Development and Capital Structure in Nineteenth Century Japan and the USA," **Masayoshi Tsurumi** (Hosei and Virginia) uses data on private industrial corporations from annual reports compiled by rating agency companies. His findings include bank support of early Japanese industrialization and the reliance of large firms on the stock market for capitalization. The last session ended with "Stock Market Liberalization, the Cost of Capital, and Economic Growth in Postwar Europe" by **Hans-Joachim Voth** (Cambridge and Pompeu Fabra). He looks at the cost to Europe of restricting capital flows after WWII. Using event-study methodology, Voth examines the extent to which restrictions on current and capital account convertibility affected stock returns. He finds that overall impact on growth was consistently and significantly negative.

The third session in this time slot, "Finance and Development," began with "The Law and Financial Development: Evidence from the Americas" by **Ken Sokoloff** (UCLA) and **Stephen Haber** (Stanford). The authors look at the connection between factor endowments, degree of inequality, evolution of institution, and the level of development in the Northern and Southern hemispheres in the New World. Their surprising conclusion

is that British heritage doesn't matter; degree of schooling, land policy, banking, and the franchise matter more. **Noel Maurer** (ITAM) then presented "Enforcing Property Rights Through Reputation: Mexico's Early Industrialization, 1878-1913" written with **Tribib Sharma**. They argue that in the absence of secure property rights and other idiosyncratic shocks to firms, credit will be made available on the basis of reputation. This will give firms an incentive to group together, leading to concentration that has little to do with increasing returns to scale or lack of capital markets. **Aldo Musacchio** (Stanford) rounded out the session with "Institutions and Modernization: The Rio de Janeiro Stock Exchange and the Industrialization of Brazil, 1889-1930. Using disaggregated date for securities trade on the stock exchange in Rio, he shows that the stock exchange and the regulated financial markets created after 1889 in Brazil played a large role in industrialization.

The last session before the presidential banquet was the plenary, "Bubble, Bubble, Toil and Trouble?" Larry Neal (Illinois) chaired the session with speakers **Peter Garber** (Brown and Deutsche Bank) and **Jeremy Siegal** (Wharton). Garber's presentation can best be described as a denial of bubbles, or the "anti-bubble" school. He argues the invocation of bubbles was based on three factors: pure rhetoric, scientific laziness, and small sample problems that make speculations appear to be great. The bottom line: irrational forces don't determine market prices. Siegal opened by reminding us that stocks are the best long-run investments. His empirical investigations have found little evidence of irrational exuberance but did find evidence of irrational despondency.

Sunday morning sessions were remarkably well attended due, perhaps, to the return to Eastern Standard Time. **Jane Knodell** (Vermont) opened "The Role of Banking Systems" by presenting her research on "Financial Deepening on the Frontier: The Antebellum Midwest, 1830-1860." She considers the financial structure of Chicago, Cincinnati, and St. Louis. Among her findings are that state-chartered banks accounted for less than half of financial intermediation and that after 1850 the share of state-chartered banks fell while intermediation rose as the states became more willing to charter insurance companies. Next up was "Economic, Political, and Legal Factors in Financial System Development: International Patterns in Historical Perspective" by **Caroline Fohlin** (CalTech). Fohlin, using a panel of countries and starting in the mid-19th century, examines the influence of economic, political, and legal factors on the development of banking systems, specifically whether a universal or specialized model is followed. Among her findings are that in certain subperiods, one type of structure would appear to perform better, but there is no evidence that either system has an advantage in the long run. Finally, in "My Word is My Bond: Reputation as Collateral in 19th Century English Provincial Banking," **Francesco Galassi** and **Lucy Newton** (both Warwick) examine the records of the Sheffield Union Bank. By looking at terms on loan contracts, they estimate the expected probability of default assigned by the directors. Their results suggest that economic factors are not the most important determinants and that trust and reputation are important in determining which borrowers received access to credit.

In what the presenters and audience felt was the most exciting session, "The Textile Industry" was examined once again. **James Bessan** (Research on Innovation) presented his thought-provoking "The Skills of the

Unskilled in the American Industrial Revolution." He claims that the Lowell mills made substantial investments in the human capital of mule spinners and power loom tenders. Accounting for skill explains most of the productivity growth from 1834-55. Technological innovation is seen as a broad social process dependent on social institutions as well as machines. In a related paper, "Did Experience Matter?," Tim Leunig (London School of Economics) uses a data set from the Lyman Mill and discovers that labor turnover does not matter much to ring spinning productivity in American mills. This appears to have been due to the ability of firms to vary wages and capital to labor ratios as the skill of their labor force shifted. Although learning took place and did matter, productivity did not seem to decrease when (the mostly female) workers took breaks from the job, as dictated perhaps by non-market responsibilities. **Michael Huberman** (Universite de Montreal), in "When Labor Hires Capital: Evidence from Lancashire, 1870-1914," compares the experiences of Oldham and Bolton. Oldham firms (unlike Bolton, where profit sharing was not adopted) introduced profit sharing, and the town experienced rapid growth. Hubermand states that profit sharing was successful in Oldham because the town's tumultuous history of labor unrest had created in workers an empathy that overrode the free rider problem. Bolton, with a peaceful history of labor relations, had workers who were indifferent to each other, and profit sharing was unsuccessful. Eventually, Oldham firms would fail to make crucial capital investments (in ring spinners!), and profit sharing would disappear.

Two concurrent sessions rounded out the conference. The session on "Finance in Colonial British North America" featured "The Demography of Debts in Colonial New England" by **David Flynn** (North Dakota), "Depression in Colonial New York: The Role of Monetary Mismanagement in Stirring Revolutionary Discontent" by **Ron Michener** (Virginia), and "Interest Rate Risk, Illiquid Assets, and Information Asymmetries: Balance Sheet Deterioration and 'Debtor' Angst in Colonial America" by **Robert Wright** (Virginia). Flynn presents new information constructed from merchant account books in Colonial New England to improve the understanding of the role of book credit. He finds that the average term of debt was 14 months, which is significantly longer than previous estimates. Michener describes the fluctuations in New York's money supply in the years leading up to the American Revolution. Specifically, King George's War and the French and Indian War brought large inflows of specie into the colonies because of military expenditures and illicit trade. This was followed by a sharp contraction due to the Currency Act, the Stamp Act, and other restrictions on trade, which made it hard for the colony to earn specie. Michener maintains that the tightness of the money supply contributed significantly to revolutionary sentiment. Wright began his presentation by illustrating how sharp increases in interest rates could lead to balance sheet deterioration for colonial businesses by lowering the value of land and slaves. The usury laws, he argues, were easily evaded. Consequently, the market determined interest rates, although quotations are hard to find. He read quotations from contemporary sources, including Benjamin Franklin, to illustrate that high interest rates were a matter of serious concern for the colonists.

The session "The Changing Aspects of Land in the US" began with "Job Mobility Over Time Across the US: Evidence on the Agricultural Ladder" by **Lee Alston** (Illinois) and **Joe Ferrie** (Northwestern). They use 1920 manuscript census schedules for

McLean County, Illinois (a typical corn belt county) and 1938 survey data from Jefferson County, Arkansas (a typical cotton belt county) to compare movement along the ladder. Results indicate that the 1910s was a decade of general ascent up the agricultural ladder and that mobility was worse in the 1920s than in the 1930s. **Price Fishback** (who claims a perfect correlation between his paper and the last session of the conference) presented "The Origins of Modern Housing Finance in the US: The Role of the Federal Housing Administration during the Great Depression" with **Shawn Kantor** (both Arizona). This work, part of a larger project on the New Deal, has as its focus the effects of New Deal spending and housing programs. They analyze a fascinating data set that describes over 30 New Deal spending, loan, and mortgage insurance programs across all US counties from 1933-1939. The final paper, "Red Wheat and the Red Queen: Biological Innovation and Productivity Growth in American Wheat Production, 1800-1940" by **Alan Olmstead** (UC-Davis) and **Paul Rhode** (North Carolina), was presented by Rhode. The authors address the significant biological innovation in American wheat production that took place before the 1930s. Innovations took the form of wholesale changes in wheat varieties and farmers employing adaptations in order to offset the growing threat of pests and diseases. Their estimates indicate that these innovations increased yields by about 40%, leading to a substantial revision in the Parker and Klein estimates of the sources of labor productivity growth in wheat farming.

In addition to discussing new research, at the banquet on Saturday night the EHA honored the best in completed research projects. **The Arthur H. Cole Prize** for the outstanding article published in the *Journal of Economic History* this past year was awarded to Lillian Li (Swarthmore) for her article, "Integration and Disintegration in North China's Grain Markets, 1738–1911," which appeared in the September 2000 issue. Daniel A. Schiffman (Bar Ilan University) received **The Allen Nevins Prize** for the outstanding dissertation in US or Canadian economic history for *Shattered Rails, Ruined Credit: Financial Fragility and Railroad Operations in the Great Depression*, completed under the direction of Charles Calomiris (Columbia). **The Alexander Gerschenkron Prize** for the outstanding dissertation in non-US or Canadian economic history went to Eona Karakacili (UC-Davis) for *Peasants, Productivity and Profit in the Open Fields of England: A Study of Economic and Social Development*, completed under the direction of J. Ambrose Raftis (University of Toronto). Carolyn Tuttle of Lake Forest College was honored with the **Jonathan Hughes Prize** for excellence in teaching economic history. **The Gyorgy Ranki Prize** for the outstanding book in the economic history of Europe published in 1999 or 2000 was granted jointly to Stephan Epstein (LSE) for *Freedom and Growth: Markets and States in Europe, 1300–1750* (Routledge, 2000); and Philip T. Hoffman (CalTech), Gilles Postel-Vinay (Institut National de la Recherche Agronomique), and Jean-Laurent Rosenthal (UCLA) for *Priceless Markets: The Political Economy of Credit in Paris, 1660–1870* (University of Chicago Press, 2000).

As we left Phildelphia's Loews Hotel, we mused on how fitting it was that this EHA meeting should be in the old Loew's Savings and Loan building – a perfect example of adaptive reuse and a reminder of the effects of financial reforms. Next year's meeting will be back in the heartland at the St. Louis Marriott Pavilion, with the theme of "Public versus Private Institutions."

Economic Growth Conference

By Paul Auerbach, Kingston University

(Kingston) The Conference "Why Economic Growth? What is a Good Measure of Society's Progress?" was held on August 30–31, 2001 at Kingston University (UK). It was sponsored by the School of Economics and the Committee for the Understanding of Society and Politics and organized by Paul Auerbach and Brian Brivati (both Kingston University). The purpose of the conference was to instigate an interdisciplinary discussion of the ways in which we choose to measure societal improvement. This summary will focus on those papers with particular relevance to economic history.

In the opening paper, "The Measurement of Economic Growth – A Tendentious Introduction," **Auerbach** notes that a proper measure of economic income, as opposed to the standard national income measure, would involve consideration of any appreciation or depreciation in the value of society's assets. This issue has been emphasized within the ecological literature in the context of natural resources. Auerbach focuses on the emphasis of possible mismeasurement of economic growth due to changes in the value of human assets. The deterioration in the stock of human assets engendered by the ferocious regime of factory work may explain the apparent relative economic decline of late 19th-century Britain. A more accurate calculation might have shown a slower rate of growth for the earlier period, in which the human capital stock was being subjected to accelerated depreciation.

Peter Swann (University of Manchester) in "No Wealth But Life: When Does Mercantile Wealth Create Ruskinian Wealth?" has made the first attempt to formalize the economic ideas of John Ruskin, the extremely influential late 19th-century British social philosopher and art critic. Ruskin posited a tension between wealth as life and mercantile wealth, with such effects of mercantile activity as pollution implying a trade-off between the two. **Nick Crafts** (London School of Economics) in "UK Real National Income, 1950-1998: Some Grounds for Optimism" notes a dramatic improvement in movements in the index of sustainable economic welfare when the results incorporate increased life expectancy.

On the second day of the conference, **Andrew Oswald** (University of Warwick) presented "National Income and Human Happiness." He concludes from his exhaustive empirical surveys over a broad range of countries that increases in national income are not correlated over time with increasing happiness. **Chris Dillow** (Investors' Chronicle) continued with "No Matter for Congratulation" where he takes the preceding conclusion as a premise and links the reasons for the failure of subjective happiness to improve with rises in national income to the nature of contemporary modes of work and to consumption externalities. **Simon Locke** (Kingston University) in "Towards a Reflexive Society: Musings of a Rhetorical Sociologist on the Issue of Economic Growth" suggests that national income attempts to do the impossible – find a unitary measure of equivalence in the context of the broad range of normative conventions which exists in society.

Via satellite link, **David Dollar** (World Bank) closed the conference with his presentation of "Globalization, Inequality and Poverty Since 1980." He concludes that there is a close relationship between the economic success of developing countries and the extent to which these countries

pursued a policy of openness to the international economy.

Despite its brevity, the conference raised several issues that generated earnest discussion among the participants. The range of papers and the subsequent discussions highlight the contentious nature of measuring economic growth throughout history, both in theory and in practice. For information regarding other papers delivered at the conference, or to obtain copies of any papers delivered, please contact Paul Auerbach at p.auerbach@kingston.ac.uk.

Call for Papers

Economic & Business Historical Society

Chicago, Illinois

April 25 to 27, 2002

The Economic & Business Historical Society welcomes proposals for presentations on all aspects of business and economic history at its 27th annual conference. In keeping with its traditions, the Society seeks proposals for both individual papers and panel sessions. Graduate students are invited to apply and may qualify for reduced registration fees.

Proposals for individual papers should include an abstract of no more than 500 words, a brief CV, postal and e-mail addresses, and telephone and fax numbers. Panel proposals should also suggest a title and a panel chair. Graduate students and non-academic affiliates are welcome. Submissions imply that at least one author will register for the conference and be present at the time designated in the conference program.

Proposals may be filed on the Society's website http://www.ebhsoc.org/papers.html or mailed to:

Malcolm B. Russell
EBHS 2002 Program Chair
School of Business
Andrews University
Berrien Springs, MI. 49104-0075

They may also be sent by e-mail to russell@andrews.edu or sent by FAX (616) 471-6236. **The submission deadline is January 15, 2002.**

Papers at the conference may be submitted for publication in the Society's peer-reviewed journal, *Essays in Economic and Business History*.

Conference Report for the 4th European Historical Economics Society

By James Foreman-Peck, H.M. Treasury

(Oxford) The 4th European Historical Economics Society Conference was held at Merton College, Oxford, UK, September 20-22, 2001. The conference theme, "Long-Term Changes in the European Economies," brought together scholars with the goal of enhancing the understanding of different European patterns of economic development and how they might be managed. Participants examined the evolution of European economic institutions and processes over time and from a comparative perspective.

The first session on "The Long View of European Economies" began with a paper by **Niels Kærgård** (RVA University Copenhagen) entitled "The Danish National Product for the Years 1000-1999." Discussant Steve Broadberry (University of Warwick) challenged Kærgård's claim that before 1800 the national product could be measured as "the number of people it will be possible to support." Broadberry commented that this claim is based on the Malthusian assumption that "most of the inhabitants were living close to the subsistence level." In contrast, Broadberry cited Maddison's study of the Danish population that used an assumption of rising per capita incomes to produce estimates of Danish GDP for the period 1500-1820. Broadberry suggested that the paper include a systematic review of other types of evidence that have been used to make inferences about living standards. He also proposed that the review include a study of the extent of urbanization and real wages. The second paper, "The Secret History of the Industrial Revolution," was given by **Greg Clark** (UC-Davis). Tim Leunig (London School of Economics) questioned Clark's wage estimates and wondered why Clark discredits an idea of the Industrial Revolution that no one holds. Leunig stated that we know from Crafts that Britain's shift into manufacturing occurred at an earlier level of wealth than in other countries and was more extensive. In that sense, Leunig said, we would do better to think of Britain as the most extreme industrial revolution, in that other countries' development was less industrial even if faster growing. Last, **Avner Offer** (All Souls, Oxford) presented "Why Has the Public Sector Grown So Large? A Political Economy of Prudence in the UK, c.1850-2000." Bob Millward (University of Manchester) noted that Offer's focus on prudential versus visceral goods provides some very useful insights but in some respects tries to accomplish too much. Millward found the discussion of the emergence of public housing and building societies from the 1920s strong, although he wondered whether the under-investment in public health was due to myopia or environmental spillovers.

The session on "Market Integration" began with a paper by **Jeffrey Williamson** (Harvard) and **Kevin O'Rourke** (Trinity College) entitled "After Columbus: Explaining the Global Trade Boom 1500-1800." Patrick O'Brien (London School of Economics) argued that contrary to O'Rourke and Williamson's assertion, historiography does not contend there was a three-century trade boom after 1492. O'Brien cited a 17th-century depression and a resumed upswing in the long 18th century and felt that these cycles should be discussed as an alternative to Gunder Frank's notion of

globalization. Next, **Bertrand Roehner** (University of Paris) and **Carol Shiue** (Texas) followed with "A Comparison of Market Integration in China and France in the 18th Century." The authors use monthly data on wheat and rice prices from a number of provinces in 18th-century China and France to compare the extent of market integration. Karl Gunnar Persson (University of Copenhagen) commented that while the correlation results suggest a tendency of intermarket price correlations to fall with increasing geographical distance between markets, the authors give no economic rationale for the "correlation length" measure. In the final paper of the session, **Lennart Schön** and **Jonas Ljungberg** (both Lund University) addressed "Domestic Markets and International Integration: Paths to Industrialization in the Nordic Countries." Tim Hatton (Essex) welcomed the new comparative measures of productivity growth by sector across the Nordic countries. He commented, however, that these and other data offered in the paper did not directly address the question that the authors sought to confront: namely, the contribution of open economy forces to the growth of these economies between 1865 and 1910. Hatton thought that further research on the nature and sources of Nordic social capital would be needed to support the paper's conclusions that these factors were the key to growth.

The agriculture session included the following papers: "The Costs of Enclosure and the Benefits of Convertible Husbandry among Peasant Holdings in Medaeval England" by **Harry Kitsikopoulos** (NYU); "Adult Life Expectancy, Investment, and Agricultural Productivity in England in the 18th Century" by **Esteban Nicolini** (Universidad Carlos III); and "Legal Institutions and Performance: Monitoring Agreements in the Early Danish Dairy Sector" by **Ingrid Henriksen** (University of Copenhagen) and **Morten Hviid** (University of East Anglia). Liam Brunt (Nuffield College, Oxford) asserted that it is not obvious that enclosure was relevant to the adoption of convertible husbandry. Moreover, since the cost of enclosure was probably large and the benefits of convertible husbandry small, it is likely that convertible husbandry was not an important technology for raising yields and output. Knick Harley (Western Ontario) praised Nicolini's paper, noting that it addresses an important historical issue, uses economic modelling in an appropriate and clever way, and uses the model to provide a useful answer. Francesco Galassi (University of Warwick) wondered why cooperatives existed in the first place in Denmark's dairy sector. He pointed out that if peer monitoring and pressure were what gave the co-ops the original advantage and if these forms of social control came to matter less with time, the co-ops should have withered away.

The first session on Saturday focused on technology, where **Nick Crafts** (London School of Economics) delivered "The Solow Paradox in History." James Foreman-Peck (H.M. Treasury) pointed out that while Crafts's paper compares the impact on economic growth of two historical technologies (electricity and steam) with that of information and communication technology, the conclusion is not firmly based, because "like is not compared with like." He reasoned that if steam is a general purpose technology comparable to ICT, then the impact should not be restricted to railways and factory engines but should include the contribution of steamships as well. **Jochen Streb** (Universität Heidelberg) followed with "Can Politicians Speed up Long-Term Technological Change? Some Insights from a Comparison of the German and the US-American Synthetic Rubber Programs During World War II." Joerg

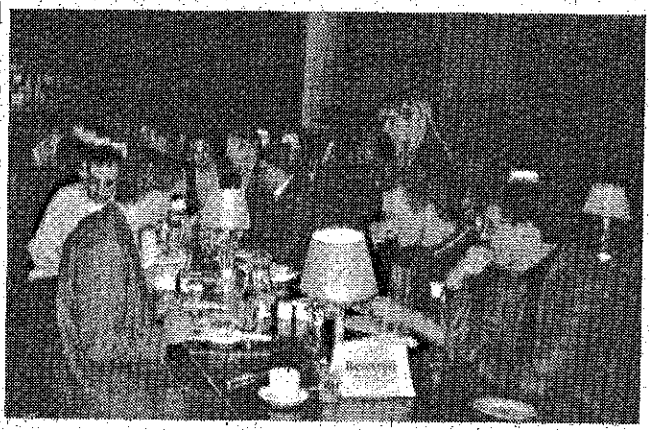
Baten (Tuebingen University) questioned Streb's use of index numbers and suggested that Streb discuss whether labor costs remained constant or whether forced labor or other irregularities could have had an effect. Finally, **Bart Van Ark**, **Herman de Jong**, and **Jan-Pieter Smits** (all University of Groningen) presented "Patterns of Technological Accumulation and Competitive Advantage in the Dutch Economy During the Twentieth Century." Peter Solar (Vesalius College, Free University of Brussels) cautioned against placing too much emphasis on the decline in R&D by Dutch multinationals. Solar observed that figures in the paper showed that productivity growth in finance, insurance, and business services had been negative for more than two decades – a fact that required explanation in light of this sector's relatively large share in the Dutch economy and its high level of investment abroad.

In the session on "Banking and Finance," **Caroline Fohlin** (CalTech) spoke on "Economic, Political, and Legal Factors in Financial System Development: International Patterns in Historical Perspective," where she analyzes political and legal factors determining financial structure. Fohlin was asked for a more detailed discussion of the variables used to measure political and legal factors and was asked to include regulatory measures in the model. Next, **Jaime Reis** (EUI) presented "Was There a Mediterranean Financial System in the 19th Century? A Comparison with Scandinavia." Reis looks at the role of financial history in the macroeconomic performance of these two groups of countries on the periphery of Europe. **Francesco Galassi** and **Lucy Newton** (University of Birmingham) finished this session with "My Word is My Bond: Reputation as Collateral in 19th-Century English Provincial Banking." Galassi and Newton develop a model of trust in a game played between the potential borrowing customer and the bank, with the bank attempting to assess the reputation and solvency of the borrower. In endeavoring to predict borrower actions, their analysis focuses on the importance placed by the bank on a customer's reputation, with the aim of providing a quantitative or money value on the asset.

Stefan Houpt (Universidad Carlos III) started the session, "European Economic Policies: 1850-1936," with "More than Ore: Modern Spanish Steel, 1856-1936." In his paper, Houpt notes that Spanish ore was particularly well suited for the Bessemer process. Given this, he asks why the steel industry did not develop in 19th-century Spain. Baten wanted Houpt to include more general considerations about the "Dutch disease" and presence of large and rich natural resources. Next, **Joan Roses** and **Blanca Sanchez-Alonso** (both Universidad Carlos III) discussed their study of "Regional Wage Convergence in Spain 1850-1936." The authors present new data on Spanish real wages by region and occupation, which suggests that the Spanish labor market was fairly well integrated. Hatton asked how the authors would explain the apparent disintegration of the labor market between 1914 and 1920 and what accounted for the negative urban/rural real wage gap observed from 1914 onwards for unskilled workers. The final paper of the session featured a study of "The Economic Policy of Fascist Italy in the Late 1930s" by **Roberto Di Quirico** (University of Pisa). He argues that the balance of payment constraint or shortage of currency explains most of the economic policy of the regime.

The session on well-being led off with a presentation of "The Anthropometric History of Early-Modern France" by **John**

Komlos (University of Munich), **Michel Hau**, and **Nicolas Bourguinat** (both University of Strasbourg). Bernard Harris (Southampton) requested that the authors discuss in detail the methods they used to control for changes in recruitment and changes in measuring practices. **Tommy Bengtsson** and **Martin Dribe** (both Lund University) followed with "New Evidence on the Standard of Living in Sweden during the 18th and 19th Centuries: Long-term Development of the Demographic Response to Short-term Economic Stress among the Landless in Western Scania." Ulf-Christian Ewert (Munich) commended Bengtsson and Dribe for their insights into the course of demographic developments before and during Swedish industrialization. In particular, Ewert noted the sophistication of the statistical analysis and the clear presentation of results. This net effect of a tremendous population growth within industrialization can be separated into age group specific mortality and fertility effects. **Andrew Godley** (Reading) closed the session with "The Convergence in Household Living Standards, 1870-1914."



High tea at the EHES meetings

"European Economic Development in the Long Run" opened the Sunday sessions. **Oliver Volckart** (Jena) spoke on "Central Europe's Way to a Market Economy AD 1000-1800: A Theoretical Model," an analysis of the emergence of the market economy in Central Europe. His results suggest that political disunity in Central Europe was the prime cause of the late emergence of markets. Millward wondered whether the Polish Commonwealth of the 16th and 17th centuries fits Volckart's framework. **Elise Brezis** (Bar-Ilan University) followed with "Population, Social Classes, and Economic Growth During Industrialization," a modelling exercise motivated by modern endogenous growth models. Harley commented that Brezis' model connects explicitly to Marxian views of the Industrial Revolution but seems poorly connected to recent research on the Industrial Revolution. **Adrianna Bunea** (University of Orleans) ended with "Economic Development by 'Conscious Design' or by 'Default'? The Case of Transition Economies in Historical Perspective." She contends that the key concepts that should determine transition programs and aid projects in the FSU and EE are "inside-out change" and "autonomy-compatible help."

"European Economic Policies: 1930-1973" began with "Winning the War, Losing the Peace? Britain's Recovery in a West German Mirror" by **Barry Eichengreen** (Berkeley) and **Albrecht Ritschl** (Zurich). Broadberry challenged Eichengreen and Ritschl's rejection of the catching up perspective. He thought that most of the puzzle could be resolved if the authors included the United States in the analysis, due to the position of the US relative to that of Germany and Great Britain. **Luciano Amaral** (European University Institute, Universidade Nova de Lisboa) presented "Education and Human Capital Accumulation in Portugal During the Estado Novo Period (1930-1973)." David Mitch (UMBC) praised Amaral's emphasis on the role of supply side factors in low productivity growth in service sector

activities like education. He also stressed that little is available in English on this topic and recommended that Amaral focus more on the primary level. The session closed with **Stefano Battilossi** (Universidad Carlos III), who presented "The Regulation of International Banking as an Agency Problem; the Deutsche Bundesbanke and the Bank of Italy under Bretton Woods 1958-71." Ritschl wondered why the Bundesbank chose not to impose the more direct controls that Banca d'Italia employed. He also felt that Battilossi should consider the effect that principal/agent problems have on the results.

The final session, "European Integration since 1945," opened with **Henrik Zobbe** (The Royal Veterinary and Agricultural University Copenhagen), author of "The Economic and Historical Foundation of the Common Agricultural Policy in Europe." Zobbe concentrates on the economic and historical foundation of the common agricultural policy in Europe, finding three reasons for the CAP: the general economic integration process in Europe, the prior history of agricultural support policies in the European countries since the late 19th century, and the effects of the price support scheme that was chosen. Jan Bohlin (Goteborg University) thought that Zobbe's argument could be strengthened by a focus on the political economy aspects of the chosen policy and added that this should include a discussion of the role of farmers as an organized interest and lobby group. **Tobias Witschke** (European University Institute) continued with "Steel Trade in the European Coal and Steel Community (ECSC) from 1952 to 1964: One Integrated Market?." Masahiro Hayafuji (WTO) stated that if domestic prices had been kept artificially high before 1958, third country producers might have undercut them. Their ability to do so would have been constrained by the level of external barriers, including tariff levels, tariff escalation, the effective rates of protection, and quotas. For similar reasons, the degree of intraregional trade liberalization would affect the extent of trade creation.

Lars Jonung (EU and Stockholm) delivered the final paper of the conference, "The Evolution of EMU; The First Decade 1999-2009," where he asks what the history of monetary unions tell us about the future of the unprecedented experiment of European Monetary Union. He concludes that EMU was bound to happen more or less spontaneously by 2010, even if actual political initiatives had not been taken. In answer to the question he poses, he follows aspects of both J.S. Mill and Karl Marx. Mill believed the "irrationality" of national currencies would cause them to disappear. Marxist predictions of the collapse of capitalism have never been falsified, because there was not a time scale assigned to the prediction – just because it has not collapsed in the last 200 years, does not mean that it will not in the next 200 years. But timing is important. Jonung contends that optimum monetary areas are created by political will rather than by economic circumstances. Yet extreme regional disparities and depopulations in Europe today raise some doubts. Foreman-Peck complimented Jonung's skill at combining historical expertise and economic analysis but noted that Jonung dismissed 1970s policies as unsatisfactory.

The next meeting of the European Historical Economic Society will be held in 2003, and the organizer of the meetings will be Leandro Padros de las Escosura. For more information, check the Society's webpage at http://eh.net/EHES/

Personal Reflections

British and International Economic History in the Twenty-First Century

By Stephen Broadberry

Reprinted from *Living Economic and Social History*, Pat Hudson (ed.)
Glasgow: Economic History Society, 2001.

An important challenge facing economic history at the beginning of the twenty-first century is the growing internationalisation of academic life. In the context of a British economics department, where I am based, research is increasingly assessed in terms of its international importance, growing numbers of students are coming from abroad, and all students are increasingly adopting a more outward looking viewpoint. However, it would clearly be inappropriate for each country to stop covering its own economic history because of pressures for international coverage. A more appropriate strategy, in my view, involves dealing with national economic history in an outward looking way as well as studying the international economy.

An outward looking approach to British economic history means more than simply comparing with other countries. Indeed, one highly inappropriate approach to modern British economic history has been the exaggeratedly pessimistic view derived from highly selective comparisons with other countries, focusing only on the successes of any particular country and ignoring all failures.[1] Thus the 'declinist perspective' on the British economy since the late nineteenth century often combines the most successful aspects of the economies of the United States, Germany and Japan into a unified example of 'modern best-practice' that represents some golden missed opportunity. A truly outward looking approach would note the failures as well as the successes in each country, and recognise that these countries all fit together in a world economy characterised by comparative advantage and gains from trade. It is thus not appropriate to assess the British economy simply by how closely it followed the business model of the United States, for example.[2]

A good example of the outward looking approach in practice is the work of Crafts and Harley on the British Industrial Revolution. Using the Chenery-Syrquin approach to derive 'European norms' for characteristics of the economy at particular levels of development, Crafts (1985) was able to show how Britain's development path differed substantially from that of other European countries. In particular, he noted the early release of labour from agriculture in Britain, so that by the early nineteenth century, Britain had an unusually small share of the labour force engaged in a relatively high productivity agricultural sector. Combined with the findings of Harley (1982) and Crafts (1985) on the relatively slow acceleration of industrial productivity growth during this period, this suggests that the key feature of Britain's Industrial Revolution was the structural transformation leading to the establishment of a large, but not particularly high productivity, industrial sector.

My own research on Britain's productivity performance compared with the United States and Germany since the mid-nineteenth

century, using a sectoral approach, complements the Crafts-Harley vision of the Industrial Revolution, as well as offering a reinterpretation of Britain's loss of overall productivity leadership. Working with a sectoral breakdown of GDP per employee, it is possible to show that both Germany and the United States overtook Britain in terms of aggregate labour productivity largely by shifting resources out of agriculture and improving their relative productivity performance in services rather than by improving their position in industry.[3] Despite its apparent iconoclasm, note that this view solves a number of puzzles in the literature. First, the debate on British and American technology in the nineteenth century following the work of Habakkuk (1962) makes much more sense if the US labour productivity lead in industry already existed in the nineteenth century rather than emerging in the twentieth century. Second, as I have already suggested, it sits rather well with the Crafts-Harley view of Britain's industrial sector being large rather than highly productive during the nineteenth century. And third, it makes sense of the dominance of London in international services during the late nineteenth century, and its subsequent relative decline.

Note the important change in the way that services and industry are viewed in this interpretation. Although there was a large US/UK productivity gap in industry in the late nineteenth century, it did not get any worse over time. On the other hand, although there was never a large productivity gap in services, Britain moved from a position of productivity leadership to a position of a modest productivity gap. Hence if we want to explain what changed between the late nineteenth and the late twentieth centuries, it is the loss of productivity leadership in services that really matters. And yet it is still conventional to read that Britain's relative decline has been exaggerated by excessive focus on industry, with services being neglected.[4] At first sight, this reinterpretation of Britain's productivity performance does appear to sit uneasily with the industrial orientation of much of the literature on British relative economic decline. Here, however, I would suggest that the central concern of the literature on de-industrialisation needs to be turned on its head. Given the expansion of industry during World War II, and the diminishing returns encountered, a movement of resources out of industry was inevitable during the postwar period, and the attempts to prevent this de-industrialisation were counter-productive.

I like to see this work on Britain's productivity performance as providing a bridge between macro-level research on international comparisons of productivity and the micro-level industry studies that have constituted such an important part of the British economic history tradition. Writing the case study chapters of *The Productivity Race,* I was struck by the wealth of knowledge embodied in these industry studies. It would be a shame if this type of work were to disappear.

The danger with the international economy as a topic must be superficiality. Hence it is positive to be able to report that there has been much good work on the international economy in recent years, particularly that based around the international monetary system. Books such as Eichengreen (1997) and Foreman-Perk (1995) provide excellent summaries of this work. Work on growth at the global level has perhaps been more mixed, since it is all too easy for research that covers all countries to lack the depth that comes from studies based on individual countries. Economic history has always drawn heavily on detailed local knowledge,

and it would be unfortunate if this strength of earlier work were discarded in the attempt to meet the challenges of relevance in the twenty-first century. A number of recent studies of European growth have succeeded in striking the right balance between breadth and depth. In particular, I would mention Crafts and Toniolo (1996) and Feinstein et al. (1997), with the latter also covering more general macroeconomic history. These works reflect the rapid emergence of a European economic history that is grounded in national cultures but also outward looking, and I would see this as one of the most positive developments of the last decade. The European Historical Economics Society has played an important role here, with the *European Review of Economic History* providing an outlet for the high quality literature that is emerging in this area.

In summary, economic history needs to internationalise. But this implies an outward orientation to British economic history as well as international economic history.

Stephen Broadberry (b. 1956) was educated at Warwick and Oxford. He has taught at Universities of Oxford, Cardiff and British Columbia, and he is currently Professor at the University of Warwick. He has published widely on the macroeconomic history of Britain, Germany and the international economy during the inter-war period. His primary research interests are now in the area of comparative growth and productivity performance, focusing in particular on Britain, Germany and the United States from the late nineteenth century to the present.

[1] See, for example, Levine (1967), Wiener (1981), Elbaum and Lazonick (1986).
[2] As in the work of Chandler (1990), for example.
[3] Broadberry (1998).
[4] See, for example, Rubinstein (1993).

Works Cited:

Broadberry, S.N. (1987), *The Productivity Race: British Manufacturing in International Perspective, 1850-1990*, Cambridge: Cambridge University Press.

Broadberry, S.N. (1998), 'How did the United States and Germany Overtake Britain? A Sectoral Analysis of Comparative Productivity Levels, 1870-1990', *Journal of Economic History*, 58, 375-407.

Chandler, A.D. Jr. (1990), *Scale and Scope: The Dynamics of Industrial Capitalism*, Cambridge, MA: Harvard University Press.

Crafts, N.F.R. (1985), *British Economic Growth during the Industrial Revolution*, Oxford: Clarendon Press.

Crafts, N.F.R. and Toniolo, G. (eds.) (1996), *Economic Growth in Europe Since 1945*, Cambridge: Cambridge University Press.

Eichengreen, B. (1997), *Globalising Capital: A History of the International Monetary System*, Princeton, NJ: Princeton University Press.

Feinstein, C.H., Temin, P. and Toniolo, G. (1997), *The European Economy Between the Wars*, Oxford: Oxford University Press.

Foreman-Peck, J. (1995), *A History of the World Economy: International Economic Relations Since 1850*, (second edition), London: Harvester Wheatsheaf.

Elbaum, B. and Lazonick, W. (eds.) (1986), *The Decline of the British Economy*, Oxford: Oxford University Press.

Habakkuk, H.J. (1962), *American and British Technology in the Nineteenth Century*, Cambridge: Cambridge University Press.

Harley, C.K. (1982), 'British Industrialisation before 1941: Evidence of Slower Growth during the Industrial Revolution', *Journal of Economic History*, 42, 267-289.

Levine, A.L. (1967), *Industrial Retardation in Britain, 1880-1914*, New York: Basic Books.

Rubinstein, W.D. (1993), *Capitalism, Culture and Economic Decline in Britain, 1750-1990*, London: Routledge.

Wiener, M.J. (1981), *English Culture and the Decline of the Industrial Spirit 1850-1980*, Cambridge: Cambridge University Press.

Call for Papers

Centre for the Economics of Globalization Conference

Trinity College, Dublin

August 29-31, 2002

The Centre for the Economics of Globalization welcomes paper proposals for "The Political Economy of Globalization: Can the Past Inform the Present?." The focus of the conference will be to ask whether the past offers guides to policy makers today as they grapple with such questions as: How has the political economy evolved in the past and can it inform the future? Will the past be repeated, or will governments preserve the benefits of freer world trade and world capital markets? If governments are able to resist the present backlash against globalization, are there particular distributional or regulatory policies which they will have to consider?

Topics might include: the impact of past globalization on income distribution; the measurement of long run trends in international economic integration; and identification of the determinants of government policy towards international trade, migration and capital flows. Contributions which deal with the experience of the European periphery, Latin America and Asia are particularly welcome.

Economists, economic historians and political scientists are invited to submit paper proposals by e-mail to:

Kevin O' Rourke kevin.orourke@tcd.ie) or
Jeffrey Williamson (jwilliam@kuznets.fas.harvard.edu).

Titles and abstracts must be received by January 22, 2002

For more information, please contact conference organizer, Kevin O'Rourke, at Tel. 353 1 608 3594 or Fax. 353 1 677 2503. You may also visit the following website: http://econserv2.bess.tcd.ie/korourke/homepage.htm

A Letter from the Editor

Happy Holidays One and All,

First, let me apologize for the tardiness of this issue. The tragic events of September 11th caused the postponement of the Economic History Association's annual meeting. Because we felt it important to cover that conference before the spring issue, we put the entire fall issue on hold. This should still reach all of you in time to get copies of the abstracts for the economic history sessions at the upcoming ASSA meetings.

Speaking of the ASSA meetings, the number of sessions devoted to economic history has increased. In addition to the two Cliometrics sessions, an EHA session has been added. Enclosed in this issue of the Newsletter you will find a set of abstracts and the schedule for the Clio and EHA sessions to be offered in Atlanta. As if you needed further inducement to attend, the secret location of the annual Clio Cocktail Extravaganza will be revealed at the sessions. This year's party, hosted by North Carolina State, will be held on Saturday, January 5th from 8-11 pm. Rumor has it that the luxurious NC State suite is located somewhere in the Hilton Hotel. Further rumor has it that Pepsi will be the only soft drink served.

This issue is proof that one is never to old to learn things that one did not even know that one did not know. That one would be me. Part of the blessing and curse of having an English major as a managing editor is that you cannot get away with using incorrect grammar or punctuation, no matter how obscure the usage may be. Hidden within the reports of this issue are four examples of a rare use of punctuation. The use is so obscure that a colleague of mine in the English Department declared that though it is correct usage, probably no more than a dozen people would know it. While that may beg the question of relevancy, we here at the editorial office (in our eternal search for truth, justice, and the proper usage of commas) will continue to tow the grammatically correct line. For those of you who have loads of free time between now and Christmas and like to spend your time looking for obscure items, let me know if you find the four above-mentioned examples. The first person to corner me at the cocktail party and regale me with the details of their adventures in punctuation sleuthometrics will win a nice prize. You can choose from among our limited edition bobble-head dolls of your favorite economic historian, our Cliom bedroom slippers, or a pen.

Finally, please allow me to draw your attention to the call for papers on the reverse side of this letter. We are hosting the annual Cliometrics Conference here in La Crosse next May and encourage your participation. La Crosse is a glorious destination in May (having said that, I have now pretty much guaranteed three days of rain and gale force winds), and the Clio conference itself is a great destination anytime and anywhere.

Wishing you and yours a safe and happy holiday season.

Michael J Haupert
Editor

Call for Papers

Cliometric Society Annual Conference
La Crosse, Wisconsin
May 10-12, 2002

The annual Cliometrics Conference is designed to provide extensive discussion of new and innovative research in economic history. Typically, 12 papers are selected for presentation and discussion. These are sent out to all conference participants in advance. In the session devoted to each paper, authors make a five-minute opening statement, and the rest of the session is devoted to discussion by all conference participants. Conference participation is by invitation only, but every attempt is made to invite a mixture of new and established scholars.

Those wishing to present a paper should submit a 3-5 page summary of the proposed paper using the application form under the Cliometrics Conference listing on the www.eh.net website. Furthermore, paper presenters, as well as those wishing to attend the conference, should provide their addresses, phone and fax numbers, and email addresses using the same. **The deadline for proposals and requests to attend the meetings is Friday, February 1, 2002.**

Although we prefer that applicants use the online application form, proposals for papers may be sent by mail to:

Lanna Miller
Cliometrics Conference Secretary
Department of Economics
University of Arizona
Tucson, AZ 85721
(520)621-2821

They can also be faxed to Lanna Miller at (520)621-8450 or sent by e-mail to lmiller@bpa.arizona.edu. For more information, please contact Price Fishback at pfishback@bpa.arizona.edu or (520)621-4421.

Those presenting papers will be notified by March 1, 2002 and are expected to provide a completed draft of the paper in the proper format for the conference volume by **April 3, 2001**.

Economic History Sessions at the ASSA meetings
Atlanta, January 5-7, 2002

Session 1: Saturday, Jan. 5, 8:00am, Hilton-Cabinet Room

The Impact of Deflation: A Historical View

Richard C. K. Burdekin (Claremont McKenna College) and Pierre L. Siklos (Wilfrid Laurier University), "Fears of Deflation Then and Now"

Randall E. Parker (East Carolina University) and James S. Fackler (University of Kentucky), "Was Debt Deflation Operative During the Great Depression?"

Chris Hanes (University of Mississippi), "The Liquidity Trap and U.S. Interest Rates in the 1930s."

Session 2: Saturday, Jan. 5, 10:15am, Hilton-Cabinet Room

Life, Death, and Work: An Economic History of Race and Labor Markets in Twentieth Century America

Wayne Grove (Syracuse University) and Craig Heinicke (Baldwin-Wallace College), "Driven from the Field or Enticed to the City?: The Cotton Picking Machine and the Great Migration from the Cotton Belt, 1949-1964."

Ryan Johnson (University of Arizona), "Did the FEPC Increase Black Employment in High-Wage Industries?"

William J Collins (Vanderbilt University) and Melissa A. Thomasson (Miami
University), "Exploring Racial Gaps in Infant Mortality, 1920-1960"

Clio Cocktail Party: Saturday, Jan 5, 8-11 pm, North Carolina State suite, Hilton Hotel

Session 3: Saturday, Jan. 5, 2:30pm, Hilton-Cabinet Room

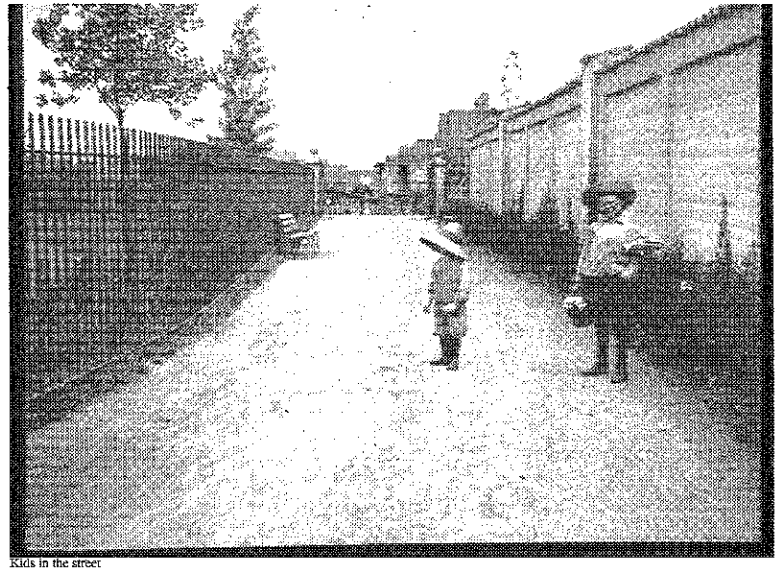
The Development and Origins of the Federal Reserve System and Its Impact on Financial Markets

Charles W. Calomiris (Columbia University) and Joseph R. Mason (Drexel University), "Resolving the Puzzle of Low National Bank Note Issuance."

Jon Moen (University of Mississippi) and Ellis Tallman (Federal Reserve Bank of Atlanta), "Why Didn't the United States Establish a Central Bank Until After the Panic of 1907?"

Michael McAvoy (SUNY at Oneonta), "How Were the Federal Reserve Bank
Locations Selected?"

J. Peter Ferderer (Macalester College), "Building a Money Market: The Case
of Bankers' Acceptances, 1914-1939."



Kids in the street

Session 1: The Impact of Deflation: A Historical View (Saturday, Jan. 5, 8:00am, Hilton-Cabinet Room)

Boys in sacks

"Fears of Deflation Then and Now," Richard C. K. Burdekin (Claremont McKenna College) and Pierre L. Siklos (Wilfrid Laurier University)

Surveys of the kind conducted by Shiller (1997) have confirmed the view that the public understands the costs of inflation, even if neither the public nor policy makers can agree on a definition of price stability. However, there is relatively little understanding of the consequences of deflation. In part this is due to the fact that episodes of sustained falls in consumer prices have been rare since the 1930s. In addition, until recently perhaps, it has also been implicitly assumed that, in the case of asset prices, there is a significant asymmetry between rising and falling asset prices. The dichotomy between consumer and asset prices on the one hand, and the belief that there are only negative consequences from deflation on the other, is neither well understood nor has the issue been adequately analyzed. During the Great Depression both goods and asset prices fell and the economy slumped. Although this experience does not characterize the entire history of deflationary episodes (BIS 1999, 78-80), Sylla (1991) suggests that nineteenth century US evidence does offer further examples of widespread speculative excesses apparently triggering a cycle of boom and bust that produced not only financial disturbances (or "panics') but full-blown economic depression. Moreover, Sylla (1991, p. 10) argues that, while depressions did not follow all such financial disturbances, in each case where this *did* happen "speculation took place on a number of fronts simultaneously and ... at least some of these fronts were unlikely to have panned out financially even if financial disturbance and depression had not ensued."

The premise that deflation, especially in consumer prices, is very costly for the economy as a whole itself has a long history in the economics profession. Keynes (1923), for example, argued strongly that deflation is worse than inflation (also see Laidler 1999, p. 109; Meltzer 1988, p. 47). Policymakers also seem to have shared this view. In an interview shortly before taking office, the Governor of the Bank of Canada, David Dodge, stated: "I think the costs of going down to [zero inflation] are high, and there are real asymmetries when you get into price deflation. We haven't got much evidence that things work a lot better at zero than they do at one or two" (Thorsell, 2001). John Taylor, reflecting on the recent Japanese experience with deflation, echoes the Governor's thinking by stating that: "I get worried about deflation and that is another reason to have an inflation target" (Snowdon and Vane 1999, p. 201). Contrasting this position is the view expressed by the Governor of Japan, for example, that in a low inflation environment with productivity gains, deflation is possible and a monetary policy that sets out to aggressively counter such price declines might be counterproductive. Indeed, Hayami (2001) states that "at a time when prices decline on account of productivity gains based on rapid technological innovation, a forceful reduction in interest rates with a view to raising prices may amplify economic swings."

In this paper, we explore public and policy maker perceptions and reactions to deflations. We analyze the views of economists and the financial press, primarily in North America and the United Kingdom, at the time of the deflation in the second half of the 19th and the first thirty years of the 20th century. During these episodes both consumer and asset prices fell. We compare these experiences to the recent case of deflations in both asset prices and consumer prices in China and Japan. We rely on econometric and descriptive evidence throughout.

We conclude that there is some similarity between the sequence of events observed in China and Japan in the 1990s and those of the United States following the Wall Street Crash of 1929. In each case large asset price declines accompanied by monetary contraction are later followed by outright deflation and falling goods prices. While the limited data available on the Chinese and Japanese experiences are insufficient to establish a demonstrable causal link between asset prices and consumer prices, a more consistent predictive role for asset prices – particularly housing prices – has been suggested by Goodhart and Hoffman (2000). The question of whether asset prices should actually be targeted in central bank policymaking remains highly controversial but has recently received increasing attention in the literature (see, for example, Kennedy et al., 1998; Bernanke and Gertler, 1999; Filardo, 2000; Vickers, 2000; Bryan, Cecchetti and O'Sullivan, 2001; and Goodfriend, 2001).

One of our objectives is to address the question of whether today's fears of deflation are well-founded, both in relation to what is known from economic analysis as well as from the reactions of the public and policy makers. Another key issue is the effectiveness of, and rationale for, policy initiatives aimed at combating deflation and whether these initiatives should take into account the movements in asset prices.

Prior to World War II, a review of the academic and business press clearly reveals that concern over the consequences of deflation focused almost entirely on commodity prices, and agricultural prices in particular. By contrast, the focus of the recent alarm over the possibility of deflation centers around the behavior of asset prices, especially financial asset prices. In a sense this is not surprising since a striking difference between pre WWII

and post-war economies stems from the relatively small fraction of the labor force in non-agricultural pursuits and the tremendous growth in financial sophistication, especially in the industrial world. Bordo and Jonung (1987; also see Siklos (1993) and Bordo, Jonung and Siklos (1997)) document these features of economic history in five countries (Canada, Sweden, the UK, the US, and Norway).

However, there is also nowadays a more general concern over the potential of deflation in an index of overall prices due to the widespread adoption of inflation objectives, either of the implicit or explicit varieties. Nor are worries about the behavior of commodity prices entirely a relic of the past. The BIS (1999, Graph IV.9) claims that inflation targeting countries are characterized by a relatively large fraction of exports to GDP. In addition, of course, these same countries are also small open economies that can conceivably suffer the twin shocks of lower commodity prices and a falling exchange rate. The latter can conceivably be inflationary, at least in the short-run, and may lead to an interest rate response in order to prevent inflation from failing to meet stipulated objectives. The former, if it feeds into the rest of the economy, can possibly lead to a "deflationary spiral". Therein lies the importance placed on the symmetric response to deviations in inflation from some central objective. If, instead, consumer prices continue to rise, then commodity and consumer prices will diverge. This is, in essence, a modern manifestation of the "scissors" problem familiar to policy makers in the early part of the century. The problem originally referred to the divergence between agricultural and industrial prices and the overall impact this distortion is said to have had on the flow of resources from one sector of the economy to the other. Of course, as commodity-based industries have fallen considerably in importance, the closing of the "scissors" is no longer as great a policy issue as it once was. Nevertheless, the divergence between overall prices and financial asset prices raises similar questions with potentially important consequences.

In reviewing how thinking about the consequences of deflation has evolved over time it is important to keep in mind that, whereas reactions in the 1990s are usually framed within some "model" of the economy, the pre-1940s discussion tends to be descriptive and not analytical. A significant reason, as explained by Morgan (1990), and Woodford (1999), is that macroeconomics as a discipline was still in its infancy, while statistical tools and the necessary data to uncover the determinants of business cycle fluctuations were only being developed or introduced by the 1920s.

Theories of the business cycle were narrative and only the simplest of interactions between markets were considered. Yet, these views, while largely accurate, are surely over-simplifications in the sense that there were attempts to achieve a consensus of sorts about the desirability of price stability, although in the context of a "model" where business cycles were thought to be more or less inevitable. Bellerby (1924), for example, goes so far as to argue for an inflation target of 3% growth in the price level, although not calling it as such, together with exemptions "in cases of real emergency". However, the proposal "would allow the continuance of either prosperity or slight depression over long periods of time" (op.cit., p. 178). Keynes (1925) also advocated a 'managed currency' in terms of commodity prices, to reduce instability in trade and mitigate its impact on employment, in a scathing review of a report on the Bank of England. But it was to no avail as his views, and those of other advocates of some form of price level or inflation targeting, fell on deaf ears among policy makers and many academics blinded by the ideology of the Gold Standard (see Eichengreen 1992). The experience of the Great Depression further reinforced the desirability of price stability not so much to prevent deflation at all costs but to reduce the variability in prices. "As to the larger conclusion, we know this: Periods of serious price disturbances are periods of industrial and financial disturbance and social unrest. Practically never one without the other. And periods of price stability are periods of industrial and social equilibrium and sanity." (Snyder 1935, p. 202). It would take the economics profession over 60 years to work out formally the notion that inflation targeting, while desirable, must confront the trade-off between the variability of inflation and output. Nevertheless, the concern over these issues is not new.

Then there is the question of expectations. Thus, while expectations play a role in descriptions of the process of deflation (and inflation) they are viewed as being more or less exogenous, if not almost deterministically driven so that depression and deflation could become self-fulfilling prophesies. "The continuous expectation of a further fall in prices has had a very restricting influence on the buying power of the general public. The steady reduction in prices has made it impossible in a great many cases to pay back money borrowed at a time when prices were higher.... Further, restriction of credit have followed, with the result that prices have been forced down still more..." (New York Times, November 27, 1921, page 8, col. 1)

There were also doubts expressed about the efficacy of monetary policy, other than perhaps in the short-run, but these had to confront the lack of hard evidence about the connection between falling prices and the real economy. These considerations make the common view that rising prices brought about by monetary causes are beneficial to production at least doubtful. It is still true that 'One wants very much stronger statistical evidence than one yet has to prove that a fall in prices diminishes perceptibly, and in the long run, the total productiveness of industry' " (Pigou 1923; the quote is by Alfred Marshall from the report of the Gold and Silver Commission).

In what follows we present some stylized facts for the US alone. In a future version of the paper we plan to add evidence from the UK, Canada, Sweden and Australia. Since deflation, as defined above, is a feature of the economic experience of the pre World War II era, data limitations preclude useful statistical testing beyond the annual frequency, especially for a cross-section of countries that we wish to emphasize.

One way of thinking about the consequences of deflation versus inflation is to examine their persistence properties. Burdekin and Siklos (1999) find, using a data set similar to the one being used here, that an AR(1) model of inflation, augmented with other variables (see below), adequately explains the evolution of inflation over long periods of time. We find that inflation is easier to predict, based on past history, than deflation, and this may be one reason for the unease with policies or events that lead to falling prices. If deflation is more difficult to forecast then unexpected movements are more likely. Overall, an AR(1) model with a memory of deflation only does a better job over the two centuries of inflation and deflation than the model that has a memory of inflation alone. An AR(1) model, while useful as a descriptive device, is hardly a complete model of inflation. Moreover, there are potentially many determinants of deflation and inflation. For example, a list of such additional determinants might include the following: stock prices, wholesale prices, NBER reference cycle dates, the term spread, a proxy for financial innovations, and the default spread. Wholesale price inflation proves to be the most reliable predictor in CPI inflation, followed by NBER recession dates, the terms spread, the proxy for financial innovation, the default spread, and, finally, stock prices. It is usually the case that inflation

is better forecasted when default risk is a determinant of inflation than when financial innovations enter into the picture.

To date, however, we have not made use of the possible asymmetry between episodes of inflation or deflation. Shiller (1997), for example, reports that there is a substantial difference between the type of "model" used by wage and price setters and the models economists are fond of estimating via econometric techniques. Indeed, Kanheman and Tversky (1979) argue that "editing" of information is commonplace. A related literature might be the one that deals with problems of "bounded rationality". We also consider a variety of estimates based on Tobit models of the inflation process. We continue to find that deflation is more difficult to explain than is inflation, as evidenced by the considerably lower explanatory power of the relevant regressions.

Criticisms of this approach to consider in future work includes asking whether individuals actually "censor" some of the data when estimating a model of inflation or deflation, as well as whether it is appropriate or not to censor the CPI series but not the wholesale price data. Nevertheless, there are some interesting additional observations that can be made about some of the results we have found so far. First, censoring inflation, which, as noted above, is a likely scenario given the history of price movements in the United States, implies, for example, that the Great Depression could not have been anticipated, despite the deflation of the 1920s. Second, even if agents use all available information, they will consistently underestimate the *severity* of sharp inflations or deflations. Finally, a model that is based solely on, shall we say, "fears" of deflation may end up predicting that the deflation will end up becoming uncontrollable.

A critical element in the foregoing analysis is that inflation forecasts are based on purely backward-looking models. Fuhrer (1997) argues that the behavior of actual US inflation is best described using a mix of backward and forward looking features. In contrast, a purely forward looking model is found to be severely at odds with the data. He also notes that his conclusions, not surprisingly, may be model specific.

Accordingly, we also estimate a version of our baseline model that mixes forward-looking and backward looking behavior along the lines suggested in Fuhrer (1997). It is rather clear from our estimates that both forward and backward-looking elements help explain the inflation process since 1876. We also find that backward looking elements explain the process relatively better when inflation is low and stable, as in the 1960s, while forward looking elements rise when there are sharp changes in actual inflation as, for example, in the presence of the two oil price shocks of the 1970s. It also appears to be the case that both forward and backward-looking elements in inflation are rather unhelpful in forecasting deflation, as is readily apparent for the most part during the 1895-1920 period. The results are consistent with those of Chadha, Masson, and Meredith (1992) but our findings but stand in contrast with Fuhrer's (1997) evidence that reports far higher estimates of the degree to which there is a forward-looking element in inflationary expectations.

As noted earlier, fears of deflation are perhaps best interpreted in light of the trade-off between the variability of inflation and output. Based on that framework, we immediately see the desirability of inflation targeting certainly versus the Gold Standard (the differences are magnified when measured in terms of variances), and even a slight preference over the Bretton Woods era. While there are considerable differences in output performance as between wars, the Great Depression, inflation variability is comparable. Clearly, the decades of the 1930s and 1940s stand out both as highly volatile in output though not necessarily in terms of inflation. Even if inflation volatility may have been lower during some of the earlier decades of the 20th century or the late 19th century, output volatility is relatively lower in every decade since World War II.

We have, so far, concentrated on the behavior of consumer and commodity prices. Not to be forgotten is the role of financial asset prices, their connection to economic activity in general as well as their role in stoking fears of deflation. We intend to explore these connections in an upcoming version of this paper.

"Was Debt Deflation Operative During the Great Depression?" Randall E. Parker (East Carolina University) and James S. Fackler (University of Kentucky)

Important questions remain regarding the transmission mechanism that linked falling prices and falling output during the Great Depression, despite recent analysis suggesting that the worldwide deflation was initiated and propagated by negative impulses associated with the interwar gold standard. A key issue is whether the deflation during the early 1930s was unanticipated prior to the onset of the Depression. If it was at least largely unanticipated, then when combined with rapidly rising nominal debt issuance, non-monetary, financial theories contribute to a satisfactory explanation of the persistent monetary non-neutralities that were present during the Depression. These theories, notably Bernanke's (1983) explanation and extension of Fisher's (1933) original debt-deflation hypothesis, hold that events in the financial markets other than shocks to the money supply can account for the paths of output and prices without invoking irrational behavior on the part of agents.

In his debt-deflation hypothesis, Fisher asserted that (nominal) over-indebtedness and deflation are the dominant forces that account for "great" depressions. Specifically, he argued that given nominally-denominated debt contracts, a protracted fall in prices and nominal incomes substantially increased real debt burdens, led to debtor insolvency, lowered aggregate demand and thereby contributed to a continuing decline in the price level and thus further increases in the real burden of debt.

Bernanke (1983), in what is now called the "credit view," provided additional details to help explain Fisher's debt-deflation hypothesis. He argued that in normal circumstances, an initial decline in prices merely reallocates wealth from debtors to creditors, such as banks. Usually, such wealth redistribution has no first-order impact on the economy. However, in the face of large shocks, deflation in the prices of assets forfeited to banks by debtor bankruptcies leads to a decline in the nominal value of assets on bank balance sheets. For a given value of bank liabilities, also denominated in nominal terms, this deterioration in bank assets threatens insolvency. As banks reallocate assets away from loans to safer government securities, some borrowers, particularly small ones, are unable to obtain funds, often at any price. Further, if this reallocation is long-lived, the shortage of credit for these borrowers helps explain the persistence of the downturn. As the disappearance of bank financing forces lower expenditure plans, aggregate demand declines, which again contributes to the downward deflationary spiral.

Analyses such as Cecchetti (1992), which carefully demonstrate that the deflation was anticipated at short horizons *once it started*, do not bear directly on our discussion. Instead, "[t]o show that debt-deflation was an important part of the contraction of the early

1930s it is important to document the accumulation of substantial medium- and long-term debt prior to 1929." In addition, "[t]he finding that deflation could have been anticipated at horizons of 3-6 months does suggest that simple debt-deflation theories for the propagation of the Great Depression must rely on the failure of agents to anticipate deflation several years in advance, not several quarters in advance." Below, we argue that debt-deflation was operative both because of a rise in nominally-denominated debt and because the deflation in the early 1930s was unanticipated *when this debt was being incurred.*

We demonstrate three facts consistent with the debt deflation/credit view explanation of the Depression. First, we document that private medium- and long-term nominal debt during the 1920s exhibited a combination of a high initial value relative to income and a rapid growth rate that is unparalleled in a consistent data set covering more than half a century. Second, using estimation results from a Markov switching model of the price process, we argue that the debt issued during the decade preceding the downturn occurred in a stable price regime. Third, using the results of this model of prices, we show that at about the onset of the Depression, the price process switched to one of deflation. These second and third facts lead us to conclude that the deflation was unanticipated when the debt was being issued. Taken together, the evidence suggests that the rising real debt burdens at the beginning of the Depression were unanticipated and the necessary conditions appear to have existed for debt deflation to be operative during the Great Depression.

References

Bernanke, Ben S. "Nonmonetary Effects of the Financial Crisis in Propagation of the Great Depression." American Economic Review 73, June 1983, 257-76.

Bernanke, Ben S. "The World on a Cross of Gold." Journal of Monetary Economics, 31, 1993, 251-267.

Cecchetti, Stephen G. "Prices During the Great Depression: Was the Deflation of 1930-1932 Really Unanticipated?" American Economic Review 82, March 1992, 141-56.

Dominguez, Kathryn M., Ray C. Fair, and Matthew D. Shapiro. "Forecasting the Depression: Harvard versus Yale." American Economic Review 78, September 1988, 595-612.

Eichengreen, Barry. Golden Fetters: The Gold Standard and the Great Depression, 1919-1939. (New York: Oxford University Press, 1992).

Evans, Martin, and Paul Wachtel. "Were Price Changes During the Great Depression Anticipated? Evidence from Nominal Interest Rates." Journal of Monetary Economics, 25, October 1993.

Fisher, Irving. "The Debt-Deflation Theory of Great Depressions." Econometrica 1, October 1933, 337-57.

Hamilton, James D. "Monetary Factors in the Great Depression." Journal of Monetary Economics 19, March 1987, 145-69.

Hamilton, James D. "Was the Deflation during the Great Depression Anticipated? Evidence From the Commodity Futures Market." American Economic Review 82, March 1992, 157-78.

Nelson, Daniel B. "Was the Deflation of 1929-30 Anticipated? The Monetary Regime as Viewed by the Business Press." In Roger Ransom (ed.) Research in Economic History. (Greenwich, CT: JAI Press, 1991).

"The Liquidity Trap and U.S. Interest Rates in the 1930s," Chris Hanes (University of Mississippi)

In the late 1990s, low inflation and nominal interest rates in the U.S. and Europe, and a major depression in Japan accompanied by short-term interest rates barely above zero, revived interest in the notion of a "liquidity trap" as a potential constraint on the effectiveness of monetary policy. Krugman (1998) argues that Japan was indeed in a liquidity trap, defining that to be "a situation in which conventional monetary policies have become impotent, because nominal interest rates are at or near zero: injecting monetary base into the economy has no effect" (p. 141). Long-term interest rates usually exceed short-term rates, of course, and remained above zero even in 1990s Japan. But most recent discussions of liquidity traps argue that *all* interest rates are bound as soon as rates at the *shortest* maturity, that is overnight rates, have hit the zero floor. Presumably, a longer-term rate equals the expected return to a series of overnight investments, plus a term premium. Term premiums are not generally believed to be affected by a central bank's open-market operations, except to the degree that the yield curve can be twisted by changes in the maturity structure of government debt, which is doubtful. Thus, in the event that overnight rates have been driven to zero, a central bank loses its ability to drive down interest rates at *any* maturity. The central bank can perhaps manipulate expectations to increase confidence that overnight rates will remain zero in the future. But if expectations do not change as desired, the central bank cannot use an increase in reserve supply to enforce a decrease in interest rates.

In the 1930s, the United States was in a situation that satisfied the conditions for a liquidity trap on this definition. Over 1929-1933 overnight rates fell to zero, and they remained on the floor through the 1930's. According to Krugman (1998), U.S. interest rates were "hard up against the zero constraint" (p. 137) in this period. At the time, however, many economists held a different view. Keynes (1936, p. 207) and his followers did not think a liquidity trap was binding in the mid-1930s. They always described the liquidity trap in terms of *long-term* interest rates. Once short-term rates had been driven to zero, they argued, short-term bonds were equivalent to money, but there was still a demand schedule for money broadly defined reflecting its usefulness as an asset free of interest-rate risk (Keynes, 1936, p. 201; Hicks, 1982, p. 263), so a central bank could depress long-term rates by increasing reserve supply through open-market operations in longer-term bonds.

In the paper, I explain the current conventional view that interest rates at all maturities are bound by the liquidity trap as soon as short-term rates have been driven to the zero floor. Using a model, I present an alternative view of liquidity traps based on monetary theory of the 1930s-1950s. The model implies that, when overnight rates are zero, longer-term rates will tend to fall in response to an increase in the supply of nonborrowed reserves *whether or not* there is a downward revision to expectations of future overnight rates. I examine the relation between reserve quantities and bond yields in the U.S. 1930s. I find that changes in yields were negatively related to changes in nonborrowed reserve supply, even if the reserve supply shocks were unrelated to news about future monetary policy and overnight rates.

I. Reserve demand, interest-rate determination and the liquidity trap

The current conventional view

Federal Reserve policymakers and much literature in economics (an example is Poole, 1968) share a common model of reserve demand and interest rate determination in the United States that rests on certain institutional facts. Banks that hold reserve accounts at the Fed use debits (credits) to the accounts to make payments to (receive payments from) other banks and federal agencies. A bank is required to maintain a zero or positive balance in its reserve account over every night, and a predetermined "reserve requirement" or "required clearing balance" on average over a "maintenance period" of several days. If the bank's balance of "nonborrowed" reserves - that is, its reserve balance before borrowing from the Fed - falls short of the minimum, the bank must cover the shortfall with a "discount loan" from the Fed. To put it another way, a bank must take out a discount loan if its "free reserve" balance - nonborrowed reserves *less* required reserves - falls below zero. The Fed pays no interest on (most) reserve balances, while a bank can lend or borrow overnight at positive interest rates through a variety of instruments. But it is impossible for a bank to predict the exact balance of debits and credits that the Fed will apply to its reserve account at the end of the day, and a bank does not receive a report of its final balance until it is too late to arrange an overnight. Thus, a bank that aimed for a zero free reserve balance would sometimes find itself short, which would be costly because discount credit is rationed by the Fed and also because banks fear that discount borrowing will be taken as a signal that the bank is in financial distress. A bank must therefore weigh the return to overnight lending against the benefit of having another dollar in reserves to cover unforeseeable reserve debits and avoid a reserve deficiency. This tradeoff creates a negative relation between the overnight rate and demand for free reserves. In periods when the Federal Reserve has a target for the overnight rate, such as the present, its open-market operations are adjusted to counteract foreseeable shocks to free reserve demand and supply, and hold market overnight rates near the target level.

The essential institutional elements of this model held in the 1920s. Overnight debt instruments included federal funds loans, repurchase agreements, interest-paying interbank deposits, and "call money" loans collateralized by a basket of stocks and bonds traded on the New York stock exchange. End-of-day payments to a bank's account were unpredictable and overnight lending markets were effectively closed before a bank received its final reserve balance. A bank facing a reserve deficiency could make up the shortfall with a discount loan but discount borrowing was subject to rationing and reputational costs; in addition discount rates were usually above comparable market rates.

Applied to any period, this model implies that a liquidity trap binds all interest rates as soon as overnight rates have been driven to zero, because reserve demand becomes *indeterminate* when there is no opportunity cost of holding reserve balances overnight. Once free reserve supply is sufficient to drive the overnight rate to zero, further increases in supply have no direct effect on asset prices (Orphanides and Wieland, 2000).

An alternative view

In the money-demand literature of the 1930s-1950s, it was often proposed that households held money not only for a transactions medium but also because money is free of interest-rate risk. For example Tobin (1958) argues that the "theory of risk-avoiding behavior" can "provide a basis for liquidity preference and for an inverse relationship between the demand for cash and the rate of interest" (p. 85). Given this relation, interest rates at maturities longer than overnight would be negatively related to money supply, *holding fixed* expectations of future interest rates and asset prices.

A similar argument could apply to banks' demand for reserve balances. A bank has special reason to avoid risk of capital loss. Regulators have generally tried to guard banks' creditors against bank insolvency by requiring a bank to maintain a standard margin between the value of its assets and liabilities, that is by imposing capital requirements.

Since the 1950s, the money-demand literature has turned away from interest-rate risk as an element of money demand. McCallum and Goodfriend (1987) point out that "rate-of-return uncertainty on other assets cannot be used to explain why individuals hold money in economies - such as that of the US - in which there exist very short-term assets that yield positive interest" (p. 779). This also holds for banks' reserve demand. Overnight loans are completely free from interest-rate risk.

Suppose, however, that there were no return to very short-term lending. Then any liquid asset promising a positive return would be subject to interest-rate risk (in addition to any other risks such as default risk). The allocation of a bank's assets between such assets and reserves would necessarily involve a tradeoff between yield and risk of capital loss.

In the paper, I present a model in which banks' need to avoid risk of capital loss creates a negative relation between reserve supply and bond yields when overnight rates are zero. This is true even though, when overnight rates are *positive*, an increase in reserve supply does *not* generally tend to reduce the spread between bond yields and overnight rates. An implication of the model is that bond yields are related specifically to the supply of *nonborrowed reserves* (reserve balances including required reserves) when overnight rates are zero, even though it is the supply of *free reserves* (reserves less required reserves) that influences the overnight rate when overnight rates are positive. Thus, the alternative view of liquidity traps is consistent with conventional propositions about interest-rate determination under "normal" conditions.

II) Interest rates and reserve quantities in the 1930s U.S.

An environment of zero overnight rates developed in the U.S. in the early 1930s. Fed funds overnight loan rates, call money rates, and repo rates were all effectively zero by early 1934, as were rates paid for interbank demand deposits (before interest on such deposits was banned by regulation). They remained zero throughout the rest of the decade.

Meanwhile, longer-term rates interest rates varied. Figure 1 plots yields to maturity on high-grade corporate bonds, long-term Treasury bonds, medium-term Treasury 3-5 year notes, and weekly auction discount rates for newly issued Treasury bills (the maturity of which varied) from April 1934 through the end of 1939.

Over the same period, there were large fluctuations in reserve quantities. Most of these fluctuations constituted shocks to reserve supply (see Friedman and Schwartz, 1963; Romer 1992). Some, but not all of these reserve supply shocks were coincident with news about monetary policy.

Figure 2 plots reserve quantities along with two closely related variables: the value of the U.S. gold stock, and total funds held by the Treasury outside commercial banks. Changes in nonborrowed reserves from 1934 through 1936 reflect Treasury purchases of gold, and a much smaller amount of silver. These boosted nonborrowed reserves when the Treasury created a certificate backed by the purchased metal, deposited the certificate in its Federal Reserve account, and spent the funds or transferred them to Treasury accounts in commercial banks. Most gold and silver purchases were a

consequence of foreign exchange operations. After March 1934 most Treasury purchases of foreign exchange were passive, depending on the volume of foreign exchange offered at the fixed rate. The inflow of foreign exchange, in turn, mainly reflected capital flight to the U.S. from political and economic disruptions in Europe and Asia. The inflow was unsteady, spurred or slowed by political events. Nonborrowed reserve supply was also affected by the vagaries of the Treasury's payment system. A purchase of precious metal did not increase reserves until the Treasury spent the resulting funds or transferred them to its accounts in commercial banks. In the meantime the value of purchased metal was booked as an increment to the Treasury's Federal Reserve balance or "vault cash." Ordinary tax payments, spending and financing operations also affected reserve supply as the Treasury transferred funds into and out of its commercial bank accounts. Over 1934-1936, the Federal Reserve made no attempt to sterilize the effects of Treasury operations. In fact the Federal Reserve undertook no deliberate open-market operations at all.

In July 1936, the Federal Reserve Board announced a change in monetary policy: reserve requirements would be hiked as of the following August. In January 1937, the Fed announced another hike in reserve requirements to become effective in March and May 1937. Nonborrowed reserves, meanwhile, were affected by a change in Treasury policy. In December the Treasury announced it would sterilize the effects of gold inflows on reserve supply, booking the value of all "inactive" gold as Treasury vault cash. In adopting these policies, neither the Treasury nor the Federal Reserve intended an immediate increase in interest rates. The goal of the policies was to make it easier for the Fed to tighten in the *future*. Treasury authorities, however, feared the hikes in reserve requirements would cause a rise in bond yields, which they wanted to avoid. In fact bond yields remained stable around the time of the first hike in reserve requirements in August 1936, but they began to rise sharply at the end of December 1936.

In early 1937 both the Fed and the Treasury began to take steps intended to lower bond yields, including open-market purchases of bonds. In September 1937, the Treasury began to release its stock of "inactive" gold into the reserve supply. In February 1938 the Treasury began to allow current gold inflow to pass through to reserves. In April 1938 Roosevelt announced that all of the Treasury's remaining gold balances would be passed through to reserves, no further inflows would be sterilized, and reserve requirements would be reduced. From that time through the outbreak of the Second World War there were no obvious changes in monetary policy.

Figures 1 and 2 together show a clear relation between reserve quantities and bond yields. This is most obvious with respect to the monetary policy changes of 1936-1937. But there was also a relation between yields and nonborrowed reserves apart from these changes in monetary policy, as is evident in Figure 3, which scatters Treasury bond yields against the log of nonborrowed reserves. Observations from all of the weeks from the Federal Reserve's first announcement of an upcoming hikes in reserve requirements (July 15, 1936) through the final reversal of policy in 1938 (April 22, 1938) are specially marked. These weeks' observations actually appear as disturbances to a negative relation between yields and nonborrowed reserves that prevailed *before and after* the policy shifts.

How might one explain the apparent negative relation between reserve quantity and bond yields? According to the current conventional view of liquidity traps, it must reflect coincidence between changes in reserve quantities and news that changed expectations of future overnight rates.

The alternative view of liquidity traps illustrated by the model presented in the paper offers a different explanation of interest-rate movements over the 1930s. In this view, yields would have been affected by exogenous shocks to reserve supply whether or not the shocks to reserve supply were accompanied by changes in expectations of future overnight rates. Reserve supply shocks could have influenced bond yields through two channels: the direct effect of reserve supply on the expected overnight yield to holding bonds, and a possible effect on expected future bond prices *given* expectations of future overnight rates.

The alternative view has some distinct or at least specific implications that can be tested. First, changes in bond yields should have been negatively correlated with changes in reserve quantities that did *not* coincide with announcements about monetary policy or otherwise constitute news about future overnight rates. Second, bond yields should have been specifically related to *nonborrowed* reserves, and unrelated to changes in required reserve balances except to the degree that the latter were coincident with news about monetary policy. Finally, the relation between bond yields and nonborrowed reserves should have been *weaker* for bonds of *longer* maturities. In the model, reserve supply directly affects the bond price because nonborrowed reserve demand is related to the expected overnight bond yield. If there are bonds of longer and shorter maturities, nonborrowed reserve supply shocks would have relatively small effects on yields to maturity for longer-term bonds, because a longer-term bond's yield to maturity does not change as much for a given change in its expected overnight yield.

Relations between bond yields and reserve quantities

1934-1939

I regress week-to-week changes in Treasury note and bond yields to maturity on weekly changes in (log) reserve quantities, excluding weeks when either the Fed or the Treasury (or both) announced changes or future changes in monetary policy. I find that changes in bond yields appear negatively related to changes in reserve quantity, specifically to changes in the quantity of *nonborrowed* reserves. Changes in *required* reserves were unrelated to bond yields. The relation between nonborrowed reserves and yields was weaker for bonds of longer maturity.

1934-1936

In the period from April 1934 through the beginning of July 1936 there were no announcements by the Treasury or Federal Reserve with obvious implications for monetary policy. No agency sterilized the effects of international flows and Treasury payments on nonborrowed reserves, or adjusted reserve supply in response to interest rates. Changes in the gold stock, net of the change in the Treasury's balances held outside the banking system, constituted shocks to reserve supply that were not counteracted by other reserve supply factors.

Confining the sample to this 1934-36 period, I again regress week-to-week changes in bond yields on nonborrowed reserves and required reserves. I also regress changes in yields on changes in the gold stock net of the change in the Treasury's balances held outside the banking system. Again I find that changes in bond yields appear negatively related to changes in *nonborrowed* reserves, this relation was weaker for bonds of longer maturity, and changes in required reserves do not matter. Also, I find that changes in bond yields are negatively related to gold inflows net of changes in Treasury balances.

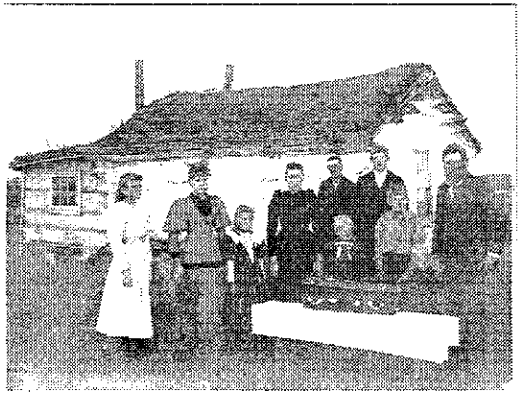
These results for the 1934-1936 period are hard to account for in terms of the conventional view. Could one plausibly argue that, within this period, changes in

nonborrowed reserves caused by interactions of gold inflows and Treasury payments were correlated with changes in expectations of future overnight rates?

4) Conclusion

The experience of the U.S. over the 1930s contradicts the view that longer-term interest rates are bound by a liquidity trap as soon as short-term rates have been driven to zero. Data are more consistent with an alternative view of liquidity traps based on monetary theory of the 1930s-1950s. The alternative view has implications for monetary policy in other times and places when overnight rates are at the zero bound. Banks nearly everywhere may have reasons to avoid risk of capital loss, which could create a defined demand for reserves when overnight rates are zero. The central bank would therefore have the ability to enforce a decrease in longer-term interest rates by boosting reserve supply through ordinary open-market operations in bonds or foreign exchange, whether or not the central bank can manipulate expectations of future overnight rate.

Session 2: Life, Death, and Work: An Economic History of Race and Labor Markets in Twentieth Century America (Saturday, Jan. 5, 10:15am, Hilton-Cabinet Room)



Death in pioneer days

"Driven from the Field or Enticed to the City? The Cotton Picking Machine and the Great Migration from the Cotton Belt, 1949-1964," Wayne Grove (Syracuse University) and Craig Heinicke (Baldwin-Wallace College)

At the end of World War II millions of workers from California to the Carolinas stooped over plants to gather almost every boll of American cotton by hand, as they had for the previous 200 years. When the mechanical cotton picker was finally perfected and commercially produced beginning in 1948, growers adopted it so rapidly that in 1970 the entire crop was gathered by machine. The connection between cotton harvest mechanization and the migration of African-Americans from the South–almost three million people between 1950 and 1970–has long been a matter of public interest; the regional relocation of blacks from the rural South to cities has had profound social and economic consequences. One effect of migration, whatever the cause, was the narrowing of the black-white wage differential (Smith and Welch, 1989). If the contribution of migration to black economic progress had been exhausted by 1965, and thus if sustained strides forward in narrowing the black-white income gap would have abated without Federal Civil Rights Legislation and enforcement (Donahue and Heckman, 1991), how were cotton mechanization and the decline of cotton involved? Both the direct wage and indirect, but powerful, political effects of migration on black economic progress have been examined, but the role that mechanization and the decline of cotton played has not been resolved.

Alston and Ferrie (1993, 2000) have addressed the question of how southern political forces powerful as they were in Congress throughout this period, first prevented but later allowed, the passage of legislation that would threaten the South's method of control over a pool of cheap labor prior to the mechanization of the cotton harvest. The retreat of the southern political effort at the federal level that once sustained the system of social control came about because the livelihood of rural landed elites no longer depended upon an isolated, discriminatory labor market that valued, above all else, a inexpensive, malleable workforce (Alston and Ferrie, 1993).

Since the cotton harvest comprised at least half of the year's labor, most significantly the mechanical picker eliminated this labor "bottleneck." The existing evidence that the cotton harvester chiefly caused the disappearance of the Old South, however, is seriously flawed. It is true that the work of Day (1967) on mechanization and Cogan (1982) on black teenage unemployment implies that harvesting machines displaced workers from the cotton fields who subsequently left the region (Wright, 1986; Wilson, 1987). Margo and Finegan (1993), however, show that Cogan overestimated the role of mechanization with respect to the decline in black teenage labor force participation. With respect to migration, Heinicke (1994), demonstrated that cotton harvest mechanization accounted for less than a quarter of black migration in the 1950s. Finally, Peterson and Kislev (1986) estimated that the lion's share of the decline in cotton hand harvesting (almost 80 percent) resulted from rising wages outside of agriculture and only a small portion was due to mechanization.

This paper revises our empirical understanding of the decline in cotton harvest employment with the use of much improved data. We find that mechanization, along with declining cotton prices and government programs which induced planters to reduce cotton acreage, formed the main impetus to rid the cotton fields of hand labor for good. The draw of better wages outside agriculture that decreased labor supply in the hand harvest market, played a smaller, although crucial, role. Our evidence constructs the crucial link concerning the effect of cotton harvest labor markets and incentives for preserving the system of social control. This, in turn, bears directly as well as indirectly on the postwar record of black progress.

The *long run* effects of mechanization, paradoxically, must have had a favorable effect on black progress, if they reduced the incentive of southern political interests to protect paternalism and block both Civil Rights legislation and enforcement (Alston and Ferrie, 1993). Once the effects of Federal legislation took hold, an "episode" of black progress ensued after 1965 (see Donahue and Heckman, 1991; Heckman, 1990; Card and Krueger, 1993). Also, if mechanization and the movement of cotton prices led to a "soft" southern agricultural labor market in the 1950s and early 1960s, that is consistent with the lack of black progress within regions during this period. Whatever the associated evidence

on political activities, our results show that after the early 1960s, the rural southern elites *no longer had the incentive* to preserve the cheap pool of labor for hand harvesting of cotton. This does not mean that southerners suddenly and uniformly welcomed social change (see Wright, 1999). In contrast to social views, the perfection of the cotton picking device abruptly changed economic incentives, reducing the need to pour substantial resources and political "capital" into preserving the system of social control that those with access to political power at the Federal level -- and with economic power within the South -- had maintained for a century following the Civil War.

COTTON ECONOMY PRICES, LABOR FORCE AND TECHNOLOGY, 1930-70

During World War II harvest wages increased compared with either general farm or industrial earnings. The sharp rise in war-time piece rate wages, cash payment per 100 pounds of seed cotton gathered, resulted in part from a rise in the demand for cotton and in part was due to the WWII-induced mass exodus from the rural South (Wright, 1986). Following the war, cotton prices and piece rates trended down.

Because of the arduous nature of cotton harvesting by hand, people have spent great effort, time and money to reduce the labor-intensity of the task. Almost an entire century of innovative activity by tinkerers and farm equipment manufacturers passed between the first recorded design of a mechanical cotton picker and its successful commercial production by International Harvester in April 1948.

Cotton Harvest Costs by Machine and by Hand

To date the displacement-replacement debate has taken place in something of an empirical vacuum. Frank Meier estimated machine harvest costs for the spindle-picker states from 1949 to 1964 in his unpublished dissertation entitled *An Economic Analysis of the Mechanical Cotton Picker*. Unfortunately, *hand* harvest costs have only been measured by using the USDA "piece rate wages," growers' payment to fieldhands for each 100 pounds of seed cotton harvested (e.g., Peterson and Kislev, 1986). These cash wages, however, poorly capture growers' total hand harvest costs. An important contribution of this paper is to advance an improved measure of this variable. First, growers care about unit costs per pound of *cotton lint*, whereas 100 pounds of unprocessed "seed-cotton" contained varying amounts of organic matter (removed at the cotton gin) that differed over time and by location.

Grove (2000) estimated a time series of hand harvest costs to match the Meier machine cost data by (1) converting piece rate wages to cash wages per pound of lint and (2) estimating non-wage costs for resident laborers, day-haul workers, domestic migrants, and foreign contract workers. Combining state wage costs and non-wage labor expenses (weighted according to the types of labor employed) yields annual state hand harvesting costs. Table 1 provides the ratio of the expected real machine harvest to hand harvest cost per pound of lint. The west-to-east pattern of picker diffusion mirrors the relative costs of the harvest method by region.

THE COTTON HAND HARVEST LABOR MARKET: MODEL AND DATA

The objective of this article is to determine the extent to which higher wages and better opportunities in the northern cities lured laborers away from the cotton belt or whether cheaper technology displaced them.

The following supply-demand model measures the shifts in supply and demand. In the cotton harvest labor market, demand for hand labor (Q) is a function of the cotton harvest wage (W), machine prices (MCOST), the (lagged) price of cotton (PCT), grower overhead expenses (OVERH), and the two government acreage restriction programs (ALLOT and SOIL). The ideal measure of harvest labor (the dependent variable) would be labor per unit time period, but it is not known how many hours and days were required to harvest a given amount of cotton. Therefore, we used the total quantity of cotton harvested by hand, i.e. the percent of cotton harvested by machine times cotton output. Lagged output has been added to account for the partial adjustment of hand harvested output (Nerlove, 1958). The "wage" here is the total compensation of labor employed in the cotton harvest (W), that is the cash wage plus the value of in-kind benefits. The price of cotton lagged one year (PCT) is inserted to reflect the fact that decisions are made on the basis of the expected price of output; since landowners sold and purchased in national markets, prices are deflated by the U.S. CPI.

EMPIRICAL RESULTS

The results of estimating the cotton picking labor supply and demand functions by two-stage least squares, regarding Q and W as endogenous, are presented in Table 2. The data used are pooled state level, annual observations for the 12 major spindle-picker states (N=180). All continuous variables are measured in logs. The coefficients are short-run elasticities and indicate elastic labor demand and labor supply. The results are reasonably robust with respect to differing specifications, but there are some exceptions.

Hand harvested employment fell by an annual average of 9.9 percent from 1949 to 1964. We seek to identify the contribution of the exogenous variables on the equilibrium quantity of labor (Table 3) by using the elasticities reported in Table 2. We first estimate the annual average rate of change in the relevant variables from 1949 to 1964. The annual horizontal shifts in either the demand or supply function is obtained by multiplying the change in the variable by its estimated elasticity (coefficient). The effect of each variable on the change in (endogenous) wages is determined by multiplying the shift in the function and the elasticities with respect to wages in the supply and demand functions. Finally, the change in the quantity of labor is calculated by multiplying the resulting endogenous change in wages and the elasticity with respect to wages in the other (supply or demand) function (see notes to Table 3).

We summarize the effect of the exogenous variables upon the equilibrium quantity of labor in Table 3. For example, machine harvest costs per pound of lint declined on average by 4.4 percent per year from 1949-64, shifting demand for hand harvest labor to the left at an annual rate of 5.7 percent (see Table 3). Assuming no change in the supply equation, the equilibrium wage was reduced by 1.47 percent annually and the equilibrium quantity of labor fell by 1.77 percent each year. Thus, these estimates imply that 18 percent of the 9.9 percent average annual decline in hand harvested cotton employment from 1949 to 1964 resulted from worker displacement due to cheaper machine substitutes.

In addition, cotton prices, non-farm wages, yields, and government programs importantly influenced cotton employment. The cotton price fell by an average of 1.9 percent a year, prompting farmers to plant less acreage to cotton which decreased hand harvesting labor by 2 percent per year (Table 3). Explaining 21% of the average annual decrease, our estimates show that falling cotton prices affected the harvest labor market more than mechanization of the harvest. Combined, these two demand-side factors, mechanization and cotton prices, account for 39 percent of the actual annual decline in harvest labor. Non-farm wages rose by 1.7 percent a year, causing labor to fall by 2.39 percent a year The pull effect of higher non-farm wages (24 percent of the total decrease in

harvest employment) was somewhat greater than the push effect of cotton harvest mechanization (18 percent) by itself.

Cotton yields rose rapidly during the 1950 and early 1960s, by 3.87 percent annually. Higher output per acre increased labor supply (in anticipation of more harvest-time employment) by 2.74 percent per year. Although the allotment variable is not significant, acreage planted in cotton fell so sharply due to the federal soil bank program that it accounts for 4.75 percent reduction in hand harvest employment in the relevant years. In those years the soil bank alone explains close to one-half of the annual decrease in harvest employment, and its effect is larger than any single factor (Table 3).

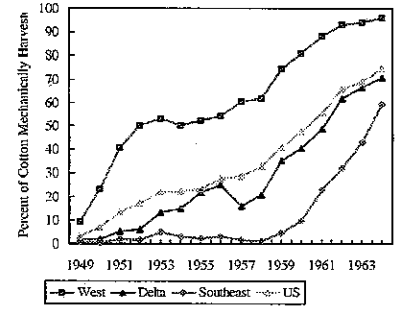
CONCLUSIONS

This article uses newly reconstructed data on hand and machine harvest costs and nonagricultural wage rates to reevaluate the causes of the exodus of hand harvest labor from the cotton fields during the 1949-64 period. Our estimates show that a decrease in labor demand contributed a greater share to declining employment in the cotton harvest labor market than decreasing labor supply during this period; this reverses to the conclusions advanced by Peterson and Kislev (1986), who found that the "pull" of higher wages outside of agriculture was greater than "push" of labor from the cotton harvest labor market. Our estimates suggest that the agricultural-nonagricultural wage differentials explains about a quarter of the decline in the hand harvest workforce, clearly an important factor. We also find, however, that the combination of mechanization and decreasing cotton prices led to a decrease in cotton harvest employment of –3.8% annually, or 39% of the total decrease in harvest employment. Add to this the effect of federal soil bank in several years (which explained 48% of the decrease in employment in relevant years), and decreasing labor demand clearly outweighs the effect of decreasing labor supply. All told, the concurrence of these events within two decades—the departure of millions of, especially black, families from American cotton fields, the decline of cotton, and the mechanization of the cotton harvest—fundamentally altered the economy, politics, and social and cultural life in those areas as well as nationally.

This extraordinary episode of demographic and technological change produced many ironies. Following the 1930s, the "tight" labor market of the early 1940s provided the incentive for the final push to perfect a mechanical cotton harvesting devise (Wright, 1986). Improvements in farm machinery exogenous to the cotton economy, aided the process. Yet, for contemporaries in 1946, and, in fact, in 1950, it was unclear that a way of life in place since the Civil War would be swept away so swiftly. The old system of social control held on for three more wrenching decades, and more turbulent time beyond that. Would the institutional system that inhibited mechanization be sustained? (see Whatley, 1985; 1987, on institutions.) We know only now that it crumbled amazingly swiftly, between 1949 and 1970. The incentive structure to maintain a low-wage economy based on poor education generally and, for blacks, fear, intimidation and institutionalized discrimination, changed as mechanization, government farm programs, and the withering of the market for raw cotton took their toll.

Perhaps the greatest irony of all is that despite the adverse effect mechanical cotton pickers had on the harvest labor market in the 1949-64 period, the incentive to retain the elaborate informal and legal codes of racial separation largely disappeared in the wake of its adoption. Add to that, the contribution of government programs that reduced cotton acreage and declining cotton prices between 1949 and 1964 and we have a collapse of demand for hand harvesting labor that rendered the Old South largely an anachronism. The old system of social control and its proponents, however, did not fade away easily. The Civil Rights movement interacting with federal politics produced change within as well as outside the South. With little reason to preserve the labor market for hand cotton picking, Federal Civil Rights legislation and the emergence of Federal programs that eroded the old system of social control had a chance. The way was then cleared for the possibility, however faltering, of some progress for African-Americans throughout the nation.

Figure 3:
Percent of Cotton Harvested by Spindle Pickers by Region and for US, 1949-64



Note: These are weighted averages according to cotton production.
Sources:
Percent of cotton crop harvested by machine per state: Statistics on Cotton (1974), 218.
Bales of cotton harvested per state: Statistics on Cotton (1974), 64-77.

Table 1
Ratio of Total Expected Real Cost of Machine Hand Picked Cotton: Selected Regions

	West	Delta	SE
1950	.64	.98	1.46
1952	.53	.93	1.29
1954	.48	.82	1.11
1959	.44	.77	1.04
1963	.32	.63	.85

Table 2:
2SLS Estimates of the Cotton Hand Harvest Labor Market
Dependent Variable: Quantity of Hand Harvested Cotton

Independent Variable:	Demand	Supply
Lagged hand harvested cotton Q_{t-1}	**0.79**	**0.68**
	(15.55)	(14.06)
Labor Compensation (W)	**-2.68**	**1.20**
	(-4.37)	(4.10)
Lagged cotton price (PCT)	**3.43**	
	(5.06)	
Mechanical Harvesting Costs (MCOST)	**1.29**	
	(5.68)	
Overhead costs of labor (OVERH)	**0.34**	
	(2.83)	
Cotton allotment dummy (ALLOT)	**0.07**	
	(1.04)	
Soil bank dummy (SOIL)	**-0.15**	
	(-3.58)	
Nonagricultural wage rates (WNON)		**-2.02**
		(-2.88)
Cotton Yields (Y)		**1.03**
		(9.09)
Pre-harvest agricultural wage (PREHW)		**-0.60**
		(-1.36)
Planted cotton acreage (ACRES)		**0.14**
		(1.31)
Intercept	-8.27	-6.71
Adjusted R^2	0.93	0.92
n x T	180	180

t-statistics in parentheses
State dummies not reported here.

Table 3
Average Annual Percent Change in Quantity of Labor Employed
In Hand Harvesting, Response to Shifts in Demand and Supply

	Annual Average Percentage Change				
	Change in Variable[1]	Shift in function[2]	Change in wages[3]	Change in qty of labor[4]	Percent of Total Change in Labor Predicted of Actual[5]
Demand Real Machine Harvesting Costs	-4.42	-5.70	-1.47	-1.77	18
Real Cotton Price	-1.91	-6.55	-1.67	-2.03	21
Soil Bank Dummy Varaiable for Years 1956-58, 1964 only	---	-15.3	-3.94	-4.75	48
Supply Real Nonagricultural Wages	1.71	-3.46	0.89	-2.39	24
Cotton Yields	3.87	3.98	-1.02+2.74	-28[6]	

Notes:
[1] Annual rate of change estimated from regression equation: $\log(x) = a + rt + sd + e$.
[2] Shift in function is change in variable times estimated elasticity.
[3] Reduced form change in wages (due to variable) is shift in function times $\left[\frac{1}{\alpha_1 - \beta_1}\right]$.
[4] Change in employment is change in wages times wage elasticity (of supply or demand).
[5] Cotton harvest labor employment decline on average by 9.9 percent annually.
[6] Negative sign denotes this counteracts the decline in labor, i.e., is an increase.

FEPC Increase Black Employment in High-Wage Industries?" Ryan Johnson (University of Arizona)

I. Introduction

During World War II the first federal government agency designed to fight racism in employment, the Fair Employment Practice Committee (FEPC), was created. Despite the existence of the FEPC, economists studying the extraordinary relative gains of black workers during the 1940s have not attributed a role to government policies specifically designed to fight discrimination. Historians, with few exceptions, have generally seen Roosevelt's FEPC as a weak agency unable to accomplish the assigned mission. Collins' (2001) study questioned the conventional wisdom by presenting empirical evidence that this agency did indeed help black workers find employment in high-wage World War II defense industries. However, the degree to which he could empirically isolate the impact of the FEPC on black employment was limited by the aggregate nature of the available data. The research presented in this paper, using a new industry level panel of race specific employment data, union data, and micro-level FEPC data, takes a closer look at the role of the FEPC in mitigating industrial crowding. It also explores the effect of the FEPC in light of the impact that changes in the structure of unionization and tight labor markets had on industrial segregation.

Due to the extraordinary gains of black workers relative to whites during the 1940s, it is important to assess, as far as possible, the degree to which various factors contributed to the gains of black labor during this period. Collins' (2001) suggests that the FEPC was able to advance black economic interests by mitigating the forces of industrial crowding. Research by Johnson (2001) indicates that the period of World War II was a period in which industrial segregation decreased within manufacturing and mining industries in Pennsylvania. Johnson showed that black integration was a function of various factors that influenced the costs of integrating black workers into a firm. These factors, such as the extent of unionization, union affiliation, the availability of labor, and the spatial and hierarchical organization of the firm differed by industry. Industries faced with tight labor markets were more likely to integrate blacks into their workforces. The leading national labor organizations had conflicting effects. The more craft-based industries organized by the American Federation of Labor (AFL) tended to have a lower concentration of black workers, while the industries organized by the Congress of Industrial Organization (CIO) in the late 1930s and early 1940s tended to open its door to more black employment. Thus black industrial integration was largely a function of the crises and institutional change that dominated much of the labor scene of the Second World War. To what degree was it also a function of the Fair Employment Practice Committee's (FEPC) actions to promote racial equality in the defense industries?

II. Crises, Change, and the Creation of the FEPC

The creation of the FEPC was very much a function of the economic crises associated with the Second World War. A. Philip Randolph, president of the Brotherhood of Sleeping Car Porters, threatened to lead one hundred thousand protesters down Pennsylvania Avenue in protest of the discriminatory practices of the U.S. armed services and defense contractors. Roosevelt, fearing among other things, the disruption of war production and racial strife that would most likely result from the protest, entered into negotiations with Randolph and Walter White of the National Association for the Advancement of Colored People. Some authors have described the results of the negotiations as a win for Roosevelt and a loss for black Americans during World War II. Roosevelt obtained greater cooperation from black protesters and black labor received the FEPC-arguably a weak agency that did relatively nothing for black Americans during World War II (Dalifume 1969 and Bernstein 1968). Factors such as a limited budget, inadequate personnel, very limited enforcement powers, and resistance from congress and other bureaucracies have been given credit for the possible short-term failures of the FEPC in helping black labor during World War II (Bernstein 1968, Polenberg 1972, Wynn 1976). The political weakness and lack of support from other bureaucracies led to the disintegration of the President's first committee after it was put under the control of the War Manpower Commission (WMC). On May 27, 1943 the committee was reformulated under Executive Order 9346.

The FEPC only investigated firms, unions, or government agencies for which there had been a complaint of discrimination docketed with the FEPC. The FEPC could investigate complaints of discrimination against any agency of the federal government, firms and unions associated with firms accepting contracts from the federal government which had non-discrimination clauses in them, and industries that were essential to the war effort. Since the FEPC lacked statutory authority it could not compel compliance. The FEPC could, however, appeal to other agencies with statutory authority for assistance. The FEPC was also able to recommend to other government agencies that the war contract of a discriminatory employer be revoked, generally an unrealistic tool of punishment due to the imperatives of war production. Collins (2001) mentions the Philadelphia transit case where white workers went on strike to protest the employment of black trolley operators. The result of the strike was the transit system being taken over by the Army, and the workers, fearing the permanent loss of their jobs and a change in their draft status, subsequently returning to their jobs. Like most examples of the FEPC receiving help from stronger government agencies, the fact that war production was being interfered with seemed to be more of an issue than discrimination. Firms that did not provoke unionized whites into disrupting war production by employing blacks in the first place did not come under similar forced compliance with FEPC directives.

III. Other Sources of Black Industrial Advancement

Generally the labor shortages associated with the war effort have been given more credit for black advancements in occupational and industrial employment than the FEPC (Bernstein 1968 and Brody 1975). Institutional change with respect to unionization may also have created opportunities for black workers to be integrated into northern industry. Throughout World War II and the second part of the Great Depression private organizations like the National Urban League, the Pittsburgh Urban League, the black press, and black churches were striving to increase black representation in high wage industries and occupations. A major institutional change that may have aided these groups in this effort was the creation of the Congress of Industrial Organizations (CIO) in the late 1930s. Unions affiliated with the CIO differed from those affiliated with the American Federation of Labor (AFL) in that the CIO affiliated unions organized on an industrial basis rather than a "craft" basis. The CIO also differed significantly from the AFL in that it incorporated constitutional provision claiming complete opposition to discrimination on the basis of "race, creed, color, or nationality" (Murray 1942). Despite the anti-discrimination declarations of the CIO, there was a great heterogeneity in the actual policies implemented by local affiliates. CIO affiliated unions that attempted to organize black labor in

Pennsylvania received support in terms of leadership and finances from black organizations, black churches, and the black press. Some of these industrial unions were able to establish collective bargaining agreements guaranteeing employees standardized promotion based on seniority, seniority based lay-off protection, standardized wages, paid vacations, and other provisions associated with internal labor markets. Significantly, these provisions, at least theoretically, applied equally to all workers, regardless of race (Dickerson 1986).

IV. The Data

Despite the general trend of scholarship throughout the 1960s and 1970s arguing that the FEPC did not do much to help the World War II black laborer, some of the more recent research on the FEPC has argued that Roosevelt's little agency did significantly help black workers during the 1940s (Collins 2001 and Kersten 2000). Collins research is very significant due to the fact that it serves as the first attempt at a rigorous empirical investigation of the impact of the FEPC on black employment. The research presented in this paper, like Collins' (2001) research, focuses on the impact of the FEPC in helping black workers obtain employment in industries for which the FEPC dealt. By combining industry employment data with firm-specific FEPC data, it is possible to better assess the impact the FEPC had on black industrial integration.

Collins' (2001) study on the impact of the FEPC on black employment in World War II defense industries made use of city level variation in FEPC cases. His city level data consisted of all cases docketed with the FEPC between July 1943 and June 1944. The data employed in this paper consists of the following information for each closed case in Pennsylvania: firm, union, or government agency charged with discrimination; industry of party charged; reason for discrimination (e.g., Negro, etc.); type of discrimination (e.g., hire, fire, promotion, working conditions, wages, etc.); sex of complainant; date of complaint; disposition (e.g., dismissed-insufficient evidence, dismissed-on merits, withdrawn by complainant, satisfactory adjustment, etc); and the date the case was closed. This data was collected from case specific microfilmed records held in the National Archives.

Collins' (2001) city level data prevented him from distinguishing between complaints which did not have merit and those that actually resulted in a satisfactory adjustment, whether the case was filed against the private sector or a government agency, and whether or not the discrimination was a refusal to hire. He also could not distinguish if the charge of discrimination was race related. The data was not sex specific and he only had one year of FEPC case data. The FEPC was given a broad mandate to enforce an order that applied to both government and industry and that applied to many different forms of discrimination. Collins' (2001) data does not allow a researcher to assess how the FEPC cases varied across industry and the extent to which the agency was able to obtain compliance. This is important due to the fact that the FEPC, an agency with very limited resources, would face tradeoffs when deciding how to allocate its resources.

Descriptive statistics from a report by the FEPC (1945) indicates why it is important to account for these factors when assessing the performance of the FEPC in increasing black representation in defense industries. In the nation, between July 1943 and June 1944, only 36 percent of the closed cases received satisfactory adjustment, 27 percent of the closed cases applied to government agencies, and only 47 percent of the cases were due to a failure to hire. These figures indicate that there was a large difference between the number of cases docketed and the subset of cases where the FEPC was actually able to compel a firm to alter its discriminatory hiring practices.

This paper also makes use of a panel of annual industry employment data for about 315 industry classifications. The union data employed in the paper are national annual union densities for 12 aggregate industry classifications. The union data are broken down by affiliation (Congress of Industrial Organization, American Federation of Labor, and unaffiliated union). These union data were derived from the data reported by Leo Wolman (1936) and Leo Troy (1965).

V. Empirical Evidence

Like Collins' (2001) paper, this paper focuses on the extent to which the FEPC increased black representation in industries for which FEPC cases were docketed. Many of the historical anecdotes on black employment indicate that much of the resistance to black integration came from white workers. Johnson (2001) found that the integration of black workers into northern industry was a function of the institutional features and events that affected the relative strength of incumbent workers' resistance to black integration. When workers were unionized by craft unions the workers' resistance to black integration was solidified, making black integration more costly. Many CIO affiliated unions had an economic incentive to open their doors to black labor and thus industries organized by the CIO were more likely to integrate black labor. Other factors such as the spatial and hierarchical organization of the firm could also impact the costs of integration. Some industries, by providing departments or areas where black workers could be employed without coming into contact with white workers, lowered the costs of integrating blacks, all else held constant. World War II and the imperatives of war production were the exogenous shock necessary to spur integration. As war time labor markets tightened, industries with either a low proportion of their workforce organized or with a significant proportion of this organization affiliated with the CIO integrated their workforces. (Johnson 2001).

Collins (2001) points out that the FEPC could also have altered the costs and benefits of integrating black workers into an industry, increasing black representation in industries for which FEPC cases were docketed. Resistance to black integration from white workers may have decreased if they had perceived that the FEPC had given their employer no choice in the matter. Also, the FEPC could have increased the costs imposed on firms that did not integrate their workforces, making integration more economically attractive. The FEPC also offered advice on how to integrate black workers into a firm with as little disturbance as possible.

So black integration across industries was a function of how union density, union affiliation, the spatial and hierarchical organization of the firms in the industry, and the relative severity of the wartime demand shock differed across industries (Johnson 2001). It is also possible that black integration was a function of the efforts of the FEPC. It is possible to asses the degree to which black integration was a function of these variables by estimating the marginal effects of these variables with respect to the probability that a randomly selected employee in industry i was black. This probability is given by

1) $P_{it} = a_i + a_1 FEPC_{it-1} + a_{2\text{-}5}UNION1\text{-}4_{it} + a_6CIO + a_{7\text{-}9}UNION1\text{-}4*CIO + a_{10\text{-}44}Year(1917\text{-}1950)_t + u_{it}$.

The fixed effects (a_i) control for industry resistance to black integration, largely associated with the spatial and hierarchical organization of the industry which do not change over time

but which vary across industry. It is possible that the number of cases investigated in an industry in a given year would be endogenous to the proportion of the industry's wage earners that were black in that year. It is also likely that an industry's employment practices would not respond instantly to FEPC pressure but that the change would be observed in the following period. For these reasons the number of complaints investigated by the FEPC in a given industry ($FEPC_{it-1}$) is lagged one year, making the variable predetermined. The $UNION1\text{-}4_{it}$ variables are dummy variables for union densities and CIO_{it} is the proportion of unionized workers affiliated with the CIO in a given industry. The year dummies ($YEAR1916\text{-}1917_{it}$) capture the year effects anticipated from World War II.

Table 1.

	%Black		
FEPC	-8.66E-06	*	*
	(0.001287)	*	*
FEPC Black	*	0.000041	*
	*	(0.00135)	*
FEPC Black Hire	*	*	-0.00255
	*	*	(0.002755)
Union2 20-39%	-0.00541**	-0.00541**	-0.00541**
	(0.001592)	(0.001592)	(0.001592)
Union3 40-59%	-0.02327**	-0.02327**	-0.02326**
	(0.00336)	(0.00336)	(0.003359)
Union4 60-79%	-0.02972**	-0.02972**	-0.02968**
	(0.004326)	(0.004327)	(0.004319)
Union5 80-100%	-0.04035**	-0.04034**	-0.0404**
	(0.007084)	(0.007082)	(0.007083)
%CIO/100	0.005972	0.005972	0.005966
	(0.004491)	(0.004491)	(0.004491)
Union2*%CIO/100	0.007319	0.007318	0.007323
	(0.005047)	(0.005047)	(0.005048)
Union3*%CIO/100	0.042124**	0.042125**	0.042128**
	(0.007739)	(0.007739)	(0.007738)
Union4*%CIO/100	0.026511**	0.026506**	0.026562**
	(0.006687)	(0.006686)	(0.006681)
Year 1917	0.006523*	0.006523*	0.006522*
	(0.003488	(0.003488)	(0.003488)
Year 1918	0.009613**	0.009613**	0.009612**
	(0.003266)	(0.003266)	(0.003266)
Year 1919	0.013446**	0.013446**	0.013443**
	(0.003563)	(0.003563)	(0.003563)
Year 1920	0.015283**	0.015283**	0.015281**
	(0.003627)	(0.003627)	(0.003627)
Year 1921	0.00884**	0.00884**	0.008837**
	(0.00383)	(0.00383)	(0.00383)
Year 1922	0.014612**	0.014612**	0.01461**
	(0.003681)	(0.003681)	(0.003681)
Year 1923	0.01983**	0.01983**	0.019829**
	(0.003865)	(0.003865)	(0.003865)
Year 1924	0.021712**	0.021712**	0.021711**
	(0.004051)	(0.004051)	(0.004051)
Year 1925	0.01702**	0.01702**	0.017019**
	(0.003335)	(0.003335)	(0.003335)
Year 1926	0.02039**	0.02039**	0.02039**
	(0.003946)	(0.003946)	(0.003946)
Year 1927	0.015455**	0.015455**	0.015454**
	(0.003512)	(0.003512)	(0.003512)
Year 1928	0.013441**	0.013441**	0.013438**
	(0.003717)	(0.003717)	(0.003717)
Year 1929	0.013401**	0.013401**	0.013398**
	(0.003418)	(0.003418)	(0.003418)
Year 1930	0.011154**	0.011154**	0.011151**
	(0.003098)	(0.003098)	(0.003098)
Year 1931	0.011063**	0.011063**	0.01106**
	(0.003275)	(0.003275)	(0.003275)
Year 1932	0.007096**	0.007096**	0.007091**
	(0.003064)	(0.003064)	(0.003064)
Year 1933	0.008258**	0.008258**	0.008253**
	(0.00302)	(0.00302)	(0.00302)
Year 1934	0.006575**	0.006575**	0.006568**
	(0.002952)	(0.002952)	(0.002952)
Year 1935	0.006164**	0.006165**	0.006158**
	(0.002971)	(0.002971)	(0.002971)
Year 1936	0.005817*	0.005817*	0.005811*
	(0.003169)	(0.003169)	(0.003168)
Year 1937	0.001912	0.001913	0.001903
	(0.003274)	(0.003274)	(0.003274)
Year 1938	0.001956	0.001957	0.001946
	(0.003308)	(0.003308)	(0.003308)
Year 1939	0.00134	0.001341	0.001331
	(0.003436)	(0.003436)	(0.003435)
Year 1940	0.002552	0.002553	0.002541
	(0.003432)	(0.003432)	(0.003432)
Year 1941	0.006465*	0.006466*	0.006452*
	(0.003524)	(0.003524)	(0.003524)
Year 1942	0.020594**	0.020594**	0.020582**

	(0.003982)	(0.003982)	(0.003982)
Year 1943	0.035533**	0.035534**	0.035521**
	(0.004315)	(0.004315)	(0.004315)
Year 1944	0.041911**	0.041909**	0.041933**
	(0.004625)	(0.004624)	(0.004623)
Year 1945	0.044971**	0.044958**	0.045075**
	(0.00479)	(0.004783)	(0.004747)
Year 1946	0.043383**	0.043377**	0.043467**
	(0.004802)	(0.004799)	(0.004791)
Year 1947	0.042467**	0.04247**	0.042437**
	(0.004474)	(0.004474)	(0.004471)
Year 1948	0.043552**	0.043555**	0.043519**
	(0.004705)	(0.004706)	(0.004702)
Year 1949	0.044286**	0.044289**	0.044253**
	(0.004972)	(0.004972)	(0.004968)
Year 1950	0.042154**	0.042156**	0.042124**
	(0.004754)	(0.004754)	(0.004752)
Constant	0.032427**	0.032427**	0.032429**
	(0.002528)	(0.002528)	(0.002528)
Adj R^2	.6718	.6718	.6718
# of Observation	10590	10590	10590

The dependent variable is the proportion of an industry's employment that was black in period t. The data are from the "Report on Productive Industries, Public Utilities and Miscellaneous Statistics of the Commonwealth of Pennsylvania" for the years 1916 to 1950. The union data were obtained from Wolman (1936) and Troy (1965). To obtain union densities the union numbers were combined with U.S. Census industry data on the number of wage earners. The union data are national. Standard errors are in parenthesis. These standard errors were produced using a White correction procedure to correct for heteroskedasticity. * indicates that the coefficient is significant at a 90% level of confidence, ** indicates significance at a 95% level of confidence. A year dummy for 1916 and union dummies for union density between 0 and 19% were left out of the regressions. All three of the FEPC variables are lagged by one year.

In table 1 the variables explaining the effects of the FEPC are presented in three different forms. The First (FEPC) is the number of satisfactorily adjusted cases investigated in industry i in period t-1. The motivation for the discrimination may have been any ethnicity, nationality, or religion. Also the investigated cases making up this variable were not just for failure to hire, but may have been for wage discrimination, working conditions, etc. So this variable captures the extent to which not only satisfactorily adjusted complaints for not hiring black workers affected the future employment prospects of black workers, but the impact of all satisfactorily adjusted complaints in the industry. The second variable (FEPC Black) is the number of satisfactorily adjusted cases against black workers in an industry, thus capturing the impact of a satisfactorily adjusted case for discrimination against black labor regardless of whether or not it was a failure to hire. The third variable (FEPC Black Hire) is the number of satisfactorily adjusted cases for failing to hire a worker because he or she was black. All three of the FEPC variables are lagged.

For each of the three lagged FEPC variables, the coefficient is practically zero and statistically far from being different from zero, implying that at the industry level, satisfactorily adjusted complaints by the FEPC did not increase the probability that a randomly selected employee in that industry was black. As found to be the case in Johnson (2001), union density had a significant negative impact on black employment while affiliation with the CIO largely mitigated the negative effects of unionization. When comparing the year effects for the 35 years of the panel one can see the extent to which black employment in the manufacturing and mining industries of Pennsylvania was impacted by the economic crises of World War II. During the Great Depression the year effects indicate that black employment fell to its 1916 level, but with the onset of World War II, rose to its maximum levels and pretty much maintained these levels throughout the 1940s.

VI. Conclusion

The empirical evidence presented in this paper lends support to the arguments of many historians, that the FEPC did not have a significant impact on black industrial employment in the 1940s. Black industrial gains of the 1940s were a function of the institutions of labor organization and the economic crises of the period. As World War II labor markets tightened, it was primarily the industries with low union density or with a high proportion of its unionized workers affiliated with the CIO that employed more black workers.

Works Cited

Bernstein, Barton J. "America in War and Peace: The Test of Liberalism." in Barton J. Bernstein (ed.), *Towards a New Past: Dissenting Essays in American History.* New York: Patheon Books, 1968.

Brody, David. "The New Deal and World War II." in Johns Braeman, Robert Bremner, and David Brody (eds), *The New Deal: The National Level.* Vol I. Columbus: Ohio State University Press, 1975.

Collins, William J. "Race, Roosevelt, and Wartime Production: Fair Employment in World War II Labor Markets." *American Economic Review* 91, n1 (2001): 272-86.

Dalfiume, Richard. *Desegregation of the U.S. Armed Forces: Fighting on Two Fronts, 1939-1953.* Columbia: University of Missouri Press, 1969.

Dickerson, Dennis C. *Out of the Crucible: Black Steelworkers in Western Pennsylvania, 1875-1980.* New York: SUNY Press, 1986.

Fair Employment Practice Committee. *First Report.* Washington D.C.: Government Printing Office.

---------"Index To Regional Cases and to Cases Referred to the Director of Field Operations, Dec. 1941- Apr. 1946." *Records of the Committee on Fair Employment Practices, Records of the Division of Field Operations.* (Group Record 228.4.3) Roll IND 1.

Johnson, Ryan S. "Racial Segregation Across Industries During Economic Crises and Change: The Case of Pennsylvania, 1916-1950" *Working Paper* available at: http://w3.arizona.edu/~econ/working.html

Kersten, Andrew E. *Race, Jobs, and the War: The FEPC in the Midwest, 1941-46.* Urbana and Chicago: University of Illinois Press, 2000.

Murray, Florence. *The Negro Handbook.* New York: Wendell Malliet and Company, 1942.
Polenberg, Richard. *War and Society: The United States, 1941-1945.* Philadelphia: Lippincott, 1972.
Troy, Leo. *Trade Union Membership, 1897-1962.* New York: National Bureau of Economic Research, 1965.
Wolman, Leo. *Ebb and Flow in Trade Unionism.* New York: National Bureau of Economic Research, 1936.
Wynn, Neil A. The Afro-American and the Second World War. New York: Holmes and Meier, 1976.

"Exploring Racial Gaps in Infant Mortality, 1920-1960," William J Collins (Vanderbilt University) and Melissa A. Thomasson (Miami University)

Great improvements in physical health are among the twentieth century's most impressive social achievements. In the United States, life expectancy at birth increased by more than 25 years; average height increased by about 7 centimeters (for white males); and a number of deadly diseases and debilitating illnesses were all but vanquished.1 However, even as the overall level of health for both whites and nonwhites improved, racial gaps in health outcomes were, by some measures at least, remarkably persistent and therefore remain a concern for current policymakers (Department of Health and Human Services 2000; Levine et al. 2001). At the center of this paper's analysis, the overall infant mortality rate (deaths of children under one year of age per 1,000 live births) fell from 85.8 in 1920 to 7.2 by 1997, but the racial gap in infant mortality rates, when expressed as a nonwhite/white ratio (reflecting the relative risk of death), did not decline over time. In fact, Figure 1 shows that while the racial gap declined in some periods, it rose substantially in others.

Thus, while we find it impossible to ignore the extraordinary gains in health enjoyed by both whites and nonwhites, the persistent gap in the relative risk of death facing the two groups is also striking.2 In this paper, we explore state-level infant mortality data in an empirical and historical framework that accommodates and sheds light on both trends. We focus on infant mortality because it is a simple, sensitive, and frequently referenced barometer of health conditions; because it conveys useful information about families' resources and environment, as well as about the effectiveness of public health policy; and because it is available in a reasonably consistent form for whites and nonwhites over a relatively long period of time.

The literature on infant mortality is voluminous, but to our knowledge, previous works have not systematically examined the determinants of long-run trends in the racial gap over the 1920 to 1970 period (e.g., see Rochester 1923; Chase 1972; Shin 1975; Grossman and Jacobowitz 1981; David and Collins 1997; Department of Health and Human Services 2000; Almond, Chay, and Greenstone 2001). We are especially interested in this period because it was one of remarkable medical and social transformation. Professional training for doctors, nurses, and midwives was substantially upgraded and standardized; federal and state governments poured resources into improving and extending hospital and clinic systems, as well as into making health services available to the poor; and of course, medical science advanced rapidly. At the same time, prevailing patterns of racial discrimination embedded themselves in 1 The life expectancy figures are from Atack and Passell (1994, p. 230). The height estimates are derived from Costa and Steckel (1997, p. 51). The infant mortality figures are from the United States Statistical Abstract (1947, 2000). 2 Though our primary interest is in "black" relative to "white" health, infant mortality data are reported by "white" and "nonwhite", and so our statistical analysis proceeds accordingly. 2 the healthcare system, perhaps compounding the disadvantages that African Americans already faced due to their geographic distribution and relative lack of financial and educational resources. Moreover, a number of historic events may have shaped the racial gap in health outcomes including the Great Migration, the Great Depression, World War II, and the Civil Rights Movement.

Our discussion is guided by a simple model of infant mortality in which the likelihood of survival is influenced by the consumption of quality-adjusted units of nutrition, housing, health-related information, and health services. We refer to this bundle of goods as "healthcare" (broadly speaking), and think of it as an input into a health production function.3 We suppose that utility-maximizing parents allocate resources over healthcare inputs and other goods, subject to resource constraints and relative prices, and that they influence infant survival probabilities accordingly. In our simple model, factors that influence infant health include income, residence, the availability of medical care, and women's education. These variables reflect the financial, environmental, and medical resources available to families having children.

To explore the relationship between these variables and infant mortality links, we construct a panel of state-level data for whites and nonwhites for five-year intervals. At each point in time, our dataset includes (by race and state) the infant mortality rate, estimates of real per capita income, the proportion of the population residing in urban areas, average years of education for women between 20 and 40 years of age, and physicians per capita. Income is important because it provides a measure of a household's ability to purchase healthcare. Nonwhites' income and wealth were considerably less than those of whites throughout the period under study, implying tighter budget constraints and less demand for all normal goods, including infant-related healthcare. Additionally, we hypothesize that for various reasons, African Americans may have faced higher costs for healthcare services due to discriminatory practices within the healthcare system. Such discrimination took many forms, including restricting blacks' access to healthcare facilities and limiting the number and quality of black doctors (Johnson 1949, Seham 1964, Beardsley 1987, Smith 1998).

Residence is important for two reasons. First, we include in our model the proportion of households in a state living in an urban area. Prior to 1920, it is clear that urban residence was relatively hazardous for infants' health (see Haines, 2001). The observed gap between urban and rural mortality rates dissolved over the course of the late nineteenth and early twentieth century as public health initiatives took root, as medical science progressed, as water and food supplies improved, and as urban hospitals increased the volume and quality of available healthcare.4

3 See Grossman (1972), Rosenzweig and Schultz (1983), Berger and Leigh (1988), Kenkel (1991), and Goldman and Lakdawalla (2001) on household production of health.
4 See Troesken (1999 and 2001) on typhoid, water, and sewerage in U.S. cities in the late 19th and early 20th centuries.

In addition to living in an urban area, living in the South may have also influenced infant health. Compared to households living in non-southern regions, infant mortality rates in the South were higher for both blacks and whites. By itself, the geographic distribution (and redistribution) of African Americans might have adversely affected their health relative to that of whites. In 1920, 85 percent of African Americans resided in the South (compared to 25 percent of whites), and in 1960, 60 percent still resided there (compared to 27 percent of whites). Throughout this period, southern per capita income was relatively low: in 1940, for example, southern real per capita income was about 64 percent of the national average (Mitchener and McLean 1999).5

Furthermore, southern blacks were relatively concentrated in rural areas, especially early in the period under study. Therefore, they often lived far from hospitals and doctors and faced considerable costs (in terms of time and effort) when seeking professional medical services. On the other hand, although blacks in urban areas may have benefitted from proximity to medical facilities, patterns of residential segregation constrained the supply of housing to blacks, thereby raising its price and exacerbating crowded, unhealthy living conditions in the emerging ghettos.6 The level of education for women is also an important determinant of infant mortality for two reasons. Education may affect infant mortality rates both indirectly, through its effect on earnings, and directly, through its effects on health-related knowledge and responsiveness to that knowledge.7 Therefore, even though education and income tend to be highly correlated, we include measures of both in the regressions, focusing on the educational attainment of women between 20 and 40 years of age. To the extent that blacks' quality and quantity of education fell short of that provided whites, blacks might have found it more difficult to acquire information about medical advances and/or the availability of professional medical services.8

Similarly, educational deficiencies might have facilitated the influence of superstition and folk remedies. 5 It is certainly possible that the relative health of southerners affected the region's relative productivity, as argued by Brinkley (1997) for the late 19th century. In this draft, however, we view income as exogenous variable. 6 Over the same period, large numbers of southerners, especially blacks, moved to other parts of the country (Collins 1997). These migrants and their children might have benefitted from the non-South's relatively abundant supply of healthcare services, relatively egalitarian education system, and relatively high levels of pay. But some aspects of the relocation might have offset such benefits. Such aspects include residential segregation/ghettoization and an unfavorable change in climate. 7 See Berger and Leigh (1989) or Kenkel (1991) for discussions of the empirical connection between health and schooling in modern data, including potential omitted variable issues. Even after adjusting for selection on the basis of unobservable characteristics (e.g., rate of time preference or ability), Berger and Leigh find that education has a significant direct effect on health. Kenkel finds that even after controlling for health knowledge, education appears to have a strong positive effect on health-related behavior. 8 In this paper, we view educational attainment as an exogenous variable. See Goldman and Lakdawalla (2001) for a recent discussion of the literature linking health and education. In an international context, several empirical studies of infant mortality and children's health emphasize the importance of women's education. See Subbarao and Raney (1995).4

Our basic regression equation is expressed in log-log form, implicitly assuming constant elasticities between the dependent and independent variables: (3) ln IMRit = (ln Xit)□+ □t + eit

where i indexes states, t indexes time periods, and X is a set of race-state-year characteristics. We run the regressions separately for whites and nonwhites, thereby allowing the coefficients to differ between race categories.9 We add the time-period dummy variables (□) to absorb unobserved period-specific factors influencing infant mortality (such as advancing medical technology), and in some specifications we add region or state dummy variables to absorb area-specific effects.10

Table 2 reports regression results from three different specifications. Columns 1 and 2

correspond to the basic specification described by equation 3. Columns 3 and 4 add state dummy

variables to the specification, and columns 5 and 6 include a dummy variable for the southern region (rather than a full set of state dummies). In general, the coefficients reported in Table 2 have the expected signs: *ceteris paribus*, higher levels of income, women's education, and physicians per capita tend to lower infant mortality rates, whereas higher levels of urbanization are correlated with higher infant mortality rates. Though the time-period dummy variables clearly absorb a great deal of the variation, the X variables' coefficients (income, education, urban, and physician supply) in columns 1, 2, 5, and 6 often exceed the 10 percent level of statistical significance. When we forfeit a great deal of the cross-state variation by including state-fixed effects (columns 3 and 4), the standard errors of the coefficient estimates increase, and some of the coefficients change noticeably in magnitude, particularly for whites.

Comparing the coefficients across racial groups (columns 1 and 2), it appears that urban residence was more detrimental for nonwhites than for whites, and that education was less beneficial for nonwhites than for whites. These gaps persist, though their magnitudes change, in columns 3, 4, 5, and 6, when state or region dummies are included in the regressions. The comparatively large urban coefficient for nonwhites in Table 2 might reflect the poor living conditions common to many nonwhite urban neighborhoods. The comparatively low returns (in terms of mortality) to years of education for nonwhites might reflect the relatively low quality of education received by nonwhites (see Margo 1986, 1990).9 Two common econometric issues deserve mention. First, there may be measurement error in the independent variables, particularly for income and education. Even if measurement error in a particular variable is random, it implies some degree of attenuation bias (towards zero) to that variable's coefficient and an unknown direction of bias to the other coefficients (Greene 1993, pp. 279-284). Second, because health and economic conditions are interrelated in complex ways, one could argue plausibly that the regressions' explanatory variables are endogenous. In theory, an instrumental variable approach could help circumvent these concerns. 10 In this case, PE tests of the linear versus log-linear form favor the log-linear approach (Greene 1993, pp. 321-322). Additionally, link tests, essentially regressions of the dependent variable on fitted values and fitted values squared, reveal no evidence of misspecification.5

Columns 5 and 6 include a southern state dummy variable to provide a sense of how different, on average, infant mortality was in the South compared to elsewhere, when accounting for state-level differences in income, education, urbanization, and physicians per capita. In both columns, the coefficient on the southern dummy variable is positive,

implying that the relatively poor health outcomes in the region were not entirely due to differences in our set of observable state characteristics. The southern white infant mortality rate is only about 5 percent higher than elsewhere (*ceteris paribus*), but the nonwhite rate is about 16 percent higher.

Even with controls for income, education, urban residence, and supply of physicians, a very strong secular trend in infant mortality is manifested in the downward march of the coefficients on the time-period dummies. Two aspects of the time-period coefficients are especially interesting. First, even as the Hill-Burton program pumped funds into the system, the secular decline in infant mortality stagnates for nonwhites from 1950 to 1965 and for whites between 1955 and 1965. The regressions suggest that the mid-century plateau evident in Figure 1 is not due to adverse movements in the independent variables offsetting an underlying downward trend. Second, during some intervals, the nonwhite time coefficient fell by substantially more than the white coefficient, and during others the white time coefficient fell by substantially more than the nonwhite one.

To what extent can racial gaps in the independent variables account for the persistent gap in infant mortality? Table 3 presents a variable-by-variable decomposition of the "explained" change in infant mortality. The explained gap is that part of the gap that can be attributed to changes in the observed characteristics between whites and nonwhites, while the "unexplained" change is that portion of the gap that occurs because of differences in the estimated coefficients between races, holding sample characteristics constant. The total gap is reported in the top row of Table 3, which provides information on how much each variable contributes to the explained change. The last row of the table shows how much of the total gap is explained solely by differences in white and nonwhite characteristics. From this perspective, the *entire* racial gap in infant mortality can be accounted for by differences in characteristics up to 1945, but by 1970, only about a third of the gap can be explained.

This apparent decline in the significance of socioeconomic differences in explaining the infant mortality gap could be interpreted in several ways. It may be that unobserved factors influencing infant health diverged even as the observed socioeconomic factors converged. The time series graph of the racial gap in infant mortality rates features two periods of abrupt decline (1941-1946 and 1966-1971) and one period of rapid widening (1948-1958). The two episodes of decline coincide almost exactly with the only two periods of substantial decline in the racial wage gap (for men) since 1920 (Donohue and Heckman 1991). To a large extent, however, the abrupt changes in the infant mortality gap remain econometrically unexplained. Fortunately, the range of pertinent data expands in the post-1940 period,6 and with it we can pursue a re-examination (albeit somewhat speculative) of these "unexplained" changes in the gap.

Table 4 reports data on neonatal (under 28 days old) and postneonatal infant mortality gaps for southern and non-southern regions. The distinction between neonatal and postneonatal data is useful because the ultimate causes of death differ substantially for the two groups (premature delivery and low birthweight dominate neonatal mortality; infectious disease and environmental factors are more prevalent in postneonatal mortality). The table also reports weights reflecting the proportion of white and nonwhite births in both regional categories, an important detail because of the dissimilar (and changing) geographic distributions of nonwhites and whites.

Clearly, the 1941-1946 period was characterized by rapidly declining levels of infant mortality for both race categories *and* a declining racial gap. Table 4 shows that the racial gap declined especially sharply in the postneonatal category. The shrinking gap in postneonatal mortality accounts for most, but not all, of the overall racial convergence in infant mortality rates: the postneonatal gap fell from 21 to 9 while the neonatal gap fell from 13 to 8. Comparing 1940 with 1945 in Table 4, it is apparent that the strong declines in southern nonwhite neonatal and postneonatal mortality dominate all other movements during the period.

There are two more likely contributors to the gap's decline in the 1940s. First, as mentioned already and reflected in Table 4, there was substantial improvement in nonwhites' absolute and relative economic and educational status. Second, and more speculatively, given the concentration of African Americans in the South, and consistent with the large decline in the southern nonwhite IMR, the federal government's expanded military and administrative presence in that region might have disproportionately benefitted nonwhites. During the war, government efforts virtually eliminated malaria, aggressively targeted venereal disease and tuberculosis, and supported the Emergency Maternal and Infant Care (EMIC) program for the wives and children of low-ranking servicemen, a program with (potentially) substantial positive spillovers to the general quality of infant care (Sinai and Anderson 1948; Beardsley 1987, pp. 173-175). Unfortunately, the downward drop in the racial gap in infant mortality reversed itself completely between 1948 and 1958. While white infant mortality continued to fall for both neonatal and postneonatal infants, albeit slowly compared to the 1940s, the nonwhite rates were nearly constant. Table 4 reports that in the South, the infant mortality rate increased slightly between 1950 and 1960. At the same time, the southern white rate declined by 6.1 per 1,000.

The widening infant mortality gap, at least among neonates, may be explained in large part by widening racial differences in birthweight. Birthweight statistics were reported for the first time during the 1950s, and Chase and Byrnes (1972) noted an increase in the proportion of low birthweight neonates (under 2,500 grams) among nonwhites from 10.2 percent of births in 1950 to 12.8 percent in 1960. Among whites the low birthweight proportion fell from 7.1 to 6.8 percent. Chase and Byrnes were not 7 able to explain the rising proportion of nonwhite low birthweight infants by changes in the distributions of age of mother, plurality, hospital delivery, measurement error, or reported gestation.11 Furthermore, the postneonatal gap, which is generally regarded as being fairly insensitive to birth weight, also increased, and so it seems that even in the proximate sense, there must be more to the story than adverse changes in the distribution of nonwhites' birth weight. Finally, the relatively strong divergence of white and nonwhite infant mortality rates in the South recommends some regional focus for the investigation.

The rapidly changing healthcare system may have also influenced the infant mortality gap. Did white infants (especially in the South) benefit disproportionately from Hill-Burton funding of hospital expansion? Although we cannot rule this out, the available evidence appears to run against the grain of the hypothesis. In the South, the proportion of nonwhite hospital births increased from 24 to 74 percent between 1945 and 1960, whereas the white proportion increased from 68 to 97 percent. Of course, these figures say nothing about the quality of hospital care nor about the availability of prenatal care, but they do suggest a rapid increase in hospital and physician services for nonwhites. Furthermore, unlike the services available to southern blacks prior to World War II, Beardsley argues

that "In new federally sponsored hospitals black patients, if still segregated, at least had benefit of modern facilities and enjoyed roughly equal treatment" (1987, p. 256).

A second major change in the healthcare system in this period was the rise of health insurance, often provided as a benefit through one's employer (Thomasson 2001). Again, race-specific information is sparse through most of the period, but prior to 1940, relatively few people of either race were covered by health insurance (about 16 percent in 1940). By 1962, however, about 74 percent of whites had hospital insurance whereas only 46 percent of nonwhites did (Hoffmann 1964). As noted above, there was substantial racial convergence in the proportion of births in hospitals between 1945 and 1960, but it is possible that the racial insurance gap had implications for the quality and frequency of prenatal and postnatal care.

In conclusion, we seek to understand the disparity between black and white infant mortality rates from 1920-1970. Clearly, the rapid descent in infant mortality rates during this period benefitted both whites and nonwhites. At each point in time, however, nonwhites were disadvantaged in terms of income, education, and location relative to whites. Using a panel of state-level race-specific data, we found that a large portion of the racial gap in infant mortality rates can be accounted for by differences in those characteristics, especially between 1920 and 1945. But between 1945 and 1970, group differences in observable characteristics lost much of their explanatory power – that is, characteristics converged, but for some reason infant mortality rates did not. We mention several hypotheses regarding unobserved 11 The potential influence of measurement error is particularly worrisome. Chase and Byrnes (1972, pp. 21-24) evaluate the hypothesis that better reporting was responsible for the shift in the nonwhite birth weight distribution from several perspectives. They conclude that the shift is not a statistical artifact, though it is impossible to rule out measurement error completely.8 factors that could plausibly have driven the gap's changing character. Proximately, the neonatal gap appears to have been strongly influenced by a leftward shift in the nonwhite birthweight distribution during the 1950s, but the postneonatal gap also failed to narrow. Differences in access to health care and other behavior traits may help to explain the post-1950 differences, but without better data, our discussion remains speculative and open to more detailed empirical research.9

References

Almond, Douglas V., Kenneth Y. Chay, and Michael Greenstone. "Civil Rights, the War on Poverty, and Black-White Convergence in Infant Mortality Rates in Mississippi." Mimeo, August 2001.

Alston, Lee J. and Joseph P. Ferrie. Southern Paternalism and the American Welfare State. New York: Cambridge University Press, 1999.

Beardsley, Edward H. A History of Neglect: Health Care for Blacks and Mill Workers in the Twentieth-Century South. Knoxville, TN: University of Tennessee Press, 1987.

Berger, Mark C. and J. Paul Leigh. "Schooling, Self-Selection, and Health." Journal of Human

Resources 24, 3 (1989): 433-455.

Bradbury, Dorothy E. "Five Decades of Action for Children: A History of the Children's Bureau." In Children and Youth: Social Problems and Social Policy, edited by Robert H. Bremner. New York: Arno Press, 1974.

Brinkley, Garland. "The Decline in Southern Agricultural Output, 1860-1880." Journal of Economic History 57, 1 (1997): 116-138.

Center for Disease Control. "Achievements in Public Health, 1900-1999: Healthier Mothers and Babies." Morbidity and Mortality Weekly Report 48, 38 (October 1, 1999): 849-858.

Center for Disease Control. "Achievements in Public Health, 1900-1999: Tobacco Use – United States, 1900-1999." Morbidity and Mortality Weekly Report 48, 43 (November 5, 1999): 986-993.

Chase, Helen C. "A Study of Infant Mortality from Linked Records: Comparison of Neonatal Mortality from Two Cohort Studies, United States, January-March 1950 and 1960." DHEW Publication No. (HSM) 72-1056. Department of Health, Education, and Welfare. Public Health Service. Series 20, No. 13. June 1972.

Chase, Helen C. and Mary E. Byrnes. "Trends in 'Prematurity', United States: 1950-67." DHEW Publication No. (HSM) 72-1030. Department of Health, Education, and Welfare. Public Health Service. Series 3, No. 15. January 1972.

Chay, Kenneth Y. and Michael Greenstone. "The Impact of Air Pollution on Infant Mortality: Evidence from Geographic Variation in Pollution Shocks Induced by a Recession." NBER Working Paper 7442 (1999).

Chay, Kenneth Y. and Michael Greenstone. "Air Quality, Infant Mortality, and the Clean Air Act of 1970." Mimeo, July 2001.

Collins, William J. "When the Tide Turned: Immigration and the Delay of the Great Black Migration." Journal of Economic History 57, 3 (September 1997): 607-632.

Collins, William J. and Robert A. Margo. "Race and Home Ownership: A Century-Long View." Explorations in Economic History 38, 1 (January 2001): 68-92.

Costa, Dora L. and Richard H. Steckel. "Long-Term Trends in Health, Welfare, and Economic Growth in the United States." In Health and Welfare During Industrialization, edited by R. Steckel and R. Floud. Chicago: University of Chicago Press, 1997.

Currie, Janet and Rosemary Hyson. "Is the Impact of Health Shocks Cushioned by Socioeconomic Status? The Case of Low Birthweight." American Economic Review 89, 2 (May 1999): 245-250.

Cutler, David M., Edward L. Glaeser, and Jacob L. Vigdor. "The Rise and Decline of the American Ghetto." Journal of Political Economy 107 (1999): 455-506.

David, Richard J. and James W. Collins, Jr. "Differing Birth Weight Among Infants of U.S.-born Blacks, African-born Blacks, and U.S.-born Whites." New England Journal of Medicine 337, 17 (October 23, 1997): 1209-1214.

Demeny, Paul and Paul Gingrich. "A Reconsideration of Negro-White Mortality Differentials in the United States." Demography 4, 2 (1967): 820-837.

Department of Health and Human Services. "Racial and Ethnic Disparities in Infant Mortality." September 2000: http://www.raceandhealth.hhs.gov/3rdpgblue/infant/red.htm.

Donohue, John H. and James Heckman. "Continuous versus Episodic Change: The Impact of Civil Rights Policy on the Economic Status of Blacks." Journal of Economic Literature 29 (1991): 1603-1643.

Edwards, Linda N. and Grossman, Michael. "The Relationship Between Children's Health and Intellectual Development." In Health: What Is It Worth?, edited by Selma

Mushkin and David Dunlop, pp. 273-314. Elmsford, NY: Pergamon Press, 1979.
Ferrie, Joseph P. "The Poor and the Dead: Socioeconomic Status and Mortality in the U.S., 1850-1860." Mimeo, 2001.
Fishback, Price V., Michael R. Haines, and Shawn Kantor. "The Impact of the New Deal on Black and White Infant Mortality in the South." Explorations in Economic History 38, 1 (2001): 93-122.
Flegg, A. T. "Inequality of Income, Illiteracy, and Medical Care as Determinants of Infant Mortality in Underdeveloped Countries." Population Studies 36, 3 (1982): 441-458.
Goldman, Dana and Darius Lakdawalla. "Understanding Health Disparities across Education Groups." NBER Working Paper 8328 (2001).
Greene, William H. Econometric Analysis. New York: Macmillan Publishing Company, 1993.
Grossman, Michael. "On the Concept of Health Capital and the Demand for Health." Journal of Political Economy 80, 2 (1972): 223-255.
Grossman, Michael. "The Correlation Between Health and Schooling." In Household Production and Consumption, edited by Nestor E. Terleckyj, pp. 147-211. New York: Columbia University Press, 1975.
Grossman, Michael and Steven Jacobowitz. "Variations in Infant Mortality Rates among Counties of the United States: The Roles of Public Policies and Programs." Demography 18, 4 (1981): 695-713.
Grove, Robert D. and Alice M. Hetzel. Vital Statistics Rates in the United States, 1940-1960. Washington, D.C.: U.S. Department of Health, Education, and Welfare; Public Health Service, 1968.
Haenszel, William, Michael B. Shimkin, and Herman P. Miller. "Tobacco Smoking Patterns in the United States." Washington, D.C.: Public Health Service, monograph 45 (1956).
Haines, Michael R. "The Urban Mortality Transition in the United States, 1800-1940." Working paper, 2001.
Hirschman, Charles and Gerry E. Hendershot. "Trends in Breast Feeding Among American Mothers." Washington, D.C.: Public Health Service, series 23, number 3 (1979).
Hoffmann, Carolanne H. "Health Insurance Coverage." Washington, D.C.: National Center for Health Statistics, series 10, number 11 (1964).
Johnson, Joseph L. "Supply of Negro Health Personnel – Physicians." Journal of Negro Education 18, 3 (1949): 346-356.
Kenkel, Donald S. "Health Behavior, Health Knowledge, and Schooling." Journal of Political Economy 99, 2 (1991): 287-305.
Ladd-Taylor, Molly. Mother-Work: Women, Child Welfare, and the State, 1890-1930. Urbana and Chicago: University of Illinois Press, 1994.
Lave, Judith R. and Lester B. Lave. The Hospital Construction Act: An Evaluation of the Hill-Burton Program, 1948-1973. Washington, D.C.: American Enterprise Institute for Public Policy Research, 1974.
Lemons, J. Stanley. "The Sheppard-Towner Act: Progressivism in the 1920s." Journal of American History 55, 4 (1969): 776-786.
Levine, Robert S., et al. "Healthy People 2010 and 20th Century Patterns of Racial Mortality." Working paper, 2001.
Linder, Forrest E. and Robert D. Grove. Vital Statistics Rates in the United States, 1900-1940. Washington D.C.: GPO, 1943.
Maloney, Thomas N. and Warren C. Whatley. "Making the Effort: The Contours of Racial Discrimination in Detroit's Labor Market." Journal of Economic History 55, 3 (1995): 465-493.
Margo, Robert A. "Race, Educational Attainment, and the 1940 Census." Journal of Economic History 46, 1 (1986): 189-198.
Margo, Robert A. Race and Schooling in the South, 1880-1950. Chicago: University of Chicago Press, 1990.
Meckel, Richard A. Save the Babies: American Public Health Reform and the Prevention of Infant Mortality, 1850-1929. Ann Arbor: University of Michigan Press, 1998.
Mitchener, Kris James and Ian W. McLean. "U.S. Regional Growth and Convergence, 1880-1980." Journal of Economic History 59, 4 (1999): 1016-1042.
Pritchett, Lant and Lawrence H. Summers. "Wealthier is Healthier." Journal of Human Resources 31, 4 (1997): 841-868.
Rice, Mitchell F. and Woodrow Jones, Jr. Public Policy and the Black Hospital: From Slavery to Segregation to Integration. Westport, CT: Greenwood Press, 1994.
Rochester, Anna. Infant Mortality: Results of a Field Study in Baltimore, MD, Based on Births in One Year. U.S. Department of Labor, Children's Bureau. Bureau Publication 119. Washington, DC: GPO, 1923.
Rosenzweig, Mark R. and T. Paul Schultz. "Estimating a Household Production Function: Heterogeneity, the Demand for Health Inputs, and Their Effects on Birth Weight." Journal of Political Economy 91, 5 (1983): 723-746.
Ruggles, Steven and Matthew Sobek. Integrated Public Use Microdata Series. Minneapolis: Historical Census Projects, University of Minnesota, 1997.
Seham, Max. "Discrimination Against Negroes in Hospitals." New England Journal of Medicine. 271, 18 (October 1964): 940-943.
Shapiro, Sam, Edward R. Schlesinger, and Robert E.L. Nesbitt, Jr. Infant, Perinatal, Maternal, and Childhood Mortality in the United States. Cambridge, MA: Harvard University Press 1968.
Shin, Eui Hang. "Black-White Differentials in Infant Mortality in the South, 1940-1970." Demography 12, 1 (1975): 1-19.
Sinai, Nathan, and Odin W. Anderson. EMIC: A Study of Administrative Experience. Ann Arbor, MI: University of Michigan School of Public Health, 1948.
Smith, David Barton. Heath Care Divided: Race and Healing a Nation. Ann Arbor: The University of Michigan Press, 1998.
Smith, James P. "Race and Human Capital." American Economic Review 74, 4 (1984): 685-98.
Smith, James P. and Finis R. Welch. "Black Economic Progress After Myrdal." Journal of Economic Literature 27 (1989): 519-564.
Steckel, Richard H. "The Health and Mortality of Women and Children, 1850-1860." Journal of Economic History 48, 2 (1988): 333-345.
Subbarao, K. and Laura Raney. "Social Gains from Female Education: A Cross-National Study." Economic Development and Cultural Change 44, 1 (1995): 105-128.
Tandy, Elizabeth C. "Infant and Maternal Mortality among Negroes." Journal of Negro Education (July 1937): 322-349.

Thomasson, Melissa A. "From Sickness to Health: The Twentieth-Century Development of U.S. Health Insurance." Mimeo, 2001.
Troesken, Werner. "The Limits of Jim Crow: Race and the Provision of Water and Sewerage Services in American Cities, 1880-1925." Working paper, 2001.
Troesken, Werner. "Typhoid Rates and the Public Acquisition of Private Waterworks, 1880-1920." Journal of Economic History 59, 4 (1999): 927-948.
United States Department of Commerce, Bureau of the Census. Historical Statistics of the United States, Colonial Times to 1970. Washington, DC: GPO, 1975.

Table 1: Summary Statistics, 1920-1970

	White	Nonwhite
Panel A: Summary statistics of log state-year values 9entered in regressions)		
Ln Infant Mortality	3.4462	3.9358
	(0.4768)	(0.4212)
Ln Income	7.5917	6.8052
	(0.4357)	(0.6704)
Ln Education	2.3419	2.1009
	(0.1174)	(0.2625)
Ln Urban	4.0954	3.9879
	(0.3207)	(0.5245)
Ln Physicians	-6.6525	-6.8232
	(0.2653)	(0.3125)
N	281	281
Panel B: Summary statistics of stat-year values		
Infant Mortality	35.43	56.32
	(18.88)	(27.12)
Income	2159	1104
	(837.2)	(659.8)
Education	10.47	8.44
	(1.183)	(2.045)
Urban	62.81	60.75
	(16.67)	(26.32)
Physicians	0.0013	0.0011
	(0.0003)	(0.0004)
South	0.3071	0.6401
	(0.4621)	(0.4808)
N	281	281

Table 2: Infant Mortality Regressions, 1920-1970

	White	Nonwhite	White	Nonwhite	White	Nonwhite
Income	-0.1407	-0.1397	0.0014	-0.1742	-0.1356	0.0628
	(0.0886)	(0.1434)	(0.1394)	(0.1770)	(0.0317)	(0.1766)
Education	-0.8416	-0.4254	-0.3746	-0.3654	-0.6770	-0.4318
	(0.2829)	(0.2203)	(0.3700)	(0.2612)	(0.2857)	(0.2198)
Urban	0.0969	0.3073	-0.0855	0.5682	0.1280	0.2573
	(0.0780)	(0.1423)	(0.1884)	(0.2362)	(0.0711)	(0.1379)
Physicians	-0.1376	-0.2057	-0.3156	-0.1666	-0.1221	-0.2350
	(0.0455)	(0.1098)	(0.0945)	(0.2147)	(0.0402)	(0.1081)
South	-----	-----	-----	-----	0.0539	0.1573
					(0.0317)	(0.0629)
1925	-0.1438	-0.1818	-0.1671	-0.1676	-0.1530	-0.1762
	(0.0211)	(0.0599)	(0.0284)	(0.0614)	(0.0208)	(0.0601)
1930	-0.2767	-0.3365	-0.3231	-0.2995	-0.2947	-0.3078
	(0.0328)	(0.0778)	(0.0413)	(0.0928)	(0.0320)	(0.0795)
1935	-0.4058	-0.5324	-0.4728	-0.5148	-0.4328	-0.4953
	(0.0525)	(0.0912)	(0.0566)	(0.1151)	(0.0466)	(0.0922)
1940	-0.5226	-0.6201	-0.6407	-0.6139	-0.5582	-0.6174
	(0.0811)	(0.0788)	(0.0809)	(0.1033)	(0.0710)	(0.0762)
1945	-0.6252	-0.7933	-0.8131	-0.7999	-0.6701	-0.8819
	(0.1104)	(0.0918)	(0.1201)	(0.0779)	(0.0947)	(0.0970)
1950	-0.8873	-1.0290	-1.0825	-1.0612	-0.9390	-1.1006
	(0.1152)	(0.0860)	(0.1213)	(0.0918)	(0.0984)	(0.0837)
1955	-0.9637	-1.0199	-1.1833	-1.0695	-1.0220	-1.1122
	(0.1175)	(0.0869)	(0.1357)	(0.0971)	(0.0997)	(0.0867)
1960	-0.9622	-0.9681	-1.1960	-1.0335	-1.0266	-1.0668
	(0.1256)	(0.0930)	(0.1470)	(0.1089)	(0.1089)	(0.0921)
1965	-0.9640	-0.9749	-1.2266	-1.0470	-1.0361	-1.1082
	(0.1443)	(0.1058)	(0.1722)	(0.1202)	(0.1258)	(0.1132)
1970	-1.1028	-1.1827	-1.3968	-1.2588	-1.1824	-1.3537
	(0.1728)	(0.1209)	(0.2000)	(0.1323)	(0.1506)	(0.1356)
Constant	5.8960	3.9867	3.4625	3.3551	5.4800	2.5960
	(0.9152)	(1.1183)	(1.1628)	(1.3395)	(0.8792)	(1.3664)
Sate Dummies	No	No	Yes	Yes	No	No
R^2	0.97	0.90	0.98	0.95	0.98	0.91
N	281	281	281	281	281	281
Mean Dep. Var.	3.446	3.936	3.446	3.936	3.446	3.936

Table 3: Accounting for the Racial IMR Gap, 1920-1970

	Total IMR Gap	Gap "Explained" by Income	Education	Urban	Physicians	Total "Explained" Gap
1920	.462	0.120	0.374	-0.056	0.036	.474
		[0.261]	[0.808]	[-0.122]	[0.078]	
1925	.452	0.119	0.324	-0.045	0.028	.426
		[0.264]	[0.718]	[-0.101]	[0.063]	
1930	.499	0.147	0.375	-0.044	0.032	.510
		[0.295]	[0.751]	[-0.088]	[0.063]	
1935	.458	0.139	0.347	-0.035	0.034	.484
		[0.303]	[0.756]	[-0.076]	[0.074]	
1940	.525	0.141	0.328	-0.029	0.038	.479
		[0.269]	[0.624]	[-0.054]	[0.072]	
1945	.460	0.122	0.282	-0.018	0.032	.419
		[0.265]	[0.614]	[-0.038]	[0.070]	
1950	.504	0.119	0.243	-0.009	0.029	.382
		[0.236]	[0.483]	[-0.018]	[0.057]	
1955	.586	0.112	0.184	-0.002	0.022	.316
		[0.192]	[0.314]	[-0.004]	[0.038]	
1960	.636	0.109	0.136	0.003	0.018	.266
		[0.171]	[0.214]	[0.004]	[0.028]	
1965	.630	0.100	0.103	0.008	0.013	.224
		[0.158]	[0.163]	[0.013]	[0.020]	
1970	.562	0.092	0.076	0.012	0.009	.189
		[0.164]	[0.136]	[0.022]	[0.016]	

Notes: The "Total IMR Gap" is the difference between the average (weighted by population) log white and nonwhite infant mortality rates in each year. Each component of the "Gap Explained By" section is the product of the relevant coefficient from Table 2, column 1 and the difference in the variable's average value for whites and nonwhites (in that year). The percentage of the year's total gap accounted for by each component is listed in square brackets.

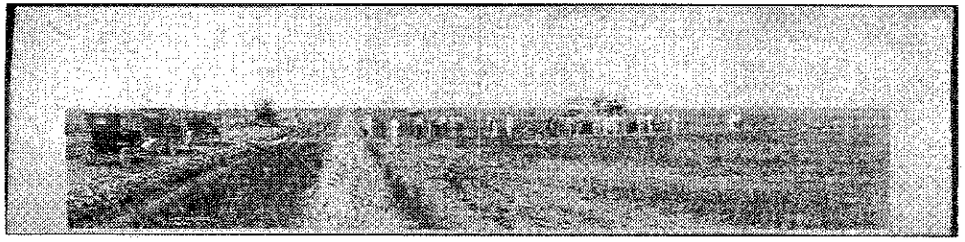


Table 4: Neonatal and Postneonatal Mortality, 1940-1970

	1940	1945	1950	1955	1960	1965	1970
White, South							
Neonatal	30.1	24.7	20.8	18.3	18.1	16.8	14.5
Postneonatal	21.7	16.1	9.8	7.0	6.4	5.9	4.2
Sum	51.8	40.8	30.6	25.3	24.5	22.7	18.7
Weight	0.31	0.30	0.29	0.28	0.28	0.28	0.29
White, Nonsouth							
Neonatal	25.9	22.6	18.8	17.5	16.9	15.8	13.4
Postneonatal	14.1	10.8	6.5	5.5	5.4	5.3	3.9
Sum	40.0	33.4	25.3	23.0	22.3	21.1	17.3
Weight	0.69	0.70	0.71	0.72	0.72	0.72	0.71
White, National	43.6	35.6	26.8	23.6	22.9	21.5	17.7
Nonwhite, South							
Neonatal	40.6	30.8	27.3	26.4	27.0	25.0	22.0
Postneonatal	35.2	25.1	18.3	17.7	20.0	18.1	11.0
Sum	75.8	55.9	45.6	44.1	47.0	43.1	33.0
Weight	0.78	0.74	0.67	0.61	0.56	0.51	0.49
Nonwhite, nonsouth							
Neonatal	36.4	34.3	28.2	28.3	26.9	26.3	21.7
Postneonatal	31.1	24.9	14.6	12.7	11.9	11.6	8.3
Sum	67.5	59.2	42.8	41.0	38.8	37.9	30.0
Weight	0.22	0.26	0.33	0.39	0.44	0.49	0.51
Nonwhite, National	73.9	56.8	44.7	42.9	43.4	40.6	31.5

Session 3: The Development and Origins of the Federal Reserve System and Its Impact on Financial Markets (Saturday, Jan. 5, 2:30pm, Hilton-Cabinet Room)

"Resolving the Puzzle of Low National Bank Note Issuance," Charles W. Calomiris (Columbia University) and Joseph R. Mason (Drexel University)

I. Introduction

During the Civil War, the federal government began to charter national banks. These banks enjoyed the privilege of being licensed to issue national bank notes, which were default-free liabilities of the banks, backed 111% by U.S. Treasury bonds deposited by issuing banks at the U.S. Treasury. The creation of these new banks, combined with a 10% annual tax on state bank note issues, soon resulted in the supplanting of state banks' notes by the new national bank notes.

Scholars have long puzzled over the observation that national banks did not take greater advantage of the authority to issue notes. The supply of notes never reached its maximum permissible level, despite calculations measuring the profitability of allocating capital toward bank note supply collateralized by bonds (e.g., as derived by Cagan 1965) indicating that national bank note issuance was more profitable than the typical profit

earned by allocating bank capital toward lending funded by a combination of deposits and capital. Friedman and Schwartz (1963, p. 23) write:

Before 1890 the amount outstanding ranged around 20 per cent of the possible maximum, by 1900 it had risen to about 28 per cent, and by World War I to about 80 percent. The maximum was in fact approached only in the twenties, when for the first time U.S. bonds deposited to secure circulation and government deposits (which also required such security) nearly equaled the total of eligible bonds. Before 1905, the capital stock of national banks set narrower limits to their maximum possible note issue than did the total of eligible bonds, but the actual issue did not approach this lower limit either. Thereafter, the capital stock of national banks exceeded the total of eligible bonds and hence was not the effective limit on note issue. Yet, despite the failure to use fully the possibilities of note issue, the published market prices of government bonds bearing the circulation privilege were apparently always low enough to make note issue profitable except in the years 1884 to 1891. The fraction of the maximum issued fluctuated with the profitability of issue, but the fraction was throughout lower than might have been expected. We have no explanation for this puzzle.

Friedman and Schwartz (1963) and Cagan (1965) argued that profits from note issue were large on the margin, because bond issues to back note issues remained cheap and because banks could easily leverage their capital devoted to those bond purchases.

James (1978) argued that cross-sectional variation in the regional supply of bank notes was consistent with regional variation in the opportunity cost of note issuance (that is, regional variation in the profitability of bank lending). In James' view, at least some of the puzzle of low bank note issuance was explicable by the high profitability of bank lending in the South and West, where note issuance was relatively low. But James' explanation was not a complete one. After 1874, there were no regional limits on note issuance, suggesting that banks in the East should have substantially increased their outstanding notes.

Hetherington (1990) showed that some of the time variation in the extent of note issue could be explained by changes in rules governing note issues. But that approach did not explain the puzzle posed by Friedman and Schwartz (1963) and Cagan (1965); like James' (1978) explanation of cross-state variation in note issuance, Hetherington's (1990) explanation of some of the variation in supply over time did not address the persistent underissue of bank notes: the level of bank notes remained far below its maximum despite the high profitability of note issuing. Champ, Wallace, and Weber (1992), recognizing that previous empirical analysis has not resolved the puzzle of national bank underissue, posit hidden transacting costs in a desperate attempt to explain why banks did not find it profitable to issue notes.

No such desperate explanations are necessary. In this paper, we resolve the puzzle of note underissue by disaggregating data on national banks and analyzing individual bank note issuing. We take account of the legal constraints facing individual banks that limited their maximum permissible note issuance. Friedman and Schwartz (1963) and Cagan (1965) compared average profitability of note issuing and lending. But that is not the relevant comparison. If, as James argued, some banks face high opportunity costs (leading them to limit note issuing), and if other banks earning lower profits from lending decide to issue the maximum amount of notes that they are permitted to issue by law, then apparent "underissuance" may still occur in the aggregate (in the sense that the average profits from lending may be below the marginal profit of note issuing) even though all banks are maximizing profits. We argue that this, in fact, is the explanation for the apparent underissuance of national bank notes.

We develop a data set linking individual bank note issuing behavior, the specific legal constraints on note issuing faced by each bank, and the profitability of bank lending (as measured by bank performance and other bank characteristics that should have influenced the note supply decision). Our measures of the opportunities for lending include characteristics of the economic environment in which banks operated. Those environmental characteristics are measured using state- and regional-level data.

By disaggregating, and analyzing the constraints faced by individual national banks in 1880, we are able to show that most banks (roughly two-thirds of all national banks) were issuing the maximum amount of notes they were allowed to issue. For the remaining banks that issued less than the maximum amount allowed, we find that cross-sectional variation in banks' opportunity costs go a long way toward explaining the extent of note issuance by these banks. Those same measures of opportunity costs also explain which banks tend to be maximum note issuers. We conclude that a combination of legal restrictions on maximum note issuing and banks' opportunity costs explain the extent of bank note issuing in a manner fully consistent with bank profit maximization.

II. Supply and Demand for National Bank Notes

The quantity of national bank notes in circulation was determined by supply and demand in the market for bank notes. While there were legislative limits placed on the aggregate quantity of notes outstanding and on the geographic distribution of note issuing, those limits were not effective constraints on individual bank issuance. Prior to 1874, whenever the amount of notes came close to reaching the maximum allowable supply, the law was changed to accommodate more note issues. In 1874, the law was changed to remove any limits on note issues, although the limited supply of U.S. Treasury bonds (to serve as 111% collateral for note issues) effectively placed a non-binding upper bound on the potential supply of notes.

Notes were essentially perfect substitutes for transactions purposes with U.S. notes (greenbacks) and coins, and traded at par with those alternative transacting media (except briefly during1873, when the special value to banks of holding greenbacks, which were a legal reserve currency, led their value to temporarily exceed that of bank notes – see Friedman and Schwartz 1963, pp. 21-2). National bank notes and greenbacks were always inframarginal sources of transacting media set by suppliers and were unresponsive to shifts in the demand for transacting media; increases in demand for transacting media on the margin were met by changes in the supply of specie currency (see Calomiris 1988, 1994, Hetherington 1990).

In essence, equilibrium in international markets under the classical gold standard simultaneously determined gold-denominated interest rates and specie flows to equilibrate the markets for goods and money (Calomiris and Hubbard 1995). The supply of national bank notes adjusted endogenously to the level of interest rates set in the money market. Calomiris (1988, 1994) shows that (so long as the supply of Treasury bonds to back national bank notes was greater than the amount demanded for that purpose) the supply of national bank notes for banks that were not at a corner solution should have been determined by (a) the yield on government bonds, (b) the profitability of bank lending, and (c) the tax rate charged on national bank note issues. The supply of notes, in this model, is

set by the profit-maximizing choices of national banks about whether to allocate capital toward (a) the business of deposit taking and lending or (b) the business of producing national bank notes, backed by government bond purchases. In any empirical model of cross-sectional differences in note issuance (for our sample of national banks in 1880) the profitability of lending is the only variable that should predict cross-sectional differences in the propensity to issue notes, since the taxation rate and market yield on bonds are the same for all banks.

Of course, this model does not apply to banks that are at a corner solution, either because of legal limits that require them to issue less than they would like, or legal limits on minimum issues that require them to issue more than they would like. Among banks issuing the maximum permissible amount of notes, cross-sectional variation in bank characteristics will have no explanatory power for note issuing. Thus, before applying the model to individual bank data, one must take account of the various limits on note issuing that might constrain banks to operate at a corner solution.

As of 1880, the limits on note issues relating to capital for national banks were as follows (see National Monetary Commission 1910). Banks chartered before March 1865 could not issue notes in excess of 90% of bank paid in capital. Banks chartered from March 1865 through July 1870 were governed by the following limits on note issue relative to capital: Those with capital less than $500,000 could issue up to 90% of capital; those with capital of between $500,000 and $1,000,000 could issue up to 80% of capital; those with capital of between $1,000,000 and $3,000,000 could issue up to 75% of capital; and those with capital in excess of $3,000,000 could issue up to 65% of capital. Banks chartered after July 12, 1870 were governed by the following limits: No bank could issue more than $500,000 in notes; banks with capital less than $500,000 could issue up to 90% of capital; and banks with capital between $500,000 and $625,000 could issue up to 80% of capital. National banks that also had outstanding state bank issues (from before they became national banks) had to include those notes in any measure of total permissible note issues. All banks also faced an effective minimum note issuing requirement, since all national banks were required to maintain at least $30,000 in bond holdings or one third of bank capital, whichever was higher. As we shall show below, these ceilings and floors on permissible note issues were often binding on individual banks.

III. Empirical Analysis

Our dataset consists of information about 1,882 national banks and the states in which they reside, for the year 1880. The dependent variable we analyze is the extent of underissue. Specifically, underissue is defined as:

$$U = [1 - (\text{Actual Notes/Maximum Permissible Notes})].$$

The median value of U is 0.66% and the mean is 9.78%. Fully 75% of national banks have values of U less than 11%. We assume that banks with U less than 2% are effectively at their maximum amount of note issue (given potential rounding effects from the minimum denomination of bond issues and random variation in outstanding notes associated with redemptions). By that definition, 67% of national banks were issuing the maximum amount of notes permitted. In other words, two-thirds of national banks were at a corner solution in their issuing of national bank notes. Figure 1 plots a histogram of the distribution of U for the other 33% of national banks.

We turn to regression analysis of U. We report three regressions: an OLS regression for the one-third of banks that were not issuing the maximum permissible amount of notes in 1880, a Probit regression for whether banks are maximum issuers or not, and a Tobit regression for the entire sample of national banks, which takes account of left-censored banks (those issuing the maximum amount of notes permitted). In all three regressions we exclude right-censored banks (those issuing the minimum amount of notes permitted). Including these banks would not materially affect our results.

Our measures of bank opportunity costs take account of a variety of potentially relevant factors. We include the return on assets (ROA) of the bank as a measure of bank profitability. Of course, ROA is a noisy indicator of long-run lending opportunities and fails to capture dynamic growth or contraction in expected loan opportunities, which would be relevant to bank decisions about allocating capital between note production and lending. Thus, as additional bank-level proxies, we also include the ratio of loans and discounts relative to assets other than U.S. Treasury bonds in 1880 (LOANRAT), as well as the change in that variable from 1879 to 1880 (LOANGR). We include other characteristics of banks that might also influence their lending and lending growth opportunities, such as asset size (SIZE) and bank age (AGE), defined as years since its national bank charter, and the interaction between the two (SIZExAGE). It occurred to us that a bank's decisions about note issuance might also be influenced by the extent of a bank's connections with the federal government as a fiscal agent of the government (or that those connections might be indicative of the opportunity set of the bank or its overall strategy), so we include the ratio of debt due to the bank from the Treasury relative to total bank assets (DFTTA) as a regressor. We interpret DFTTA as a measure of the extent to which the bank acted as an agent for the government in the bond market.

We also include variables that capture aspects of the economic environment in which banks operate, which might be relevant for measuring opportunity costs, and which might not be fully captured by our noisy bank-level measures of lending opportunities. These include: DIVCAP (dividends paid by national banks in the state / capital of national banks in the state), URBAN (an indicator variable that takes the value 1 if the bank is located in New York, Philadelphia, Boston, Chicago, New Orleans, or San Francisco, and zero otherwise), and six regional indicator variables (MIDATL, MIDWEST, SOUTH, APPAL, WEST, and the excluded category, EAST). To control for regional differences in links between banks and the federal government, we interact regional variables with DFTTA. Finally, we include the ratio of national banks in the state to total banks in the state (NBRAT) to control for the possibility that the presence of other national banks in a bank's state might influence its note issuing behavior.

Table 1 reports regression results for each of the three regressions (OLS for underissuers, Tobit for all national banks, and Probit for all national banks). The results of the three regressions are broadly consistent and indicate substantial variation in the extent of underissuance that is traceable to bank-specific, state-specific, and region-specific characteristics. Some of the regressors have more explanatory power in some of the regressions than in others.

Interestingly, LOANRAT and LOANGR have opposite signs in all three regressions; LOANRAT is associated with less note issuing and LOANGR with more (after controlling for LOANRAT). One interpretation of this finding is that there are adjustment lags in note issuance and redemption. Banks whose high LOANRAT is due to recent growth in loans may have more notes outstanding than they would like to have in the long run.

Banks with high DFTTA tend to issue more notes, and this varies according to the region of the country. As in James (1978), banks in the West, South, and Midwest tend to issue less notes. ROA, SIZE, and AGE are highly significant in the Probit and Tobit, but not in the OLS.

IV. Conclusion

Most of the puzzle of underissuance of national bank notes disappears when one disaggregates the data to the level of individual banks and takes account of the limits banks faced on their maximum permissible note issues. 67% of banks in our 1880 sample were maximum note issuers.

The hypothesis that bank opportunities in lending can explain cross-sectional differences in bank note issuing is confirmed by our regression analysis. Banks with poor lending opportunities, or those operating in regions with poor lending opportunities, issued more notes or were more likely to be maximum issuers.

We conclude that the puzzle of underissuance of national bank notes is an object lesson in the value of disaggregating data, and thus avoiding "representative bank" analysis relating average bank behavior and opportunities.

Table 1: Regressions on National Bank Note Issue, 1880

Model Type	OLS	Tobit	Probit
Dep. Var.	Underissue	Underissue	Maximum Issue=1
N	604	1785	1785
Uncensored Values	604	604	604
R^2 / Log-Likelihood	0.3411	-671.16	-1030.27
Adj R^2 / Unrestricted Log-Likelihood	0.3185	-876.95	-1180.91
	Coef	Coef	Coef
	Std. Error	*Std. Error*	*Std. Error*
Intercept	-0.208	0.657	-3.552
	0.446	*0.570*	*2.133*
LOANRAT	0.253	0.217	-0.316
	0.053	*0.067*	*0.248*
LOANGR	-0.065	-0.169	0.503
	0.029	*0.040*	*0.150*
SIZE	0.026	-0.054	0.293
	0.034	*0.044*	*0.163*
AGE	0.027	-0.093	0.481
	0.029	*0.037*	*0.141*
SIZExAGE	-0.002	0.007	-0.037
	0.002	*0.003*	*0.011*
NBRAT	-0.009	-0.004	-0.025
	0.076	*0.098*	*0.366*
DFTTA	-6.531	-10.724	23.500
	1.056	*1.672*	*6.166*
MA_DFTTA	-7.015	-6.875	17.813
	2.061	*2.839*	*10.318*
MW_DFTTA	-12.305	-18.084	44.026
	1.997	*2.959*	*10.884*
SO_DFTTA	-12.204	-32.095	100.045
	9.747	*11.736*	*44.802*
AP_DFTTA	-9.073	-19.905	71.429
	5.320	*7.081*	*26.381*
WE_DFTTA	-7.012	-26.783	79.941
	17.001	*19.478*	*79.419*
ROA	-0.529	6.409	-24.213
	2.168	*2.733*	*10.275*
DIVCAP	2.842	-1.171	8.088
	1.667	*1.909*	*7.183*
URBAN	-0.048	-0.021	-0.083
	0.028	*0.039*	*0.151*
MIDATL	0.033	0.085	-0.340
	0.040	*0.058*	*0.220*
MIDWEST	0.161	0.302	-0.855
	0.042	*0.062*	*0.236*
SOUTH	0.205	0.448	-1.350
	0.121	*0.159*	*0.627*
APPAL	0.068	0.272	-1.145

References

Cagan, Phillip (1965). *Determinants and Effects of Changes in the Stock of Money, 1875-1970*, New York: Columbia University Press.

Calomiris, Charles W. (1988). "Price and Exchange Rate Determination During the Greenback Suspension," *Oxford Economic Papers* (December), pp. 189-220.

Calomiris, Charles W. (1994). "Greenback Resumption and Silver Risk: The Economics and Politics of Monetary Regime Change in the United States, 1862-1900. In M.D. Bordo and F. Capie, eds., *Monetary Regimes in Transition*, Cambridge: Cambridge University Press, pp. 86-132.

Calomiris, Charles W., and R. Glenn Hubbard (1995). "International Adjustment Under the Classical Gold Standard: Evidence for the U.S. and Britain," Working paper, Columbia Business School.

Champ, Bruce, Neil Wallace, Warren E. Weber (1992). "Resolving the National Bank Note Paradox," *Quarterly Review*, Federal Reserve Bank of Minneapolis (Spring), pp. 13-21.

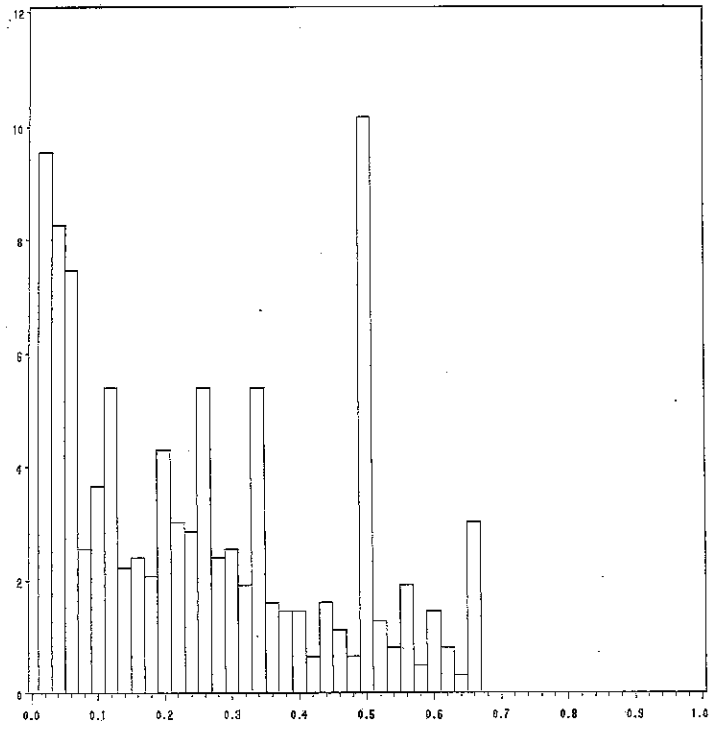
Friedman, Milton, and Anna J. Schwartz (1963). *A Monetary History of the United States,1867-1960*, Princeton: Princeton University Press.

Hetherington, Bruce W. (1990). "Bank Entry and the Low Issue of National Bank Notes: A Reexamination," *Journal of Economic History* (September), pp. 669-76.

James, John A. (1978). "The Conundrum of the Low Issue of National Bank Notes," *Journal of Political Economy* (April), pp. 359-67.

National Monetary Commission (1910). *Laws of the United States Concerning Money, Banking, and Loans, 1778-1909*, Washington D.C.: U.S. Government Printing Office.

Fig 1: Histogram of National Bank Note Underissue, 604 Underissuing



Underi

"Why Didn't the United States Establish a Central Bank Until After the Panic of 1907?" Jon Moen (University of Mississippi) and Ellis Tallman (Federal Reserve Bank of Atlanta)

I. Introduction

The establishment of the Federal Reserve System in 1913 is conventionally viewed as the inevitable outcome of the turmoil surrounding the Panic of 1907. Before 1907, there had been support from bankers and politicians for a central bank (or system of centralized reserves -- terms we shall use interchangeably), and there had been severe financial crises and bank panics before 1907. Yet these earlier panics had not led to the establishment of a central bank. So what made the Panic of 1907 different from earlier National Banking Era panics?

One reason is that the Panic of 1907 was focused uniquely on the New York City trust companies. Moen and Tallman (1992) show that New York City trust companies were singled out by depositors for withdrawals in 1907, unlike the New York City national banks. During the Panic of 1907, deposits at New York City trust companies fell precipitously by nearly 37 percent. In contrast, deposits at New York City national banks increased by over 8 percent. A key difference between the panic of 1907 and the earlier crises was that trusts did not play a major role in intermediation during the earlier instances of financial panics.

We argue that this unusual deposit behavior was a result of the fact that New York City trust companies were not members of the New York Clearinghouse. Timberlake (1984) and Gorton (1985) describe how the private clearinghouses employed specific mechanisms to stem panics during National Banking Era panics. In panics before 1907, the New York Clearinghouse addressed runs on member institutions by providing adequate reserves to solvent member banks, and closing down insolvent ones. During the 1907 panic, the Clearinghouse lacked sufficient timely information about the trust companies' balance sheets to make reliable inferences about the solvency of these institutions. Eventually a coalition of New York bankers led by J.P. Morgan provided aid to the trusts, presumably because letting the trusts go it alone posed a risk to the entire financial system. We therefore provide a hypothesis for why New York bankers perceived a risk to their own balance sheets arising from the potential actions of trusts faced with depositor's panic withdrawal demands: the national banks were linked to the trusts companies through the call loan market, the overnight loan market at the New York Stock Exchange.

II. Background and Previous Research

There are many extensive studies on the evolution of the US financial system and the establishment of a central bank (White 1983, Livingston 1986, McCulley 1991, Broz 1997 are a few examples). Each takes a different view of the key factors that eventually led to the general acceptance by the US legislature and electorate of the creation of a central bank . The political economy surrounding specific pieces of legislation are dealt with in detail within those studies. Our goal is different; we emphasize how the Panic of 1907 was different from earlier panics, and then we indicate how such differences posed much greater risks for the financial system than the risks observed in earlier panics.

E.N. White's study of American banking regulation is of particular relevance to our argument; he views the Panic of 1907 as the galvanizing force in forming a US central bank (White 1983). His research focuses on the dual banking system, and his analysis at the national level shows that national banks were the dominant intermediary in the early part of the National Banking Era. The rapid growth of the state-chartered trusts and banks weakened the dominance of national banks and the influence of the clearinghouses over aggregate financial assets. White notes that the effectiveness of the clearinghouses in combating panics declined as the proportion of assets outside the clearinghouse banks increased. He notes also how the circumscribed coverage of the clearinghouse was an observable flaw when, during the 1907 banking panic, the trusts were not able to get quick liquidity assistance from the New York Clearinghouse. Because some state banks in New York were clearinghouse members and no New York trusts were members in 1907, the distinction between state banks and trusts appears warranted. White's conclusions remain relevant to our narrower focus on the trusts and their "outsider" status in New York City, as a refinement of his argument. We propose that the trusts in New York City imposed risks on the financial system and more specifically the payments system coming from outside the national banking structure.

Until 1907, New York national banks handled financial crises adequately on their own, at least in terms of protecting the clearinghouse member banks. It is possible that they may have even profited from crises (Donaldson 1992, 1993). Under such

circumstances, one would expect little enthusiasm for the institution of a central bank in the US coming from New York banking forces.

III. Theoretical Models of Banking Panics – Motivations for a Central Bank

The theoretical research on individual bank runs has recently been extended to the analysis of the propagation of runs into widespread banking panics. The papers on banking panics have implications regarding whether key financial market participants would support the establishment of a central bank. We focus on three particular models, showing that our hypothesis about the establishment of a central bank can resolve some inconsistencies in the implications of each model, implications concerning the motives for establishing a central bank.

Timberlake (1984) and Gorton (1985) discuss the role of private clearinghouse behavior during the National Banking Era (1863-1913). These papers provide a sense in which the existing clearinghouse system may have been sufficient to quell panics, and that the establishment of the Federal Reserve System may have been unnecessary.

Donaldson (1992, 1993) presents a model of banking panics that implies a more controversial view of clearinghouse behavior. Donaldson suggests that the large New York banks, especially the members of the New York Clearinghouse, were able to earn risk-adjusted excess rates of return from loans of cash during the banking panics. If Donaldson is correct, why would the large New York banks that earned excess profits have been in favor of establishing a central bank, when this new institution would remove the monopoly on cash reserves from the New York Clearinghouse?

McAndrew and Roberds (1995) present a model of banking in which optimal private clearinghouse behavior allows for the possibility of bank panics. Nonmember institutions free ride on the reserve holdings of the clearinghouse in this model, but the clearinghouse lacks the power to enforce any direct penalty on nonmember institutions for holding lower reserves. In the model, the existence of banking panics is a mechanism to punish nonmember financial institutions for holding lower than optimal.

Why then, given these theoretical results, would any coalition of bankers support an outside central bank? A key reason is that the assets of trust companies had grown tremendously since 1896; by 1907 total assets of trust companies and of national banks in New York City were about the same (Moen and Tallman 1992, p.612). By 1907, the increased size of the trusts left a significant proportion of financial assets held by intermediaries beyond the examining authority of the Clearinghouse. Thus, unlike in earlier panics the Clearinghouse was an unreliable candidate to provide liquidity in 1907 to the troubled nonmember institutions, the trusts.

IV. Systematic Aspects of National Banking Era Panics

Many financial structures present during the 1907 panic had also been present in earlier panics and can be ruled out as the immediate causes of the panic and for establishing a central bank. The pyramid structure of reserves across central reserve city, reserve city, and country national banks existed in 1907 and earlier, and branch banking was still pretty much nonexistent. Clearinghouse loan certificates, IOUs used between clearinghouse member banks in lieu of cash to settle clearing balances and therefore to free up cash to pay depositors during panics, had also been used in earlier panics. The problem with the inelastic currency also remained (Friedman and Schwartz 1963, pp. 168-73).

While the general financial structure remained similar across the panics of the National Banking Era, the behavior of balance sheet items (loans and deposits) was quite different in 1907. For example, Sprague notes that loans and deposits at New York national banks contracted somewhat in August during the Panic of 1893 (Sprague 1910, p. 190). Loans and deposits increased at the New York national banks as depositors fled the trusts during the Panic of 1907 (Moen and Tallman 1992). We therefore examine the loans, net deposits and reserves of national banks from the weekly statements of clearinghouse banks in New York City during the most severe National Banking Panics of 1873, 1893 and 1907. Loans are defined as total loans, deposits are defined as net deposits, and reserves are defined as specie and legal tender, and each series is the aggregate of clearinghouse member banks in New York City (in A.P. Andrew, *Statistics for the United States 1867-1909*). What we find in those figures is that the deposits, loans and reserves all contract during 1873 and 1893 when the panic strikes. The pattern observed in the series are consistent with the idea that depositors "panic" by withdrawing their funds from the intermediation system in general, lowering bank cash reserves, and occasionally forcing liquidation of demandable loans when reserve levels are perceived as low

In Graph 1, the series for deposits, reserves, and loans for New York national banks in 1907 displays behavior that is not consistent with the same panic story. In contrast, national bank loans and deposits increase while reserves decline. It is possible that this result is due to an idiosyncrasy of the 1907 panic. However, we then look at deposits and reserves in other reserve city national banks in 1907. Looking at the ratio of deposits to reserves among New York City national banks over the three major panics (Graph 2), we see that the deposits to reserves ratio did not fall in 1907, whereas it was typical for the ratio to fall in prior panics. Graphs 3, 4, and 5 present the behavior of loans, deposits, and reserves, respectively, in New York versus other reserve city national banks in 1907. We aggregate these series into a measure of reserves and deposits at reserve city banks outside New York, and compare those aggregates to those of New York national banks in 1907. In the aggregate, deposits and reserves contract during the panic in ways consistent with the standard panic characterization described above, in clear contrast to the behavior of those series in New York national banks. The correlation coeffieients between deposits and reserves at New York national banks in 1873, 1893, and 1907 are 0.89, 0.97, and –0.72; the distinct difference noted for 1907 supports the patterns observed in Graph 1.

We interpret the unique decline in the reserves at New York national banks as an indication that there was some form of disintermediation taking place there. Notably, it was not depleting the assets of the national banks. Rather, it was depleting their reserve holdings. The increase in deposits, however, suggests that the panic-related withdrawals from trusts were re-deposited in banks so that the increase in national bank deposits may reflect partly a transfer of trust deposits into national bank deposits. The increase in loans among New York national banks likely reflected the purchase of trust loans by banks, perhaps call loans as discussed below. Hence, the dis-intermediation was affecting the trusts but not the payments system. The runs on trusts forced them to find additional sources of reserves because the trusts held a low proportion of reserves to deposits. Trusts likely borrowed reserves from the Clearinghouse member banks or acquired them by selling assets. In either case, the drain on trust deposits reduced the level of reserves among New York City national banks and contributed to the increased level of loans held by national banks.

V. Fear and Contagion in the Call Loan Market

Even though the trusts were tangential to the payments system and, from a systemic perspective, should not have threatened a major disruption in the payments system, the New York bankers wanted to prevent large-scale withdrawals from trust companies to protect the assets and solvency of national banks. The link between the New York national banks and the trusts was the call loan market at the New York Stock Exchange. New York City national banks participated heavily in the call money market, providing demandable loans (collateralized by stock equity) to the stock market to support the trading on the floor of the market. These short-term demandable loans were a convenient mechanism for earning interest on reserve city holdings of banker's balances and could be settled quickly on demand during normal times. Nearly 30 percent of the assets of New York banks were tied up in these demandable loans (see Moen and Tallman 1992).

Trust companies also participated in the call loan market, although it is likely that they held fewer call loans. The liquidity provided by these loans was less important to the trust companies because their volume of clearings was so much lower than the national banks. Still, the influence of trust companies in the stock market was not insignificant. Trust companies still held sizable proportions (estimates of from 10 to 25 percent) of their assets as call loans, as well as owning some stock shares outright (on average, 20 percent of trust assets were in stocks and bonds – we have no direct data on stock proportion).

During the panic of 1907, the call money rate peaked at an unprecedented 125 percent, indicating a significant lack of liquidity provision and/or perceptions of risk to stock market investments. The stock market index (Macaulay 1938) declined about 25 percent over the three months of the panic. But this measure does not capture fully the degree of turmoil that struck the stock market. Even given standard discounts for the collateral asset values, some of these loans already would likely be contributing to bank losses if they were liquidated at depressed market prices. Banks were at risk of further losses if trust company assets related to the stock market were liquidated under financial duress, which would place additional downward pressure on stock prices. Given the degree of the panic, there was a possibility of a downward spiral of prices and further liquidation of call loans that would leave the financial system in a shambles. Instead, banks likely (we have no direct documentation) bought over the loans from trust companies rather than allow the collateral from those loans to be liquidated at fire sale prices. Lower stock prices would exacerbate the liquidity problems, sending call money rates again into triple digits

With the stock market and the call loan market linking the banks and trusts in a way that the payments system did not, we suggest that the rational national bank or clearinghouse member would want to aid the trusts. The increase in deposits and loans at the national banks that took place during the Panic of 1907, while puzzling at first, can therefore be seen as scramble by the banks to protect assets and intermediation rather than to profit off of the misfortune of the trusts.

VI. Conclusion

In the aftermath of the Panic of 1907 the New York bankers recognized the growth of relatively unregulated intermediaries as a new and growing risk to the financial system. The fact that these intermediaries were beyond the influence of Clearinghouse monitoring contributed to the change in New York banker opinion that moved them toward finally supporting the creation of a central bank. While the Panic of 1907 convinced important New York bankers that the time for a system of centralized reserves had come, they nevertheless were unable to control the ultimate form that such a system took.

REFERENCES

Andrew, A. Piatt (1910) "Statistics for the United States, 1867-1909." Publications of the National Monetary Commission, vol. 7 (Washington, DC).

Broz, J. Lawrence (1997) *The International Origins of the Federal Reserve System*, (Ithaca, New York: Cornell University Press).

Donaldson, R. Glen. (1992) "Costly Liquidation, Interbank Trade, Bank Runs, and Panics," *Journal of Financial Intermediation* 2: 59-82.

_____. (1993) "Financing Banking Crises: Lessons from the Panic of 1907," *Journal of Monetary Economics*. February, 31: 69-95.

Friedman, Milton, and Anna J. Schwartz (1963) *A Monetary History of the United States, 1867-1960* (Princeton University Press: Princeton NJ).

Gorton, Gary, "Clearinghouses and the Origins of Central Banking in the United States," Journal of Economic History (June 1985): 277-84.

Livingston, James (1986) *Origins of the Federal Reserve System: Money, Class, and Corporate Capitalism, 1890-1913.* (Ithaca: Cornell University Press).

McAndrews, James and William Roberds (1995), "Banks, Payments, and Coordination," *Journal of Financial Intermediation* (October), pp. 305-327.

McCulley, Richard T. (1992) Banks *and Politics During the Progressive Era: The Origins of the Federal Reserve System, 1897-1913.* (Garland Publishing, New York and London).

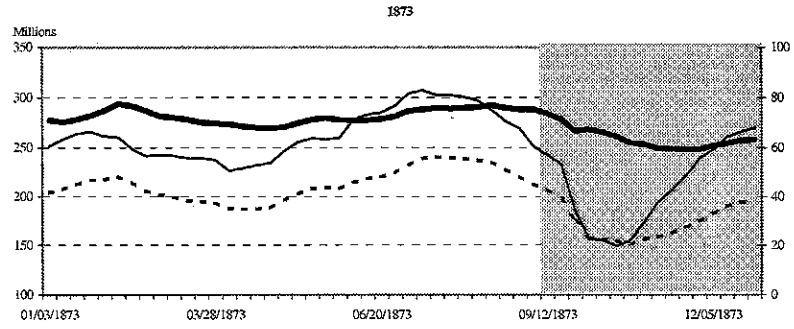
Moen, Jon and Ellis Tallman (1992) "The Bank Panic of 1907: The Role of Trust Companies", Journal *of Economic History* 52, no. 3 (Sept. 1992): 611-30.

O.M.W. Sprague (1910), History *of Crises under the National Banking Era*, National Monetary Commission (Washington: Government Printing Office).

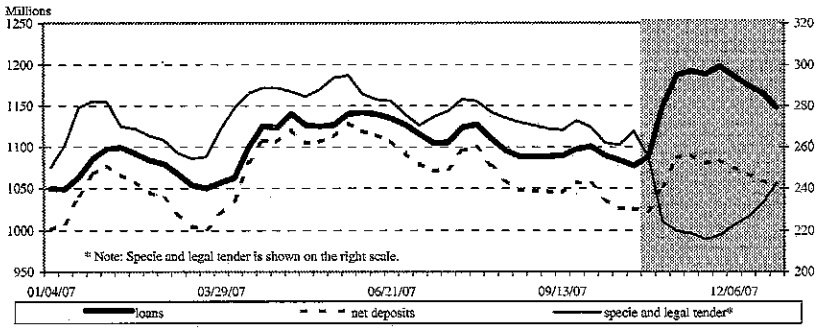
Redlich, Fritz, The Molding of American Banking: Men and Ideas (Johnson Reprint Co., New York: 1968).

Timberlake, Richard, "The Central Banking Role of Clearinghouse Associations," Journal of Money, Credit, and Banking 41 (Feb. 1984): 1-15.

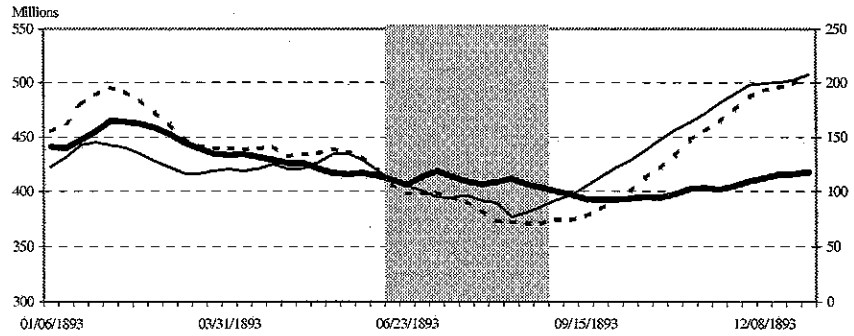
White, Eugene N. (1983) *The Regulation and Reform of the American Banking System: 1900-1929.* Princeton, New Jersey: Princeton University Press.



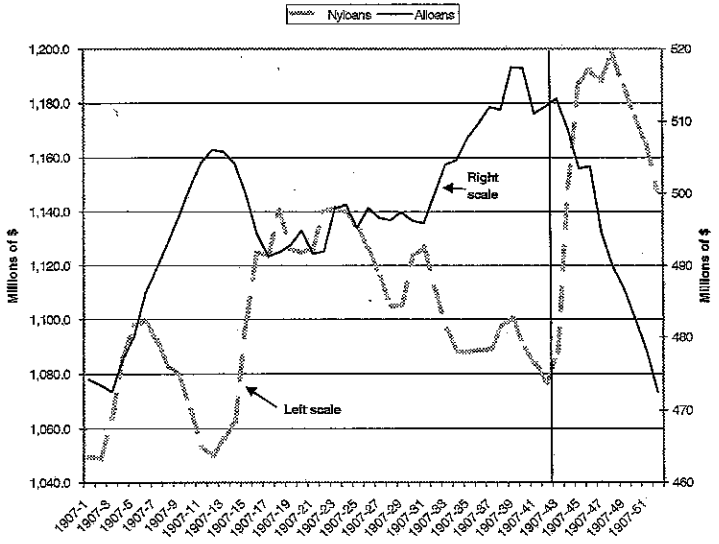
1907



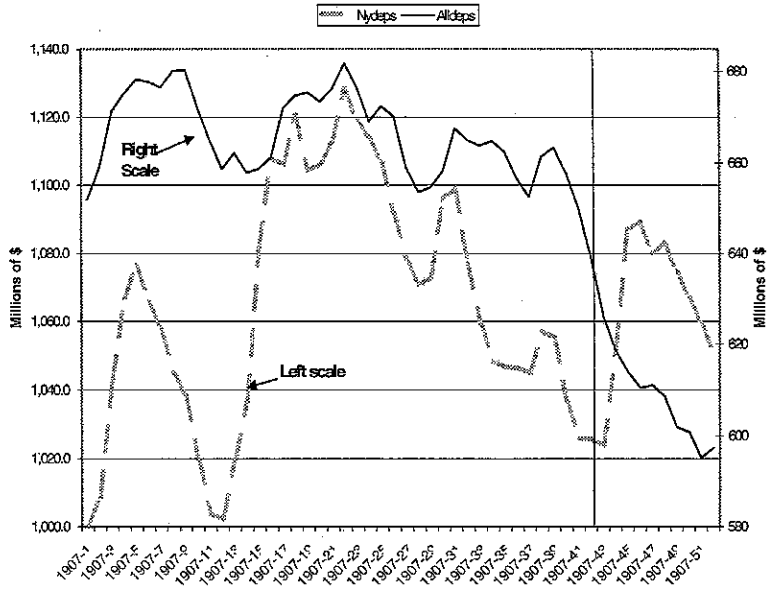
1893

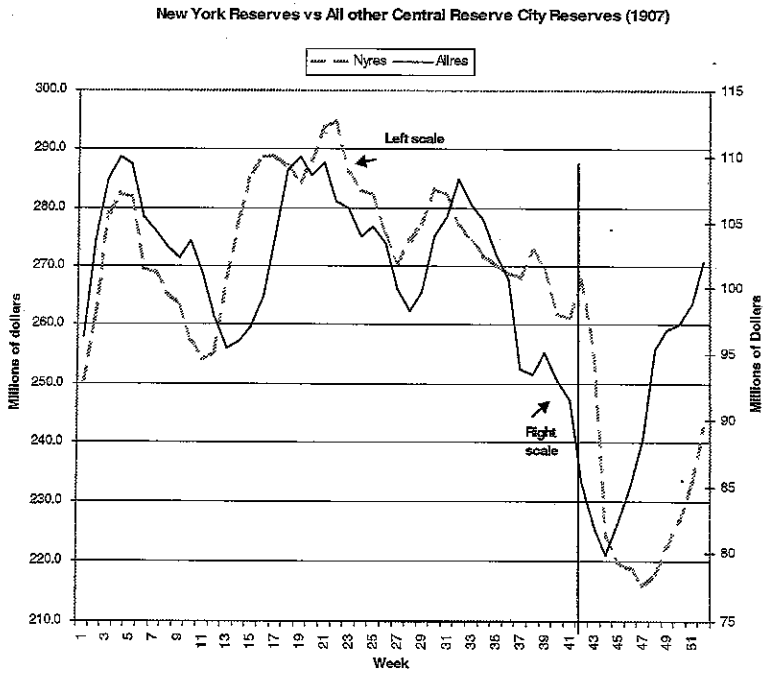


Loans: NY vs All other Central Reserve Cities



Deposits: NY vs All Other Central Reserve Cities





"How Were the Federal Reserve Bank Locations Selected?" Michael McAvoy (SUNY at Oneonta)

Political interference was alleged to influence the Reserve Board Organization Committee for the selection of at least five of the 12 Federal Reserve cities in April 1914: Atlanta, Cleveland, Dallas, Kansas City and Richmond. H. Parker Willis, a committee advisor, alleged that politics interfered with the selections and some recent financial histories rely upon his accusation. This essay examines the committee's selection decision. Probability choice models are estimated for the 37 cities requesting consideration for selection on proxies obtained from ex-post statements by parties involved in the selection. The results confirm that reserve cities were selected systematically upon information claimed by the committee. Proxies for political and personal influence fail to improve the predictive ability of the estimated models.

Today, an examination of the map of the Federal Reserve System shows two Federal Reserve banks in Missouri, many Reserve banks located along the eastern coast, and one Reserve bank on the West coast. The map drawn at the opening of the System in 1914 appears little different today. Given the institutional importance of the Federal Reserve System that the System fails to adjust locations to changes in population or production is an interesting observation.

How did the Reserve Board Organization Committee decide to locate Reserve banks in 1914? The committee could have established some principles on which to derive selection guidelines. Instead, politics was alleged to influence the selection process. For instance, Congressman Sloan condemned the committee's decision:

> "... the requests of banks have not controlled. I have cited instances here where the size of the cities did not control. I am talking about a case where the lines of transportation and the course of business did not control. I have cited instances where the area does not control, instances where population did not control, where banking preferences did not control. *I do not know a single basis or rule that controlled in one case which has controlled in another.* The gentleman (Carter Glass) will pardon us if we mildly say that politics must have had something to do with it, that there may have been favoritism shown. This is worse than politics, because politics carries with it party responsibility, while favoritism never appears twice the same way and no responsibility attaches" (Italics added, *Congressional Record* 1914, 51: 6441).

To Sloan, the decisions of the committee were the result of not systematic rules but arbitrary choices of politically favored cities and regions.

H. Parker Willis, the selection committee's hired academic expert, concurred that politics played a role in the selection of locations in his massive 1923 history on the founding of the Federal Reserve System (562-563). As a consequence, some recent financial histories agree that politics was a factor of selection. For example, West observes that key persons involved in the legislation and the selection decisions had the position as a basis to influence some of the selections. West hypothesizes that such influences favored the selection of some cities in favor of others (1977, 210-211). Primm lists as relevant factors, "some banking reality and a lot of politics" (1989, 44-45).

However, new work suggests objective bases for the selection of some of the controversial locations are evident as well as questions Willis' motivation for his 1923 allegation. Odell and Weiman (1998) find that committee members likely evaluated the future prospects of cities as banking centers and recognized that the inland distribution centers were ascendant to older seaports. Recent research on the 1914 districting and location decisions reveals Willis wrote during the winter of 1915 that he had recommended that the committee select nine or ten, and, in contradiction again, all 12 locations in his unpublished 1914 "Report on Districting." Willis claimed that eight or nine selections would have been preferable in 1923 (Hammes 2001, endnote 13).

Which factors perform better at predicting selection: those factors defined by Willis in his published history, political characteristics, or instead those by the committee in its published public documents? The effects of political characteristics in the two frameworks are included and considered. If politics was important, then a model of political variables or one that incorporates political variables should perform better than the non-political specification. The evidence to be presented finds that the committee selected the correct cities on economic factors and did not behave politically. Political characteristics of these cities are considered and they fail to improve the predicted results. The conclusion drawn here is similar to that of Odell and Weiman (1998): the committee did not behave in a partisan manner.

THE SECTION AND CONTROVERSY OF THE RESERVE BANK LOCATIONS

The Reserve Board Organization Committee organized the Federal Reserve System during 1914 after President Wilson signed the Federal Reserve Act on December 23, 1913. The committee consisted of the Secretary of the Treasury, William G. McAdoo,

the Secretary of Agriculture, David Houston, and the Comptroller of the Currency, John Skelton Williams. The Federal Reserve Act provided for the selection of eight to 12 cities for the locations of regional reserve banks to be administered by the Federal Reserve Board located at Washington D.C. 37 cities requested consideration as a location for a Reserve bank by the committee. This paper does not address the issue of the correct number of reserve cities, perhaps a political decision. The committee decided to select all 12 districts and locations (Houston 1926, 1:108).

On April 2, 1914, the Reserve Board Organization Committee announced that Boston, New York City, Philadelphia, Cleveland, Richmond, Atlanta, Chicago, St. Louis, Minneapolis, Kansas City, Dallas and San Francisco were the locations for a Federal Reserve Bank. A short statement that accompanied the committee's decision implied that the relevant factors had been considered in determining the best location for Reserve cities without explanation. The selections of Cleveland instead of Cincinnati or Pittsburgh, Richmond and not Baltimore or Washington D. C., Atlanta and Dallas rather than New Orleans, and Kansas City instead of Denver or Omaha brought criticism and charges of political interference by the Congress and press, among others (*Congressional Record* 1914, 51: 6227-6228, Primm 1989, 46-48, Willis 1923, 587). The committee issued an explanatory statement on April 10, 1914, but the selection decision continued to be tainted with charges of politics.

What regions or cities should have received Federal Reserve Banks? In his 1914 "Report," Willis recommended that if the committee were not to locate a bank within Texas, the committee should select Atlanta, Boston, Chicago, Cincinnati, Cleveland, Kansas City, Missouri, Minneapolis, New York, Portland, Oregon, Philadelphia, and St. Louis (Hammes 2001). Rather than select Portland or Cincinnati, the committee placed Reserve banks at Dallas in a Texas district and at Richmond.

GROUPS OF ATTRIBUTES AFFECTING SELECTION OF CITIES

A baseline sample consists of the 37 cities that requested consideration by the committee for the location of a Federal Reserve Bank. A reduced sample of 21 cites, the considered sample, evaluates cities considered on the margin, or the cities in the baseline sample less obvious choices (Boston, Chicago, Minneapolis, New York, Philadelphia, San Francisco and St. Louis) and obvious rejects (cities that were not among the 29 suggested cities as compiled from the Comptroller's poll of the 7471 National banks). In the reduced sample, the committee chooses five cities from these remaining 21.

Groups of attributes for economic, development, and political factors are used to estimate the odds of selection via maximum likelihood estimation of the logistic function and obtain predicted probabilities of-selection. Many of the variables are measured at the state level since city level data are difficult to locate for this time period; however, the organization committee had stated its intent to closely follow state lines for defining the limits of the reserve districts (Reserve Board April 2, 1914, 135).

Willis proxies.

Willis prepared a preliminary report for the committee in which he debated several principles on which recommended factors could be provided (1923, 585). Willis listed five categories for successful reserve cities and their proposed districts: present and prospective commercial importance, financial importance, business habits, railway facilities, and total bank capital (566). Willis believed that the most important characteristic in the selection of a reserve city was the quality of communication and rail facilities (Willis 1937, 93-4).

City population and the proportion of the state population that is urban suggest commercial importance. Urban areas also relied upon on bank checks more than currency. Larger city populations in 1910, and the proportion of a city's home state population that was urban in 1910, are expected to increase the odds of selection.

Banking variables used include the deposits and number of National banks in the city on March 4, 1914, and indicated the local use and thus support of the National Banks, the banks that would be required to join the system. With respect to predicted selection, the greater the value of deposits is expected to increase the city's probability of selection. Greater numbers of National banks at a location is expected to have a positive impact on the probability of selection.

Federal Reserve Banks required rail and telegraphic services to communicate with the member banks within its district, other reserve banks and the Federal Reserve Board, as well as to receive clearing services and currency shipments. The ratio of total railroad mileage to the product of state population and state land area in the city's home state, RAIL, is expected to have a positive impact for the odds of selection.

In addition, an economic variable for banking market structure controls for states that permitted state incorporated banks to open branch banks. State chartered banks permitted to open branches might be preferred to National charters by local bank organizers, so fewer banks might organize as National Banks in those states. As a result, states that permitted branching would have less representation, support and investment in the National Banking System. A dummy variable takes the value one if the home state of the city permitted branch banking to some extent, and zero if otherwise as recorded in Samuel Weldon's 1909 summary of state banking laws (Bradford 1940, 13). The committee is expected to select cities in branch banking states less often.

Political influence proxies.

A set of political attributes controls for direct influence on and by the selection committee. If committee members behaved with favoritism for their home states there would be a positive impact on the estimated odds of selection. Houston went to Washington from his position at Washington University in St. Louis, McAdoo was raised in Georgia, and Williams was a Richmond banker and capitalist. A dummy variable takes the value one if the city's home state is Georgia, Missouri or Virginia, and the value zero if otherwise. Overall political power of the Democrats within particular states may also have affected the decision. If politics mattered, the numbers of Democratic Representatives and Senators from the city's home state and are expected to have a positive impact on the odds of selection.

Committee proxies.

When announcing its selections, the selection committee stressed that it had considered the characteristics of total bank capital, business habits, railway facilities, and population (Reserve Board April 2, 1914, 135). In defending its controversial selections, the committee compared recent development of a location's National banking infrastructure. In addition, the committee emphasized that the polling results of 7471 National banks preferences for Reserve bank locations (Reserve Board April 10, 1914, 147-153).

As in the Willis specification, city population in 1910, the proportion of the home state population classified as urban in 1910, and the railway ratio are expected to impact positively the odds of selection. Banking attributes measure the average annual rate of change between the fall seasons of 1904 and 1914 for individual deposits and capital. Capital is measured as paid-in capital, surplus and profit and loss. These measures are expected to have a positive impact on the estimated odds of selection. Another attribute the committee emphasized was support for locations of reserve banks as provided by National banks. The number of votes a city received as the preferred first choice for a Reserve bank is expected to increase the probability the city is selected.

ESTIMATION OF PROBABILITY CHOICE MODELS AND PREDICTION OF SELECTIONS

Estimated logit models are evaluated by their ability to predict the actual outcomes. Abbreviated results are presented in Tables 1 and 2. As usual for logit estimations, predicted probabilities of selection that exceed 0.5 are predicted selections, and those less than 0.5 are predicted non-selections. The correct outcomes are the predicted actual selections and the predicted actual non-selections. Incorrect outcomes are false predicted selections and non-selections not actually observed. False predicted selections are those cities predicted by the model for selection but not actually selected. False predicted non-selections are predicted non-selections although actual selections.

For the baseline sample, the estimated Willis model, Table 1 (1), is not statistically significant and correctly predicts the selection for eight of 12 of the reserve cities. When the sample is reduced to 21 cities, Table 2 (5), the Willis model lacks statistical significance again.

The political models estimated perform most poorly among all the models estimated; however, the estimated models of the full and reduced samples, Tables 1 (2) and 2 (6), are weakly statistically significant at the 10 percent level. Overall, Democratic Congressional representation and possible selection committee bias correctly predict the selection of eight of 12 cities in the baseline sample and two of five in the reduced sample. Among the controversial selections, Dallas is not predicted for selection in the baseline sample, and Atlanta, Cleveland and Richmond are not predicted selections in the reduced sample. Democratic representation may influence selection overall, but performs poorly as a predictor if considering the cities on the margin for selection.

When models for selection are estimated on the Willis and political variables jointly for the full and reduced samples, Tables 1 (3) and 2 (7), the estimations are significant at least at the five percent level, and the directions are all as expected. Atlanta, Kansas City and Richmond fail to be predicted as selections in the baseline sample, but only Atlanta is not a predicted selection in the reduced sample. This model performs better in the reduced sample among the controversial selections than either the Willis or political models alone.

By contrast, the attributes of the selection committee models estimated, Tables 1 (4) and 2 (8), are individually and jointly significant at least at the 10 percent level or better. All the cities selected are correctly predicted for selection in the baseline sample and only Cleveland is not predicted for selection in the reduced sample. Overall, the results confirm the selection committee's contention that it based selection on systematic guidelines and not on arbitrary judgments. The greatest difficulty with this interpretation is that committee statements regarding selection criteria are available only *ex-post* and made public in an environment of political acrimony. Yet, the committee's systematic rules do explain jointly the entire selection decision based upon the statements without relying upon political factors.

CONCLUDING REMARKS

The factors in the committee's *ex-post* published documents best predict the probabilities of selection. Political representation in the Congress and other influence proxies are poor predictors of selection. Factors suggested by the committee's hired expert appear ignored by the selection committee, an observation that Willis concurred with in his history of the establishment of the Federal Reserve System (1923). Increased private investments in the National banking system, or greater local reliance in the National banking infrastructure in the immediate decade prior to the establishment of the Federal Reserve System was rewarded by the selection committee. The selection committee is shown to have behaved impartially and not arbitrarily.

REFERENCES

Bradford, F. A. (1940), *The Legal Status of Branch Banking in the United States*. New York: American Economists Council for the Study of Branch Banking.

Comptroller of the Currency (Various years), *Annual Report*. Washington: GPO.

Congressional Record Containing the Proceedings and Debates of the Sixty-third Congress, Second Session (1914), 51. Washington.

Hammes, D. (2001), "Locating Federal Reserve Districts and Headquarters Cities." *The Region*, Federal Reserve Bank of Minneapolis, http://minneapolisfed.org/pubs/region/01-09/hammes.html.

Houston, D. F. (1926), *Eight Years with Wilson's Cabinet, 1913 to 1920, With a Personal Estimate of the President*. Garden City, New York: Doubleday, Page.

Odell, K. A. and D. F. Weiman (1998), "Metropolitan Development, Regional Financial Centers, and the Founding of the Fed in the Lower South." *Journal of Economic History* **58**, 103-125.

Primm, J. N. (1989), *A Foregone Conclusion: The Founding of the Federal reserve Bank of St. Louis*. St Louis: Federal Reserve Bank of St. Louis.

Reserve Board Organization Committee (April 2, 1914), *Decision of the Federal Bank Organization Committee Determining the Federal Reserve Districts and the Location of Federal Reserve Banks Under Federal Reserve Act Approved December 23, 1913*. In Comptroller (1915), 1: 135-146.

_____ (April 10, 1914), *Statement of the Reserve Board Organization Committee Relating to the Decision of April 2, 1914*. In Comptroller (1915), 1: 147-153.

United States Senate (1913), *Hearings before the Committee on Banking and Currency United States Senate*. Washington: GPO.

_____ (1914), "Vote for Reserve-bank Cities." *Location of Reserve Districts in the United States,* Senate Document No. 485, 63rd Cong., 2nd Session. 347-357.

Willis, H. P. (1923), *The Federal Reserve System, Legislation, Organization and Operation*. New York: The Ronald Press Company.

Willis, P. B. (1937), *The Federal Reserve Bank of San Francisco, A Study in American Central Banking*. Morningside Heights, New York: Columbia University Press.

TABLE 1
Estimated Logistic Models, Full Sample

	Willis	Political	Willis & Political	Committee
	(1)	(2)	(3)	(4)
Wald	9.99	7.72[c]	17.22[b]	16.04[b]
Correct Selections	8/12	8/12	8/12	12/12
Incorrectly Predicted as Not Selected	Atlanta Dallas Kansas City Richmond	Boston Dallas Mpls SF	Atlanta Kansas City Minneapolis Richmond	
Incorrectly Predicted as Selected	Pittsburgh	Cincinnati Columbus Pittsburgh Savannah	Birmingham Pittsburgh	Pittsburgh Portland
Correctly Classified	86.49%	78.38%	83.78%	94.59%

Note: [a] indicates statistical significance at least at the 1% level; [b], at the 5% level; and [c], at the 10% level.

TABLE 2
Estimated Logistic Models, Considered Sample

	Willis	Political	Willis & Political	Committee
	(5)	(6)	(7)	(8)
Wald	4.48	5.55[c]	21.15[a]	12.66[b]
Correct Selections	2/5	2/5	4/5	4/5
Incorrectly Predicted as Not Selected	Atlanta Kansas City Richmond Louisville	Atlanta Cleveland Richmond Fort Worth	Atlanta	Cleveland
Incorrectly Predicted as Selected		Houston	Houston New Orleans	Baltimore
Correctly Classified	80.95%	76.19%	85.71%	90.48%

Note: See Table 1.

"Building a Money Market: The Case of Bankers' Acceptances, 1914-1939," J. Peter Ferderer (Macalester College)

> The importance of the Money Market does not lie in its size, because the total amount of funds in the market is small in comparison with the total banking funds of the country. The importance lies rather in the liquidity of the market, it its capacity for furnishing cash at a few hours' notice.
> -- W. Randolph Burgess, 1926

I. Introduction

Unlike Europe, the United States did not have a market for bankers' acceptances at the start of the twentieth century. Rather, the U.S. market was given birth by the Federal Reserve Act of 1914, which allowed the new Federal Reserve Banks to rediscount acceptances. By giving the Reserve Banks the power to rediscount, it was hoped that the "discount market" would become more liquid and bankers' acceptances would be an attractive investment vehicle.

The framers of the Federal Reserve Act were motivated by three broad goals. First, they wanted to build a market for bankers' acceptances so that the U.S. could become a banker to the world. Second, a U.S. market for bankers' acceptances would stimulate foreign trade by lowering the costs of financing imports and exports. Finally, with the financial turmoil of 1907 fresh in their minds the framers believed that a more liquid money market would act as a "prophylactic against panics."

This paper explores the development of the discount market for bankers' acceptances between 1914 and 1934. We address three questions. First, did the liquidity of the discount market rise over the first twenty years? Second, what were the institutional and cyclical factors that influenced liquidity? Third, was liquidity priced? That is, was the cost of acceptances financing affected by the willingness of dealers to "make the market"?

II. Overview of the Market

Figure 1 shows the volume of bankers' acceptances outstanding on a monthly basis between 1917 and 1933. Figure 2 classifies bankers' acceptances by the different transactions they financed.

Growth of bankers' acceptances was fostered by several liberalizations in the "rules of the game" that governed the market. The original Federal Reserve Act only granted permission for National Banks to create acceptances used to finance the importation and exportation of goods. As Figure 2 shows, the majority of bankers' acceptances were used for this purpose even as late as 1924. By 1917 the Act had been amended to allow banks to create acceptances for domestic shipment of goods and holding marketable stables in inventories in the United States.

Development of the acceptance market was further promoted in 1919 when the Federal Reserve Board approved an amendment making it possible for member banks to accept up to 100 percent of their capital. In 1921 the Board permitted Federal Reserve banks to purchase six-month import and export bills. In 1926, the American Acceptance Council helped get legislation passed in New York State that made acceptances a legal investment for life insurance companies.

In the early 1920s when banks were allowed to create acceptances to finance imports and exports between foreign countries and storage of readily marketable staples in foreign countries. The first recorded use of American bankers' acceptances for this

steps beginning February 3, 1928. Both the level and volatility of the call renewal rate rose dramatically, though volatility did not approach levels reached in 1920. Liquidity in the long end of the bill market fell in response. Spreads on 120- and 180-day bills remained at ¼ of one point between December 14, 1928 and August 30, 1929. By contrast, spreads for the 30- and 90-day bills rose to ¼ only briefly in 1928 and 1929.

Another shock hit the bill market in the fall of 1931 when the Federal Reserve raised its discount rate to prevent capital outflows following Britain's departure from the gold standard. Once again liquidity in the market fell:

> Very Seldom does the bill market come to the standstill condition of the past two weeks... Heretofore there has been an ebb and flow of bills that has kept the wheels moving but on this occasion nearly all the bills are in the Federal Reserve Banks and as the Federal buys from but never sells to the market the total bill volume is therefore locked up to be held to maturity.

Spreads on 120- and 180-day bills remained at ¼ of one point between October 16, 1931 and January 8, 1932. In contrast, the increase in bid-ask spreads for 30- and 90-day bills was temporary, only rising to ¼ for one week in October. It appears that the short end of the discount market had become much more resilient and less susceptible to liquidity withdrawals by the early 1930s.

III.B. Econometric Evidence

Table 1 provides results from regressions used to explain variation in bid-ask spreads. The explanatory variables are the level of the call loan rate and the conditional standard deviation of call rate changes. Unfortunately, we do not have weekly data on trading volume. However, the variation in acceptances outstanding over the year suggests that trading volume follows a seasonal pattern. Thus we test for volume effects by including dummy variables for the months of January through November in the regressions. The month of December, which typically has the highest level of acceptances outstanding (and presumably the greatest trading volume), is reflected in the constant term.

The results in Table 1 suggest that the call rate exhibits a weak relationship with the spreads. Only in the case of the 30-day bill is the call rate coefficient positive and significantly different from zero at the five percent level. In contrast, the volatility of the call rate has a positive and significant relationship for five of the six spreads. Also, note the size of the coefficients for the long-term bills are three to four times the size of those for short-term bills. These findings suggest that increases in return volatility created greater risk for dealers making markets in bills with more than 90 days to maturity. Finally, the month dummies, though typically negative, are insignificant in models for 30-, 60- and 90-day bills. In contrast, coefficients on month dummies are negative and significantly different from zero more frequently in the regressions for the longer-term acceptances. The months with the largest negative coefficients are January (120-day bills), August (150-day), and April (180-day bills). These findings are consistent with the hypothesis that liquidity in the long end of the discount market was influenced by seasonal variations in trading volume.

To the extent that the discount market was less developed in its early years (i.e., dealer wealth was low and they were less able or willing to bear risk), we should see return volatility exert a more powerful influence on bid-ask spreads during this time period. To test this hypothesis, Table 2 reports bid-ask spread regressions for 30-, 60- and 90-day acceptances estimated over two sample periods: February 1919 to December 1926 and January 1927 to November 1934. The results clearly show that changes in return risk affected spreads in a more powerful manner in the early sample period. This finding suggests that the bill market became deeper and more resilient over time.

The final issue we address is whether liquidity was priced. That is, did changes in liquidity of the bill market affect the level of the bill rate? This is an important question. There is a great deal of evidence from cross-country studies that economic growth and financial development are highly correlated (see Levine (1996)). However, it is very difficult to determine the direction of causality. If exogenous forces (i.e., the purposeful activities of the Federal Reserve and the American Acceptance Council) caused bill market liquidity to rise over time and greater liquidity lead to lower costs of financing production and trade, then there is evidence that causality runs from financial development to economic growth.

To examine the extent to which liquidity is priced, we estimated regressions for weekly changes in the bill rate on: a) the level of the bill rate lagged one week, BA(T-1); b) the weekly change in the discount rate set by the Federal Reserve, DISRATED; c) the bid-ask spread, SPREAD; and d) dummy variables for January through November. If bill rates are mean reverting, the coefficient on the lagged bill rate level should be negative. Changes in the Federal Reserve's discount rate should have a positive influence on the market rate for obvious reasons.

The results are reported in Table 3. The coefficient on the lagged bill rate is negative in each of the six regressions, but is marginally significant. Thus there is some evidence of mean-reversion. As expected the discount rate set by the Federal Reserve has a positive and highly significant impact on the bill rate. However, note that coefficients are well below one suggesting that other forces are influencing the bill rate. The coefficients on the month dummies are generally negative with the coefficient on the April dummy often taking on the largest negative value. Thus there does appear to be some seasonal variation in the bill rate. Most importantly, the bid-ask spread has a positive and statistically significant impact on the bill rate for maturities of 90 days and less. While the coefficients on the spread in regressions for longer-term rates are positive they are not significant. Thus there is somewhat mixed evidence regarding the link between liquidity and interest rates.

IV. Conclusion

The market for bankers' acceptances grew dramatically between 1914 and 1929 partly because the Federal Reserve Board liberalized the "rules of the game" that governed market activity. The Federal Reserve also acted as the market maker of last resort by rediscounting paper and this activity reduced the risk borne by private dealers and induced them to make the market.

The American Acceptance Council also played a key role by publishing the monthly Acceptance Bulletin, a vehicle used to educate banks and investors on the benefits of bankers' acceptances. The editorial pages of the Bulletin appealed for banks to be guided by more than self-interest: "There can be no question of the duty of the big banks in the

large centers... nevertheless, their [smaller banks'] duty as well as their profit lies in the direction of carrying as a secondary reserve acceptances of American banks." Clearly, there was a sense that private profit motives would not suffice to overcome the coordination problem that exists when market participation and liquidity drive one another (see Pagano (1989)).

In the end, these efforts paid off. By the late 1920s the size of the market had grown dramatically and the United States was becoming a banker to the world. Increasing liquidity of the discount market played an important role in this process.

Data Appendix

1. Bankers' Acceptances Outstanding (end-of-December from 1917 to 1919, end-of-April and end-of-December from 1920 to 1924, and end-of-month from 1925 to 1933). Source: Facts and Figures Relating to the American Money Market (1931) and various issues of the Acceptance Bulletin (1931-1933). Data not seasonally adjusted.
2. Bankers' Acceptances Bought in the Open Market by Federal Reserve Banks (Wednesday figures for each week from 1919 to 1933). Source: Banking and Monetary Statistics (1946), Table No. 103. Data not seasonally adjusted.
3. Classification of Bankers Acceptances Outstanding (end-of-month from 1925-1933). Source: Facts and Figures Relating to the American Money Market (1925-1930) and various issues of the Acceptance Bulletin (1931-1933). Data not seasonally adjusted.
4. Bid and ask rates for Bankers' Acceptances (closing rates reported by dealers on various days of the week from 1917 to 1923 and Fridays from 1924-1934). Source: various issues of the Acceptance Bulletin.
5. Interest Rate on Stock Exchange Renewal Call Loans in New York City (weekly average of daily renewal rates from 1919-1934). Source: Banking and Monetary Statistics (1946), Table No. 121. Data not seasonally adjusted.

References

The American Acceptance Council. Acceptance Bulletin, various issues 1919-34.

The American Acceptance Council. Facts and Figures Relating to the American Money Market, New York: The American Acceptance Council, 1931.

Amihud, Yakov and Haim Mendelson. "Asset Pricing and the Bid-Ask Spread," Journal of Financial Economics 17 (1986), 223-249.

Amihud, Yakov and Haim Mendelson. "Liquidity, Maturity, and the Yields on U.S. Treasury Securities," The Journal of Finance XLVI (Sept. 1991), 1411-26.

Burgess, W. Randolph. The Reserve Banks and the Money Market, New York and London: Harper & Brothers Publishers, 1946.

Demsetz, Harold. "The Cost of Transacting," Quarterly Journal of Economics (1968), 33-53.

Fry, Morton. "The Future of the Acceptance Market," Acceptance Bulletin (Nov. 1922), p. 9.

Hackley, Howard H. Lending Functions of the Federal Reserve Banks: A History, Washington: Board of Governors of the Federal Reserve System, 1973.

Jacobs, Lawrence Merton. Bank Acceptances, Washington: Government Printing Office, 1910.

Kamara, A. "Liquidity, Taxes, and Short-Term Treasury Yields," Journal of Financial and Quantitative Analysis 29(3) (1994), 403-17.

Levine, Ross. "Financial Development and Economic Growth: View and Agenda," Journal of Economic Literature XXXV (June 1997), 688-726.

Melton, William C. and Jean M. Mahr. "Bankers' Acceptances," Federal Reserve Bank of New York Quarterly Review (Summer 1981), 39-55.

O'Hara, Maureen. Market Microstructure Theory, Malden Mass.:Blackwell Publishers Inc., 1997.

Pagano, Marco. "Endogenous Market Thinnes and Stock Price Volatility," Review of Economic Studies 56 (1989), 269-288.

Shen, Pu. "Bid-Ask Spreads: A Theoretical Model with Tests on the Treasury Bill Market," Manuscript, Federal Reserve Bank of Kansas City, (July 1993).

Shen, Pu and Ross Starr. "Liquidity of the Treasury Bill Market and the Term Structure of Interest Rates," Journal of Economics and Business 50 (1998), 401-417.

Stoll, Hans R. "The Theory of Dealer Services in Securities Markets," The Journal of Finance 33(4) (September 1978), 1133-1151.

Thralls, Jerome. "The American Discount Market," Acceptance Bulletin (February 1925), 5-8.

Tinic, Seha M. "The Economics of Liquidity Services," Quarterly Journal of Economics (1972), 79-93.

Buckaroos in paradise