

The Cliometric Society

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International and Interdisciplinary Perspectives on German Economic and Social History: The First German Cliometrics Conference

by Scott Eddie and Rob Gray, University of Toronto

(Toronto) Peter Temin (MIT) opened the first German cliometrics conference with a keynote address on "Culture and Economic Growth in Germany, Japan, and New England" at the Goethe Institut. Temin emphasized the role of culture in determining both rates and types of investment. He argued that there was a continuum of cultures from the individual-oriented culture of the United States to the community-oriented culture of Japan, with Germany in the middle, tending more toward the Japanese end. He argued that these cultures resulted in different modes of behavior: people in the US act more instrumentally, while those in Japan and Germany act more according to custom. Constraints on actions are furnished more often by custom in Germany and Japan than in the US, which relies more on explicit laws. Customs are efficient in the short run but can pose problems in the long run when change is needed. Temin closed by speculating that some of the current economic troubles of Germany and Japan may stem from their inability to change customary behavior with rapidity.

The academic sessions took place at the Centre for International Studies, University of Toronto, opening with a welcome from Michael Marrus, Dean of the School of Graduate Studies. John Komlos (Munich) followed with an overview of the state of cliometrics in Germany. He had long ago been puzzled by the failure of cliometrics to gain many converts in Germany, despite the strong position of economic history in German universities and the friendly reception of anthropometric

history on the part of his German colleagues. Richard Tilly, who obtained a chair in social and economic history at Münster in the 1960s, had in 1968 delineated several obstacles to the progress of cliometrics in the German environment. Komlos finds these obstacles still in force: the predominance of economic history in history rather than economics faculties, the traumatic intellectual legacy of the 1930s, as well as path-dependent results of the German intellectual tradition. Komlos expanded the list by referring to the small size of economics faculties, the weakness of the econometric tradition in Germany, institutional structures which reduce incentives to produce new ideas and largely eliminate competition for graduate students, and the *Habilitation* (effectively a second dissertation) that is required of anyone taking up a Professorship. Since cliometrics, with its stress on theories and counterfactuals, is alien to the German historical tradition, it is likely to remain on the Central European intellectual periphery; that is, most cliometric work on Germany would continue to be undertaken elsewhere. Komlos suggested that cliometricians could broaden their appeal by incorporating new types of analysis from economics and other fields, and by exploiting funding opportunities from German sources. German funding agencies are willing to support

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Executive Director's Notes

Letter to Members

In September 1983, after 25 years of informal meetings, D. McCloskey and I sent a letter to interested parties announcing that, "As the time seems ripe to bring the field together more, a number of historical economists have proposed the formation of The Cliometrics Society." (We dropped the "s" years later.) We went on to say that "the Society would in no way compete with national societies or associations and thus the Society will have no officers or a journal, but maintain a mailing list and distribute a Newsletter." We also announced the First World Congress of Cliometrics, in May 1985 at Northwestern.

Following the Congress, we published our first *Newsletter*. It covered both sides of one sheet of paper and included a Congress report and a offer to become a member of the Society. (Membership was \$3; joint membership and *EEH* subscription was \$34.) In 1989, John Lyons joined me as Associate Editor of the *Newsletter*, and we recruited Lou Cain to join our team in 1991. The following year Debbie Morner became the administrator of the Society, and the four of us have been editing and publishing the *Newsletter* ever since. Anyone who is familiar with my editing skills knows the debt I owe to John, Lou and Debbie.

Over the last 14 years, the Society has grown into a well-established organization and added many services. Yet, I think we have maintained our original goal to promote communication among and about Cliometricians. I am pleased that the Trustees have decided to continue the organization, but I feel it is time someone else took over. During my time as the executive officer, we were pioneers in using the internet to help the field. In 1993, we started a gopher called Clionet and a list called Econhist. These services have continued to grow and EH.Net became a separate organization in 1996. As EH.Net has grown, it has occupied more and more of my time. I do not plan to fade away, but I plan to concentrate my energy on making EH.Net an even better service to the profession.

There is so much exciting new Cliometric work being done around the world, I am sure the new Executive Director and Editor will keep us enlightened.

The plan is that the new officers (Executive Director and *Newsletter* Editor) will take over at the start of the new year. I hope that your membership renewal letter will come from a new office in January. My last official act will be organizing our Fourth World Congress in July.

Further, it is with great regret that I must announce that Debbie is leaving our office to take another position at Miami University. I am sure all of you who have dealt with Debbie over the past eight years share with me a sense of loss. Debbie has been a great resource for our profession and a friend to many economic historians worldwide. We wish her success in her new position.

Finally, I want to say that I am proud to be a member of Clio Clan. We have been able to do serious work and not take ourselves too seriously. We are a group that has been able to stay fresh by always listening to what someone is saying instead of being concerned about who is talking.

Report on The Cliometric Society Trustees Meeting

The annual Trustees meeting was held Friday, October 8, during the Economic History Association meetings in Baltimore. Trustees discussed the following items of business:

The Future

After extensive discussion, it was decided that The Cliometric Society will continue, and, for the time being, the *Newsletter* will continue as a hard copy publication.

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THE CLIOMETRIC SOCIETY

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AN INTERVIEW WITH PETER TEMIN

Editors' note: Peter Temin is the Elisha Gray II Professor of Economics at the Massachusetts Institute of Technology, where he has taught since 1965. He is a Research Associate of the National Bureau of Economic Research and has been a Visiting Fellow at the Charles Warren Center for Studies in American History at Harvard University (1976-1977), and Pitt Professor of American History and Institutions at the University of Cambridge (1985-1986). He was president of the Economic History Association in 1995-1996. Our interview took place in his office at the Sloan School, MIT, on October 5, 1999, and was conducted by John Brown (Clark University), who writes:

I first became acquainted with Peter Temin's work as a graduate student at the University of Michigan, when we members of Gavin Wright's seminar in American economic history wrestled with the controversy over labor scarcity. Immediately thereafter, Peter reappeared in our discussions as a major protagonist in the battle over the "Soundness School" interpretation of the Panic of 1837. He contested the long-held view that Jackson's mistakes – his veto of legislation re-authorizing the Second Bank of the United States in 1832 and then his issuance of the specie circular in 1836 – prompted the panic and the subsequent economic downturn. We encountered Peter Temin a few weeks later as we worked our way through the debate over antebellum slavery and yet again in what at that time was a bold attack on the orthodoxy of the Friedman-Schwartz perspective on what caused the Great Depression. Peter was also at a conference on the performance of the Victorian economy that I attended in the third year or so of graduate school, so that it was clear to me that his interests extended across the Atlantic. Moving east brought me gainful employment as well as the opportunity to attend the Economic History Workshop at Harvard. There, my appreciation for Peter's approach to economic history grew in the give-and-take of the seminar, as he probed visitors on the logic and, just as important, on the quality of evidence they offered in support of their arguments. For these and other reasons (particularly given his research on the cotton textile industry and the European economy), I was pleased to have the opportunity to interview him.

You started your career with research focusing on the American iron and steel industry, expanding to include the diffusion of steam power, banking policy, and the macroeconomic history of the Jacksonian economy. You later made an important contribution to the debate over slavery. What prompted you to go into economic history and, in particular, the study of iron and steel?

I had been interested in history before graduate school; the influence that led me into economic history was Alexander Gerschenkron. I was a student at MIT in the era of Walt Rostow, who was teaching his book, *Stages of*



Economic Growth. He wasn't doing a very good job, probably because his mind was half in Washington – this was in 1959. Gerschenkron came through and gave a smashing seminar. I went up to Harvard and took his course in the spring term. I was totally captivated by him. I wrote a paper for him, I got more interested in the subject, and he put me in contact – actually, he got me an office – with Paul David and Al Fishlow at Harvard, in the original Economic History Workshop on Linden Street in Harvard Square. The stimulation that I got from Alex, first of all, and then from Paul and Al, was just incredible and very attractive, so I started working. I was choosing at that time between econometrics and economic history, and this led me into economic history. I don't recall how I got interested in the steel industry, but my thesis was an attempt to write a narrative about how an important industry developed. I remember very much the process of writing it and not at all the process of beginning.

This was the period, '62 to '65, when you were a Junior Fellow at Harvard. As Junior Fellow, did you have some teaching responsibilities as well?

No, it was just a chance to work on my dissertation, which was largely finished in the first year. I turned it into publishable form and then began to read the literature rather more seriously. That's how I got into some of the subsequent projects. The Jacksonian Economy was stimulated by reading Doug North's book on the antebellum period. He was giving the rather familiar cotton-cycle story of the 1830s, and it just didn't seem to have the right tone. I tell my students today, when you read something and it seems wrong, little bells go off, and you should try to figure out what happened.

What was it that "went off"? Was it perhaps intuition you had developed from course work you had done?

North argued that there were diminishing returns to cotton agriculture that led to the rise in the price of cotton in the 1830s. I did not believe that there were diminishing returns at that time or that the price rise was the result of supply factors. So I began to look in a totally different direction, which then led me into the macro area. I benefited a lot from the work of van Fenstermaker in collecting banking data. There was a lot of data available, and I was able to put together a different story. One of my disappointments in economic history is demonstrated by the questionnaire that was sent around a couple of years ago, saying "What is your view of the Jacksonian economy?" Only about half the people seemed to have come around to this new view, which I think is now a very old view, and which I would have thought would have become a standard view. I don't understand why the previous view continues to be held, or what I could have done then, what I could do now, to make this story more convincing.

That is, the old view stemming from North's focus on the cotton economy as well as the Soundness School?

The old view was that Jackson was responsible for what happened. It was loose banking rather than increase in the specie supply that led to inflation. That is not what happened.

Your 1966 paper on labor scarcity opened up another line of research and discussion quite unlike the macro-economic analysis of the Panic of 1837 or your monograph on the iron and steel industries. It places the labor scarcity debate in the framework of a general equilibrium model, which must have set a precedent for the *Journal of Economic History*. It also is a classic of a cliometric approach to economic history.

The article about labor scarcity came out of doing two things at once. One was reading Habakkuk's classic book, and the other was taking a reading course from Paul Samuelson. In that course, Samuelson told me about a paper that he had written about the so-called non-substitution theory, when capital is embodied labor. If everything depends on time, then the wage rate doesn't matter, only the interest rate. I was taking the theory and the Habakkuk book and putting them together – and, of course, they didn't fit. And so I wrote that up. But the idea came from knowing the economic theory, and I recommend to all students that they learn a lot of economic theory, even if it seems highly irrelevant, because you cannot tell where an idea will come from. I made this very simple-minded model to demonstrate the point, which acquired a life of its own in the trade literature.

Your comments raise an issue that I would like to pursue. In the late 1950s – before your time – the New Economic History and the cliometric conferences were starting up. When did you get involved in cliometrics? Clearly, you knew Paul David and Albert Fishlow. Were you also actually involved in the conferences?

I was not at the first conference, but I was at several of the early conferences when they were in West Lafayette at Purdue University. And they were very exciting conferences. Everybody participated. People were very concerned about where the data came from, which was a very good lesson for a young cliometrician, and there was a lot of excitement in the work being done.

Was the excitement because a new generation was taking on older problems, equipped with economic theory and econometrics?

Yes, there was a sense that we had the tools and, since we had the tools, that we were in a sense messengers of the Messiah. Fogel, of course, was the most evangelical of all, but we were all swept up in the same spirit, that there was no nut that we couldn't crack with this particular intellectual nutcracker.

Did you have a sense of conflict with more traditional economic historians?

Yes, to go from one metaphor to another, we were St. George and the conventional historians were dragons, and we had to slay them. It was very definitely set up as a contest between Us and Them, very much a feeling of

Them and Us. Other people have talked about the problems with the *Journal of Economic History* and how it was going to bridge these cultures. But within the cliometrics meetings, there was no feeling that we had to bridge cultures; we were the True Believers.

Would you say that people were actually choosing fights or intellectual battles by taking particular old chestnuts of economic history and subjecting them to another kind of analysis?

I think that's a very fair description of what was happening, that people would look for received wisdom and then with a flourish be able to demonstrate with a little bit of economic theory, and some new data, and so on, that in fact things were absolutely different and with any luck, exactly the reverse.

So there's a certain rhetorical expectation in terms of the work.

Absolutely.

Could we pursue this? Was there any realization among cliometricians that this approach might actually involve some tradeoffs? Clearly there was a lot to be won by doing this, but, if you focus on particular debates, are you also writing a new history? Did you believe that you had to win all this new territory for cliometric/economic history, and that it might not be necessary to fill in the gaps of a historical narrative?

Well, that's a complicated issue, because most of the young cliometricians – and certainly Paul and Al and I – had appointments in economics departments. The audience that we were writing to, seeking to get tenure in our universities, was composed of economists. They wanted to see a clever use of economic theory – and then testing of economic hypotheses. The fact that this excluded – or refuted or even angered – traditional historians was just kind of a second-order issue. I think it's not really until we got a lot older and got tenure that we began to think about things, that we began to say, "Okay, it's true that these are good techniques, but they don't answer all questions," and to go back and look more at traditional history.

There's the paper on the Habakkuk hypothesis that appeared relatively early in the intellectual history of the cliometrics movement. It seems to me that it also sparked both new thinking about cliometrics, a debate with Robert Fogel, and subsequent iterations.

That wasn't much a debate with Fogel. I wrote the initial paper, and then everybody jumped up and down and wrote papers saying it couldn't be so, and then, how could each of them modify my model in a different way to show this. The debate got to be kind of shapeless. I made a restatement of the theory five years later, and have come back to it periodically. But it's not been a debate like the debates on *Time on the Cross*, where the same people lined up for quite a long time. Rather, it was that there was a paper that stimulated a lot of discussion, and then there were one or two iterations. I wouldn't think of this as a struggle between me and Bob, even though Bob wrote a paper opposing me. It was an isolated paper.

I guess I found it interesting because it was a kind of exchange that one would expect to see in an economics journal rather than in an economic-history journal. For the time, it must actually have been unusual. The paper plowed new ground by developing a straightforward, but, nonetheless, a general equilibrium model. Someone else then comes in and argues that there is a model misspecification issue here. That's a kind of discussion I wouldn't have expected to see.

I think in retrospect you're right; it's very much within an economics mold, and so it has that kind of quality. Since we were in economics departments, that was the kind of activity to do. And it wasn't just economic historians who got into this; Larry Summers wrote an article in this debate...[laughter]

I didn't realize that! [laughter]

Which I think may have been a term paper in Gerschenkron's course, but still it was part of the debate. Then the model got written up by Ron Jones who worked out its characteristics, its formal properties. The model went into the economics literature, and, in a way, you're right; it stood at the edge of history and economics and went both ways. It went into the trade literature where the model got worked out and also into the historical literature, as people thought about what caused industrialization in New England.

Yes, that's kind of an unusual case.

I think actually it is.

We've mentioned Fogel, which brings to mind the extended review essay of *Time on the Cross* that you and Paul David wrote. That appeared in the *Journal of*

Economic History; it's now found in the Whaples and Betts volume of readings. The essay prompts questions about the intellectual history of cliometrics, the approach followed in its critique of *Time on the Cross*, and the role it might have played in your own intellectual development. So there are a couple of questions I want to ask. First of all, I'm wondering what prompted you and Paul to get involved in a critique that went to such considerable depth; the detail is impressive. What really pulled you into this debate?

Part of Bob's genius is being able to write about hot-button issues, choosing the issues that get people excited. The issue about race and slavery in America is probably the biggest issue, and the one that has the most emotion attached to it in American history. I think we got swept into the debate for two reasons. One is that we got caught up in all of this emotion that Bob and Stan had stirred up, and, then, second, we were not convinced by the evidence that had been put forward. So partly this was a debate about the American past and an emotional issue, and partly it was a debate just like the debate over labor scarcity, which is, are you using the right model? Have you specified this properly? Have you taken account of other characteristics? It is methodological in the same way, and comes a decade after the labor scarcity debate, so it is that much more technical than the earlier one was. But I think that that controversy kind of began for me a slightly different exposition, or a different trend. In the early '60s I was part of the True Believers, very much thinking, with the economics model as my Excalibur that I would slay all dragons, could defeat all enemies ahead of me. As I began to think about slavery – and I was beginning to think about health economics which I was getting interested in at about that time – I began to think that maybe there were issues that couldn't be explained by straight economic models. I had to think a little more about – I would say now, think about culture, but I wouldn't have said then – think about culture. It's the beginning of the strand in my work that comes out in some of the business history and modes of economic behavior and more current stuff in my Presidential Address.

What did it mean for both of you to take on this kind of format: a 20-30 page review essay that offered a written critique in a published form? Was the strength of the critique at issue when you and Paul were putting it together?

There was a lot of emotion connected to this debate, and a lot of the issues were raised without ever being fully

settled. I think that's probably because there was so much emotion that people couldn't resolve them. But close reading of things has been a feature of my work, not just in that case but in other cases. I think it comes partly from the way I teach, which comes from my early training at Swarthmore as an undergraduate where we had a seminar system. We had just half a dozen or eight people together with a professor, and we discussed the work that we were doing. In my classes I still try and maintain this discussion quality, so a lot of the class – even the class that I just taught – involves looking at a paper, looking at the documentation, and saying, well, is this the right documentation? Have they actually proven their point? Have they done the tests carefully? And so on. It's a characteristic of the way that I have approached these fields, that we make incremental progress by building on the work of others, and that one of the characteristics that I've had is taking other people seriously. When they write something, I assume they mean it. I want to learn from it –

The foundation should be solid –

And if there's a problem with it, then I go on. For example, my work on the Industrial Revolution, which is in a sense very much like this – a critique of some other work – comes directly out of teaching. There are two views of the British Industrial Revolution, and I kept inviting students to write a term paper on this, because I thought that this was just the kind of thing that one could test. Since none of the students took me up on it, I did the test myself. Now it has come out, and there's getting to be a literature on it, too. But my paper came out of trying to teach this material, trying to take seriously what the people who have written on the subject have said, and trying to ask the students to think about it: does this make sense, is this consistent, have they proven their point? In the Fogel and Engerman case, I think we went into this – *Time on the Cross* was a major study, a lot of work with a lot of parts to it – and as we got into it, I think we had the sense that Bob and Stan had gotten convinced of their position and so, perhaps, had not always looked at the evidence as carefully as they should. Once we began to see their position as an ideological position, we began to look even more critically at the evidence that they had marshaled.

Your comment reminds me of a point you raised in the review essay about the methodology of economic history. You wrote that "the slant of their quantitative work (that is, Fogel and Engerman's) reflects the

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“One Kind Of Freedom Reconsidered” African American Economic Life in the Segregation Era

By Brian Alnutt, Lehigh University

(Bethlehem, Pa.) Even the recent visit of Hurricane Floyd could not dampen the enthusiasm of those present at “*One Kind of Freedom Reconsidered*”, an interdisciplinary conference focused upon “African-American economic life in the segregation era”, held at Lehigh University September 17-19, under the direction of Lehigh faculty members Anthony O’Brien (Economics), William Scott (African Studies) and William Shade (History). The conference was organized as both celebration and reconsideration of *One Kind of Freedom: The Economic Consequences of Emancipation*, the groundbreaking 1978 study by Roger Ransom and Richard Sutch.

Session I, chaired by Mary Washington (Lehigh), was opened by James Irwin (Central Michigan) and Anthony O’Brien with their paper “Economic Progress in the Postbellum South? Implications from the Growth in Incomes of African Americans in the Mississippi Delta, 1880-1910.” Joining the ongoing debate over black economic progress between Emancipation and the Second World War, Irwin and O’Brien note that scholars have clustered into two camps, “pessimists”, including Ransom and Sutch who have found little evidence of economic gain during the period, and “optimists”, who have presented evidence of substantial improvement. Focusing upon African-American families in the Mississippi-Yazoo Delta region, Irwin and O’Brien first construct an occupational order dividing heads of household into four categories – laborers, sharecroppers, cash renters, and farm owners. Taking account of the limitations of available data, they then set forth to discover whether African Americans tended to move upward in this hierarchy during the period in question, and whether incomes in general increased. PUMS from 1880, 1910, and 1920 indicate that the percentage of black household heads recorded as “laborers” decreased dramatically from 1880 to 1910, while

the percentage of blacks recorded in the broad category “farmers” increased. Basing their estimates upon MSS agricultural census files from 1879 and a US Department of Agriculture study from 1913, Irwin and O’Brien find that mean income for farm laborers and sharecroppers increased. Furthermore, comparing information on black

family size and the occupational distribution from the 1910 PUMS and the 1913 USDA study with 1880 MSS census data, Irwin and O’Brien find that *per capita* income for Delta blacks increased roughly 45% between 1880 and 1910.

Discussant Kyle Kauffman (Wellesley) praised the work, noting that it supports the “agricultural ladder” hypothesis in which he also believes. He

cautioned that census records contain distortions because sharecroppers and true tenants are combined into a single category. Since tenants earned much more than croppers, this overstates cropper income. He also questioned whether the Mississippi-Yazoo Delta, with its excellent soil and productivity, was representative of conditions elsewhere in the South.

Vernon Burton (Illinois) followed with “Revisiting the Myth of the Matriarchy,” which addresses the long-standing debate over whether the institution of slavery had promoted a matriarchal family structure among African Americans. While a consensus existed among white observers and black scholars such as DuBois and Frazier into the middle years of this century that slavery had indeed “damaged” the black family by eroding fatherhood, that consensus was later upset by the work of Gutman and others, whose evidence showed that slave families were generally male-headed and stable. More recently, counter-revisionists including Orlando Patterson have challenged Gutman *et al.* From detailed evidence for Edgefield County, South Carolina, Burton



Photo: Lehigh University

Leon Litwack with Lehigh hosts Anthony O'Brien and William Scott.

argues that black families in the rural South were indeed as patriarchal as the surrounding white milieu from the era of slavery into the late 1870s. Scholars who have asserted otherwise have been hampered by insufficient research or a bias toward urban data. In addition, "matriarchy" has not been adequately defined. He notes that even under the strictures of the slave regime, fathers were overwhelmingly recognized as heads of household by slave owners and the general community. Edgefield County MSS census schedules from 1870 and 1880 indicate that percentages of male-headed households were nearly identical among whites and blacks. Furthermore, membership in African-American community organizations such as churches and social orders was generally restricted to males. Male-headed households overwhelmingly dominated the tenant farming and sharecropping economy, although black women were employed outside the home far more often than their white counterparts. If there has been a latter-day problem of a disproportionate number of female-headed black households, Burton locates the origins of this tendency in small urban and village centers of the South during the waning days of Reconstruction. African-American families had flocked to these urban areas during the early postbellum years, where men had access to a wide range of employment and women could find domestic work. However, conservative white Redeemers forcefully closed all but the most menial urban opportunities open to black men by the late 1870s, making it difficult for males to support families and, consequently, leaving a large number, though far from a majority, of urban black women to fend for themselves and their children.

Discussant Susan Carter (UC-Riverside) had high regard for Burton's paper, citing its importance to current questions regarding single motherhood. She offered photographic evidence from the 1930s supporting the predominance of patriarchy among African Americans in the rural South. For the next step in his research, Carter suggested that Burton help scholars develop a practical definition of "matriarchy." She also suggested that he present information on the representativeness of Edgefield County.

Closing the Friday afternoon session was Thavolia Glymph (Penn State), with her presentation of "One Kind of Freedom: The Work of African American Women in the Postbellum South." Glymph's paper urges scholars to take a more nuanced view of the "withdrawal" of southern African-American women from the rural workforce that was lamented by many postbellum white planters and has become a commonplace in historical

studies. The reduced presence of women as full-time agricultural workers was not always voluntary (mechanization of some Louisiana sugar plantations allowed farmers to lay off their female workforces) and did not necessarily reflect an attempt by black women to enter a sphere of Victorian domesticity. Citing a number of personal narratives, account books, and other sources, Glymph notes that the "domestic" production activities of African-American women, such as gardening and dairying, often resulted in marketable produce that made a substantial contribution to their families' material welfare. After emancipation, many women forsook the full-time fieldwork of the slave regime for flexible part-time arrangements that allowed them to combine farm or domestic labor with individual household economic activities. Glymph is disappointed that recent historical work, including studies informed by quantitative economic methodology, have continued to treat African-American women's activities as marginal to the southern economy. She calls upon scholars to better appreciate the complex nature of African-American women's role.

Carter noted that she has located significant quantitative evidence supporting Glymph's thesis: a review of 1880 MSS census records revealed that a far higher proportion of black women reported gainful employment than was indicated in the published Census. Apparently such part-time labor listed by enumerators was not deemed true employment at the Census Bureau. PUMS data from 1870 and 1880 indicate that southern black women had a far higher level of workforce participation than did white women, but Carter noted that these rates were still far lower than the roughly 90% workforce rate assumed for slave women. Furthermore, black women's workforce rates decreased upon marriage. Apparently emancipation brought women opportunities to adjust their work schedules better to accommodate family needs.

Rexford Ahene (Lafayette) chaired Session II, which opened with Lee Alston (Illinois) and Kyle Kauffman presenting "Croppers and Competition: A View from Plantations in the Early 20th Century." Agreeing that a rural "occupational ladder" existed during the postbellum period, Alston and Kauffman ventured to discover whether a truly competitive labor market existed for blacks and whites (*i.e.*, whether blacks and whites holding equivalent ranks in the hierarchy received similar compensation). One principal tool employed in the project is a comprehensive 1911 survey of 39 Georgia plantations compiled as part of an abortive Census Bureau project. The survey results indicate that turnover among black

agricultural workers was very high as competing farmers bid for labor, and that the compensation of white and black sharecroppers was roughly similar, even on farms employing both black and white labor. Testimony offered by North Carolina landlords in 1887 and 1900 support the conclusion that black and white laborers received equal compensation. However, as blacks were far more clustered than whites in the lowest positions in the agricultural hierarchy, overall African-American incomes were lower than those of whites.

Discussant Joseph Reidy (Howard) remarked that it is hard to draw conclusions about the competitiveness of the southern labor market without broader statistics. Anecdotal evidence of income equality between some black and white sharecroppers sheds little real light on the reality of race and labor in the southern context. For instance, some white sharecroppers had family ties with landlords, and, as whites, could expect a certain level of respect. Furthermore, the fact that black sharecroppers were less likely than whites ever to escape the lowest rungs of the agricultural order implied that many black croppers were older than the white counterparts to whom their earnings are being compared.

Thomas Maloney (Utah) followed with "Migration and Economic Opportunity in the 1910s: New Evidence on Occupational Mobility." Amid the superheated industrial economy of the World War I years, did northern-born blacks have greater opportunities for upward mobility than southern-born black migrants? Maloney's comparison of the 1910 and 1920 PUMS provides some insight. In the 1910 sample, African-American men in the north were far more likely than whites to hold unskilled or service positions, and there was little occupational dissimilarity between northern- and southern-born blacks. By 1920, occupational dissimilarity between whites and southern-born blacks remained nearly unchanged, but the gap between whites and northern-born blacks had narrowed considerably. Occupational dissimilarity between young (age 15-34) white and northern-born black workers was reduced even further. To study this development in greater detail, Maloney collected samples of black and white males living in the north and constructs probability equations for their presence in five employment categories ranging from white collar to service. In 1910, African Americans were over-represented in the unskilled labor and service jobs at the lower end of the scale regardless of birthplace; by 1920, while young northern-born whites had gained near-parity with whites in semiskilled occupations,

southern-born blacks had become even more concentrated in unskilled labor positions. The causes of such a difference are unclear, but could be linked to fewer employment-networking resources available to recent migrants or to differences in educational opportunities for blacks in the different regions. Maloney then presented evidence from a longitudinal study sampling black and white males in Cincinnati. He utilizes draft registration records from June 1917, June 1918, and September 1918, and compares these with a 10% sample of black males from the 1920 MSS census and 1920 PUMS information to construct a profile of occupational mobility among those persons who could be linked between 1917-18 and 1920. There were difficulties in linking black individuals, but Maloney is nonetheless able to reach a preliminary conclusion that blacks lagged dramatically behind whites in upward mobility, and that, in this instance, southern-born blacks suffered no mobility deficit as compared with the northern-born. This latter conclusion may be affected by the timing of the draft registration relative to migrants' arrival in the city.

Discussant Bart Shaw (Cedar Crest) praised the design of Maloney's research and his careful qualifications of its data. He noted the limitations of Maloney's census material, since, as Maloney conceded, it fails to indicate when particular migrants moved from the South. He also suggested that data be collected showing the occupational mobility of southern white migrants.

Session III convened with John Smith (Lehigh) in the chair. Price Fishback (Arizona), Michael Haines (Colgate) and Shawn Kantor (Arizona and Federal Home Loan Mortgage Corp.) began the session with "The Impact of New Deal Programs on Black and White Infant Mortality in the South." Infant mortality was selected as an appropriate measure of whether New Deal social welfare initiatives, filtered as they were through state and local authorities, significantly aided those at the bottom of the socioeconomic scale. Due to improvements in nutrition, housing, and hygiene, the US infant mortality rate began declining substantially in the 1870s, and Progressive-era health initiatives continued the decline through the 1920s. In fact, the rate continued to decline, for both blacks and whites, through the Depression-wracked 1930s. To investigate the effect of New Deal programs on this decline, the authors assembled data, covering roughly 3,000 counties from 1930-1939, on infant death rates and New Deal program spending. New Deal spending is divided into two periods dominated by different sets of programs: 1933-35 and 1936-39. After appropriate methodology was employed to limit biases in the data, a set of variables

was used to estimate New Deal spending in particular counties. In general, Federal Emergency Relief and Home Owners Loan Corporation programs could be associated with reduced infant mortality from 1933-35, while the Public Works Administration, Works Progress Administration, and Federal housing programs had a similar effect after 1935. Because of data limitations, a smaller sample of 725 counties in the southern US is used to gain reliable race-specific data on infant mortality. New Deal programs could not be associated with decreases in white infant mortality in these counties, but significant reductions in black infant mortality were associated with Farm Credit Administration programs and HOLC loans into 1935 and with Farm Security Administration and Public Roads Administration programs thereafter. Therefore, New Deal programs can be said to have countered some of the difficulties facing southern blacks at the local level.

Discussant Howard Bodenhorn (Lafayette) noted that the actual improvements in black infant mortality that can be tied to New Deal policies was mathematically quite small. He called attention to the "lag effect", noting that public health measures that improve maternal health can have a positive effect on infant mortality years later.

"Arbitraging a Discriminatory Labor Market: Black Workers at the Ford Motor Company, 1918-1947," by Christopher Foote (Harvard), Warren Whatley (Michigan) and Gavin Wright (Stanford) was the next presentation. Given Ford's record of employing unusually large numbers of African-American production workers during the interwar years, the challenge was to see whether the company arbitrated the labor of black workers; that is, did it take advantage of racial constraints to offer blacks lower compensation than whites. At first glance, the fact that African Americans could be found throughout Ford plants, including on assembly lines, in far higher numbers than at other auto companies and at wages similar to whites, places such arbitrage into question. However, the observed concentration of black workers in the least desirable jobs, including the hot and hazardous foundry of the River Rouge plant led the authors to test a hypothesis that the company was using an elaborate strategy of cost reduction. Unlike white workers, African Americans had few alternative sources of semiskilled industrial employment in interwar Detroit, and so were not in a position to demand the premium pay whites would have required for dirty, hazardous foundry assignments. The authors develop a complex regression formula to verify that Ford maintained roughly even pay scales

regardless of employee race or assignment to foundry or non-foundry work. Then, a sample of the voluminous work history files Ford maintained on each employee was drawn from the years 1918-47. By measuring the rates at which black and white workers quit jobs in foundry and non-foundry plant areas, the authors develop an image of how much workers valued the jobs they held. Quit rates of black foundry workers and white non-foundry workers differed little, but white foundry workers had far higher turnover. Thus, white workers, who had much better prospects for alternate industrial employment than blacks, "voted with their feet" and confirmed that foundry work was not to be endured at existing wage levels. Blacks had no such choice. Posing the question of why the company used such subtle methods to gain extra value from black workers, the authors note that overt wage discrimination between whites and blacks could violate expectations of wage fairness and had, at other firms, caused disruptive tensions between black and white workers (whites feared that blacks would take their jobs at lower pay rates). Secondly, by maintaining a population of loyal black workers (often recruited through black churches), the company may have been attempting to promote an anti-union, Republican voting bloc.

Discussant Robert Weems (Missouri) suggested that the reality of the black relationship with Ford was more complex than the paper allowed. Blacks did, after all, work in parts of Ford plants other than the foundries and rose to responsible salaried positions unheard of in other Detroit area firms. He drew a parallel between Henry Ford and Branch Rickey: for both, employing blacks might be a means to increased profits, yet both might be said to have had sincere, if paternal, concern for blacks. Throughout Detroit during the 1920s and '30s, African Americans held Henry Ford in high regard, and he showed interest in their community welfare. One striking example was Ford's "rescue" of a small, predominately black Michigan town wracked by the Depression. He offered residents industrial jobs and used company resources to bring health care, street cleaning, and other benefits to the town. Perhaps Ford's concern for blacks mingled with paternal racism and avarice in a singularly complex personality.

Session IV, chaired by William Scott, was devoted to a roundtable discussion of the legacy of *One Kind of Freedom*. Authors Ransom and Sutch (both UC-Riverside) presented a lively reconsideration of their work entitled "IKF: Reconsidered, Jump Started, Turbo Charged."

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Sixth Hughes Prize Award

by Ann Harper Fender, Gettysburg College

(Baltimore, MD) In 1992 the Economic History Association announced the creation of the Hughes Prize for Excellence in Teaching Economic History to honor the memory of Professor Jonathan R. T. Hughes, a distinguished teacher and scholar at both Purdue and Northwestern Universities. The prize also illuminates the importance of superb teaching to the continued vibrancy of economic history as a discipline by recognizing annually an exemplary practitioner.

This year's recipient, Dr. Robert Whaples of Wake Forest University, has amply exhibited both traits for which Professor Hughes was known: distinguished, generous scholarship and excellent teaching. Professor Whaples's research and his formal writings about teaching are well known, as are his selfless contributions to the development of internet sources for colleagues and students alike. Perhaps less well known, at least beyond the confines of Wake Forest and the University of Wisconsin-Milwaukee, are the affection and respect with which his students view him. In selecting him as "one of the best", the Wake Forest University newspaper described Professor Whaples as a true gem who could take the dry subject of economics and make it interesting to all, even as he challenged the students with difficult, thorough tests and assignments. Formal student evaluations, letters solicited from students, and interviews with still other students all

depicted him as an excellent teacher with strong command of the material, imaginative assignments diligently graded, the ability to entice students to apply economics to interesting historical situations, and a quirky sense of humor that enlivened the classroom. Students uniformly mentioned his ready accessibility and interest in their doing well despite his commitments to family and community. His imaginative and carefully constructed course syllabi and assignments entice the reader to piracy of these tangible objects that reflect his teaching philosophy and knowledge of the discipline.

Letters from and interviews with his colleagues at Wake Forest University and the University of Wisconsin-Milwaukee and elsewhere made clear that Professor Whaples's influence is not limited to his current and past classroom students. All unanimously and enthusiastically endorsed him as an excellent scholar and teacher, citing how much they had learned from him about economics, history, teaching, or all three.

The Teaching Committee of the EHA faced this year the pleasantly daunting task of choosing the Hughes Prize winner from among a pool of excellent nominees. Because he so clearly exhibits the attributes which the prize recognizes, we are pleased to honor Robert Whaples as the 1999 recipient.

On Receiving the Hughes Prize

by Robert Whaples, Wake Forest University

Winning the Hughes Prize was quite unexpected. Not suspecting that anything was afoot, I skipped the banquet to spend time visiting family members who live in Maryland. My apologies. My thanks go to my students and to two economic historians who were my teachers in graduate school at the University of Pennsylvania, both of whom are very well known for their teaching abilities – Claudia Goldin and Bob Margo.

First a brief sketch of my career as a teacher. In graduate school, I never really taught a class. I was Claudia Goldin's teaching assistant in one class and ran weekly

review sections, but I delivered exactly one lecture. Therefore, I didn't really know what to expect and was quite ill-prepared when I began my duties as an instructor (and later assistant professor) in the Department of History at the University of Wisconsin-Milwaukee. I was so nervous that I even forgot which axis was which on the supply-and-demand diagram! Fortunately, after about a week, I decided that I liked teaching. After three years of teaching at UWM, I moved to Wake Forest University's Department of Economics. During the on-campus interview, I surprised the department by asking to sit in on a lecture to see what Wake Forest students

were like in action. I found them to be bright and eager and have been lucky to serve them over the past nine years. (Still, my most challenging and interesting students have been my children.)

Because I am nearer the beginning of my teaching career than the end, my comments on teaching are probably not all that insightful. Nevertheless, here are some "observations."

– Learning and teaching go hand in hand. If you know and understand things, you will feel compelled to spread that knowledge to others. As an added bonus, you will learn things much better when you teach someone else what you've learned.

– The teacher's first duty is to motivate students. A teacher's enthusiasm is infectious – enthusiastic teachers have motivated students. This means that you can have fun in the classroom. [Apparently, one (all?) of my students commented to the Hughes Prize Committee that I have a "quirky" sense of humor. I'm glad they noticed and they've let me know that humor can be an important part of teaching.] Needless to say, teaching isn't built on fun and games. The enthusiastic teacher needs to see the innate, God-given potential in all students, be well organized and knowledgeable, and teach a subject which is relevant and important. I try to bring all of these things to my teaching. I'm lucky that my students have been eager to learn and that economics and economic history are such powerful tools for understanding how the world works and how we can make it work even better. Still, enthusiasm comes first. I hope that my students (from those in my economics classes, to those that I coach on the Quiz Bowl team, to my Sunday school students, to my own children whom my wife, Gina, and I home school) can see in me the true joy of learning and understanding – I can see it in them.

– A mind is capable of great things only if it is exercised. Learning cannot really take place if the student is spoon-fed, and the world is immensely complicated so oversimplification is unacceptable. Unfortunately, some students shy away from tough, rigorous classes. Fortunately, most won't. Most students are very capable and when they are prodded, respond by putting in the effort and hours needed for mastery. (A few years ago, a poll in the student newspaper voted me one of the two most difficult

professors on campus. Fortunately, students have continued to fill my classes.)

– Teachers are not omniscient and should humbly, candidly admit this to their students from the start. Let them know that you are a student too, that everyone in the profession is a student. On the first day of my American Economic History class, I give students a quiz, based on the questions asked in my *JEH* article, "Where Is There Consensus among American Economic Historians?" I then reveal the "answers" and point out that there is often disagreement among this group of knowledgeable, careful, well-intentioned scholars. This helps frame the entire semester as a search for understanding – a group effort that involves the students, the professor, and the authors of books and articles that we invite into our classroom. (I also emphasize that these economic historians are not faceless scribes, but are real people whom I've been lucky enough to get to know. This seems to make students more eager to learn – at least they have told me this.)

– There must be a purpose to an education. You cannot only educate the mind. The soul must not be ignored. I try to bring this vital point to my teaching. As my students and I examine the technical results embodied in a table of regression coefficients or the intricacies of a new model, we must also carefully, rigorously examine moral issues – from slavery to immigration policy to banking insurance. Our ultimate purpose is to use our knowledge to act justly and this is the ultimate lesson that I try to teach to all my students.

Finally, I'd like to thank the most influential teacher in my life – my father, Gene Whaples. Under his picture in his high school yearbook is a quote. Most of his friends' quotes are witty or silly or cryptic, but his quote reads "Education makes the man." He has lived by this motto, teaching children as a 4-H Agent, earning a Ph.D., becoming a college professor and eventually president of the Adult Education Association of the US, and (most importantly, from my point of view) imbuing in his four children the importance of education. Following his example, I became a teacher. I hope that my kids – Thomas, Nina, Becky, Rose, and Charlie – will be the teachers of the next generation. (Check out this link to the International Adult and Continuing Education Hall of Fame: <http://www.occe.ou.edu/halloffame/whaples.html>. It includes a great photo of my dad.)

Report on NBER/DAE Summer Institute

by Richard Sylla, New York University, and John Joseph Wallis, University of Maryland

(Cambridge, Mass) The "Development of the American Economy" (DAE) program of the National Bureau of Economic Research held its annual Summer Institute meetings at the Bureau's home office July 12 - 15, 1999.

Monday's session, featuring six papers, began with a presentation by Carolyn Moehling (Yale) and Richard Steckel (Ohio State) of "Long-Term Trends in Wealth Inequality: Evidence from Property Tax Records." They presented preliminary results of their work using property tax records to measure patterns of wealth inequality from the late 19th to the early 20th century. Their results suggest that inequality was increasing, and that much of the increase in inequality occurred at the upper end of the wealth distribution. The authors explore several alternative ways to measure inequality, and examine wealth distributions in Ohio and Massachusetts in detail.

Next, Lee Alston (Illinois) presented "Did U.S. Agricultural Policy Subsidize Canadian Farmers? The Capitalization of Farm Policies into Land Prices", written with Randy Rucker (Montana State) and Marc Weidenmier (Claremont McKenna). This paper tries to estimate the effect of US agricultural price supports on the price of farmland by examining land prices along the Canadian/US border. The econometrics should be able to control for year to year changes in weather, yields, and international factors that affected wheat prices and land prices. To their surprise, the authors found that increasing price supports for American wheat did not have an effect on the price of American relative to Canadian land. Two possible conclusions can be drawn: one, that farm price supports had only a temporary effect on land values; the other, that American price supports raised the price of wheat in both countries, so that both Canadian and American land values were increased.

The morning session closed with a paper by Robert Margo and William Collins (both of Vanderbilt), "Race and Homeownership, 1900-1930." The evolution of racial differences in wealth has not been so extensively analyzed as racial differences in income. This paper uses IPUMS data to study trends in racial differences in home ownership since 1900. At the turn of the century approximately 20% of black adult males (ages 20 to 64) owned their homes compared with 46% of white men, a gap of 26

percentage points. By 1990, the black home ownership rate had increased to 52% and the racial gap had fallen to 19.5 percentage points. All of the long-term rise in the rate of black home ownership, and almost all of the corresponding long-term decline in the racial gap, occurred after 1940, with the majority of both changes concentrated in the 1960 to 1980 period.

The afternoon session began with "The Impact of New Deal Funds on Economic Activity at the Local Level," by Price Fishback, William Horracc, and Shawn Kantor (all of Arizona). Horracc made the presentation (Fishback had lost his voice and was reduced to asking questions and making comments by non-oral means - flash cards and the like). During the 1930s the Federal Government dramatically expanded the use of intergovernmental grants to fight the Depression and to relieve unemployment. Using county level data, Fishback, Horracc, and Kantor attempt to determine whether federal spending actually stimulated economic activity. They employ spatial econometric techniques to capture any spillover effects that New Deal spending in one county may have had on its neighbors. They find some evidence that politics played a role in the allocation of grants. They do find spillover effects and that, in rough terms, at the margin each additional dollar in New Deal spending might have increased retail sales by an aggregate total of from 20 cents to 40 cents over the period from 1933 through 1939.

Next, Claudia Goldin and Lawrence Katz (both of Harvard) presented their paper on "The Returns to Skill in the United States across the 20th Century." This paper makes use of detailed educational records on individuals from the Iowa State Census of 1915. The paper finds that returns to schooling in Iowa were large. Not only did obtaining a high school education enable graduates to enter more lucrative jobs, but also, within occupations, increased schooling led to increased earnings. With some caveats about comparability, it appears that the returns to schooling in 1915 were larger than the returns to schooling in the 1940s. This result is consistent with the compression in wage structure that occurred in mid-century. With such high returns to education in the early years of the century, it is no surprise that the high school movement took root in those years.

The Monday session closed with Dora Costa (MIT) presenting "The Rise of Joint Retirement." Costa uses census data to document the increase of joint-retirement decisions among all older couples and among all couples in which both husband and wife had been in the labor force during the previous year. For all couples, Costa finds that the main determinant of increased joint retirement was the rise of married women's labor force participation. For couples in which both husband and wife were in the labor force when they retired, Costa finds that the joint retirement decision was related to similarities in their demographic characteristics, their wages and pension arrangements, and to an increased desire for joint leisure. Retirement migration (*e.g.*, moves to places with better climates) and retirement travel are factors that affect joint-retirement decisions. Costa speculates on the future of joint retirement as married women increasingly approach retirement age after having "careers" rather than just the "jobs" that many recent retiring women took on after raising their families. Two-career couples may well have different joint-retirement behavior than one-career, one-job couples.

Tuesday was a half-day to allow a free afternoon for other activities in the rich academic and intellectual environment of Boston and Cambridge. It began with Thomas Weiss's presentation of "The Public Finances of the Colonies of the Lower South," joint work with Peter Mancall and Joshua Rosenbloom (all of Kansas). Weiss noted the absence of efforts to compile systematic quantitative records of the public finances of the American colonies. Most of the previous work dealt with political issues related to finance or to issues related to the colonies' uses of fiat currencies. Weiss then presented preliminary estimates of revenues and (especially) expenditures covering the years 1732 to 1776 based on compilations from a variety of colonial records, published and unpublished, for the states of South Carolina and Georgia. Although overall levels of public spending were not large by later standards, he observed that the data could nonetheless be used to study the composition of spending over time and the changing relative importance of specific governmental functions, for example, Indian affairs. The data also contain good information on rates of pay established by the colonies for public officials and military personnel.

Tuesday's second paper was by Naomi Lamoreaux (UCLA), "Accounting for Capitalism in Early American History: Farmers, Merchants, Manufacturers, and their Economic Worlds." Lamoreaux assays a large and

ongoing debate among American historians, and between historians and economic historians, on the nature of the early American economy and society. In recent decades, a group of historians attacked earlier historical notions that capitalism came to America in the first ships from Europe. They have argued that, even in the late 18th and early 19th centuries, most American farmers (*i.e.*, most Americans) were pre-capitalist or even anti-capitalist in their mentalities, their family and community relationships, and their culture. Only gradually, and often reluctantly, were these farmers brought into the capitalist fold, dragged along by the increasing importance of the capitalist merchants and manufacturers with their markets, prices, and pursuit of profits. Some economic historians counterattacked by showing that early farmers, even in the colonial era, kept account books, participated in markets, and responded to price signals. Lamoreaux takes a different tack, criticizing the historian revisionists for assuming without investigation that the capitalist merchants and manufacturers were different from the farmers. She shows that their account books were rather like those of the farmers, that they were just as attentive to family and community relationships, and that they were part of the same culture and faced the same social and cultural constraints as did the farmers. It seems, then, that there was a transition to capitalism for all of American society, and that it likely came around the turn of the 19th century. Lamoreaux indicates that documenting the transition involves defining fairly precisely what is meant by capitalism, and that the rate of transition most probably varied from sector to sector of the economy. Her paper was different from usual DAE fare, and it provoked an animated discussion of historiography.

The third and last paper Tuesday was by K Coleman Strumpf and Paul Rhode (both of UNC), "A Historical Test of the Tiebout Equilibrium: Reduced County-Level Stratification from 1850 to 1990." The Tiebout hypothesis is one of the most famous in all of economics, certainly in public finance; it predicts that individuals will move between political jurisdictions to obtain their most preferred mix of public and private services. Following Tiebout, Strumpf and Rhode argue that, as mobility costs fall, citizens should sort themselves into more homogeneous communities and that inter-community variation should increase. From 1850 to 1990 census data, and contrary to expectations, it appears that counties have become more rather than less alike. That is, there is little evidence for Tiebout sorting going on over the long term. It appears that Americans have decided to live in more

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Undergraduate Paper Prize Award

by Martha Olney, University of California-Berkeley

As the nominating instructor, it is my pleasure to "introduce" the prize-winning paper by University of California at Berkeley graduate, Charmaine N. Go, "Unemployment Relief Distribution in the Bay Area During the Depression."

Charmaine's paper for my undergraduate seminar in US Economic History was the third in a series of papers. The first, her honors thesis for the History Department, used archival records of the Dean of Women and the Dean of Men to examine how the University of California assisted students during the Great Depression of the 1930s. She found that the Deans instituted a policy established by the university President to insulate the students from bad economic times: "No one shall leave the University for financial reasons." Fees were reduced, tuition remained at zero, deans assisted students in finding housing with families, financial aid was increased.

Charmaine's second paper, her honors thesis for the Economics Department, asked whether the presence of the University insulated the city of Berkeley from the

Great Depression. Student enrollments had not fallen and thus employment had been maintained. Retail sales dipped only slightly, buoyed by student spending. Comparing Berkeley with neighboring cities and counties, Charmaine was able to show that Berkeley experienced the mildest recession in the Bay Area.

The paper for which Charmaine received The Cliometric Society prize is published below. Charmaine uses archival evidence and the records of U.C.'s Heller Committee for Research in Social Economics to examine the pattern of relief distribution in Berkeley and surrounding communities. She evaluates the design of the relief program in light of the nutritional benefits enjoyed by recipients.

Charmaine's paper is an excellent example of the quality of work our undergraduate students can produce with a sharp mind, attention to detail, access to archival sources, and a well-defined question. My heartiest congratulations to Charmaine Go on her receipt of The Cliometric Society Prize for the Best Undergraduate Paper in Economic History, 1998-1999.

Unemployment Relief Distribution in the Bay Area During the Depression

by Charmaine Go, University of California-Berkeley

The 1920s will always be remembered as a decade filled with growth, energy, and prosperity. Families surrounded themselves with modern durable goods such as radios, appliances, and automobiles which made life easier and more convenient. However, a few years later, many of the same Americans found themselves in the opposite position: instead of having disposable income to spend on durable goods, they were lacking enough money to pay for daily necessities such as food and shelter.

During the Depression, the 1920s aura of endless opportunity and wealth quickly dissipated as feelings of uselessness and shame overtook American society. Unemployment grew to an all time high in 1933 and for the first time in American history, the Federal Government began to expand its role as caretaker of society as it offered relief and aid to those without jobs. As unemployment became a mass phenomenon and relief demand increased, it is necessary to question whether the local city welfare

systems utilized the best distribution method for allocating scarce relief resources. More specifically, the Bay Area used three different methods of food relief allocation and, in light of research, it can be concluded that San Francisco county's commissary system was the most effective method for relief allocation.

The intent of this paper is multifaceted. Section One discusses the highlights of the Depression so that Bay Area statistics can be placed in a frame of reference. Section Two looks at the unemployment and relief situation in the Bay Area; examines the general statistics of unemployment in San Francisco, Oakland, and Berkeley; and describes the Bay Area providers and recipients of relief. Section Three examines the different relief distribution methods used by agencies in the Bay Area and argues that San Francisco's commissary system was more efficient than Berkeley's cash system or Alameda's grocery-order system.

Table 1.2 Personal Consumption Expenditure 1929-1939

(In millions of dollars)

Year	Current prices	1929=100	Constant prices	1929=100
1929	77.2	100.0	139.6	100.0
1930	69.9	90.5	130.4	93.4
1931	60.5	78.4	126.1	90.3
1932	48.6	63.0	114.8	82.2
1933	45.8	59.3	112.8	80.8
1934	51.3	66.5	118.1	84.6
1935	55.7	72.2	125.5	89.9
1936	61.9	80.2	138.4	99.1
1937	66.5	86.1	143.1	102.5
1938	63.9	82.8	140.2	100.4
1939	66.8	86.5	148.2	106.2

Source: *Historical Statistics, Series F47.*

Table 1.3 Unemployment in the United States 1929-1939

(In thousands of persons 14 years old and over. Annual averages)

Year	Unemployed	% of civilian labor force	Unemployed (1929=100)
1929	1,550	3.2	100.0
1930	4,340	8.9	280.0
1931	8,020	16.3	517.4
1932	12,060	24.1	778.1
1933	12,830	25.2	827.7
1934	11,340	22.0	731.6
1935	10,610	20.3	684.5
1936	9,030	17.0	582.6
1937	7,700	14.3	496.8
1938	10,390	19.1	670.3
1939	9,480	17.2	611.6

Source: *Historical Statistics, Series D8-D9.*

Deflation was prevalent during this time period. From 1929 to 1933, prices fell approximately one-quarter of their original amount. Thus the trend of falling income and disposable income during the Depression was somewhat compensated by falling prices of goods.

Table 1.4 Disposable Personal Income 1929-1939

(In billions of dollars)

Year	Current prices	1929=100	Constant prices base=1958	1929=100
1929	83.3	100.0	150.5	100.0
1930	74.5	89.4	139.0	92.3
1931	64.0	76.8	133.7	88.8
1932	48.7	58.5	115.1	76.5
1933	45.5	54.6	112.4	74.7
1934	52.4	62.9	120.4	80.0
1935	58.5	70.2	131.8	87.6
1936	66.3	79.6	148.4	98.6
1937	71.2	85.5	153.1	101.7
1938	65.5	78.6	143.6	95.4
1939	70.3	84.4	155.9	103.6

Source: *Historical Statistics, Series A6, F282.*

Table 1.5 Consumer Price Index 1929-1939

(1967=100)

Year	All Items	1929=100
1929	51.3	100.0
1930	50.0	97.5
1931	45.6	88.9
1932	40.9	79.7
1933	38.8	75.6
1934	40.1	78.2
1935	41.1	80.1
1936	41.5	80.1
1937	43.0	83.8
1938	42.2	82.3
1939	41.6	81.1

Source: *Historical Statistics, Series E135.*

Section 2. Unemployment and Relief in the Bay Area

“Unemployment is perhaps the most dreaded disease of modern industrial society”, and in the 1930s it was a disease that few could escape.⁴ The unemployment trend in California mirrored that of the United States. In 1930 when 8.7% of the US workforce was unemployed, 7.6% of the California workforce was unemployed. Within California, in 1930 San Francisco had a 7.3% unemployment rate; Oakland, a 7.4% unemployment rate; and Berkeley, a 4.9% unemployment rate. In 1932 when unemployment in the US was 23.6%, 28% of the California workforce was estimated to be unemployed.⁵

After 1932, census data for unemployment become scarce; in fact, “there are no statistics of unemployment for either San Francisco or Alameda County covering 1932-1934.”⁶ However, there is a “Special Census of Unemployment”, which in January 1931 measured unemployment rates in 19 cities including San Francisco. The number of unemployed in San Francisco increased from 24,467 in 1930 to 46,045 in 1931, a 47% increase in one year.⁷

Relief Providers

While there are no consistent data for unemployment from 1932-1934 for the Bay Area, we can examine relief aid data to better understand the unemployment effects of the Depression. Since there is a positive correlation between unemployment and the number of family relief cases, it is evident that as unemployment increased, the number of people who sought relief aid also increased.

Until August 1933, unemployment relief in San Francisco was administered mainly by private agencies. The agencies that played a large role in the county were the Associated Charities (later known as the Citizens’

Agency), the Eureka Benevolent Society, and the Italian Board on Relief.⁸ In August 1933, the Citizens' Advisory Relief Committee, a public agency, closed all of the private agencies in the county and took the sole responsibility of administering most of the family relief in San Francisco.

As seen in Table 2.1, unemployment did not greatly impact San Francisco until 1931. From 1929 to 1930, expenditure for relief aid only increased by 29.2%, while from 1929 to 1931 expenditure increased by 583.3%. Then from 1931 until 1934, relief expenditure increased an average of 1572.7% over 1929. The enormous rise in expenditure from 1931 to 1934 implies that unemployment was rising rapidly in San Francisco during those years.

More precisely, Table 2.1 also shows the average number of families per month on relief rolls. Once again, the data show that the number of families on relief did not start to increase dramatically until 1931, which means that unemployment did not become a major problem in the city until 1931. From 1929 to 1930, the average number of families on relief per month only increased by 31.4%. In the following year, there was a 148.9% increase over 1929. By 1933, the average number of families per month on unemployment relief reached its peak, with a 960.9% increase over 1929. Finally, in 1934, the average number of families on relief began to decline by 14.8%, but the number of families was still 818.3% over the number of families on relief in 1929.

Table 2.1 Unemployment Relief in San Francisco

Year	Expenditure for family relief (In Thousands of Dollars)		Number of families	
	Amount	% increase over 1929	Average per month	% increase over 1929
1929	436	...	1,704	...
1930	564	29.2	2,239	31.4
1931	2,985	583.3	4,241	148.9
1932	4,320	889.1	14,202	733.5
1933	7,081	1,521.1	18,076	960.8
1934	10,518	2,307.8	15,648	818.3

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed ...* p. 9.

The staggering numbers clearly illustrate that the Depression had a major impact on the unemployment rates in San Francisco. As a result, it is evident that unemployment relief aid and the agencies that distributed it were an important part of survival for the citizens of San Francisco.

From 1929 to May 1933, four agencies administered relief aid in Alameda County. There were three private agencies in Berkeley, Oakland, and Hayward, and one city agency in Alameda. "These agencies contracted with the county to investigate applicants for relief and to supervise the distribution of county relief funds."⁹ However, on May 1, 1933, the Alameda County Charities Commission was created and took the entire responsibility of handling all family relief cases in the county. This paper will focus specifically on the city of Oakland, whose relief distribution prior to 1933 was supervised by the Oakland Associated Charities, and the city of Berkeley, whose county relief funds prior to 1933 were administered by the Berkeley Welfare Society.

Unlike San Francisco, in Oakland unemployment began to have a major impact in 1930. Table 2.2 illustrates that in 1929 unemployment relief expenditure increased only 39.7%, while it increased by 228.6% in 1930. For the next two years, relief expenditure increased by an average of 632.1% over 1928. These statistics show that as expenditure for relief aid continued to climb, unemployment also continued to grow in Oakland.

Similarly, Table 2.2 also shows that the average number of families on relief per month began to increase unusually in 1930 when there was an 82.1% rise over 1928. By 1932, the average number of families on relief per month reached 8,813 cases, a 586.4% increase over 1928. Unfortunately, due to the lack of data, it is not possible to determine conclusively whether 1932 was the peak year for relief expenditure and whether it had the largest average number of families on relief in Oakland. As reported by the Census Bureau, the United States as a whole had the highest unemployment rate (25.2%) in 1933, which leads to the assumption that the 1932 Oakland relief expenditure and number of family cases were approximately the highest during the decade.

Table 2.2 Unemployment Relief in Oakland

Year	Expenditure for family relief (In Thousands of dollars)		Number of families	
	Amount	% increase over 1928	Average per month	% increase over 1928
1928	170	...	1,284
1929	238	39.7	1,371	6.8
1930	561	228.6	2,338	82.1
1931	1,016	495.1	5,141	300.4
1932	1,484	769.1	8,813	586.4

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed ...* p. 12.

As Table 2.3 shows, relief expenditure in Berkeley did not reach abnormal levels of increase until 1931 when expenditure jumped by 291.6% over 1928. During the next year, in 1932, expenditure for family relief rose dramatically to 641.8% over 1928. The relief statistics imply that Berkeley was not seriously affected by unemployment until 1931.

Table 2.3 Unemployment Relief in Berkeley

Expenditure for family relief (In Thousands dollars)			Number of families	
Year	Amount	% increase over 1928	Average per month	% increase over 1928
1928	52	...	275	...
1929	56	6.6	339	23.3
1930	89	68.7	519	88.7
1931	207	291.6	971	253.1
1932	393	641.8	2,131	674.9

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed ...* p. 13.

Table 2.3 also shows that the average number of families receiving relief aid in Berkeley did not rise abnormally until 1931. In 1929 there was a 23.3% increase in the number of families receiving aid from the previous year. This number increased to 88.7% in 1930 and, in 1931, the average number of families receiving aid increased by 253.1% over 1928. The average number of families receiving aid in Berkeley had increased 674.9% over 1928.

In summary, relief agencies were necessary to the Bay Area during the Depression. San Francisco, Oakland, and Berkeley were economically impacted by the negative effects of the Depression through unemployment.

Relief Recipients

Since relief providers in the Bay Area have been discussed and expenditure data examined, the next step is to tell a story about the other side of unemployment, the relief recipients. Stereotypically, people who receive welfare or unemployment aid from the government are perceived to be lazy or burdens to society. However, by examining Bay Area data collected by the Heller Committee for Research in Social Economics from 1932 to 1934, it is evident that the stereotype is wrong. This paper will focus on four characteristics of relief recipients: age, family size, foreign/native born and occupation to prove that, unlike popular belief, relief recipients were young, hardworking, and mostly native-born whites.

To begin with, Table 2.4 describes the ages of male heads of families who applied for unemployment relief in 1929 and 1932, as well as the 1930 census data for San Francisco, Oakland, and Berkeley. Between 1929 and 1932 there was not a significant change in the ages of men who applied for relief aid. Approximately 75% of the men in both 1929 and 1932 were younger than 44. However, when compared to the 1930 unemployment census, it was found that only 60% of the people were under 44 years of age. Therefore, the male heads of families who applied for unemployment relief aid in San Francisco, Oakland, and Berkeley tended to be younger than the population as a whole.

Table 2.4 Age of Male Heads of Families in the Bay Area

Age (years)	Male heads of families of specified age		Per cent of Total
	Unemployment cases		
	1929	1932	
Total	100	100	100
Less than 25...	8.3	6.9	4.2
25-34...	37.8	36.0	24.4
35-44...	31.8	31.6	30.5
45-54 ...	18.4	18	26
55-64 ...	3.7	7.5	14.9

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed ...* p. 18.

According to the Heller Committee, "one might expect to find relatively few families applying for relief in a 'normal' year whereas in the later years of a depression, such families would have exhausted their resources and forced to apply for relief."¹⁰ Table 2.5 supports this statement. For example, in 1929 64.6% of families who applied for aid had two or fewer children. Meanwhile, in 1932 86.4% of families receiving relief had two or less children. This illustrates the point that smaller families were more likely not to apply for unemployment relief aid until their resources were exhausted. Another way to view these statistics is to see that in 1929, 35.4% of families had three or more children. In contrast, during 1932 that number dropped to less than half, as only 13.6% of families who received relief aid during that year had three or more children. These statistics illustrate the idea that families who received unemployment relief asked for help because they truly were in need of financial assistance. The fact that smaller families did not apply for relief aid until their financial resources were depleted shows that relief recipients were reluctant to ask for federal help.

Table 2.5 Sizes of Families in the Bay Area in 1929 and 1932

# of children under age 21	Families with specified number of children under age 21		San Francisco, Oakland, and Berkeley 1930 Census
	1929	1932	
	Per cent of total		
Total ...	100	100	100
None ...	20.8	36.7	46.1
One...	22.4	26.5	25.7
Two...	21.4	23.2	16.1
Three ...	13.3	7.3	7
Four ...	13.6	4.1	2.9
Five ...	3.6	1.2	1.2
More than five	4.9	1	1

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed...*, p.20.

This reality goes against welfare recipient stereotypes which say that these people are relief recipients because they can be, and because it is easier to get help from the government than to work.

Often when resources (relief money) are scarce, people are more likely to blame outsiders for exhausting their resources. Table 2.6 compares the head of families' ethnicity from the Bay Area in 1932 to that of the 1930 Census. The difference between native-white recipients in the Bay Area (67.7%) and the 1930 Census (65%) is insignificant. Thus the white population in the Bay Area was similar to that of the nation. The Heller study also reported that the number of "foreign-born whites" and "other races" in the Bay Area was less than the general United States. On the other hand, there were more blacks receiving unemployment relief in the Bay Area than in the 1930 Census. These statistics prove that the foreign-born population was not "contributing more than its fair share to the burden."¹¹

Table 2.6 Nativity and ethnicity in the Bay Area

Nativity and ethnicity	Heads of families, 1932		Males aged 20-64 1930 Census
	unemployment cases		
	Percent of total		
Total	100	100	
Native white	67.7	65	
Foreign-born white	19.6	27.5	
Black	8.5	1.6	
Other races	4.2	5.9	

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed...* p.26.

Relief recipients are also often stereotyped as unskilled workers. While it is true that there is a higher proportion of unskilled workers in the Bay Area in both 1929 and 1932 than in the 1930 Census, the fact that all categories are represented shows that not all relief recipients are

unskilled. In fact, many unskilled workers are receiving aid because their jobs tend to be temporary and also tend to fluctuate with business cycles. For instance, many unskilled workers are employed in the construction industry. As a result, when the economy is in a depression or a recession, there is a decrease in the demand for new buildings and construction workers are no longer needed. Since their jobs are temporary, it is harder for unskilled workers to save money in case of financial emergencies. Thus, when the economy declines, it is more likely to find unskilled workers needing relief aid at the beginning of an economic crisis such as in 1929. Table 2.7 also shows that the number of relief recipients working as clerks and skilled workers increased from 1929 to 1932. One reason for the increase could be due to the fact that clerks and other types of skilled workers are more likely able to save money for future use. As a result, when the Depression hit the Bay Area, this category of workers lived by dipping into their savings until they were depleted. As a result there was no need for them to apply for relief aid until later in the Depression.

Table 2.7 Occupation in the Bay Area 1929 and 1932

Regular Occupation	Male heads of families in		All male gainful workers in San Francisco, Oakland, Berkeley 1930 Census
	unemployment cases		
	1929	1932	
	Per cent of total		
Total	100	100	100
Professional persons	2.8	3.3	9.3
Proprietors, managers, & officials	4.6	5.1	13.1
Clerks and kindred workers	9.8	16.8	24.4
Skilled workers & foremen	26.1	34.3	20.7
Semi-skilled workers	18.3	16.2	16
Unskilled workers	38.4	24.3	16.5

Source: Huntington, Emily H. *Unemployment Relief and the Unemployed* p. 34.

To conclude, a majority of relief recipients were not unskilled workers; most of them were skilled, semi-skilled, managers, or professional persons. The workers who are unskilled find themselves in need of relief aid because of the temporary types of jobs available to them since they possess fewer skills.

In summary, relief recipients in the Bay Area were not lazy immigrants who were in the margins of society prior to the Depression. They were mostly young, native whites, a majority of whom were skilled or semi-skilled, and who were reluctant to ask for government help until their own savings were depleted. These descriptions do not fit the stereotypical relief recipient.

Section 3. Comparison of Bay Area Relief Allocation Systems

During the Depression, different cities and counties in the Bay Area used different methods for distributing relief aid. More specifically, three methods were predominantly used: the grocery-order system (Alameda city), the commissary system (San Francisco County), and the cash-relief system (Berkeley and Oakland). By comparing these three methods of distribution in terms of cost and effectiveness, it will be evident which system was most optimal during the Depression.

Before costs are compared, it is first necessary to give a brief description of each kind of allocation method. Under a grocery system, a family is given an order every two weeks to the grocery of their choice. The order is a limited list of set items that they can buy at set prices. These prices are set regularly by grocers' associations and chain stores. The goods are priced at wholesale, plus an additional 10% off. In a commissary system, families have the choice of having boxes of food sent to them or coming to the groceteria to choose their own food. Milk is delivered to their homes and a supplement check is provided for fresh meat and bread, which they buy at regular retail groceries. In a commissary system, there is the least amount of individual choice of food. Finally, in a cash-relief system, families are given more freedom to choose their provisions. Families in Berkeley were given "orders for specified amounts which were redeemable in groceries for these amounts," but they did specify which goods.¹²

In order to compare the costs of the different systems, we will turn to a study conducted by the Heller Committee for Research in Social Economics. In December 1933, Alameda County approached the Committee and asked them to calculate the cities' current relief costs under the current grocery-order system and to project relief costs if Alameda used the commissary or cash-relief systems.

Table 3.1 Estimates of Alameda Relief Costs for Food Distribution Methods

	Grocery Order	Commissary	Cash Relief
Total food cost	24.74	19.04	26.37
Breakage and Spoilage	...	0.33	...
Overhead Cost	0.57	1.32	0.29
Sales Tax	0.66
Total Cost	25.31	20.69	27.32
% of present cost	100.00	81.70	107.50

Note: The estimates above are for a family of four members (a man, his wife, and two children between the ages of 9-13). Also note that these total costs are for one month.

Source: Heller Committee for Research in Social Economics, *Report on the Probable Costs of ...*, p. 3.

As seen in Table 3. 1, the smaller "total food cost" for the commissary system more than compensated for the higher "breakage and spoilage" and "overhead cost" of the San Francisco County method. One way to compare the different costs is to look at total costs when it is calculated as a percentage of Alameda's present cost. It is clearly evident that the commissary method is the least expensive of the three methods, followed by the grocery-order method, and then the cash-relief method.

Another way to determine which distribution method is most effective would be to examine families of different relief systems and see which are attaining the necessary dosage of vitamins and minerals to maintain healthy lives. Unfortunately, there is a lack of data to do a complete comparison of all three cities. However, in 1932 the Berkeley Welfare Society (which utilized the cash-relief system) approached the Heller Committee for Research in Social Economics and asked them to conduct a study to determine whether Berkeley residents were attaining proper nutrition. Thus, from this study it will be possible to determine whether the cash-relief system makes up for being the most expensive by being the most nutritionally full.

The food standard used was "drawn up in November 1931 by Ruth Okey and Emily H. Huntington as a basis for relief to families of children receiving aid and similar cases."¹³ In their food standard, Okey and Huntington use five different minerals (calories, protein, calcium, phosphorous, iron) to determine whether a person's diet is "adequate."

Table 3.2 Number of Berkeley Families Below Okey-Huntington "Adequate" Food Standard

	Number of families	% of families
Calories	14	56
Protein	23	92
Calcium	25	100
Phosphorous	23	92
Iron	22	88

Note: 25 Berkeley families were surveyed in the study.

Source: Okey, Ruth, *The Foods Chosen By Dependent Families ...* p. 32a.

As seen in Table 3.2, a majority of families were below the "adequate" food standard in all mineral categories except for calcium. This means that under a more liberal distribution system (cash-relief system) where relief recipients were given the ability to choose their own provisions, they were unable to choose the best combinations of food which would give them the greatest nutritional value per

federal dollar. According to Ruth Okey, "As might be expected, these relatively unrestricted food purchases tended primarily towards a choice of those foods which appeal to the taste rather than towards ones which contain the needed nutritive qualities at low Cost."¹⁴

In summary, when the grocery-order, commissary, and cash-relief systems were compared in terms of cost and nutritional effectiveness, it was found that the cash-relief system was most expensive and it did not provide the needed adequate nutrients to relief families.

Section 4. Conclusion

In light of the data examined, it can be concluded that the cash-relief system (or a system where individuals are given free reign to pick their own provisions) is the least effective allocation method. This is especially the case when financial resources are scarce as they were during the Depression. While it cannot be concretely concluded that the commissary system is the best method for food distribution in terms of nutrient content, it is the least costly. In fact, it seems "more or less obvious that better values for the money can be attained if relief funds are expended accordingly to some plan which provides for wholesale purchase of food and its allotment to individual families according to nutritional requirements, . . . in other words, a central commissary."¹⁵ However, while it might seem that a commissary system might be more effective, it is also important to keep in mind that a commissary method takes away any individual choice in the type of food consumed. Therefore, another question emerges: Should the government have the ability to prescribe the types of food that people consume? The answer to this question is left to the individual.

Studies regarding distribution methods have important current and future public policy implications. For instance, every day thousands of Americans are without food and shelter; this study shows that government aid in terms of set nutritionally-filled food is the most effective way to feed more hungry Americans. In fact, these conclusions can be applied in situations and countries outside the United States. They are especially applicable in Third World countries where resources are scarce.

Endnotes

¹Galbraith, John K. *The Great Crash*, Boston: Houghton Mifflin Company, 1979.

²Ibid., p. 1.

³Gary Walton and Hugh Rockoff, *History of the American Economy* (Fort Worth: Dryden Press, 1998), p. 520.

⁴Huntington, Emily H. *Unemployment Relief and the Unemployed in the San Francisco Bay...*, p. 5.

⁵Ibid., p. 6.

⁶Ibid., p. 7.

⁷Ibid., p. 6.

⁸Ibid., p. 10.

⁹Ibid., p. 11.

¹⁰Ibid., p. 19.

¹¹Ibid., p. 24.

¹²Note: Description of different relief distribution systems were found in Heller Committee for Research in Social Economics, *Report on the Probable Costs of...* pp. 4-6.

¹³Okey, Ruth, *The Foods Chosen By Dependent Families*, p. 32.

¹⁴Ibid., p. 35.

¹⁵Ibid., p. i.

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Cliometric Society Sessions at the 2000 ASSA Meetings

The Cliometric Society will sponsor three sessions and a cocktail party for Members and Friends at the 2000 annual meetings of the Allied Social Science Association in Boston, January 7 through 9. ASSA Coordinator Kyle Kauffman selected and grouped papers sure to hold your interest. Chairs, discussants, and paper presenters are assumed to have read each full paper slated for their sessions. Other attendees are encouraged to read the abstracts in this pull-out section and the expanded summaries published on the EH.Net web site: <http://www.eh.net/Clio/> Complete papers are available from the authors on request; both the following abstracts and the summaries include contact information for the authors of correspondence. Sessions will be conducted in traditional "Clio style": a brief presentation by the author and discussant, followed by discussion from the floor.

Slavery and Race

Friday, January 7, 10:15 am **Marriott/Maine**

Chair: Kyle D. Kauffman, Wellesley and Harvard

Joshua Rosenbloom, Peter Mancall, and Thomas Weiss, Kansas: *Slave Values and the Economy of the Lower South, 1740-1815*

Robert A. Margo and William Collins, Vanderbilt: *Race and Homeownership, 1900-1990*

Siddharth Chandra, Pittsburgh: *Colonial Policy, Wage Inequality, and the Birth of Nationalism: The Case of the Dutch East Indies*

Discussants: Jonathan B. Pritchett, Tulane
Sumner LaCroix, Barnard and Hawaii
Jens Ludwig, Georgetown

Monetary and Financial History

Friday, January 7, 2:30 pm **Marriott/Maine**

Chair: Richard Sylla, New York University

Eugene N. White, Rutgers, and Cormac Ó Gráda, University College, Dublin: *Who Panics during Panics? Evidence from a Nineteenth Century Savings Bank*

Joseph Mason, Drexel, Ali Anari and James Kolari, Texas A & M: *The Stock of Closed Bank Deposits, Duration of Credit Channel Effects, and the Persistence of the U.S. Great Depression*

Arthur Sweetman, Victoria, and Aidan Hollis, Calgary: *Higher Tier Agency Problems in Financial Intermediation: Theory and Evidence from the Irish Loan Funds*

Discussants: Richard Sylla
Robert Whaples, Wake Forest
Peter Temin, MIT

Frontiers in Economic History

Saturday, January 8, 8:00 am **Marriott/Yarmouth**

Chair: J. Bradford De Long, UC-Berkeley

Werner Troesken, Pittsburgh, and Karen Clay, Carnegie Mellon: *Did the Trusts Have Market Power? Evidence From Distilling, 1881-1898*

Maristella Botticini, Boston, and Aloysius Siow, Toronto: *Why Dowries?*

William K. Hutchinson, Miami: *United States International Trade: Proximate Causes, 1870-1910*

Tarik M. Yousef, Georgetown: *Market Integration in the Age of Economic Liberalism: Egypt, 1850-1939*

Discussants: Dean Lueck, Montana State
Stephen Margolis, North Carolina State
J. Bradford De Long
William Collins, Vanderbilt

Cliometric Society Members and Friends Party

Saturday, January 8, 8:30-10:30 pm

Clery's Irish Pub **113 Dartmouth Street**

Exit Copley Square Mall through Dartmouth Street door, turn right, walk one-half block to corner of Dartmouth Street and Columbus Avenue.

Slavery and Race

Slave Prices and the Economy of the Lower South, 1722-1809

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Slavery shaped the economic growth of the lower South in the 18th century. The region's primary export staples – rice and indigo – were both produced primarily on large plantations relying on slave labor. The importance of slavery was clearly reflected in the region's population statistics. With the introduction of rice at the beginning of the century, the slave population of South Carolina grew nearly five-fold between 1700 and 1720, at which time blacks outnumbered whites in the colony by a margin of more than two to one. Although the share of whites crept upwards after 1720, it was not until the rapid expansion of settlement in the backcountry after the Revolution that South Carolina's free population outnumbered its slave population. Once the prohibition of slavery was lifted in Georgia in 1749, the slave population of that colony also shot upward rapidly, reaching 45% by 1770. Only in North Carolina, where lack of accessible ports inhibited the growth of export oriented agriculture was slavery's role more limited; and even here, over one-third of the population were slaves by the Revolution.

Although recent works by Philip Morgan (1998), Joyce Chaplin (1993), and Peter Coclanis (1989) have elucidated many aspects of the slave based economy of the lower South, none of them has given more than passing attention to the evolution of slave prices. As the most important productive asset of the economy, and a key component of the region's wealth, however, information on slave prices is a crucial indicator that can shed new light on the pace and pattern of economic growth in the lower south. That this topic has previously been neglected is especially puzzling in light of the extensive data available on slave prices in the probate inventories that have provided the foundation of much of the recent work on the region's economic history.

In this paper we make use of data from several samples of probate inventories to construct and analyze a time series of slave prices for South Carolina and Georgia from 1722 to 1809. We find that slave prices increased considerably over the nearly 90 years covered by our data, and almost all of this increase can be attributed to rising export prices. Combining our slave price series with figures on slave population, and data on other variables reflecting shocks to supply and demand, we estimate an econometric model of the market for slaves in South Carolina. The model indicates that both short-run supply and demand functions were relatively price inelastic. It also confirms the apparent correlation between the price of rice and the value of slaves. These findings concerning slave prices have several important implications for our understanding of the economic development of the lower South.

First, the relationship between rice prices and slave values suggests that the growth of productivity in rice cultivation was

much less pronounced than most recent accounts have suggested. Widely accepted estimates by Peter Coclanis suggest that total factor productivity in rice cultivation increased by between 30 and 50% over the course of the 19th century. Such an increase should, however, have been reflected in a rise in input prices relative to output prices. Our data indicate that prices of slaves – the most inelastically supplied input did not increase relative to the price of rice between 1722 and 1809. And it seems implausible that increases in the prices of land and capital could have been large enough to be consistent with Coclanis' conjectures.

Second, rising slave prices appear to explain much of the dramatic increase in wealth in the region. This does not, of course, alter estimates of regional wealth, but it does place them in a rather different light. Instead of reflecting the continued accumulation of real assets by the region's inhabitants, increased wealth was due largely to rising asset prices caused by the growth of world demand for the region's primary export staples.

Race and Home Ownership, 1900-1990

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The historical evolution of racial differences in labor incomes has received considerable attention from economists. In contrast to labor incomes, far less attention has been paid to the historical evolution of racial differences in wealth. This scholarly neglect is unfortunate because racial differences in wealth were – and still are – much larger than racial differences in labor incomes and also because wealth *per se* is an important determinant of living standards, independent of earnings. This paper uses IPUMS data to examine the historical evolution of racial differences in one aspect of wealth – home ownership – from 1900 to 1990.

Information about home ownership is available in every micro sample of the census since 1900, with the exception of 1950. We extracted from each census micro sample observations of male household heads between 20 and 64 years of age who were not in school. The white home ownership rate was at all times considerably higher than the black rate, and for the most part, the black and white rates moved in the same direction from decade to decade. Remarkably, not until 1970 did the black home ownership rate reach the level of the white rate at the turn of the century (46%). The slight declines in both the black and white rates between 1920 and 1940 were followed by sharp rises from 1940 to 1960 (24.2 points for whites and 18.6 for blacks) and continuing increases until 1980 when the rates leveled off.

Although the white level of ownership was always higher than the black level, the size of the gap varied over time. The gap jumped by 5.5 points between 1940 and 1960, and then collapsed from 1960 to 1980, falling 7.8 points. We use both regression and decomposition analysis to study the factors behind the changes in the home ownership gap over time. The

increase in the gap in the 1940s was due to changes in the racial gap in characteristics influencing the likelihood of ownership, chiefly migration from the rural South into central cities, which was negatively associated with the likelihood of home ownership. Although changing racial differences in household characteristics were partly responsible for the fall in the gap after 1960, much of the decline was explained by a decline in the negative effect of race *per se* on home ownership.

We also use the IPUMS data to study the historical evolution of racial differences in mortgage status and in the value of owner occupied housing. At the turn of the century black home owners were less likely than white home owners to hold mortgages, but the racial gap declined substantially by 1990. Similarly, we find that, while the value of black owned housing was far below that of white owned housing in 1940, the black-to-white ratio of housing values increased sharply between 1940 and 1980, primarily because of improvements in the relative quality of black-owned homes. However, holding housing characteristics constant, black-owned homes did not appreciate as rapidly as white-owned homes between 1960 and 1980.

Our results have important implications for the literature on racial differences in housing markets. First, previous work has suggested that various institutional changes in housing finance in the 1930s severely inhibited the ability of black households to accumulate housing equity prior to the passage of fair housing legislation in the late 1960s. Our results, however, suggest that, while such discrimination was present, blacks were increasingly able to become home owners after World War Two, possibly because these institutional changes accelerated the "filtering" process by which owner-occupied housing became available to prospective black home buyers. Second, our results suggest that fair housing laws have been a mixed bag in terms of their effectiveness: although the laws seemed to have enhanced the ability of black males to become home owners, they do not appear to have had a similar effect on the relative value of black-owned housing.

Colonial Policy, Wage Inequality, and the Birth of Nationalism: The Case of the Dutch East Indies
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The origins of the local nationalist and anti-Chinese Sarekat Islam movements (1912-16) are analyzed in the context of colonial policy and movements in industrial wages for subjects of the Indies for the period 1908-17. The analysis reveals a sharp increase in inequality between the ethnic Malay and Chinese communities between 1910 and 1916, the period of the birth and rise of the first movements in Java. It also reveals that this inequality was most severe in a number of residencies of Java, where the movements first took hold. The fact that this increase in inequality was accompanied in the later years by a rise in real wages for both communities suggests the importance of relative wages over absolute wages as a source of political unrest. This study reinforces the assertion of Anderson

(1999), that nationalist movements are not born from notions of "absolutely splendid ancestors." Their origins lie in humbler, and often economic, phenomena.

Monetary and Financial History

Who Panics During Panics?
Evidence from a Nineteenth Century Savings Bank
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Contagion is greatly feared in today's world financial system. The possibility that the collapse of one country's equity market or banking system can spill over to other countries is a grave concern of policy makers. Contagion can occur in all types of financial markets. In this paper, we examine the most traditional form of contagion, runs on a single bank in the uninsured 19th-century American banking system.

There are generally thought to be two types of banking contagion (Aharony and Swary, 1983; Lang and Stulz, 1992; Park, 1993; Kauffman, 1994). Industry-specific contagion occurs when information about one bank adversely affects all banks, including healthy ones that share few characteristics with troubled banks. Runs of this type are referred to as noninformational contagion. This type of contagion has some similarities with the mechanism that brings about a run on individual banks in models by Bryant (1980) and Diamond and Dybvig (1983). In these models, runs occur when each depositor believes that others will run on the bank and force it into liquidation. Fear of being last ignites a run.

The second type of contagion, bank-specific contagion, occurs when new information about one or more banks produces runs on other banks in the system that have some common characteristics with the former, but not all, banks. This type of run is considered the result of informational contagion. Contagion of this kind may be generated by asymmetric information among depositors. Jacklin (1983), Smith (1984), Gorton (1987), Chari and Jagannathan (1988), Jacklin and Battacharya (1988), Williamson (1988) and Calomiris and Schweikart (1991) have looked at the informational problems in the bank-depositor relationship. A run occurs because enough agents receive negative signals about the value of a bank's assets. Other depositors may respond with a withdrawal of deposits because they cannot easily observe the value of bank assets or the motives of other withdrawing depositors, producing a run.

These two types of contagion can also be characterized as the product of banking runs, by either poorly-informed or well-informed depositors (Frankel and Schukler, 1996). Bank-specific runs should begin with the well-informed making use of their information, while industry-specific contagion should be precipitated and spread by a run of the poorly-informed. Unfortunately, there is very little empirical work about what type of informational asymmetries caused banking runs and panics.

Most of the contemporary empirical literature has focused on contagion in equity returns for banks, not contagion among depositors.

In this paper, we provide some historical evidence on the nature of banking runs, drawing on the records of the Emigrant Savings Industrial Bank. The ESIB was subject to three serious runs in its early years. The first run occurred as the result of a local panic in 1854 when the Knickerbocker Savings Bank failed. The second run was part of the panic and nationwide crisis in 1857, and the third run took place in 1861 at the outbreak of the Civil War. The three episodes provide a natural experiment that allows us to observe how different types of information or shocks affected depositors. The Knickerbocker Savings Bank failed because of mismanagement, while other savings banks and banks were solvent and ultimately did not fail. This episode may be considered a potential example of bank-specific contagion panic. Looking at the ESIB, an apparently strong bank, will provide an opportunity to see if there is any evidence of noninformational contagion. Although the panic of 1857 was precipitated by the failure of the Ohio Life and Trust Company, the proximate cause of the panic was the collapse of the market for speculative western land and railroad securities, which was linked to the political uncertainty over whether Kansas and Nebraska would become slave states (Calomiris and Schweikart, 1991). This crisis affected banks differently, depending on their investments in these markets. A run in 1857 should be a mix of informational and noninformational contagion. Finally, the run in 1861 was the result of a general economic shock to the system, where one would expect the informed depositors to act first. Given the stable character of the ESIB deposit base, these three episodes allow us to determine how different shocks generated runs among the bank's depositors.

To examine who panicked, we analyze who closed accounts during panics and who kept them open. The ESIB's test books contain the names, addresses, and occupations of account holders. Usually, they also provide data on nationality, and date of arrival in New York. The account ledgers detail all transactions and interest payments for each account. Together these yield a profile of each account holder. We use a probit model to examine the determinants of account closure. Our study of 1854 and 1857 compares the 234 and 503 accounts closed during the panics with a sample of accounts that remained open. This sample is a one-in-ten sample of all accounts opened from the date of the creation of the bank in 1850. We find that account holders were less likely to close their accounts the longer they were resident in the US, suggesting a greater familiarity with the savings' bank. Unskilled account holders and those with small opening balances had a greater probability of closing an account. The variables measuring banking activity – the greater the frequency of deposits and withdrawals, and the size of the account prior to the panic – suggest that familiarity with banking reduced the likelihood of panic. This evidence indicates that it was the less informed depositors who panicked in 1854 and 1857.

The Stock of Closed Bank Deposits, Duration of Credit Channel Effects, and the Persistence of the U.S. Great Depression
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Bernanke (1983) hypothesized that the persistence of the Great Depression was linked to the longevity of credit effects. This hypothesis was not directly tested in his paper; instead, he relied upon published survey and anecdotal evidence to evaluate persistence related to credit channel effects. We formalize these anecdotal insights by constructing a data series that reflects the incipient excess money contraction due to bank failures. This contraction is the result of an involuntary transformation of liquid deposits into illiquid securities. Although these securities are technically bank liabilities, they are unable to fund new lending. Furthermore, this decline interacts endogenously with asset market overhang and asset price declines, as these phenomena affect the persistence of the contraction. The capital crunch that results from the excess money contraction and concomitant asset price decline promotes the financial accelerator phenomenon, thereby increasing borrowing rates and credit rationing and further embedding the credit channel effect.

The present paper empirically estimates the relationship between the duration of credit channel effects and the persistence of the US Great Depression. We accomplish this by taking more fully into account the amount of time it takes to "...rehabilitate insolvent debtors..." or liquidate their collateral. More specifically, instead of proxying credit effects using the flow of deposits in closed banks as in Bernanke (1983), we employ the amortized stock of deposits in closed banks, which reflects the slow liquidation of failed bank assets during and after the Great Depression. The stock of deposits in closed banks more appropriately measures the involuntary substitution of liquid deposits for illiquid claims on bank collateral. This substitution affected bank depositors' consumption decisions and banks' ability to make loans on the basis of limited capital. In the theoretical and empirical papers cited above, the substitution of liquid for illiquid claims is believed to be the incipient excess money stock contraction that leads to credit channel significance and business cycle persistence. Interestingly, casual inspection of our stock proxy for the credit channel *versus* the traditional flow-based proxy suggests that the banking crisis lasted 10 years, rather than five years as previously believed.

We employ vector autoregression (*i.e.*, VEC and VAR) models to investigate the explanatory power of the amortized stock of failed bank deposits during the Depression as a proxy for the time it takes to rehabilitate insolvent debtors or liquidate their collateral. Results from the forecast error variance (FEV) decompositions indicate that the stock of closed bank deposits was as important as the money stock in terms of explaining output changes over forecast horizons from one to 10 years. Impulse response functions reveal that bank credit shocks measured by the stock variable last for about five years. Together these findings suggest that the prolonged banking crisis was a

major factor in explaining the persistence of the Great Depression into the late 1930s. Further results show that bank credit shocks had permanent negative effects on money supply and transitory negative effects on prices. Thus, consistent with recent theoretical work cited above, the dynamic effects of credit disruptions were cumulative and pervasive during the Depression. We conclude that our results using the stock of closed bank deposits strongly support Bernanke's inference that the duration of credit channel effects is important in explaining the persistence of the US Great Depression.

An important policy implication of our results is that deposit insurance can reduce the depth and persistence of severe business downturns by maintaining depositors' access to funds so that they can be reinvested quickly and efficiently in the financial sector. The deposit insurer, by acting as an asset management company, can also help mitigate asset price volatility by liquidating bank assets at a controlled, reasonable rate that reduces asset market overhang. This implication constitutes an important, often overlooked, macroeconomic role for deposit insurance beyond the microeconomic objective of containing market failure and contagion in the financial services sector.

**Higher Tier Agency Problems in Financial Intermediation:
Theory and Evidence from the Irish Loan Funds**

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Intermediation replaces the contract between saver and borrower with two tiers of contracts in which the intermediary, or bank, stands as borrower at the higher tier and lender at the lower tier. This paper examines approaches to managerial moral hazards used by the Irish loan funds, a microcredit institution which operated during the 19th century, to address the higher tier problem. Higher tier problems of corruption and incompetence by clerks and managers appears to dominate lower tier loan defaults as a source of risk to depositors. The unusual structure of the funds, which had capital but no residual claimant, allows us to focus on the role of depositors in monitoring. We show that the loan funds used a combination of high salaries and monitoring by depositors to mitigate managerial moral hazard, and that the nature of the combination varied systematically with fund characteristics. It is not, of course, possible to observe monitoring directly, and so we develop a model of the loan funds that highlights the substitutability of monitoring and compensation. This is particularly motivated by the relatively high wages paid to some clerks in the Irish loan funds. One way of interpreting these "excessive" salaries is that they were used as a kind of efficiency wage to keep clerks honest.

The Irish loan funds began in the early 1700s as a single small fund, created and managed by notable author and nationalist Jonathan Swift, to assist the "industrious poor" who could not

obtain credit elsewhere, and to smooth consumption among the poor. Swift's idea slowly took root and peaked in the early 1840s as a widespread institution of about 300 funds, although we use data from the period after the Great Famine when only about half of that number of funds operated. Their principal purpose was making small loans at interest and for short periods up to 20 weeks. The funds were limited to making loans to a maximum of £10 per person, and the average loan was £4, a little smaller than the average *per capita* income of the poorer 67% of the population. Funds were local and small; the median fund made around 1500 loans annually. Nevertheless, together they constituted one of the larger financial organisations in Ireland and at their peak were making loans to around 20% of all households annually. Profits were either retained by the fund or distributed to local charities.

Two first order conditions that capture the basic trade-offs depositors make in deciding on salary and monitoring follow from a model that is developed in which depositors have utility functions that are increasing in their own wealth and in the profitability of the fund, and decreasing in monitoring effort. First, monitoring is costly because of the effort involved, but reduces the probability of absconding, and, second, the salary also has incentive effects on the probability of absconding, but reduces the profits and hence the size of donation that the loan fund can make. It is evident in this formulation that monitoring and salary are to some extent substitutes in creating an incentive not to abscond. If monitoring is empirically important, then the relationship between salaries and capital (in this case, donations and retained earnings) will be positive; otherwise, the reverse will be true. A positive relationship thus implicitly indicates the presence of the unobservable monitoring.

The data set is a 22-year unbalanced panel with information on 145 funds. A series of panel regressions explore the relationship between the capital ratio and the dependent variable (salaries normalized by assets) controlling for a range of variables. It can be clearly seen that the capital ratio is statistically significant in excess of the 1% level in all of the regressions. There is a clear and large relationship between capital and salaries. On average, funds with very low levels of capital have the lowest salaries. Salaries increase fairly rapidly until the capital ratio is just under 50%, the range containing most of the observations.

Some perspective on the magnitude of the differences being observed can be obtained by considering the levels. Gross profits divided by assets is about 11% on average. As the capital ratio increases from zero to 50%, average expenses go from about 40% of gross profits to about 60%. This is a substantial increase and suggests there is scope to alter net profits quite considerably by increasing the efficiency of the operation. Further, monitoring by depositors appears to have been important in disciplining fund managers.

Frontiers in Economic History

Did the Trusts Have Market Power? Evidence from Distilling, 1881-1898

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During the late 19th century, the American economy was transformed by the emergence of trusts and other large combinations. Although economists and historians have studied the trusts extensively, there is little scholarly consensus as to how these combinations affected consumer prices and welfare. Broadly construed, existing studies fall into one of two camps, market-power interpretations and efficiency interpretations. According to market-power interpretations, the trusts were able to influence prices in both the short and long run because they controlled large market shares and because they acted strategically. Several studies, however, raise questions about the efficacy of the trusts' predatory strategies and suggest the trusts were only able influence prices in the short run because of market entry. According to efficiency interpretations, the trusts reduced operating costs by introducing new technologies and by exploiting scale economies. Scholars disagree as to whether these cost reductions were passed along to consumers.

The controversy over the trusts stems largely from the fact that nearly all existing studies rely on evidence that is subject to multiple interpretations. For example, authors advancing market power interpretations often cite statistics showing the trusts controlled large shares of industry production. Yet the trusts might have controlled large market shares because they adopted the most efficient production and management techniques or because they brought consumers lower prices. Data on market share could also be a deceiving indicator of market power if the threat of market entry were significant. Along similar lines, authors advancing efficiency interpretations often cite statistics showing that output expanded and prices fell with the rise of the trusts. Yet, while expanding output and falling prices are consistent with the view that the trusts reduced operating costs and enhanced consumer welfare, they are also consistent with alternative explanations and do not rule out the possibility the trusts exercised significant market power.

In this paper, we provide direct evidence on the market power of the Whiskey Trust, which dominated the production of alcoholic spirits from 1887 through 1895. At both the firm and industry level, we have monthly data on sales, output prices, and the prices of the three primary determinants of (marginal) cost in whiskey distilling: corn, malt, and the federal excise tax. We also have data that allows us to control for technical improvements that reduced the cost of distilling. With these data, market power is estimated using methods from the new empirical industrial organization (NEIO).

In the NEIO, four procedures have been used to estimate market power. The first procedure exploits non-proportional shifts in

industry demand to identify how prices respond to changes in the elasticity of demand. In a perfectly competitive industry with constant returns to scale, changes in the elasticity of demand would not induce a change in equilibrium price. A second procedure uses regime shifts. For example, in his study of a railroad cartel, Porter compares railroad rates during price wars, when rates were set at marginal cost, to rates during periods of effective collusion. A third procedure, suggested by Panzar and Rosse, examines how output prices respond to changes in factor prices. One application of this approach examines how changes in tax rates affect the price of cigarettes. This approach assumes that the cigarette industry is characterized by constant returns to scale, and by demand curves with a constant price elasticity. Given these conditions, an increase in the tax on cigarettes should cause the price of cigarettes to rise by the exact amount of the tax, if the industry is competitive. In contrast, in a monopolistic industry with constant costs and price elasticity, an increase in the tax would cause prices to rise by more than the tax increase because monopolists set prices at a multiple of marginal cost. A fourth procedure examines how changes in output affect prices. In a perfectly competitive industry with constant returns to scale, changes in output generated by shifts in demand would not affect equilibrium price.

At the industry level, we use three of the four procedures outlined above to estimate market power. First, we consider how regime changes affected price. Because the industry data extend from 1881 through 1898, it is possible to examine prices under three distinct regimes: periods of competition when there were no trusts or pools, periods when a pool effectively restricted output, and periods when the trust dominated the industry. Following Porter, we use periods of competition as a benchmark, and assume price equaled marginal cost in the absence of the trust and pool. Second, we consider how changes in output affected the price of spirits. Because distilling was characterized by constant returns to scale, changes in output would not have affected equilibrium price if the industry were competitive. Third, we consider how changes in factor prices affected the price of spirits. Given the industry's cost structure, competition predicts that changes in the tax and the price of corn and malt would have generated proportional changes in output prices.

We conduct similar tests at the firm level. In addition, the firm-level data allow us to estimate the demand curve the Whiskey Trust faced, which in itself would be revealing given the paucity of data usually available on the trusts. At both the firm and industry level, we address the concern that the trust's price-setting behavior varied over time.

Overall, the results suggest that various cost-reducing techniques implemented by distillers enhanced consumer welfare, and that the Whiskey Trust generally exercised little or no market power, although it did increase prices significantly for two short periods of time. Entry and the threat of entry prevented the trust from exercising market power for longer periods. These findings have implications for economic history and industrial organization. Regarding history, these findings

undermine narrow market power interpretations, and suggest a more eclectic interpretative framework. Regarding industrial organization, economists frequently use the trust movement to test larger questions about cartel stability, regulation, strategic behavior, mergers, and new methodologies such as event studies. Our results highlight the importance of being careful about history when using the trust movement to examine these larger questions. For example, without a close reading of the historical sources, we might have missed the fact that the Whiskey Trust exercised significant market power for short periods and left an unduly sanguine interpretation of the trust.

Why Dowries?

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Parents transfer wealth to their children in many ways. The dowry is distinctive because it is a large transfer made to a daughter at the time of her marriage. In an insightful essay, Goody (1973) proposed that the dowry is a pre-mortem inheritance to the bride. A daughter obtains a wealth transfer from her parents as her dowry whereas a son obtains his as a bequest. His observation has been confirmed in different dotal (dowry giving) societies. We develop a theory of dowries that explains his observation. Our work builds on Becker's seminal research on marriage markets and the research program on economics of the family (Rosenzweig and Stark 1997). A theory of dowry has to articulate the role of wealth in marriage. Abstracting from non-pecuniary considerations, the expected wealth that a bride or groom brings into a marriage is largely a public good within the marriage. There are two benefits to an individual by having more wealth when he or she enters the marriage market. Own wealth is a direct contribution to own welfare. Since the wealth of the spouse is a public good in the marriage, more own wealth allows the individual to match with a wealthier spouse and again gain higher utility. Thus each female uses her own wealth to compete with other females to marry as wealthy a male as she can attract. Likewise, each male uses his wealth to marry as wealthy a female as he can attract.

The standard model of the dowry, as a spot price which clears the marriage market, fits into the above wealth-matching framework. Its users often note the substitutability between dowry and a bride's other attributes in determining her expected wealth contribution in marriage. Still, the question of dowry is brought into sharp relief in the above description of the marriage market. There are many ways for parents to transfer wealth to their children. Within a dotal society, why use dowries for daughters and bequests for sons? Why are dowries used in some societies and not in others? We argue that in virilocal societies, where married daughters leave the parental home and their married brothers do not, altruistic parents use dowries and bequests to solve a free-riding problem between siblings. In virilocal societies, married sons continue to work with the family assets after their marriage. If married daughters share in the parents' bequests, the sons will not get the full benefits of

their efforts in extending the family wealth. Thus they will supply too little effort. In order to mitigate this free-riding problem, altruistic parents give bequests to sons and lump sum payments to daughters.

The model predicts that dowry contracts, which may be complicated, should not contain claims on shares of income generated with the family assets. In other words, a married daughter is not only excluded from her parent's bequests as observed by Goody. Our model predicts that she is also excluded from *inter vivos* claims on income generated from her natal family's assets. When sons have primary claims on parental bequests and daughters do not, family demographics affect dowry prices. The bride's parents have to forecast the bequest due a prospective son-in-law in order to determine his expected wealth. Similarly, the groom's parents have to forecast the likelihood of a prospective daughter-in-law getting further non-dowry transfers in order to determine her expected wealth. The number and gender composition of the siblings of a prospective spouse will affect his or her expected wealth, and therefore the dowry that she will give or the dowry that he will receive. Some historians argue that, in medieval dotal societies, parents transferred more wealth to their sons than to their daughters. A naive application of this argument implies that the dowry should fall when the fraction of sons in a family increases. Our model implies that the dowry should instead rise because daughters are concerned about being excluded from parental bequests. A theory of dowry has to explain its disappearance in previously dotal societies. As the labor market becomes more developed, as the demand for different types of workers grow, children are less likely to work in the same occupation as their parents. They are also less likely to work for or live with their families. The use of bequests to align work incentives within the family becomes less important. Since it is costly to pay a dowry, the demand for dowry (within the family) will fall as the need to use bequests exclusively for sons to align work incentives falls. Instead of the dowry, parents will transfer wealth to both their daughters and sons as bequests. So the development of labor markets will be important in reducing the role of dowries.

We test our model of dowries with two types of evidence. The primary source of evidence comes from notarial deeds and the Florentine *Catasto* (census) of 1427 housed at the State Archives of Florence. The deeds record marriages in the Tuscan town of Cortona and 44 villages in its countryside between 1415 and 1436. The Florentine *Catasto* of 1427 supplied information on the paternal households of the brides and grooms. The model's prediction on contractual form is matched against the terms found in the marriage contracts. We merge the value of dowries from the marriage contracts to family characteristics found in the *Catasto* to test the model's predictions on family demographics and dowry values. Dotal marriages in medieval Cortona support the model presented here. Currently, we do not have data on the decline of dowries in Tuscany. In general, there is little data on the decline of dowries in a society due to the large time span of historical data needed to track its decline. A singular exception is the insightful study by Nazzari (1991) who studied the decline

of dowries in Saõ Paulo, Brazil, from 1600 to 1900. Although her theory is different from ours, the factors which Nazzari considered responsible for the decline of dowries there are consistent with our model.

***United States International Trade:
Proximate Causes, 1870-1910***

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Explanations of international trade traditionally rely on the variation in relative factor endowments that are said to result in each country having a comparative advantage in a particular good or set of goods, *e.g.*, the Heckscher-Ohlin model. The predominance of trade among countries that have similar factor endowments has led economists in at least two directions: a) to seek alternative explanation for trade, *e.g.*, the Linder Hypothesis and intra-industry trade theory; and b) to disaggregate factors into more specific sub-groups. This paper explores both approaches in a search for the proximate causes of international trade in 31 industries between the United States and seven major trading partners during the period 1870 to 1910.

***Market Integration in the Age of Economic Liberalism:
Egypt, 1850-1939***

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Over the past quarter century, economic historians have made a concerted effort to examine the role of technological advances and institutional change in promoting the economic transformation of the North Atlantic economies in the late 18th and 19th centuries. With a few exceptions, the countries of the Southern Mediterranean Basin including the Middle East have yet to be fully integrated into this research agenda even though the engines of global economic integration in the late 19th century were operating with force in much of the region. The expansion of trade with Europe and North America necessitated investments in modernizing transport infrastructure, which in turn required capital inflows from abroad. The commercialization of society which accompanied integration into the global economy often induced fundamental changes in the social and economic institutions governing society.

These dynamics were clearly present in the economic history of Egypt from the mid-19th century through the inter-war period. While becoming gradually integrated into the global economy, Egypt was increasingly governed by a liberal economic ideology that fostered the growth of free markets and narrowed the role of the state to building legal institutions and transport infrastructure for the market. This process was sparked to a large extent by the rapid expansion of cotton exports and the invasion of European

imports into Egypt. The growing, processing, and marketing of cotton and other cash crops involved extensive marketing arrangements between rural peasants, interior merchants, city buyers and export agents in Cairo and Alexandria. It also necessitated a revolution in the transportation system that would give the state an active role in the creation and integration of domestic markets. The marketization of the economy received further stimulus with the introduction of reforms affecting the institutions of private property, administration and regulation, and foreign participation in the economy. Did Egypt's age of economic liberalism coincide with the rise of unified market economy? Did the revolution in transport infrastructure and legal systems promote efficiency in local and regional trade? These questions remain largely unanswered despite their centrality to evaluating the achievements of Egypt's experiment with economic liberalism for over a century prior to WWII. With a few exceptions, economic historians by and large have been skeptical of the efficient working of Egyptian commodity markets, even while acknowledging the importance of the noted transportation revolution and legal reforms. This paper presents empirical evidence on the integration of commodity markets during this unique period in Egyptian economic history.

The historical setting of inter-war Egypt is unique since it allows us to abstract from trade barriers, taxation, and other factors suspected of undermining the empirical validity of the law of one price (LOP). We provide a rich test of price integration using a high-frequency panel of prices for a large basket of essential commodities. The data consists of quarterly retail prices for 26 commodities across 34 markets during the period 1921-39. Our sample of 34 cities spans the entire administrative and geographical map of Egypt: 13 are in the Delta, 17 are in the Nile Valley, and the remaining four are coastal cities on the Mediterranean and Red Seas. We are able to reject the random walk null hypothesis (no market integration) for all but four commodities at the 95% level. We find rates of convergence to LOP that are considerably higher than those found in cross-country and time series examinations. In fact, we show that Egyptian commodity markets in the early 20th century were as closely integrated as those in the US in the late 20th century. Finally, not only do transportation costs explain the extent of price integration, but they also condition the rates of convergence to LOP.

Although the empirical results in the present investigation of price integration in Egypt are still preliminary, a number of conclusions are evident. First, previous notions of segmented markets in local trade must be revised in light of the overwhelming evidence of market integration. Second, the revolution in transportation and communications has contributed to the creation of a nationally integrated economy. Finally, the liberal economic experiment in inter-war Egypt should not be judged only on the basis of its failure to industrialize the economy.

German Cliometrics *continued from page 1*

cliometric work and German economic history journals are willing to publish cliometrics papers.

In the discussion, Paul Hohenberg (RPI), speaking from editorial experience, called on cliometricians to refashion their prose and write for a wider audience; this could only improve the product. Richard Tilly observed that receptiveness to quantitative and theoretically oriented work is growing in Germany but noted that since the real power in economics departments lies with business administration professors, more effort should be devoted to topics in business history to get their attention. Joachim Voth (Pompeu Fabra, Barcelona) wondered how Komlos's recommendations could do anything about the structural problems. Albrecht Ritschl (Zürich) suggested that the true problem might lie with the economists who do not want to go back to the Historical School, but whose interest might be sparked by studies of the major role of economists in policy-making in the Third Reich. Ritschl also pointed to a dilemma of publishing: Germans might want to publish in English to become better known in the wider world, so perhaps the non-Germans should direct their efforts toward publishing in German. Stephan Klasen (Munich) suggested we try to interest more of our economics colleagues in doing cliometric work and that cliometricians should try to publish more in economics journals. David Good (Minnesota) closed by referring to the problem of audience: if social and cultural historians are dominant in history departments, then cliometric studies of such topics as nationalism and nation building might interest them.

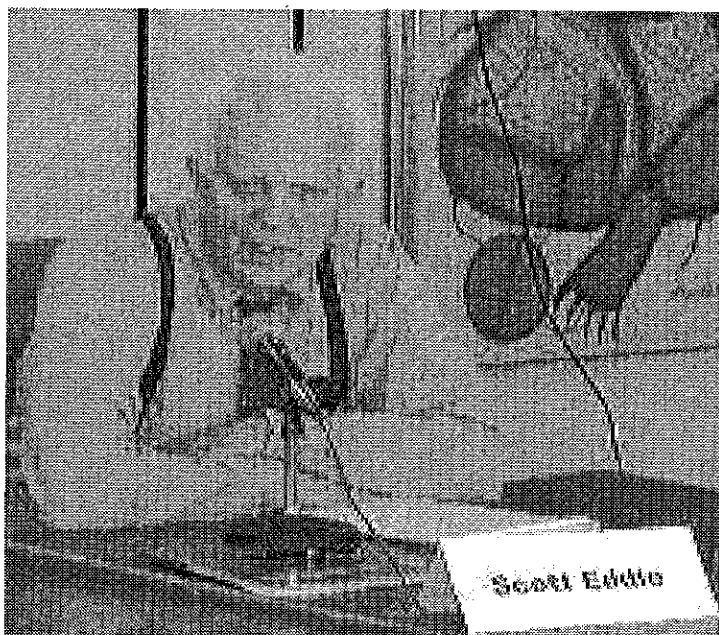
After his talk, Komlos presented a token of appreciation to Scott Eddie for organizing the conference. Thanking Komlos, Eddie noted how inspiring was the enthusiastic response to the call for papers and re-emphasized the importance of Komlos's call to publish in Central Europe.

Kris Inwood (Guelph) moderated the second session, on "Long Term Comparative Performance." The first paper, by Stephen Broadberry (Warwick), "Human Capital and Productivity Performance: Britain, the United States and Germany, 1870-1990", considers Britain's relative economic decline with respect to productivity and human capital accumulation. When human and physical capital

are considered substitutes, German overtaking can be attributed to an expansion in vocational training. When they are considered complements, the growth in higher education in the US leads to "social capabilities" that explain its growth.

Inwood asked Broadberry to elaborate on the adoption of apprenticeship in Germany *versus* its relative absence in Great Britain. Broadberry distinguished between intermediate and higher level outcomes

and noted that changes in flows of training act as leading indicator of changes in stocks. Komlos asked why Broadberry aggregated labor skills in terms of units of unskilled labor rather than, say, the value of the inputs that go into producing human capital. Broadberry replied that the capital stock values could be obtained from flows. Joerg Baten (Munich) thought that the construction of the labor productivity estimates might result in a spurious negative contribution of human capital to service-sector productivity in the German-UK comparison. Broadberry replied that the low German service-sector productivity estimates come from benchmarks at points in time (especially 1935) and time series in between; several other scholars have found low service-sector productivity. Voth raised two issues, asking first about the lag between vocational training and the time its benefits can be observed and how low the cost would have to be to yield a net gain. Broadberry suggested that a sectoral breakdown would show that the payoff to people would come at a time appropriate to their discount rates. Second, since working hours in Germany have fallen over recent decades, how would this affect the flow of benefits from skills in the workforce and the incentive to invest in



Conference Organizer Scott Eddie

human capital? This flow is recent and its effect would take a generation to appear; moreover, the lack of observable effects may be the result of government subsidies. Finally, Michael Reutter (Munich) also raised two questions, the first about the role of involuntary unemployment in the development of German labor productivity compared to the UK. Broadberry replied that this is unlikely to have a large effect, since before 1973 there was full employment in Germany, and, up to 1990, there was a rising trend in Britain's involuntary unemployment as well. Second, how do low levels of higher education in Germany contrast to university enrollment expansion? Broadberry had taken at face value the OECD data which show increasing university enrollments for most countries, but also that both Germany and the UK have lagged behind the US. Klasen offered an explanation that the data are driven by the percent of a cohort that goes beyond secondary education; this ratio is small in Germany compared to Britain and the US.

In the second paper, "The Economic Growth of Central and Eastern Europe in Comparative Perspective, 1870-1989," David Good (Minnesota) and his co-author Tongshu Ma (Utah) construct GDP data for present day Central and Eastern European countries in order to address the discrepancies in measures of their long-term economic performance which appear in previous work.

Hohenberg wondered about the discrepancy between the modest growth shown for Austria in the Good-Ma paper compared to the depression in the same period in the Schulze paper that followed. Sean Rogers (New Brunswick) suggested comparing the convergence of a given economy to an appropriate counterpart (*e.g.*, Australia to Britain) on grounds of a close trading relationship instead of making the traditional comparison to the USA. Cynthia Taft Morris (Smith) suggested it was inappropriate to use the convergence literature as the standard for interpretation. Differences in institutional capacity for growth are not reflected in GDP *per capita* figures; thus low GDP *per capita* does not necessarily mean a higher growth potential. Good agreed. Several attendees voiced data concerns. Komlos questioned the basis for the new growth rates for Austria-Hungary. Good responded that proxies were used for early data. He also noted that both Germany and Austria had the same ratio of GDP relative to the USA in 1870, but that in 1989 Hungary had only half the GDP *per capita*. Both of these seemed odd. Broadberry suggested an examination of the data across war shocks to see if growth was in fact poor, since the series are not well linked. John Brown (Clark) wondered

whether, because of their structure, the East European economies had not been more sensitive than other economies to the energy shocks of the 1970s. Ritschl pointed to recent downward revisions of the economic performance in East Central Europe, which were not reflected in the 1995 Maddison data. Both Inwood and Terence McIntosh (UNC) asked how exactly the break points were estimated. Good answered they came out of the data, rather than from choosing the 1973 oil shock *a priori*.

The final paper of the session was "Patterns of Growth and Stagnation in the Late 19th Century Habsburg Economy" by Max-Stephan Schulze (LSE), using new income estimates for Austria and Hungary. He finds that Austria grew at a slower pace in earlier periods than previous estimates had calculated, but at a faster rate from 1890 to 1910. Hungary's growth rates were similarly revised downward, but remained faster than Austria's and ranked at about mid-European levels. The slow pace of labor migration into an expanding industrial sector might be a possible explanation.

Inwood thought the shift of economic momentum from West to East in the late 19th century and the slowness of structural change in the economy raised interesting issues. Klasen asked about the similarities between labor productivity in services in Austria and Germany. Schulze replied that a problem with the labor force data is that they provide only a rough estimate of productivity and thus similarities are not extractable. Ritschl asked about the downward revision in growth rates in the depression period, wondering whether the implicit assumption of a constant share of wages in value added was defensible. Schulze replied that this assumption does not introduce bias into the main sector affected, mechanical engineering. Komlos then asked about the possibility of doing an estimate for the service sector independent of Kausel's. Schulze responded that Kausel's results are based on previous estimates by Rudolph that had massively overestimated the rate of growth over the period as a whole. Schulze also examines a much larger number of sectors than did Kausel, uses different weights for the contribution of each sector to overall growth, and includes additional sectors that were disproportionately affected by slowdowns but not by upturns. Together, these account for the drastic reduction in the growth rate. Schulze noted that service sector estimates warrant as much revision as those for industry, but that, currently he has produced only rough "guesstimates" for Hungary.

The third session, moderated by John Munro (Toronto),

was on the topic of "Demographics and Migration." Stephan Klasen's paper, "Gender Bias in Mortality in a Comparative Perspective", focuses mainly on sex differentials in life expectancy. In most industrialized countries, women have had an advantage in life expectancy in the 20th century. This is not true for some underdeveloped countries and, more importantly for this study, for Germany and other countries in the 18th and 19th centuries. Several theories have been advanced to account for Excess Female Mortality (EFM). This paper analyzes those theories, contrasting their repercussions with recently assembled data on sex-specific mortality rates from a large number of countries. Economic theories of intrahousehold resource allocation (IRA) seem to account for the EFM effect. Germany does not appear to be an outlier when compared with other countries over time.

Munro pointed out that Klasen's position differs from the accepted view of Jan de Vries regarding the importance of maternal mortality in the EFM. According to Klasen's work, EFM is found mostly in rural, peasant economies with peculiar IRA patterns. Munro concluded by noting the importance of the relationship of changes in EFM and changes in the demand for both female and child labor. This issue, as well as the relevance of age groups in EFM ratios, is open as a question for Klasen to address. Klasen began his reply by mentioning the mortality differential between married and single people, which is higher for women than for men. Likewise, it is important to compare European and non-European societies. On female mortality for those aged five to 15, he pointed to factors such as tuberculosis, although no economic model has addressed this issue. Voth asked why the "East" model life tables were used rather than other models. Klasen responded that the choice of table used made no difference to the results of the work; the "East" seemed to adjust more to the demographic characteristics of this population. Mark Hallerberg (Pittsburgh) inquired about other influences on mortality in Klasen's work, such as religion. In addition, it seems plausible that men from towns looked for rural women, so that there was a higher migration of women to the cities due to this factor. Klasen said he found no significant differences beyond a slightly lower mortality rate in Protestant areas.

The second paper was "Urban Demographic Stagnation in Early Modern Southwest Germany: A Computer Simulation", by Terence McIntosh. The paper analyzes the historical demographers' general contention that immigration was necessary to maintain or increase the population of cities, because of the high urban mortality rate.

The intricate relations between demographic characteristics and behaviors of different groups mixing in the cities complicate the study of this phenomenon. The work of Allan Sharlin is discussed, and McIntosh comes to opposite conclusions about whether "the law of natural decrease" applied for small towns.

Munro began by asking about the relevance of median age of marriage, since McIntosh shows that a significant proportion of brides were recent migrants. He proposed discussion of the importance of servants in the model and the contrast of these findings with those of Sheila Ogilvie. Also, why was 10,000 inhabitants chosen as the dividing line between small towns and cities for this analysis? McIntosh explained that 10,000 was a direct result from the data and not from theory, but that this number may in fact be irrelevant for the analysis. Regarding servants, his work focuses on the net reproduction rate, whereas servants were only a flow to and from the town, so they did not affect the main demographic variables, particularly the net reproduction rate. Finally, regarding the work by Ogilvie, the towns were not as closed as previously thought, and mobility from the countryside was significant.

Simone Wegge (Lake Forest) closed the session with her paper, "Self-selection of Nineteenth-Century German Emigrants: Evidence from Nineteenth-Century Hesse-Cassel." Wegge's work focuses on how the groups migrating from Hesse-Cassel to the USA were self-selected according to their social and economic position. She selects three main groups, farmers, artisans and laborers, for this analysis. Farmers had enough wealth to migrate, but perhaps lower incentives to do so. Laborers, although they may have had higher incentive to migrate and higher expectations in the new country, did not have the liquidity to pay for travel and settlement. Artisans were the most likely to migrate, since their liquidity constraint was not so strong and they may have had a higher disposition to migrate. Moreover, their main wealth was human capital, which is more easily transferable and lucrative in a new country.

Munro referred to the importance of German migration to the US, in which Hesse-Cassel was a special and important case. While underlining how this work presents a clear analysis of migration factors for each social group, he also inquired about data availability to expand the analysis to wage differentials between the US and Germany, as well as to differentials in land values. Wegge noted that 1848 was not important in Hessian emigration.

It seems the data are not complete for this year. From 1852 onward the data improve and include illegal emigrants. The ideal analysis would be to follow persons or groups closely to their new positions and real wages in the US, but the data required for this are not available. There are some data on land prices, but only for some regions, and not through time. Komlos commented that Wegge's model works well for laborers and artisans, but does not seem to do so well for farmers, and asked about ways to modify the model in order to account for this anomaly. Mass migration in a short period of time did not occur, and therefore the suggestion of fire sale prices for land is not easily supportable. Another issue is that land may be an argument in the utility function of the farmers, which changes the economic scenario for migrating. Along the same lines, migration may be motivated by changes in relative rather than absolute utility. Voth asked about the role of institutions in migration. Broadberry pointed out that available data suggest a huge difference in land prices between Germany and US in the middle of the 19th century. Klasen noted that some authors have found strong evidence against assuming that human capital has the same value in a new country, and very often the occupations (and especially wages) are far from those in the country of origin. With regard to unclear motivation for farmers, Wegge noted there are factors beyond mere transactions costs affecting farmers' decisions: social status, closely correlated with land ownership, makes this group less likely to move.

Closing Friday's sessions was "Credit and Finance", moderated by Kurt Huebner (York). The first paper, "Company Law, Stock Market Regulation, and the Development of the German Financial System, 1880-1913", by Caroline Fohlin (Caltech), examines company law and stock exchange regulations from the 1870s until the onset of World War I. It also investigates the impact of the legal framework both on the functioning of the exchanges and on the development of universal banks. Furthermore, issues of whether combining securities underwriting and trading with commercial banking services would naturally lead to the internalization of securities trading within banks, and of how such market internalization affects secondary market and banking institutions, are considered. Fohlin also examines whether the structure of the German financial system resulted at least partly from regulatory pressures in the early industrial period rather than as a natural response to economic backwardness, as hypothesized by Alexander Gerschenkron.

Huebner opened discussion by raising the issue of time

path dependence of the empirical results. Ritschl was concerned about the lack of control for changes in the volatility of stock prices, considering the increasing volume of German stock trading within the period covered. He suggested that in order to show the real result of the German financial change, the regression should use the tax rate rather than the presence of the tax. Broadberry argued that the real effect of the 19th-century German financial revolution was similar to the situation in Britain; *i.e.*, the real effect of the revolution was from the side of the retail banks, not the investment banks.

In "Germany's 'Financial Revolution': The Importance of Links Between Public and Private Finance in the Early Stages in the Early Stages of Industrialization," Richard Tilly applies the concept of "Financial Revolution" to 19th-century German history. The paper focuses on the extent to which German states adapted their financial policies to the norms of financial capitalists, and, with much evidence from Prussia, on exploring how the observed changes in state financial policies might have affected the behavior of financial capitalists. Further, the paper emphasizes capital market pricing that partially covers the evidence documenting the connections between public and private finance.

Michael Haines (Colgate) said that what Tilly had pointed out about the monetary union of the Zollverein was a very important issue, that Prussia essentially bribed the smaller states to give up their independent currencies in return for a share of the Zollverein revenue. He then wondered if there were real lessons here for the current situation in Europe. Temin was skeptical of the implicit idea of "hard money" in Tilly's paper. Klasen and Broadberry both criticized the method of comparison on which the analysis was based, suggesting that Tilly's paper does not provide sufficient reason to justify the sudden comparison of the financial revolutions of Germany and of Great Britain. Broadberry argued that the nature of the two revolutions was so different that one could not get much significant inference from the comparison.

Saturday began with the fourth session on "Labor, Education and Growth" moderated by Cynthia Taft Morris. Oliver Grant (Oxford) opened the day with "Max Weber and *Die Lage der Landarbeiter im ostelbischen Deutschland*: a statistical examination", which analyzes agricultural prices, wage data and budget studies. It employs traditional historical analysis to evaluate Max Weber's view of why the dominant household economy east of the Elbe was largely replaced by a system of wage

labor, in which mostly seasonal Polish workers were replacing German contractual workers. Weber argued that institutional changes altered the attitudes of Eastern landowners, which also resulted in the introduction of sugar beet cultivation. In his paper Grant also looks at alternative explanations for the change in the contractual system.

In her comments, Morris said that the conclusion drawn by Grant about Weber's proposition on the importance of capitalist attitudes is too strong. Haines also commented that the growth of labor demand in industrial regions could well indicate that the movement to sugar beet cultivation could be endogenous: that is, in response to German flight from the land, employers decided to grow sugar beets and import Polish labor in the short term. Finally, Wegge

noted that Grant mentions a difficulty in measuring farm size because workers not only spent some time on their own farms but also on the farm of the landowner; she suggested it would be helpful to obtain some sort of measure of how much of this was still going on after 1907. Grant responded that the distribution of work by size of farm could not be measured by the statistics currently available.

In the second paper, "Job Tenure and Demand Dynamics during High Industrialization: The Case of Germany before WWI", John Brown estimates a hazard model for job quits from the official labor registers for over 11,000 workers. He then estimates separate distributions of job quits for entering workers by age in order to account for high labor market flexibility during the rapid industrialization before World War I.

Morris first noted that Brown's use of the hazard model works less well on the supply-side influences of job tenure because the economic approach does not take into account major long-term supply side changes; his cross-section view of job quits cannot necessarily be

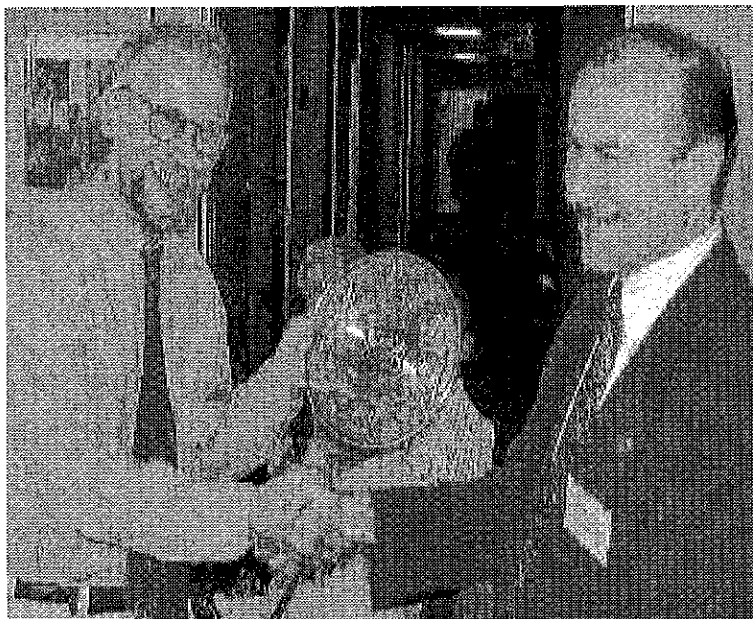
expected to represent individual behavior well over time. Klasen pointed to the demand-side story and indicated that information about either output or employment of the firms needs to be given, since when workers are fired and firms reduce employment, this is not a voluntary quit but a demand change. Brown admitted that he had no

definite structure for the demand side, but thought one solution might be to adjust the hazard by including a variable for employment demand shifts for every year, but doing so would be complicated.

In the third paper, "Educational Development and Labor Markets: The Case of Higher Education in Germany, 1820-1941", Claude Diebolt and his co-author Bachir El Murr (both of Montpellier I) develop a mathematical model to analyze the cyclical

behavior of student enrollment in German universities. The core hypothesis is that there was an organic reciprocal relationship between training and employment, and their aim is to analyze econometrically the dynamic relationship between the number of students enrolled and the labor markets.

Moderator Morris reminded us that long-term dynamic influences often cannot readily be incorporated into econometric models, that in a changing economic environment one would expect the weights of different causal influences to change over time. Voith commented that because of the estimation of the cross elasticities, he did not feel that what drives the model is what the authors claim – namely, the deterioration in the benefits in entering a profession after a significant number of people have done so earlier. He proposed that what the paper shows is that some cycles are not fully synchronized between two different courses of study, which is different from what the authors state. Diebolt replied that in the future he would try to produce a dynamic cobweb model that could create a better understanding of the phenomenon.



Richard Tilly receives a lifetime achievement award for his pioneering and continuing work in German cliometrics from Conference Organizer John Komlos.

Hal Hansen (Harvard) concluded the fourth session with "Artisans and Industrialization: The Craft Character and Sources of Germany's Modern System of Education and Training." Hansen applies traditional non-quantitative analytical techniques to contrast the historical causes, which he believes are quite efficient in modern Germany's system of regulated location of training and certification, with market models that are less well suited for analyzing historical problems of collective action in labor markets.

Morris observed that Hansen's paper brings out a part of German development that has not been explored very widely and that it emphasizes things that econometric models handle very poorly. Klasen noted that the compulsory systems of craft educational training and certification are seen as an advantage in the economy, but that there is no mention of their costs, for example, who bears the burden of the restrictions to entry? Hansen agreed that the costs should also be examined and added that policy makers tried not to restrict entry but only to restrict who was certified to trade. Wegge suggested that Hansen should go back further in time and evaluate whether there was a path dependence aspect to industrial development that was related to regional differences in guild practices.

Saturday morning continued with a session on "Agriculture and Agricultural Markets", moderated by Sean Rogers. It began with "Rational Investment Behavior and Seasonality in Early Modern Grain Prices", by Michael Reutter with Ulrich Woitek (Glasgow) and Walter Bauernfeind (Munich), in which the authors put an investment model on trial. They analyze grain prices in Nuremberg from 1490 to 1855 and tackle the issue of whether storage behavior can actually be described by rational investment models. The evidence presented indicates that grain prices did not move in the expected saw-toothed pattern.

Rogers cautioned that although the investment model is a supply-side model, short-run demand factors should not be ignored. Reutter replied that if demand fluctuations are foreseeable the supply should react such that, in equilibrium, arbitrage conditions are fulfilled. Ritschl pointed out that, since the market for grain was heavily regulated, one could look at inter-regional price movements across time. Finally, Wegge suggested that the authors could observe another market in Germany, where the market set the prices of grain, and then compare it to the one that was analyzed.

The second paper by George Vascik (Miami U.), considers "The Role of Sugar Production in the 'Agrarian Crisis' of the 1890s." Vascik uses the beet sugar industry, which not only monopolized the market but also was a major exporter, to test the revisionist hypothesis that a short but sharp downturn in cereal prices between 1892 and 1894 was transformed into a "crisis" of agriculture by those pursuing their own political and ideological beliefs. After observing the evidence available he concludes that this "crisis" could be seen as only partly economic; it was mostly political and psychological.

Grant asked how competitive would the German sugar industry have been if there had been free trade? Was this system benefiting the whole industry or just the export-oriented large mills? Vascik indicated that sugar exports were important to the German economy, and he believed that the sugar industry would not have grown as extensively as it did without the large export market, since domestic German sugar consumption was extremely low. Hallerberg recommended that Vascik extend his paper from a narrative to an analytical narrative. Munro suggested constructing a regression involving livestock prices, especially when there was a significant shift in relative price movements in favor of livestock (the obverse of the shift in prices against grains) from the late 1870s and 1890s. He also suggests that the regression in the paper could use sugar prices for both beet and cane sugars from external sources.

The final paper of the session, "Grain Price Fluctuations and Witch Hunting in Bavaria" by Joerg Baten, written with Ulrich Woitek, develops and tests, with the use of the Poisson regression model, competing models of the determinants of witch hunting (represented by trials and accusations in Bavaria in the period 1345-1750). The focus is on grain price fluctuations and their influence on the intensity of prosecution. Their evidence shows that a moving average of grain prices was in fact correlated with the number of accusations in the late decades of the 16th and the earliest and last decades of the 17th centuries.

Rogers commented that the paper presents a neat economic model of witch hunting, but that the problem with using the Poisson distribution is that the mean of the variable observed is equal to its variance. If, in reality, this were not correct, then there would be over dispersion. Perhaps a threshold model could be used, in order to obtain some mean level of witch burning that occurred each year and then explain deviations from that average. Baten responded that there indeed is a threshold, which

can be seen by the close correlation in some years. Broadberry contended that grain prices could in fact be innocent, and recommended that the authors formulate an alternative model against which they could run their present model and as a result show that their current model yields a better fit. Komlos remarked that in exceptionally rainy weather rye tends to mold and during high prices people are more likely to consume moldy rye which could cause hallucinations; there is, in fact, a theoretical link between high grain prices and witch burning.

Saturday afternoon began with the sixth session, a "Workshop on Data and Methodology", moderated by Gillian Hamilton (Toronto). In contrast to the other sessions, each author was given a few minutes to talk about his data and the methodology. Scott Eddie opened the workshop with his paper "Estimating the Value of Land from Prussian Wealth Tax Data." Eddie explained his procedure for estimating the market value of land in Prussia by using recently discovered price data from individual land transactions that originally were collected for purposes of the 1893 wealth tax.

Hamilton began discussion by asking about data quality; were there systematic biases (e.g., in discounts for relatives)? Vascik wondered why landowners had any reason to hide land values, and Munro requested explanation of the purpose of the graph of the ratio of crop price index to animal products price index. Eddie answered that there was really no opening for nepotism, since there was nothing to gain for either the assessors or the landowners. As a check, he had estimated the discount for relatives from actual, rather than net, price data; it amounted to around 20%. The motivation behind his work was to find out how much of the national wealth landowners had owned, given both the enormous increase in non-agricultural wealth in 19th-century Germany and the persistence of the landowning elite at the top of the socio-political pyramid. Wegge asked whether small properties had sold for more per hectare because they tended to be closer to town. Eddie replied that small properties were indeed likely to be located closer to town, but that what was interesting were the marginal prices: other things equal, people paid more per acre for a large property (over 100 ha.) than for a smaller one. McIntosh asked how the methodology was to be expanded from one district to all of Prussia, while John Brown—marveling at Eddie's high R^2 —asked whether there was room for von Thünen. Eddie replied that he was attempting to estimate in small steps. The next step would be estimations for other districts, then finally he would estimate values for each district of

Prussia. Because a riding (*Kreis*) is normally so small, locational factors are minimal; hence von Thünen gets little play.

Ben Tipton (Sydney) then presented his paper, "Tales of Hoffmann: Output and Labor Productivity in German Industry, 1850-1937", which shows why Hoffmann's estimates need revision. It offers a factor income alternative to physical product estimates. He pointed out that some historians both use Hoffmann's estimates uncritically and then make statements, such as the importance of 1871 as a turning point in German economic history, that flatly contradict what Hoffmann's data show.

Both Broadberry and Ritschl expressed reservations about the accuracy of income-based estimates of total output. Broadberry noted that the ratio of capital to wage income varies over time and across sectors, and even over the business cycle in the same sector; Ritschl argued that data from tax revenues are likely better than either income- or product-based estimates. Tipton replied that he had used Hoffmann's own ratios to make the point about how different estimates produced widely-varying results, and guessed that his estimates would not be very sensitive to alternative assumptions in any case. Information on prices would be the key to sorting out divergences among estimates, especially for the pre-1890 period when tax-based estimates are not available.

Michael Haines's paper, "Religion, Statistics and Demographics," deals with the relation of religion and culture to biases in reported data. In particular, they seriously affect the accuracy of data on stillbirths and suicides. A comparison of Catholic and Protestant areas of Germany demonstrate the point.

Good asked about the mechanism by which stillbirths were turned into alleged live births followed immediately by infant death, and pointed out that culture always affects the way data are reported. Hamilton asked if there were quality variances by region or by rural/urban that were independent of religion. Klasen explained how priests were called in to baptize babies during difficult births, partly in order to avoid having to bury them in unconsecrated ground. He also asked about the treatment of miscarriages as well as stillbirths, mentioning Wrigley's recent contention that English fertility increases in the 18th century were mostly the result of a decline in stillbirths, not an increase in pregnancies. Haines noted studies that showed certain factors (e.g., age of mother, venereal disease, other prenatal difficulties) are strongly

associated with stillbirths, and that the male/female birth ratio also increased as a result of the decline of stillbirths. He offered other examples comparing largely Protestant Saxony with its largely Catholic neighbor, Bavaria.

The session's final paper was from René Schiller (Technical University of Berlin), "Results of Database Supported Socio-economic and Cultural Analysis of the Owners of Prussian Landed Estates (the Example Brandenburg)." The paper makes two main points. First, relational databases prove to be a good means to link difficult data sets, and, second, there are strong biases in Max Weber's work about the Junkers, which has become the conventional wisdom in German history. Schiller commented that Weber could not be objective about his ideological enemy and that his positions were based on the results of a single set of questionnaires filled out by Junkers themselves. So, much of his work is simply wishful thinking about the Junker class.

Broadberry asked whether the British pattern of aristocrats marrying into bourgeois wealth as a method of maintaining their estates was also common in Prussia. Schiller responded that marriages of nobles to non-nobles were very rare in Prussia. Vascik raised the issue of defining the nobility, and Voth wanted to know what the standard of comparison was in Schiller's remarks about the relative "openness" of the Junker class. Ritschl pointed out that the data do not necessarily contradict the notion of the decline of the Junkers – what of the changes in composition of the noble class itself, through new ennoblements? Schulze suggested that it would be useful to get data on the asset composition of the wealth of the nobility, perhaps from probate records, to see if they had significant holdings of non-landed wealth. Schiller commented that the gentry/aristocracy distinction was not found in Prussia. Around 1900, there were about 3,500 noble land owners out of about 80,000 noble persons, among whom was virtually no one ennobled in the 19th or even the 18th centuries; most were very old families.

Saturday concluded with the eighth session, on "Weimar Economic Performance and the Great Depression", moderated by Paul Hohenberg. The first paper "Peter Temin and the Onset of the Great Depression in Germany: A Reappraisal", by Albrecht Ritschl, presents new data supporting Temin's view that the Great Depression in Germany was not triggered by the drying up of American credit, as the conventional wisdom holds. Ritschl uses domestic orders of the German machinery industry as a leading indicator of business sentiment.

This and adjusted stock prices show a downward trend from the middle of 1927, suggesting that the economic outlook turned sour in Germany. At the same time in the US, stock prices continued to rise, accompanied by a strong investment demand, thus showing that the Depression reached Germany first. The author also finds that capital imports slumped in Germany before US capital exports peaked, calling the notion of an American-bred recession into question.

In the second paper, "With a Bang, Not a Whimper: Pricking Germany's 'Stockmarket Bubble' in 1927 and the Slide into Depression", Hans-Joachim Voth examines one of the possible starting points of the recession: the stock market crash of 1927. The crash was caused by the intervention of the Reichsbank, which feared the consequences of a speculative asset-price bubble. Voth believes that the bank's move was a mistake because it cannot be proved that stocks were overvalued. The spectacular rise in prices preceding the crash can be explained by the low interest rates of the period, and by the normalization of the economy. Formal tests reject the hypothesis that share prices were too high relative to fundamentals.

The two papers were discussed jointly. Klasen mentioned an alternative interpretation of the results: in 1927 there was indeed a recession in Germany caused by domestic factors, but in 1929 international forces entered the picture, blocking recovery and leading to the Great Depression. Ritschl admitted that his data make it possible to forecast the definitive slump only from May 1929, while Voth pointed out that the real effects of the stock market crash were not of sufficient magnitude to explain the Great Depression. Temin joined the debate, expressing his view that the distinction between a business cycle recession and the Great Depression is false. The Great Depression began as a cyclical recession that was later worsened by policy mistakes, but the Big Slump cannot be traced back directly to the causes that the two papers focus on. Temin asked the authors why they had not stressed the importance of international capital movements, a factor he considers crucial. He mentioned that excessive capital imports in 1927 were among the main reasons why the Reichsbank felt it necessary to step in. Voth acknowledged the significance of capital imports, but he regards them as transmission mechanisms, not as autonomous factors.

Christian Stögbauer (Munich) investigates how the worsening economic situation affected the electoral results of

the Communist and Nazi parties in "Measuring the Impact of the Depression on the Radical Vote in the Weimar Republic: A Spatio-temporal Approach." First, the voting behavior of the individual is influenced by many factors: where he lives, to which social group he belongs, and how much he earns, to name a few. In order to identify the impact of changing economic circumstances, one has to control for all other possible factors. It is a hard question, however, which particular variables can satisfactorily represent the effect of the social structure. Instead of choosing specific theories, the author uses a purely exploratory technique to control for socio-structural variables. His figures verify the results of preceding studies, that in cross section unemployment was negatively related to the Nazi vote.

Hohenberg suggested this outcome could be due to the fact that the NSDAP enjoyed greater popularity in rural areas, where unemployment remained more hidden than in big cities. He added that rising unemployment over time did increase the number of people who voted for the Nazis. Stögbauer replied that, while the urban/rural difference in voting patterns exists, it does not explain the result because that was one of the variables for which he had controlled. Broadberry remarked that the changing signs of unemployment and income in the regression make it hard to interpret the results.

To conclude the Saturday sessions, Arthur Van Riel (Utrecht) presented "Wage Formation, Economic Policy and Labor Market Performance in Germany, 1925-1936", which he wrote with N. H. Dimsdale (Oxford) and N. Horsewood (Oxford). They argue that the dramatic rise in unemployment during the Depression was due to two coexisting factors. Although politicized arbitration in the labor market was a structural deficiency that led to inertia in nominal wages, its effects were felt only under the deflationary monetary policy resulting from the debt crisis, when decreasing prices together with sticky salaries drove real wages excessively high and caused mass unemployment.

Ritschl raised two questions concerning the data. First, how did Van Riel correct the official employment data that were subject to a reporting bias? Second, because official wage data in the Depression did not reflect true salaries, how had the authors computed their wage figures? Van Riel responded that he had used corrected employment data, and that his wage figures were the sum of wages divided by these employment figures.

The ninth and final session was held on Sunday with Loren Brandt (Toronto) as moderator for the topic "Institutions, Politics and Economics." The session began with Mark Hallerberg's paper, "The Political Economy of Taxation in Prussia, 1871-1914." He concentrates on how political institutions affected the mix of taxes levied by the state. The electoral system assured that large landowners controlled the parliament, which had the power of determining most of the direct taxes, and the tax code was consistently changed to reduce the burden on land. Prussia could afford to maintain low land taxes because it had extra revenues from railroad taxes.

Baten mentioned the possibility that taxes on agriculture were low simply because the return on agriculture was modest. He cited estimates from official data, according to which the return on land was around 1%, while on industrial investment it was around 11%. Eddie commented that, assuming well-functioning capital markets, such a disparity is hardly believable. Direct yields from agriculture probably were just part of the benefits that a large landowner enjoyed. Capital appreciation and increased social and political status also played an important role that one cannot ignore. To illustrate the notion that extra rights were capitalized into the price of large estates, he pointed out that, *ceteris paribus*, large estates sold for more per hectare than small ones. In his pilot riding a seat in the local assembly seemed to be worth about 20,000 marks, since that was the premium paid for a knightly estate. Hallerberg supported Eddie, arguing that observed sales tax increases imply increasing land prices, and thus capital appreciation.

In "Creating Firms for a New Century: Determinants of Firm Creation in Southern Germany around 1900", Joerg Baten uses historical data to find answers to a current question: What makes entrepreneurs? He first performs a standard regression analysis that confirms the findings of studies related to firm creation. He also finds a strong autocorrelation in firm creation rates, while the parameters of the seedbed effect and agglomeration effect became insignificant when the level of aggregation was the same as in other studies using recent data.

Brandt remarked that the number of small and medium enterprises (SME) that was used as a proxy variable for the seedbed effect could be endogenous, since high creation rates can lead to high number of small enterprises. Brandt also mentioned that attention has been concentrated on firm creation because of its supposed link with economic growth, but an enterprise contributes

to economic performance only if it survives, so survival rates are the interesting objects of study. He asked if there is anything about survival in the data set. Baten answered that he recognized the problem of endogeneity of SME numbers, and that he had performed some tests. When he included the lagged creation rate in the regression as a dependent variable, the parameter of SME number remained significant when he was examining the data on the level of cities. Voth added that human capital also might have a positive role in the birth of new enterprises. He suggested the proximity of educational institutions as a proxy variable. Baten responded that he had checked for educational institutions, but their impact was highly correlated with the agglomeration effect. Wegge asked whether proto-industrialization had had any effect on firm creation, to which the author replied that proto-industrial activity might have had a beneficial effect on growth, but, by increasing the demand for labor, it made conditions more difficult for new enterprises.

Closing the conference was "Why Chamberlain failed and Bismarck succeeded: The Political Economy of Tariffs in British and German Elections", in which Douglas A. Irwin (Dartmouth) and Adam Klug (Ben-Gurion) analyze the elections of 1906 in Britain and 1877 and 1878 in Germany. In both countries the fate of free trade was decided in these votes. They test a model in which the economic interests of voters are supposed to determine

how they voted. The results mainly fulfill expectations: employees of those industries that showed strong export performance tended to choose parties that supported free trade, and vice versa. The biggest surprise is that the role of agricultural laborers is totally insignificant in the German elections, contradicting the accepted view that the Junkers, having mobilized the laborers on their estates, determined the outcome of voting.

Since neither author could be present, Brandt summarized the paper and concluded by remarking that there can be sharp differences within sectors in terms of international competitiveness, but that these do not necessarily correlate with the sectoral distribution of performance.

At a final plenary session of the conference a special award of encouragement for a graduate student was presented to Michael Reutter for his paper. A selection of papers will form the content of a special issue of the *European Review of Economic History*. There was unanimous agreement on the desirability of a second conference to be held in 2001.

Note: John Komlos and Joerg Baten also helped to organize the conference. Scott Eddie and Rob Gray were assisted in preparation of the conference report by András Fülöp, Leila Gharani, Oleg Igoshev, Jordan Lee, and Giovanni Romero.

Director's Notes *continued from page 2*

Trustees then agreed that the position of Executive Director and *Newsletter* Editor could be separated or remain one position. If there are to be two positions, the *Newsletter* Editor will be responsible to the Executive Director. The By-Laws were then amended to reflect this change. Susan Wolcott and John Wallis will serve as co-chairs of the Search Committee. [See Announcement, page 51.]

Undergraduate Paper Prize

The Trustees debated whether to continue the Undergraduate Prize. It was decided that the December membership renewal letter will include a poll asking members to indicate the likelihood of their nominating a paper in the

next two years. If enough members "pledge" that they intend to submit nominations, the prize may continue.

Trustees Election Process

The majority of members voted to have a choice of nominees on the ballot. The ballot will have four nominations for two Trustee positions, with the top two nominees elected.

Other Business

Dick Sylla was re-elected Chair of the Board of Trustees for 2000.

Trustees voted to appoint Sam Williamson as Cliometric Society representative to the IEHA through the year 2002.

Temin Interview *continued from page 6*

economists' professional habits of mind and the methodological pull of the tradition established by Conrad and Meyer's studiously depersonalized approach to the history of slavery's profitability..." You were particularly critical of the whole effort of Fogel and Engerman to try to compute a statement of comparative welfare from evidence on consumption and other data without addressing the fundamental question of whether it was possible to make comparisons in welfare between the well-fed, but unfree slaves and the underfed and poorly housed, but free, white workers in the North. Does this statement offer insight into where cliometrics was at this point in its development? I mean, it seems to me that *Time on the Cross* occasioned so much controversy, primarily because of the issue of slavery, but at the same time, something else must have also been afoot.

It wasn't just this issue. You have to remember that Bob was trying to proselytize the New Economic History in America and England. I would say that the mid-70s may be the high point of, let's call it, pure econometrics, in the sense that all you needed to know was the economic theory and that, if you just understood the competitive model, you could deal with any problem at all. I think that since then, there has been somewhat of a retreat from this. Bill Parker's Presidential Address said that maybe we've beaten this horse to death. We were trying to say, maybe you needed in *Time on the Cross*, maybe you needed a more expansive view of what was going on. It was not a decline in the use of economic models and evidence, but rather it was an erosion of the belief that these were the only kinds of evidence that were relevant. It was the beginning of a notion that it's not Us or Them, but maybe that it's together that we need to do this. I think it's really at that time we begin to make this approach, back and forth.

So in a way the controversy may actually have prompted a realization that cliometrics must cooperate with more traditional historians or historians rooted in other kinds of traditions?

I don't know. This reconstruction seems to have more order than I think it had as I went through it. At the same time that Paul and I were responding to *Time on the Cross*, I was also engaged at M.I.T. in trying to encourage different approaches to economics. I had a grant for scholarships for students trying new approaches, and I

was onto the pharmaceutical industry. As I got further and further into the pharmaceutical industry and into trying to understand the demand for pharmaceuticals, I got more and more convinced that a straight economic model was not explaining everything. I began to think about alternatives. The notion of satisficing was around, scarce information was just beginning to be information as a commodity, and you had to think of people acting without having enough information. In my article on modes of economic behavior in (I think) 1980, I argued that economic behavior, "instrumental behavior," is only one of several different kinds of behavior, not the only behavior, as I think we would have said *circa* 1970. It is important to think, particularly in places where you don't expect the market to be working completely, like health or perhaps like slavery, to think about other modes of behavior that people are using.

Could you offer some background on what drew you to the pharmaceutical industry, and then to the study of the break-up of the Bell system, which offers a kind of *ex post* "inside view" of the events – certainly a methodological innovation?

Looking back on it, I have had a continuing interest in industry history, which has now molded into business history. It starts with the iron and steel industry, goes through the pharmaceutical industry, the telecommunications industry, and ends up with the business history conferences that Naomi Lamoreaux, Dan Raff, and I have been putting together. They all can be seen as continuing one from the other, but each had its own individual cause. The pharmaceutical industry was interesting to me because I was trying to think about uncertainty and lack of information, and how people act without full information. I thought briefly of trying to model this problem, and then acknowledged to myself that I was an economic historian, not an economic theorist, and said, "Let me find a place in which there is a lot of uncertainty and figure out what kind of institutions were developed to take care of it." That gave rise to my study of the pharmaceutical industry. The Bell System was an interest that follows on that, but it was stimulated by a totally different thing. I had consulted a little in the AT&T case and gathered some data for them that was not used in the trial because they wanted to argue that the government had done something in the early years of the 20th century which I discovered the government had not done. So I was not a cooperative witness.

That's interesting; this is the work you did on the shifting regulatory regimes and the pricing, kind of

the allocation of cost, and so forth.

No, no, this is before the book, work that hasn't been published. I don't think it's very interesting because it was just trying to establish priors that turned out not to be true. But I knew the people involved, and when the idea of writing the history came up, I volunteered to write it. In fact, I argued to AT&T that they wanted an economist to write this book rather than a straight historian, harking back to the earlier debate in a sense. I argued that the pricing issues, economic aspects of regulation, were critically important, that cross subsidies were key, and that you needed an economist to understand how much of the issue revolved around prices. I did convince them, and I got the opportunity to write what I hope was a good book. It certainly was an enormous amount of fun and fascinating to write.

I can't say that I finished it, but I've certainly found it very interesting!

People in the field have given me good feedback. When you write what I call contemporary history, then there are other people who have lived through it. I have been very cheered when I get people who are in the industry who say, "Yes! That was really the way it happened!"

How did this experience differ from what you had done before, drawing on the experience of key participants, most of whom were still alive and accessible, rather than trying to reconstruct events second-hand? Did you bring a different kind of critical thinking to the process of writing this history?

It's quite different. There are lots of overlaps in the kind of logic that you bring to the issue, but I did a lot of oral history. I interviewed people like you're interviewing me. Then, of course, I had to decide how much of what they told me was reasonable, and what was unreasonable. Some of the people who had made critical decisions had very vivid memories and could tell you what happened each day. Other people had no memory at all. The question then is, does that bias the story? You have a whole new set of things to think about when you're doing contemporary history. And, of course, there are a lot of people to show it to, to get feedback.

Did you have a documentary record to draw upon to frame the discussion?

I had a long documentary record, and I had a lot of support

from people: Lou Galambos, Bob Lewis, and a staff of people that included Bob Garnet, Ken Lipartito and George Smith. The paper trail, though, was only partial. We had the regulatory proceedings, which were voluminous, and we had the internal AT&T documents, which were exactly the opposite. You have to realize that AT&T had survived the anti-trust suit brought in 1949 and settled in 1956. After that, the lawyers made sure that there was nothing that was really substantive in their files. You could always find out what they were talking about, but you could only seldom find out what they said. Consequently, the paper trail within the firm was composed of some reports that were written internally, so you could see them, and a lot of speeches that were made internally, which were recorded. This was a kind of a formal communication, and I had to try to think what was behind the statements where the CEO was trying to marshal the troops. What was he trying to marshal the troops for? It turns out that deButts was a colorful character with a rather extreme view, so it was pretty easy to know what he was doing. Romney before him and Brown after him were more subtle characters, and they were a little harder to get out of the record. Of course, I could talk to Brown (and I couldn't talk to Romney), so I could get some additional information from him.

Now that you have both reconstructed the events through oral history and worked with what documentary evidence there was, did the exercise give you insight into how to treat the documentary evidence that any historian must use? Essentially, we must try to reconstruct the series of events that would have actually led to an important event such as the breakup from the fragments left to us.

I think doing a project like this has to give you a lot of humility as you approach the more distant past because it is very hard to fill in these blanks. Without the kind of evidence that you get from being able to ask people what goes on, you always have in your mind that you may just have gotten it wrong, because there was partial evidence and you put it together in a way that a contemporary just wouldn't recognize at all. I must say – if I can tell one story – that I wrote a paper about the Koreaboom in Germany, which is not contemporary history, but it is relatively recent history. I gave a seminar on it then in England. Alec Cairncross was one of the heroes of this episode, at least as I construed it, but I was arguing against a whole intervening literature that had seen this differently. I gave the seminar, and Cairncross came to the seminar. He remembered the incident, and said, "Yes,

that's what happened!" He remembered it, and gave me some more stories about it. I thought, "My!" It was an extraordinary feeling that I "passed the test," that I put the story together from the documentary evidence, and, by the grace of God, an eyewitness came back, a participant came back, and confirmed that, yes, this was the view that was accurate for that time. That was a wonderful experience for me.

Our discussion of contemporary history brings to mind the work you have done integrating business history and economic history, particularly your work with Naomi Lamoreaux, Dan Raff and others. So far, you have organized three NBER conferences, each of which has resulted in a published volume. Prompting some of the research presented at the conferences and noted in your Presidential Address is a dissatisfaction with the unwillingness in economics to account at the macro level for the evolution of, and importance of, economic cultures in shaping economic development. Economic historians are also often unwilling to abandon the economics learned in elementary classes when carrying out micro-level analyses and turn instead to more sophisticated models of economic behavior, including imperfect information, path-dependence models of learning, and so forth. The Social Science Research Council recognizes another feature of this problem with its Program in Applied Economics. Students in most economics Ph. D. programs do not receive the breadth of training that would prepare them to carry out economic history research. What could be done to train the next generation of economic historians, given the content of the Ph.D. programs they are enrolled in? How can graduate students in economics in general be made aware of the questions that the research presented at the NBER business history conferences tries to address?

Our hope in doing these conferences was that we would provide reading that could be used in a variety of courses, and in particular, that could be used in economics classes. We were hoping that even in elementary economics classes people would be willing to assign some of these stories to try and say, "You know, we have these black boxes called 'firms,' and here is what firms do." You can see a firm trying to make some decision or trying to work out some problem. We were hoping that would help people understand economics, and also attract people to this kind of work.

Your question about the next generation of economic

historians is a very difficult one, because the market for economic historians has not been a particularly buoyant one in recent years. There is a steady demand for economic historians but, while the demand for economists has been growing, the demand for economic historians has been pretty stagnant, with one or two economic historians at each school and, in fact, we like economic historians better if they're part-time economic historians who can also teach money and banking or macro or labor or econometrics or whatever. And so it has been hard to attract people into economic history as a professional activity. On the other hand, I hope that these conference volumes, and also other articles that get written, bring economic history into the range of activity of ordinary economists; that they would think that it would make sense to do an essay or do part of their thesis on an historical topic. This has been rather successful here at M.I.T., and there are many dissertations that have economic-history chapters in them, some of which go out and become articles in the journals – Peter Berck's article on blast furnaces many years ago, Keith Head's article on learning-by-doing in the steel industry, or Matt Slaughter's article on price convergence in the early 19th century. These are all papers that grew out of their economic history at M.I.T., done by people who are not primarily economic historians. The problem is that economic history can't survive on the isolated articles done by economists who recognize economic history as part of their activity. We have to lobby our colleagues to have some more specialists to maintain the framework from which these other people can learn, and maintain the framework that those people can hang onto as they do their individual essays and individual contributions to the field – typically quite good contributions to the field, but still episodic. None of the people that I mentioned are economic historians; they are people who have done some economic history. But let's remember Conrad and Meyer – Conrad tragically died very early, but Meyer was not an economic historian; he's a transportation economist, an IO economist. Yet their foray into this debate touched off an enormous discussion. It's really very important to the field not only to have professional economic historians to identify as economic historians but also to have other people, typically economists, coming into the field because topics or questions or theorems interest them, and having an impact on the field.

What is the response from the economists at the NBER conferences? You really have three kinds of groups, the business historians, the economists, and the economic historians.

That's right. We had all these people together, and one of the wonderful things was getting them to talk with each other. The conferences have been very popular; people have wanted to come to them, and I think have enjoyed coming to them. It has not always been easy to get everybody to talk to everybody else. We had resistance to what we were doing from the traditional business historians, and we also had some resistance from the straight economists. I think that over the course of the three conferences we have swept more people into this conversation and convinced more people that this was a legitimate thing to do, but it's still only a small number of people that we could reach through the conferences. The wider group we try to reach through the volumes. I know they've been popular volumes and they get well reviewed, but I don't know how much they change people's thinking.

You only see that as time goes on. You have been so generous with your time. I'll restrict myself to one final question that concerns a reference made in the most recent volume that you and Dan edited ...

All three of us were editors.

Okay. There was a very brief allusion to a break-up of economic and business history that took place in the depths of times past. Gerschenkron was on one side and on the other was the entrepreneurial school. It seems to me that these kinds of conferences are also efforts to overcome what has been a schism, maybe not quite of historic dimensions as is true of other schisms, but certainly an important one for the development of economic history. Are there barriers that prevent more interactions of this kind? Are they primarily the availability of tools – a barrier to working with some other kinds of historians – or does it go deeper? Do they reflect deep methodological differences, the inductive work of the business historian *versus* the deductive approach of the economic historian: think of a model and then work from there?

We talked about the early days of cliometrics and the kinds of conflicts then between the economists and the

historians. For general historians, communication with them has only become more difficult over the years, because they have gotten interested in other questions which aren't of interest to economics. It's not as much a problem of different methodologies as it is of different interests. Business historians are a different group than regular historians. Partly, they have gone to general history and corporate culture and so on. But partly they have thought about the economics and, just as history has changed, economics has changed. Economics has gotten far more subtle in its use of information and understanding of bargaining and understanding of how small groups, whether it be an oligopoly or a board of directors or government agency, operate. There has been room for a lot more discussion back and forth between economic and business history. Which goes both ways. For business historians, one can draw on a lot of economics. Maggie Levenstein is an example of that. Economists also are acknowledging the importance of history. For example, the GM purchase of Fisher Body has been the classic observation in this economics-as-contracting principal-agent literature. Economists have gone back to look at the history of the Fisher Body story, and there'll be an article in the *Journal of Law and Economics*, in the next couple of issues, talking about that history and arguing that, in fact, when you understand the history, it doesn't support the literature that has developed. That's an interesting kind of two-way street which I think is bringing people to those fields.

It's late in the afternoon, so I'll let you go. Do you have any other comments you'd like to make for the record before we conclude our discussion?

The comment is that one of the things that attracted me to economic history was that, back when I was a graduate student, this was just an enormous amount of fun to do. And now today, many, many years later, I still think it's an enormous amount of fun to do, and I wish somehow we collectively as a group could convey that to our students so that we could attract more people into economic history.

Hear, hear!

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Lehigh Report *continued from page 10*

They questioned whether the book could still be seen as valid after 20 years, and proposed that it might not, given advances in research methodology and dramatic increases in computing power. For instance, anyone undertaking such a study today could reasonably investigate the entire (ex-Confederate) South, incorporating more than 1.2 million farms at the 1900 census, rather than just the "Cotton Belt" and its 500,000 farms. Data from the "peripheral South," beyond the Cotton Belt, indicate that whites were still far more likely to be landowners than were blacks; and 1880 census records suggest that blacks throughout

the South remained mired as farm laborers, stuck on the lowest rung of the rural hierarchy, over a longer span of their lives than whites. The authors reviewed the "lock-in" model developed for *One Kind of Freedom* to explain small farmers' loss of self-sufficiency to credit merchants. Financially, small growers would have been better off devoting sufficient acreage to corn production for consumption and sale, but, by committing to grow cotton for furnishing merchants, many were forced into costly corn purchases to feed their families. Testing this model using 1990s computing resources, the authors calculate the corn production capacity of cotton land, compare information on 1880 corn and cotton prices and merchant credit prices for corn, and find that about 30% of small farmers were truly "locked-in." It is also more feasible now, they noted, to test for land-tenure choices. Why did black farmers own so little land relative to whites? Was it due to race or some other factor? The authors presented their new discrete tenure choice model in which, controlling for factors such as the age of the household head, family size, and risk-taking behavior, race emerges as the most powerful factor controlling land ownership. The authors conclude that, were they to produce a new version of the study, which they do not anticipate doing, they would of course use 1990s methods, but would expect to reach similar results.

Panelist Peter Coclanis (North Carolina) paid tribute to



Photo: Rob Upton

Panelists Ransom, Engerman, Wright, Woodman, and Coclanis;
Sutch in foreground.

One Kind of Freedom as an esteemed, well-researched study, but stated that he found the "lock-in" model unconvincing. Small farmers may not have felt themselves bound by local merchant monopolies, and, in fact, the authors' portrayal of merchants was excessively harsh. Much historical evidence indicates that some customers found furnishing merchants flexible and helpful. Rural southern merchants served as part of a tradition of vendors serving poor clients that continues today in the form of check-cashing agencies and pawnshops. These firms have high prices but offer services their high-risk impoverished customers need. Coclanis did agree with Ransom and Sutch that the South

was evidently impoverished and African Americans oppressed. He concluded by calling for scholarship comparing the southern experience with other parts of the world, noting that the former exploitative reputation of Indian merchants in rural Burma, similar to that of southern credit merchants, has recently been challenged.

Panelist Stanley Engerman (Rochester) also called for an international comparative approach. He noted that the end of slavery in the US differed from its end elsewhere in the Americas because it occurred without compensation to slave owners. A variety of results followed the abolition of slavery: sheer poverty in Haiti, the disappearance of the plantation in Jamaica, and the continuation of plantation production in Barbados. In Trinidad and Brazil, ex-slaves tended to leave plantation agriculture after emancipation, but were replaced by East Indian and European labor respectively. He suggested that the postbellum era in the US be considered as two periods: 1865-90, which featured economic gains, and 1890 onward, marked by economic stagnation and racial violence.

Panelist Harold Woodman (Purdue) praised *One Kind of Freedom* as a valuable study and a landmark in the ascent of the cliometric approach. He suggested some areas in which it could be refined, noting that the authors had paid little attention to the close landlord supervision of

croppers that developed over time. He also urged a closer look at how the changing fortunes of rural whites affected the welfare of black croppers and tenants, and emphasized that laws, not just proximity, enabled the dominance of small farmers by merchants. He suggested one way to reconcile the differences in how merchants were regarded is to understand that tenants and croppers had differing relationships with merchants depending on how their landlords operated.

Panelist Gavin Wright questioned whether the term "debt peonage" as used in the book could really be applied in the American context, given a supposedly greater freedom of movement on the part of small farmers here compared with those in other countries. He suggested the volume could serve as a useful starting point to a wider regional study of the postbellum southern economy.

Ransom and Sutch agreed that they had employed some "strident" language in the book, and acknowledged that the southern legal framework was instrumental in defining the merchant-farmer relationship. They noted that some questioned their decision to end *One Kind of Freedom* with the advent of the boll weevil in 1890 and acknowledged that the situation of southern blacks grew worse after that point.

Session V opened on the morning of September 19 with Thomas Hyclak (Lehigh) chairing and William Collins and Robert Margo (both of Vanderbilt) presenting "Race and Home Ownership, 1900 to 1990." Many studies, the authors noted, have focused on income differences between blacks and whites, but race-specific wealth differences are even greater but have not been investigated as completely. Not only is home ownership the main component of family wealth but, since government programs have had a direct impact on access to home ownership, studies such as this allow consideration of the impact of government policies on personal wealth. As a baseline, the authors consider wealth holdings by southern blacks during the late 19th and early 20th centuries, from state government reports on assessed wealth, and find that black wealth was very low, with a median of 5.8% relative to white wealth. The study then proceeds to sample PUMS data on home ownership among black and white male household heads from 1900-90, finding that black ownership has always trailed white, although the gap has narrowed over time. One vivid exception to closing this gap occurred between 1940 and 1960, when the difference actually increased. The age profile of black homeowners also shifted upward at the time, and the

authors link the increase in the gap to the "Great Migration" which brought mobile blacks into northern central city areas where home ownership was less common. Linear probability models are employed to test household characteristics that might influence home ownership; race was found to be a material factor, although in combination with other (probably racially-influenced) factors such as income and literacy. Data on black *versus* white home values from 1940 to 1980 indicate that homes of blacks increased in physical quality, but the increase was partially offset by concentration of blacks in central urban areas. The authors assert that scholars' arguments that HOLC and Federal Housing Administration policies (which "redlined" heavily black neighborhoods) inhibit black home ownership are possibly "overblown," since the data did not support any specific negative impact of these policies; FHA-VA programs enabled some blacks to acquire suburban housing.

Discussant Peyton McCrary (US Department of Justice) praised the paper's focus on wealth and called for more such studies. He did question the statement that other scholars have "overblown" the negative effects of HOLC and FHA programs, noting that rather than accusing these programs of inhibiting black home ownership, scholars have simply noted that they promoted housing segregation. Further, McCrary mentioned that Federal policies which attempted to overcome segregation beginning in the late 1960s had little enforcement power until the late 1980s. Finally, he questioned the authors' acceptance of a second grade education as an indicator of "literacy."

Next, James Heckman (Chicago), Thomas Lyons (American Bar Foundation) and Petra Todd (Pennsylvania) presented "Understanding Black-White Wage Differentials over the Last Fifty Years." Census information from 1940 to 1990 indicate that wages of working black males continue to lag behind those of white males, but that the gap has closed considerably over the last 50 years. Many "demand side" scholars have emphasized the importance of outside interventions, such as civil rights laws, in bringing about the improvement. Others have emphasized the "supply side" of increased human capital in the form of education and other assets possessed by blacks in the workforce. The authors endeavor to construct a reliable model to evaluate what types of human capital factors have contributed to black earnings progress. They conclude that previous scholarship, which focuses on improvements in the education of southern-born blacks, might be of

doubtful value, since their earnings model finds little impact from this factor.

Thomas Hyclak and Robert Margo served as discussants. Margo noted that this is one in a series of recent papers emphasizing supply-side factors in improvements in the black employment situation and acknowledged its relevance to current debates about public school spending and quality. He praised the authors' elaborate modeling methods but urged caution in trusting the data, since census records of earnings become less reliable the farther one goes back in time.

William Sundstrom (Santa Clara) presented the closing paper, "Discouraging Times: The Labor Force Participation of Married Black Women, 1930-1940." As other papers presented at the conference noted, the work force participation of married black women has historically been substantially greater than that of married white women. While the participation rates of women in both races were on a gradual upward trajectory early in the 20th century, the gap between the rates narrowed substantially during the Depression decade, when white female participation increased while that of black females remained steady. Why was there such a striking divergence of workforce experience during this stressful decade? Sundstrom uses PUMS samples of married women with spouses present from 1920-70 to gain insight. Since black men relied heavily upon public relief employment, could the relative reduction in black women's participation stem from the requirement of Depression relief agencies that men seeking such employment have no other source of household income? Or might the weakness in the black participation rate stem from the "discouraged worker" effect in which the search for work appears futile amidst high unemployment? One potential complication of the study data, different approaches to recording seasonal farm employment in the 1930 and 1940 censuses, was overcome when the author determined that much of the workforce participation difference occurred in urban areas. By putting a subsample of urban married women drawn from the 1940 PUMS through a series of counterfactual evaluations, Sundstrom establishes that factors such as the husband's status had far less effect on women's presence in the labor force than race and the local unemployment rate. The discouraged worker effect, traceable to racial discrimination in hiring, appears

to be the culprit in weakening black women's, and therefore black households', earnings.

Discussant Marcellus Barksdale (Morehouse) noted that historians such as John Hope Franklin have also attested to the presence of extreme unemployment and "discouraged worker" symptoms during the Depression. He reminded the audience that census takers might not have reliably recorded the informal employment activities of black women.

A highlight of the conference, on Saturday evening, was a compelling keynote address by Leon Litwack (UC-Berkeley): "Nothing but the Blues: Work and Betrayal in the Age of Jim Crow." Accenting his speech with the colorful and world-weary lyrics of African-American blues, Litwack traced the ways in which black expectations of freedom after emancipation were dashed by a century of betrayal. He told of the disappointment of freedmen on Edisto Island, South Carolina, shortly after the Civil War, as General O. O. Howard told them the lands they had been working would be returned to white owners. Howard instructed them to make contracts with the planters, but Litwack noted that for decades afterward, the free labor principle embodied in contracts between black tenants and landlords perished in the realities of oppression and the unfair "settle." Meanwhile, Booker T. Washington and others who urged southern blacks to greater industry never overcame a cruel paradox: southern whites would violently suppress any African American who became too successful lest he forget his "place." Exploited in the plantation economy and barred from rising above menial labor, many blacks became "internal exiles," barren of hope, divorced from conventional values, seeking escape in dissipation. Litwack cited the 1960s as landmark years in which institutionalized segregation was overcome, but lamented that when white Americans realized by the 1970s that integration would have costs, many backed away. He linked conservative attitudes expressed during the 1980s and '90s, such as opposition to affirmative action and studies questioning black intelligence, to the racism of the 1890s. Drawing inferences again from black music, he noted that optimistic Motown group names from the early 1960s had given way to ominously named rap groups by the 1990s, and wondered how far America had really come in escaping its racist legacy.

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diverse, rather than more homogenous, communities over the course of the 20th century.

The Wednesday sessions featured six papers on the broad theme of financial history. It began with a paper by Lee Craig, Jack Wilson, and Robert Clark (all of N.C. State), "Emerging Financial Markets and Pension Portfolio Management: The US Navy Pension Fund, 1800-1842." Wilson presented the paper which shows that the current debate, on whether public pension money such as Social Security funds should be invested in private equities, is not exactly a new one. Early in US history the Federal Government operated such a fund, the Navy Pension Fund, which paid disability pensions to naval personnel and, later, widows and orphans. It was funded by "prizes," enemy ships and cargoes captured by the Navy. The fund was not well managed. Some of its investments in private equities such as bank stocks smacked of "crony capitalism," and, when banks failed, Congress had to appropriate revenues to bail it out. In addition, interest groups lobbied to expand eligibility for pensions and to extend them to widows and orphans. Although there is a world of difference between a pension fund for disabled naval personnel and one that covers an entire society, the lessons of the paper for those who would privatize Social Security are not very encouraging.

Sonali Garg (Ohio State) followed with a paper on "Rise of the New York Stock Exchange to Preeminence: Causes and Consequences." It investigates trading volumes and bid-ask spreads on the Boston, New York, and Philadelphia stock markets/exchanges from 1832 to 1854. The New York market appeared to have by far the largest volume of trading throughout the period, and its bid-ask spreads were usually, but not invariably, below those of the other cities. Garg concludes that the New York market became preeminent only in the 1840s, but gaps in the data make that a tentative conclusion. The discussion noted that the types of securities traded in the three city markets varied, and that ideally a test of trading efficiency would look at bid-ask spreads for the same securities traded in each city. It was noted that a few securities were at times traded in two or three of the cities, which might allow such refined testing.

The morning session ended with Marc Weidenmier's paper, "War News, War Debt, and Confederate Bonds in London." During the Civil War, the Confederacy in 1863 issued three million pounds sterling of bonds in

the London market, with the French financial house of Emile Erlanger and Co. serving as investment bankers. The Erlanger bonds, as they were called, carried an unusual feature, an option to take payment in cotton delivered at Confederate ports. Weidenmier shows that the option had value and that the Confederate bonds increased in value in 1864, even as the Confederacy's military fortunes were waning, when the price of cotton rose. Weidenmier's evidence also shows that war news mattered for bond pricing, as others have found in different contexts. His attempts to disentangle the two effects led him to the conclusion that the cotton market was the more important fundamental factor in the market's pricing of the Erlanger bonds.

After lunch, Peter Rousseau (Vanderbilt) presented his paper, "Share Liquidity and Industrial Growth in an Emerging Market: The Case of New England, 1854-1897." Rousseau develops new measures of prices and returns for Boston bank and industrial stocks, and a measure of liquidity, the number of shares per \$1,000 of par capitalization in the stock market. He utilizes chiefly the records of Joseph Martin, a 19th-century broker. The measure of industrial stock liquidity increased over time as par values of stocks fell. Rousseau then uses VAR methods to show that liquidity raised stock values, presumably cutting the costs of capital to industrial firms, and that both liquidity and the market value of equity in banks and industrial stocks were associated with rising earnings of industrial workers. Rousseau concludes that financial developments were important for US industrialization, although the ensuing discussion brought out the point that this industrialization had begun well before the period covered in his paper.

Hugh Rockoff (Rutgers) followed with a paper that we now know forecasted the 1999 Nobel award to Robert Mundell, "How Long did it take for the United States to become an Optimal Currency Area." Using Mundellian theory, Rockoff argues that, at least until the 1930s when federal fiscal transfers increased and the country got bank deposit insurance, the US was not an optimum currency area. For more than a century before then, the US paid a price for having a uniform currency. Shocks in financial or agricultural commodity markets would affect some regions of the country more than others, with banking contractions in those regions producing monetary contractions aggravating the disturbance. Ensuing debates over financial structure added to financial and investment uncertainties. Rockoff contends that the US would have been better off had it used several regional currencies, so

that exchange rate changes among regions could have alleviated the stresses caused by shocks.

The final paper of the day was that of Eugene White (Rutgers) and Cormac Ó Gráda (University College, Dublin), "Who Panics during Panics? Evidence from a Nineteenth Century Savings Bank." The general issue of the paper is the nature of the contagion that occurs in banking panics as depositor runs on one or several banks lead to runs on more and more banks. Is such contagion industry-specific, where runs on one bank lead to runs on all types of banks, or is it "bank specific," where the run spreads only to banks with similar characteristics? White and Ó Gráda use the detailed depositor records of the Emigrant Industrial Savings Bank (EISB) of New York City to investigate their title question during three different sorts of panics: one in 1854 when another savings bank failed, one in 1857 when a major commercial bank failed, and that of 1861 when the general shock of the Civil War hit the financial system. The paper was only partially completed when presented, but White and Ó Gráda find that in 1854 the "uninformed" depositors, those with recently-established and small accounts, tended to close their accounts with greater frequency than did depositors with older, larger accounts. In 1857, a more general banking panic, the evidence was mixed; informed as well as uninformed depositors closed their accounts. The analysis of the 1861 panic remained to be carried out.

Wednesday evening was given over to a clambake at the Harvard Faculty Club, and to formal Bastille Day celebrations in the streets, traditional in Cambridge since 1999.

Thursday morning's closing session featured three papers, and was opened by Karen Clay (Carnegie Mellon) and Werner Troesken (Pittsburgh) with "Did the Trusts have Market Power? Evidence from Distilling, 1881-1898." At the end of the 19th century, large trusts emerged in several US industries, including the market for alcoholic spirits - the Whiskey Trust. Clay and

Troesken attempt to disentangle whether the Whiskey Trust formed because of gains in efficiency, gains that were ultimately passed on to consumers in the form of lower prices, or whether the trust was formed to capture and exploit market power. The results suggest the former, that barring two occasions the Whiskey Trust produced lower prices for consumers. On those two occasions, the trust mistakenly raised prices and encouraged entry of potential competitors, competitors who eventually caused the demise of the trust.

Next, Sukkoo Kim (Washington U.) presented "The Growth of Modern Business Enterprises in the the 20th Century." The growth of large, multi-unit firms is a striking feature of the modern American economy. Alfred Chandler has explained the growth of multi-unit firms as the result of gains to scale and scope. This paper examines the growth of multi-unit firms in the United States, showing that most firms are not multi-unit enterprises, but that those few multi-unit firms are quite large. Multi-unit firms employ roughly half of all workers at the end of the 20th century. The paper then examines the development of multi-unit firms in wholesale and retail trade. This history suggests that multi-unit firms arose as a response to economies in marketing rather than economies in scale and scope.

The 1999 DAE concluded with Lee Alston and Joseph Ferrie (Northwestern) on "Up and Down the Agricultural Ladder: The Experience of Farmers in the American South, 1890-1937." This paper examines mobility within agriculture by utilizing a survey of tenant farmers. The farmers gave retrospective interviews, detailing how their careers in agriculture had played out. Using this data, it is possible to create tenure histories for each of the farmers.

The 1999 DAE Summer Institute sessions were a great success. Your humble reporters, Sylla and Wallis, will be organizing the program again next July.

NOMINATIONS REQUESTED POSITIONS OPEN FOR CLIOMETRIC SOCIETY OFFICERS

After many years of dedicated service to The Cliometric Society, Sam Williamson is retiring from the positions of Executive Director and *Newsletter* Editor. The Society needs to find a replacement for each or both of these positions.

Executive Director

Term: 4 years, renewable.

Duties: Oversees the work of the Society. Maintains membership records. Manages the finances of the Society. Handles relations with affiliated journals. Appoints the *Newsletter* Editor and the meetings coordinator with the advice and consent of the trustees. Ensures that funding is secured for the annual Cliometrics Conferences. Works with *Newsletter* Editor and oversees content of web page. Works on future World Congresses. Maintains liaisons with other associations.

Newsletter Editor

Term: 4 years, renewable.

Duties: Oversees collection of material for *The Newsletter of The Cliometric Society* and edits submitted information. Identifies conferences of interest to members and secures a reporter for each such conference who will provide a report for inclusion in the *Newsletter*. Arranges interviews of respected cliometricians for inclusion in the *Newsletter*. Arranges to have timely announcements from related organizations and other items of interest to members included in the *Newsletter*.

The Cliometric Society Board of Trustees seeks your suggestions and nominations for these two important positions. If you would like to nominate either yourself or someone else for either or both of these positions, please contact the Nominating Committee of John Wallis and Susan Wolcott at Susan's address:

Susan Wolcott
Department of Economics
Holman Building
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Susan will acknowledge your nomination. The Nominating Committee will collect nominations, confirm that nominees are interested in serving and present the results to the Board by **December 20th**. The Board hopes to make the appointments no later than the end of January.

- Richard Sylla
Chair of the Board of Trustees
The Cliometric Society

Fourth World Congress of Cliometrics

Next Year in Montreal

The Fourth World Congress of Cliometrics will be held July 6 - 9, 2000, in Montreal, Canada. The Program Committee is now in the process of putting together an outstanding international program from the 90 excellent proposals submitted. The program will be posted on The Cliometric Society web site in early December.

All members of sponsoring organizations are invited to attend. Registration will be open but conducted in advance so participants can receive their copies of the Congress Book and read the papers prior to the Congress. Sessions will be held in traditional Cliometrics Conference format: Instead of formal presentations, authors will provide a brief introduction to their work, followed by an extended period of discussion involving session participants.

- We will offer travel and lodging grants for graduate students and international scholars who otherwise cannot afford to attend. Presenting authors will have preference. Applicants are encouraged to use the form on The Cliometric Society web site, or submit the required information *via* e-mail, fax or post to the address below. Applications for grants will be accepted **December 15, 1999, through January 15, 2000.**

- Complete information about registration and lodging and the Congress Registration Form will be available on The Cliometric Society web site in early January 2000. Using the web site registration form will allow us to provide immediate confirmation of your registration.

- Once registered, participants will be able to make their own reservations with the Congress hotel, the Holiday Inn Montreal-Midtown, at the special Congress rate. A limited number of dormitory rooms at McGill University also will be available to participants.

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