The Newsletter of

The Cliometric Society

July 1992 Vol. 7 No. 2

Summary of Papers and Discussion from the 32nd Annual Cliometrics Conference

Miami University, May 15-17, 1992

by
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(Oxford, OH) The sky cleared just before the 32nd Cliometrics Conference participants assembled for the first of twelve thought-provoking discussions, time enough to enjoy the gardens near the Marcum Conference Center at Miami University. The very pleasant accommodations arranged by the conference hosts (Sam Williamson, Bill Hutchinson, Len Kloft and John Lyons) helped everyone feel at home as they settled in for the grueling work of providing the presenters with helpful comments and constructive criticism. A summary of the discussions follows; abstracts are included as an insert.

Alex Field (Santa Clara) opened the conference with his paper on the importance of the telegraph to American business, with the meat-packing industry as his primary example. Field argued the telegraph was indispensable to the meat-packers because they could not effectively have managed such a high-throughput business in a perishable commodity without it. Discussion centered on appropriate measures of indispensability and the packers' use of the telegraph to maintain collusion. Jeffrey Williamson (Harvard) offered the first comment, suggesting that a description of industry operations pre- and post telegraph might help Field make his point. Later in the discussion Sam Williamson (Miami) came back to the issue of industry structure to ask if communications changes could also be linked to the 20th century decentralization of the meat-packing industry. Mary Gregson (Illinois) requested that Field more precisely describe his counterfactual world. Field responded that the counterfactual involved communications at railroad speed only. Lou Cain (Loyola and Northwestern) pointed out that FTC records contain direct evidence that the telegraph was used to support collusion, and Jeremy Atack (Illinois) suggested the Armour company papers might contain quantifiable evidence on the importance of the telegraph to the firm. Elyce Rotella (Indiana), Jocelyn Hammaker (California-Riverside) and Albrecht Ritschl (Princeton) questioned Field on his measurement of indispensability, asking whether other social costs and benefits might also be considered, especially if the telegraph was the means by which the "Greatest Trust in the World" thrived.

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Secretary/Treasurer's Notes

ASSA meetings

We have a full slate of papers scheduled for the ASSA meetings to be held January 5-7, 1993 in Anaheim, CA. Eugene White and Bill Sundstrom have put together four sessions for the meetings and the California group has promised a nice reception in the area. The October Newsletter will have all the details. We hope to see you there.

International Economic History Association

I am pleased to announce that the Cliometric Society will be nominated to become an associate member of the International Economic History Association at the 1994 Milan meetings. In addition, we will sponsor a three and a half hour C session there. The Trustees will be appointing a program committee for that session; a call for papers will appear in the October Newsletter. The committee will select some papers from those submitted and may also include some invited papers in the program.

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Cliomets

For the few of you who used it, perhaps, the "Cliomets" file server was an interesting experiment that never quite worked. I like to think that we were ahead of the times. Regardless, we will soon move into modern times: we plan to install a server using new developments in wide-area networks through the internet system. I will not make any promises as to how soon it will be operational, but we are working on it this summer. Some of you who have given us an e-mail address will be asked to test the server when (we think) it is working. Look for further announcements in the October Newsletter.

Trustees Meeting

The trustees had their annual meeting at the May Conference. Several things were discussed. It was reported that we currently have 348 paid up members, and that our financial picture is "tight." Most of the meeting was spent working on the proposed by-laws for the Society. We made considerable progress and I hope that we will be able to complete the task by September. If any member wishes a copy of the proposal we are working on, please write the Society's office.

Society Staff Change

Debra Morner has just joined us as Society Secretary and Newsletter Assistant Editor. She was initiated immediately by having to deal with Society book-keeping, Newsletter preparation, several new soft-ware packages, and the idiosyncrasies of the editors. We are grateful for her timely arrival, and look forward to further productive collaboration.

AN INTERVIEW WITH STANLEY LEBERGOTT

Editor's Note:
Stanley Lebergott is Chester Hubbard Professor of Economics, Emeritus, at Wesleyan University. He moved there in 1963 following a stint at the U.S. Bureau of the Budget, by way of a year at Stanford. The interview took place in Middletown, Connecticut on June 17th, 1992, and was conducted by Fred Carstensen, who writes:

Sometimes we don't do the obvious thing until circumstance forces it on us; then we realize how much we have missed. I have circled Stan Lebergott for nearly three decades without having much of a conversation with him before this interview. Some of my ancestors were involved with founding Wesleyan, and a forebear was an early president, but my brother and I were the first men (it was until recently, remember, a men's college) not to go there; if I had, Stan and I would have arrived together, and I would probably have been his student. I got closer when I went to Yale for graduate work, but Yale's global vision (i.e., its insularity) somehow didn't include Middletown. And then after a decade, I returned to Connecticut, now to the north and east of Wesleyan. Though I am a bit of a seminar groupie, my trips to Cambridge, New Haven, and Manhattan didn't bring me together with Stan. So when I was asked to do the oral history interview for the Newsletter, I was delighted. It would give me the opportunity to do something very interesting and the excuse to open up a conversation with what – based on his public persona and those marvelous quotes with which he so often opens chapters and articles – has to be one of the most engaging of scholarly minds. Indeed. What a wonderful conversation. I got excited about economic history all over again. Stan, how about lunch?

Two of your questions focus on how I got involved in economic history, and what I am engaged in. During my work in the government, the primary motivation for a whole variety of projects was one question: "What is par for the course?" The first major project that comes to mind was in the post-war division of the BLS, which had heady ambitions. They were going to plan the labor market after the war. My first assignment — perfectly appropriate for someone fresh out of college — was to take all the programs lying around that proposed to get the economy out of the Depression. I was to assess what would be possible after the war. Lunatic, of course, but appropriate if you are in your twenties. I will never understand why Davenport, who was then in charge of operations in that division, wanted this. He came from the Harvard Business School and had dreams of a bureaucratic empire. So I spent about nine months writing a book summarizing all the programs, assessing all their virtues and limitations, and thinking about this issue. It was very instructive; I learned a lot. What the taxpayers got is another matter.

I then spent some time assessing schemes for chasing money around the economy, primitive anti-recessionary schemes, like time-dated currency. One scheme was supported by a guy of some importance in the agency, and two of us had great fun trying to tell how this proposal might fail to work as desired. This led me to learn more about statistics. We didn't make any friends, because this was a superior officer, but we learned a lot. I think we stopped him from making much more trouble with.

Many years later I got involved with the Brookings model. It was a group effort by very good people. Lawry Klein and Jim Duesenberry were co-directors of the project. Among others, we had Frank Fisher, Dale Jorgenson, Bob Eisner, Ed Kuh, Danny Suits. The model may have had 260+ behavioral equations, plus how many identities I hesitate to think. We had, I guess, three summer sessions of two weeks apiece. Boy, it was the fastest, hardest learning I've ever done; it was marvelous. When you have X offering his views about what determines the short rate, and Y immediately saying, "Hell no, it's another" and everybody then piling in, you learn.

For all the proposals and programs, one asked "What variables count?" It's the whole quantitative emphasis that we economic historians live with, variables. Sherm Maisel said, "I can give you residential investment, if somebody develops a marriage model." (Vacancy rates are affected by marriages, of course.) Lawry asked me, "Why don't you do the marriage model too?" So I did a marriage model, and I had to have Vietnam conscription and avoidance in, as well as crossing status and caste and age lines. You clearly get into sociology's domain, but that was what made things go ... Remember Charlie Wilson's wonderful book on Unilever? How do you understand that company if you don't understand these two crazy sets of people trying to work together, each with their own separate orientations? You've got to understand this firm. Talking about maximizing money income or present value doesn't explain it. So when I had this pseudo-sociology in the marriage model (outside customary age lines for marriage and moving outside the customary groups), that was a revelation of what happens when you really try to model behavior. There are great opportunities to tackle what we don't now know, as long as it's only these two or three variables.

You remind me of the caveats of Adam Smith at the end of *Wealth of Nations* about the dangers of specialization. That doing really good scholarship means that you must not respect the putative barriers between fields.

Well, I don't remember that, but I agree you must not. I will add that we can only do so much. I am prepared to say, "That's sociology." I don't know of anything in our training that gives us any great competence for it, but I am not prepared to say that it's superficial. But someone like Gary Becker comes along, fine. Let's hope the next young guy or gal who comes along will be as good, or better.

Were you at Williamstown?

Yes. I was on the committee, perhaps because I had been in the Income Conference. It was a delight to meet Hal Williamson. Simon had surpassing virtues, is still a superb idol. But Hal had that grace. And, of course, he was a tremendous honest workman. The high point of that meeting was unquestionably the Conrad-Meyer piece. Their discussants could not accept the idea these guys would fit their simple model to a reality like slavery. Granted they were cocky, but if John Meyer didn't have the right to be cocky, then who does?

But I must add, about a year or so before I had been wandering through *Agricultural History* and came across an article by Robert Worthington Smith. Smith, bless his modest heart, was "just a historian." But he had laid out all the elements correctly. There they were. Same elements, not with the pizzazz, not with the clear understanding of the economic factors, and not with the heartiness of "Look what we've done, guys." But there it was. Now, had the profession, had the historians, the wit to take the article to heart, we would have had fewer forensic results over the next half century. Conrad and Meyer started with an observation that only economists would make and Smith did not—why is the ratio of male to female prices so different from the ratio of male to female hire rates? That is an absolutely critical question, of course. That isn't why there has been so much attention since; that's the reason why it is a superb article, or one of the reasons.

It's one of those things that screws up wealth, welfare-maximization ...

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his scheme. Then John Pierson, who had been a Yale assistant professor, became assistant division chief. He had a scheme for expanding consumption, so we put that in our net. At the end of it, it became clear that we didn't know a helluva lot about all this.

My next assignment turned out to be orthogonal to much of that. Chuck Stewart, my immediate superior, sensibly said, "Well, one of the things the Labor Department has got to deal with after the war is unemployment." Those were the days when everyone, except Woytinsky, knew there was going to be a great crash. Stalin's favorite economist, Varga, was writing that it was going to be the end of the world. Samuelson and Hagen published something for the NRPB which said we were probably going to have massive unemployment after the war, and I was no less ignorant in a lead article in *Harper's* in 1945. So that was the central economic problem for the Labor Department. Chuck said, "We don't know how much unemployment there was in the Depression." We had conflicting estimates, including NICB's negative employment, but didn't have "par for the course." What could we hope to return to? Our concern eventually became labelled "normal unemployment."

Does that mean the "natural rate," as some would have it now?

Well, I don't believe in "normal unemployment," but there *is* an average or median historical unemployment. We didn't even have that, so I spent about nine months working on unemployment estimates for the Depression, and I continued thereafter on and off. When I got pretty well finished with those at the BLS, I was offered a job at the ILO. I continued to work on the estimates on my own. Then, for whatever reason, I decided that there was an antecedent period; the world didn't start in 1929 on Wall Street, so I pushed my estimates back to 1900. Much of our orientation, at least in the generation I came from, was the Depression. But, what came before the Depression? America, as a major economic leader with tremendous confidence, had no history behind it; the national accounts start in '29. Not that there aren't data before it, but we didn't have anything that gave us a run. (For quite separate reasons, I am now running consumption back to 1900 in that gory 103-item detail the PC permits.) What did the period of earlier prosperity accomplish; what did it look like? That ought to be studied if we are trying to establish a reasonable policy goal.

Now, there was a related inquiry. The Joint Economic Committee asked me, Mo Abramovitz and Ray Goldsmith to testify, partly, I imagine, because Paul Douglas was on the committee. The JEC wasn't interested in writing history; they wanted to use history to get some sense of par for the course on mobility (my topic), growth (Abramovitz) and capital (Goldsmith). This goes to the second aspect of why we're interested in economic history. They wanted some sort of broad model of reality. What are the variables and the variances? I remember emphasizing that we had a sea-change with the GI-bill and federal subsidies to housing. When somebody has planted his crabgrass, he's going to want to hang around to see how it came out. Now that was partly a graphic illustration for them, but it was partly because we had just bought a house, and I was trying to grow grass in the damn Maryland clay. The Lord did not mean grass to be grown there, but I was trying. I remember Senator Sparkman laughing and taking it up for a couple minutes in the hearing. What the policy guys were asking for was a sense of a relevant model with its constituent variables, with a hunch as to the variance.

Somewhat earlier, while I was still at the Budget Bureau, my boss became the American representative to the UN contributions committee; they were trying to establish how much each nation should kick in to the UN. The League had used industrial production, which was not very helpful if you're talking about all those then new states in Africa. So we moved to national income; Simon [Kuznets] was the key adviser to that operation. During the late years of the war there was a lot of work over at the War Production Board on what was national income. Why were they interested in national income? Very simple. The military and Congress (representing the civilians) were struggling over the size of the pie. The first thing you have to do is make some statement as to what the size of the pie is.

I don't want to get us too far off, but you've told an extraordinarily interesting tale about how a concern to formulate intelligent policy creates a recognition that you must have some understanding of history. If you don't know what par for the course is, you don't know what reality looks like. I am curious: where do you think we stand now? Have policy makers become less aware of the critical importance of continuing that effort, are we at the same level, has it expanded?

It's done both. Fortunately, young economic historians are filling in pieces that strike them as of intellectual interest. There is enough sheer intellectual curiosity among people who came out of the 1960s, and, like those of us who came out of the 1930s, they see acute problems in this society they think ought to be fixed. They want to inquire in ways that are relevant to that orientation. The new social history is motivated, as the Marxist historiography was, by "I'll show you what the bastards were really like." Fine. Look at what Marx dredged up, what he put together. You need a fire in your belly to move you through the archives and all the dull documents.

So I take it you agree with McCloskey's recommendation that we ought to pay more attention to the new social history and talk to these people.

They're working in the vineyard; anybody who is doing honest work in the vineyard, whatever his motivation, is someone from whom we can learn. I once read a Wisconsin editor named Pomeroy, who was a leading Copperhead, his autobiography; it was the most touching piece. Now Lincoln is the only person in American history that I think of as really special — John Adams is close — and there was Pomeroy saying, "Kill Lincoln." As historians, we want to know why was he taking that attitude. Original sin is just the starting point. Pomeroy, this impoverished kid from the Midwest, at the age of six or seven, walks — you know what walking in a Midwestern winter can mean — from farm to farm with a pail. He collects ashes from the farmers to supplement the family income. He shows up at one farm—apparently in a Methodist area — where the housewife looks at him and says, "Go away, you damned Presbyterian." Such differences burn into the soul and shape behavior. Mere money maximization is a long way from what is going on. I didn't feel I fully understood Pomeroy after that, but I felt I had a bit more comprehension about him. I've never had the altruism to figure out what motivated Joe McCarthy, or Richard Nixon, but they had reasons, obviously.

As historians, we have to look around at anybody who has a piece of information and a loud voice—or, like the new social historians, every one of them has done homework. Since they are young enough, much of it is honest work. They tracked sources, they made discoveries. Indeed, one of them showed a flyer from the Lowell Mills; just printed the flyer. All he was interested in was that hours were long and life was miserable. But there was a line in the flyer about vaccinations. I began to think about why the Lowell Mills took an interest in vaccinations. After awhile it fit into a piece I am writing. There it was. I wouldn't have had it without this social historian. Bright young people have bees in their bonnets. If you don't have a bee in your bonnet, you're not interested in anything. They give the field vitality; they give us an endless vision of understanding.

I guess I distracted you.

No, that was a couple of questions. There are only two other things on the model of reality. After summarizing all those recovery plans, and finding it was stimulating but very frustrating, I moved on to a more systematic look. Having recently labored through Tinbergen, I felt Tinbergen as well as Keynes was part of the new revelation. Tinbergen even more so. Obviously copying him, with some changes, I developed a small scale econometric model to forecast national income. It appeared in the *AER*. I was very pleased; it was a big thing for me. I looked back a few years ago, and it seemed to me a sort of sensible model.

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Conference Report *(Continued from page 1)*

Roger Ransom (California-Riverside) brought up the issue of timing and pointed out that a missing element in Field's story was the increase in urban demand for fresh meat. Charles Calomiris (Pennsylvania) and Rick Steckel (Ohio State) stressed that Field's argument hinged on uncertainty of final demand for meat. Tom Weiss (Kansas) and Susan Carter (California-Riverside) suggested that Field add evidence from other industries, while Martha Olney (Massachusetts-Amherst) suggested that other communication innovations such as the telephone were plausible extensions of Field's thesis. Bill Lazonick (Barnard) said that Field might re-focus his study on the importance of the telegraph as a precondition for the telephone. This prompted the first of several discussions throughout the conference on the interpretation of the "Chandlerian firm." Eugene White (Rutgers) and John Wallis (Maryland) concluded that, while Field had identified potentially important industries that could have benefited from the telegraph, the paper stressed neither the telegraph itself nor the Western Union Company.

Those attending a Clio conference for the first time were, from left to right: David Levenstam, Wade Shilts, Tawni Hunt, Albrecht Ritschl, Ben Polak, Solomos Solomou, Michael Huberman, Chetan Sanghvi, Bill Boal, Jocelyn Hammaker, Alan Taylor, Leonid Borodkin, Tom Maloney, and Sophia Twarog.

Lou Ferleger (Massachusetts-Boston) and Bill Lazonick provided an interpretation of the history of the U.S. Department of Agriculture (USDA) as a Chandlerian "managerial revolution." They attributed the high social rates of return on USDA research in part to the ability of the Department to coordinate and screen projects, and they described the centrally-managed USDA as model for the centrally-managed cabinet-level departments of the post-World War II era. The lively discussion focused on the absence of a political economy story and on the potential usefulness of a comparative approach.

White wanted Ferleger and Lazonick to develop an alternative hypothesis, such as a public choice story, and wanted to know who originally supported the critical Hatch and Adams Acts. Barry Eichengreen (California-Berkeley) seconded the motion to put the politics back into the paper, and Lynne Kiesling (Northwestern) wanted the authors to consider rent seeking as a means to legitimate the existence of the USDA. Price Fishback (Arizona) stressed that the authors should recognize the differences between political and corporate processes: politics deals with voters and corporations with stockholders. Shawn Kantor (Arizona) stressed that USDA activities are a public good funded by government, and urged the authors to examine agricultural committees of Congress to see how incentives were changing. Wallis added that the USDA — and other departments — underwent dramatic change during the 1920s and 1930s, and wanted the authors to explain why research was undertaken by the government originally and why the government continued to finance the research. Roger Ransom asked Ferleger and Lazonick to say more about the USDA-farmer relationship because farmer-county agent cooperation was critical to USDA research success. Peter Lindert (California-Davis) asked why, if rates of return were so high, did the USDA not do more research? He urged the authors to look at returns to research in the context of other farm policies that were less desirable, such as price supports. Joel Mokyr (Northwestern) noted there was an implicit assumption that coordination is desirable, but duplication or overlap can give unintended benefits. John Nye (Washington Univ.) was skeptical of Ferleger and Lazonick's acceptance of previous rate-of-return estimates for USDA activities. He suggested that, before the authors' argument could be convincing, they would have to make the case that government did a better job providing agricultural research than the private sector might have. Ritschl noted that in Germany, mixed public-private farms were created for research purposes. Gregson wanted Ferleger and Lazonick to put the farmer back into the story. She noted that the demand for research was evident in the pre-Hatch Act agricultural journals. Calomiris commented the authors' argument left room for skepticism because of governmental incompetence in other spheres, and suggested a comparative perspective to demonstrate that the USDA was a success.

John Wallis completed the trio of Friday afternoon papers by offering estimates of the (KABOOM!) implosive concentration of government revenues and expenditures at the state and local levels. He sought and rejected an explanation based upon costs of taxation and economies-of-scale in public goods provision. Wallis tentatively concluded that much of the increased centralization of government at the lower levels seems to have been the result of federal grant policies.

Carol Leonard (Harvard) wanted to know how important transfer costs were to Wallis' argument. Kantor suggested that Wallis try a Nash formulation to model state and local government cooperation. Olney and Ferleger wanted to know about regional trends, specifically, how Southern state and local governments fit into the national trend towards centralization. Mokyr warned Wallis that he must make the federal government integral in his future analysis of concentration. Alan Taylor (Harvard) asked why some demographic and economic variables in Wallis' analysis affected the state share in expenditures in one direction and the local share in the other. Bill Boal (Ohio State) noted that the opposite sign results were a feature of normalization, and that Wallis might want to include a state-specific time trend. Ransom wanted more information on the innovation of the income tax. Did this tax create a ratchet effect: the government spends whatever it receives? Tim Hatton (Essex) turned the discussion to revenues, and asked Wallis to explain not just the composition of revenues but what happened to total tax collections and why. Many participants urged Wallis to expand on the revenue side of his story.

Kevin O'Rourke (Columbia) and Jeff Williamson tried to shore up the historiography of the Hecksher-Ohlin model by examining factor price equalization in the period 1870-1913, just prior to the model's formulation. They conclude that the convergence of commodity prices explains about half of the convergence of factor prices during this period.

The discussion covered a variety of points, with many participants wanting O'Rourke and Williamson to link the results at hand with other debates. Taylor suggested the paper be placed in the context of the wider convergence debate and wanted to know if the authors could determine more about the time path of convergence. Eichengreen asked how the evidence for factor price equalization related to the standard of living and labor scarcity debates, and what assumptions the authors had made about technological change over time. Lindert noted that the U.S. was not truly a national market during the period, and wondered if regional dispersion affected the robustness of the results for international convergence. David Levenstam (Iowa) wanted to know why sugar was an anomalous case. Sugar prices diverged, and he was curious about how policy might have contributed to the divergence. Calomiris suggested that O'Rourke and Williamson analyze price changes using a model of random walks with reflecting barriers. Ritschl suggested that the expected time trend of price and wage gaps is not linear at all, and any projected trend should be of higher order. Sophia Twarog (Ohio State) wanted to know how international migration was handled in the model.

Hammaker asked if wage shocks reflected any employer market power. Lazonick stressed that there was no discussion of price shocks or monetary regime changes such as specie resumption in the U.S. George Grantham (McGill and L'INRA) suggested that the periodization be changed to 1850-1900 because of the very large increase in meat and other prices just before World War I.

Grantham then presented his new estimates of the stock of French agricultural capital in structures, 1789-1940. He reconstructed the stock of buildings by back projecting from the age distribution of buildings recorded in a 1965 census. His technique treated the depreciation rate of structures of different materials as a "mortality rate," allowing him to estimate the "population" of buildings given his estimated stock (which was equal to the number of farms) at benchmark dates. The participants wanted Grantham to expand in particular on two issues: comparison with alternative capital stock estimates and the implications of the estimates for French total factor productivity growth.

Participants in attentive mode at Marcum Center conference room.

Fishback wanted to know if the drawbacks of Grantham's method were less serious than the drawbacks of alternative methods such as tax records, and White concurred, requesting a more detailed comparison of the new estimates with the Levy-Leboyer series. Grantham replied that serious questions had been raised about tax record-based studies of agricultural capital. Levenstam asked Grantham to explain his application of the depreciation rates. Stefano Fenoaltea (Princeton) rejoined on the issue of depreciation accounting, asking how Grantham dealt with periods of intensive restoration of buildings. Mokyr and Wallis questioned the 1965 census responses: is the census reporting considered accurate? Were the buildings' uses in 1965 representative of historical uses? Wallis emphasized that Grantham's two major assumptions — that buildings were used for agriculture and dwellings in constant proportions over time, and that there were only as many farmhouses as farmers at the benchmarks — were not innocuous ones. Leonard agreed and asked if the counts of farmers at the benchmarks were accurate. Rotella and Ritschl wanted Grantham to expand on the discussion concerning the use of demographer's tools. Rotella suggested that Grantham look at the debate on the merits of back projection in the demography literature. Ritschl suggested that the agricultural crisis of the late 1800s could be treated as an epidemic that caused farm structure abandonment. The discussion ended with questions about the implications of Grantham's estimates. Cain asked how the new estimates of capital stock affected the author's own previous study of technological change in French agriculture. Lindert wanted to know if Grantham took the new productivity growth rates to mean that French agriculture was more competitive than previously believed. Grantham said he certainly hoped so.

Louis Johnston (Bowdoin) fearlessly defended himself against the hungry crowd in the dreaded before-lunch Saturday session. Johnston's paper questioned the ability of neoclassical growth models to explain American economic growth during the so-called "Grand Traverse" of 1840-1890. Johnston showed that simulations generated from his model, if he assumed that there were substantial "transitional dynamics," did not closely resemble the actual growth path of the economy. He argued that neoclassical models should be considered suspect and that "new growth" models should be seriously considered. The participants expressed diverse opinions regarding the merits of the exercise, with some questioning the ability of the experiment to add to our understanding of long term growth, and others defending Johnston's effort to bring history to the attention of macroeconomists.

The first comment was made by the no-longer-36-year-old J. Williamson, who suggested that Johnston be clear that his neoclassical growth model was not quite the model employed by a (then-36-year-old) author cited in the paper. Williamson also pointed out that the traditional emphasis of the 19th century story of U.S. growth was on the role of wealth accumulation and distribution, a story that was downplayed in the paper. Mokyr asked Johnston to isolate the assumption that made his model "neoclassical." Johnston replied that the key assumption was a constant elasticity of substitution. Eichengreen said it was probably this assumption which had caused the simulations to go awry. Gregson questioned Johnston's arbitrary apportionment of half of growth to transitional dynamics in the simulations, and asked whether transitional dynamics should be considered unimportant if they accounted for less growth. O'Rourke asked for a discussion of the shocks that might have put the economy off the steady-state growth path in the first place. Olney and Kantor then suggested that Johnston report the parameter values that make his simulation work, but Mokyr interjected a warning against such "tweaking." There were several comments on model specification. Taylor asked if a more complex model, one that took into account important factors such as the closing of the frontier, might perform better while still falling under the rubric "neoclassical." Carter suggested that Johnston change the model to take into account lags in the economy. Tawni Hunt (Washington Univ.) asked how behavior towards risk might be incorporated. White suggested removing the closed-economy assumption from the model, since in fact the 19th century was one of large capital flows. Calomiris, Kiesling and Nye agreed that the paper was an important exercise because it would bring history to the attention of growth theorists. Nye followed up by saying that an important next step would be to look closely at the data. Then followed considerable discussion by Field, Richard Sylla (NYU), Fenoaltea, and J. Williamson concerning the data on the investment/output ratio. The investment/output series Johnston compared to his simulations, that drawn from recent revisions by Gallman, showed no trend over the century. Discussion turned to changes in the composition of capital across industries, with the participants pointing out that Gallman's revised investment appeared flat because he had included a larger amo[illegible] of investment in structures and land clearing in the pre-Civil War years, but there was a shift in composition of capital towards transport and industry after the Civil War.

Leonid Borodkin (Moscow) and Carol Leonard opened the Saturday afternoon sessions with their initial findings concerning urban-rural wage gaps in Russia during the period of early industrialization. They tested for integration of labor markets using series of average daily wages for specific industrial occupations (construction, factory, railroad) and agricultural day laborers. While the statistical results were mixed, Borodkin and Leonard concluded that there was considerable labor market integration during the 1885-1913 period of state-directed growth. There were two primary veins of discussion: the specific data and the general method. Grantham, Carter, and Ritschl commented on the potential problems of using daily wages that were averaged over the year and across so large a country. They wondered if it would be possible to get regional breakdowns or more frequent observations. Borodkin and Leonard replied that they intended to verify the archival sources of the data and hoped to find more

(continued on page 14)

Miles and Miles of Nothing but Miles and Miles

It was with mixed emotions that your faithful servant and the Mullah Nasra Din boarded the silver bird that would deposit them at the annual Spring rites of the clioms. As always, he looked forward to the wisdom that would gush forth during the deliberations and to the camaraderie that prevailed. His reservations sprung from two sources. The fabled Jayhawks had not done as well as anticipated in the annual roundball tournament, and, worse, the famous White Knight of the Hoosiers had done reasonably well. Some impudent clioms might just bring the topic up for discussion. Fortunately, the Wildcats from the southwest had fared as well as ever. More to the point, we would be boarding an even bigger silver bird shortly after the cliometric ritual which would take us back to the Middle East. The possibility that Mullah Nasra Din would remain there indefinitely, perhaps never to return to another cliometric rite, could not be ruled out.

He vowed to throw himself into the proceedings even more fully, listening more intently than ever for those pearls of wisdom that he has come to appreciate. He was, of course, concerned that the scholar from the southwest whose bits of wisdom convinced him that these meetings were fruitful exercises would be absent. She would be ably represented by two of her colleagues, at least one of whom has uttered some inane remarks on his own, but still the prospects of any wisdom forthcoming from the desert Sands were slim. Also missing from the action this year would be the great Hawkeye rhetorician and the garrulous gourmet from the bay. Could he who has studied the potato(e) at great depth make up for these absences?

Offsetting these deficiencies in the makeup of the participants was the fact that this Spring ritual has been going on for 32 years, enough time for even the slowest learner to catch on. Little in academia or the entertainment world has prospered for so long. The clioms have now outlasted Johnny Carson and are almost certain to hang on longer than Regis Philbin. There is, after all, some justice in this world.

As has been true each year, Mullah Nasra Din's misgivings were ill-founded. In fact, this year the sayings were spewed forth with alarming frequency in the opening sessions. The problem appeared to be one of excess, making for a difficult and time-consuming decision. Fortunately, the law of diminishing returns prevailed, and this fixed body of intelligence produced less and less wisdom as the ritual progressed. Still, there was a large number to choose from, in part because several of the experienced clioms had become adept at picking these phrases out of the humdrum discussion. The Oxfordian lion of

Peter Lindert after receiving The Can from Jeff Williamson, who advises: "Forget the Nobel Prize; it's The Can that counts!"

proverb-listening was hard at work, and his efforts were amply supplemented by the neo-full professor who teaches by the side of the river. And, having resigned himself to the fact that the inaugural winner ("Never open a can of worms larger than the universe") was not likely to be emulated, he found more of the sayings worthy of consideration.

As always, Mullah Nasra Din had to remind himself that the best proverb must be pithy, and must contain wisdom for all times and places. He is fond of saying that universality is a useful generality. The lack of it explains why many excellent sayings are not among those chosen for perpetration. This year, for example, the fabled truth that "The younger you are, the more careful you work," was uttered in self-defense. While this had the potential for generality, there seemed a widespread belief that it was not true of any cliom in attendance. The winning proverb should also benefit society. Many a pithy and catchy phrase simply has little to offer. "Tweaking is a really important issue," may seem worthwhile in comparison to whether Murphy Brown's values lie at the heart of urban unrest, but Murphy will hold our attention much longer.

Finally, for this group, the winning aphorism should have a special quality. It might be related to the market, as in the words of he who has studied the potato(e), "It works because there are a lot people who don't know what they are doing;" or to methodology, as exemplified by the remark of the beleaguered historian of agricultural institutions, "Counter examples are preferred to throwing-up." In his view, they must be carefully chosen as he expressed great reluctance "to create a counter-factual that didn't exist," apparently preferring those that did. At a minimum, the winning phrase should have a quantitative dimension.

Almost anyone can think up a wise saying, even more can think up seemingly-wise sayings, but this group has a comparative advantage in devising seemingly-wise proverbs that might be measured or subjected to empirical tests. The thought of subjecting truth to empirical verification has long intrigued Mullah Nasra Din, at least since the time that it was alleged that Ramses III had 92 children. Given the length of Ramses's sarcophagus, it would appear that the Egyptian economy could not have sustained such a high birth rate, but yet the legend persists.

It appears that some of the clioms have been perusing the Mullah's own collected book of sayings, a fine new edition of which Old King Coal donated to the clioms' library. How else explain how one of them could think up the idea that "Misquotes are often better than correct quotes." And what else can explain the extent to which these august scholars postulated self-deprecatory remarks: "I don't know if I have anything on my mind." "When I did it, I didn't know what I was doing." "This is a useless, meaningless and ultimately fruitless exercise." And, "The estimates are secure within the limits of the exercise." Of course, some needed a little help from their friends with comments like, "As a friendly suggestion, this is the most boring paper I have read in a long time." Or "That is completely the wrong way to look at it."

After sorting through the misquotes, the near misses, and the lengthy diatribes, and with little discussion among the senior members of the tribe, the field was narrowed. Among the semi-finalists was a quote from the scholar who left with the oil can that fits beneath the seats of the silver birds. His opinion that "You run a marathon, then you are dust," without doubt contains wisdom for all time. Indeed, it is wisdom that has spanned the ages, for it appears he has yuppified the ancient notion "from dust, to dust." The newest member of the Illini tribe believes one should "always try to get the creditors with their pants down." Perhaps he with silk monogrammed shirts feels the need to do this, and it may be a good idea, but its universality is weakened by its slight sexist tone, suggesting that all creditors wear pants. Equally restrictive was the line by she who is enamored of British accents, "The first-born male child is almost as valuable as an eye tooth." Again, perhaps true, but a bit too feminist for some of the first-born elders. One bit of methodological wisdom made the semi-final list. According to he who is not sure whether he lives in France or Canada, "It is not impossible to guess the right answer." There is much truth here, perhaps even universal truth, but, at the same time, it does not advance our understanding much. How do we know right from wrong? What is the probability of guessing correctly, and is it significant? It thus raises fundamental issues of the cliom religion without providing answers.

While these were worthy contenders, similar to the Bearcat tribe making the final four, they had little chance to win the title. The top three had too much wisdom, too much pithiness, or the element of quantification. The distinguished visitor from Germany was in the hunt throughout, but rose above the pack with "Nobody lives to the average life expectancy." Such boldness, such cliometricality; if only it were true. Old King Coal held up his own reputation as a purveyor of pith with "Don't clean out the stable with a giant river." This has the sort of clarity and succinctness that Mullah Nasra Din admires, a truthfulness that need not be empirically verified. Moreover, it had the ring of "Never open a can of worms larger than the universe," or that wonderful non-winner, "There is more than chewing gum between the U.S. and Canada." Its utterance raised the specter that Monty Python had opened a southwest branch of his school of silly talk.

Perhaps in some other year this might have won the grand prize, but not this one. He who thinks the telegraph has been underrated chipped in with this year's winner: "Distances in the U.S. are larger [sic] than in Europe." How long has this been true? What significance, if any, does it have for path dependence? Are distances being measured in a straight-line? Are distances yet larger in the western part of the United States? Are they even larger west of the United States? If so, how far west? Are the distances affected by jet lag? Is the same true of height and length? While a robin is not a robin, is a rose not a rose not a rose? Has Gertrude Stein rolled over in her grave? The saying raises innumerable imponderables, all waiting to be pondered. It sets off other trivial questions, while begging others.

The evening concluded with the leading general equilibrium gap analyst and his Tower of Babel continuing the tradition of announcing the winner of the can that fits beneath the seat of the silver bird in musical fashion. What this piece of performance art lacked in clarity and musicality, it made up for with enthusiasm and originality. As we prepared to board the silver bird for Egypt, the Mullah mused on the pressure that now lies on the new king's head to continue the tradition and to surpass the extravaganza witnessed that evening.

Submitted humbly by the faithful and obsequious servant of the Mullah Nasra Din

Ode to the Cliometrics Can*

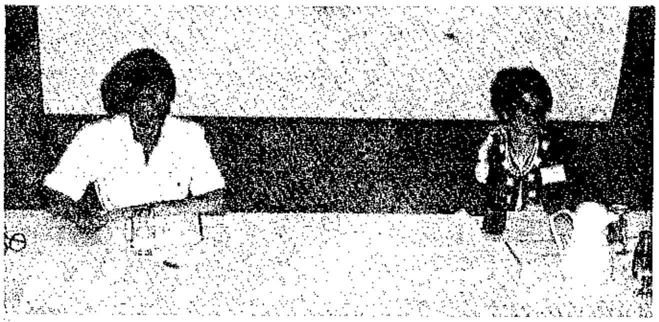
by Jeffrey G. Williamson

This is the case that holds the can
The can is the best award that am
It goes to a Cliometrician that gives so much
Even though you may feel you're losing your touch
So, here's to the Can Cli-o-met-rix
May it give the recipient great big kicks.

*To be recited in English, with antiphonal line-by-line and simultaneous translation into Russian, French, Danish, Dutch, Hebrew, Italian, British, German, and Tagalog

Conference Report *(Continued from page 10)*

detail. Atack asked if there was evidence on non-wage compensation in agriculture that might make the gap disappear altogether. Fenoaltea wondered if the return to farm operators might be a more appropriate comparison to urban wages than daily farm wages. Nye and Mokyr wanted the authors to discuss the specific institutional context of the transition from serfdom to labor market participation. White wondered what the finding of integrated labor markets implied about the effectiveness of the internal



Marty Olney presiding; Lynne Kiesling presenting.

Russian passport system. Finally, there was a lengthy discussion led by Wallis, Calomiris, Hatton and Fishback concerning how wage gaps ought to be measured. Hatton defended the usual lagged specification of the co-integrating regressions, while Calomiris suggested that the usual integration tests could be improved by taking into account potential migrants' reactions to the expected variance of the wage gap over time, as well as the gap at any point in time.

Lynne Kiesling presented her evidence that mid-Victorian cotton textile workers in Lancashire used family networks to avoid having to participate in public poor relief. She found that there was more than a year's lag between onset of the Cotton Famine and the mass movement of people onto public relief. Kiesling argued that this lag represented the widespread functioning of family networks for self-insurance.

In the main, the participants wanted additional evidence on other cyclical downturns and an explanation for the wide variation in poor law participation across Kiesling's sample towns. Thomas Maloney (Michigan), Fishback and Twarog especially wanted to see what happened to the number of cotton workers on poor relief during the severe recession of the 1870s. Twarog also asked about timing of the representative family's progression from self-insurance to private charity to public relief. Kantor rejoined, and asked if the opportunity cost of using the poor law declined as the Famine worsened. Ben Polak (Harvard) wondered if there was any evidence that poor laws became more generous as the crisis deepened. Kiesling replied that the maximum benefit was kept quite low. Michael Huberman (California-Davis), John Lyons (Miami), and Wallis asked about the cotton workers' responsiveness to other types of aid. Lyons suggested that Kiesling discuss further the extent of private charity. Wallis wanted Kiesling to estimate a more complex model, rather than her reduced-form equations, to measure people's responsiveness to other sources of aid more accurately. Huberman also noted that the incidence of short time in the mills was an important type of insurance negotiated between employers and workers. Atack asked about the intermingling and identification of normative and positive concepts in the poor law: "deserving" and "non-deserving" and "able-bodied" and "non-able-bodied," and Atack, Lazonick, and Wade Shilts (Iowa) had questions about the differences in poor relief participation among towns. Kiesling explained that the poor law participation rates varied from 5 to 25 percent across her six locations because the towns specialized in spinning different types of cotton. Lazonick noted that not only did the towns use different technologies suited to different cotton fiber lengths but also that they were different in terms of distribution of firm size, union organization and average wages. Shilts asked if Kiesling had impressionistic evidence of perceptions of the stigma of Poor Law relief in the different towns. Finally, while Hunt questioned the ability of the Lancashire families to insure themselves against systematic risk of the textile industry if they all participated in that industry, Hatton and Chetan Sanghvi (Rutgers) supported Kiesling's intuition. Sanghvi believed that the empirical results seemed weak because of second-order auto-correlation. Hatton suggested that Kiesling emphasize the large numbers of textile workers who never received public relief.

Wade Shilts, Saturday's last presenter, provided evidence on the value of limited liability to investors in British companies in the years after 1856, following the adoption of administrative registration (as opposed to Parliamentary grants). Inferring investors' gains from the rates-of-return to 222 companies that switched status from unlimited to limited liability, Shilts concluded that gains to investors were small, at least in the period immediately after limited liability became widely available. The participants unanimously agreed that the question was an important one. The discussion focused mainly upon alternative ways to measure the benefits of limited liability.

Several participants made the point that the greatest benefits of limited liability are expected to accrue to investors in new firms that would not have been formed but for the legal innovation (the pooling hypothesis). Avner Greif (Stanford) stressed that limited liability induces investment in risky or unfamiliar ventures. Field underscored the value of limited liability to create an atmosphere of free entry. Cain and Nye suggested that Shilts compare the British experience with the U.S., French and Scottish experiences. Mokyr said that the paper was important as a window onto the political economy of *laissez faire;* White agreed and suggested that Shilts consider the Parliamentary interests that resulted in the passage of the law. Kantor said that Shilts might consider modelling the firm-level choice to switch to limited liability, while Fishback suggested that case histories of particular firms could be illuminating. Eichengreen expressed concern about the additional inferences that had been made from just a few firms' rates of return, but encouraged Shilts to pursue the topic further. Calomiris directed Shilts to the finance literature for discussion of the various expected effects of limited liability to firms and for examples of event studies of major regime shifts. David Augustin (Loyola, Chicago) commented that, while the social return would clearly be higher than the private, with a small sample of relatively risky stocks, the distribution of *ex post* observations was not necessarily the same as that of *ex ante* expectations. Rotella pointed out that from the investors' point of view, unlimited and limited liability shares were likely to be portfolio complements rather than substitutes. Lazonick suggested that Shilts consider measuring the benefits of limited liability later in time, perhaps by estimating the reduced cost of capital to the largest industry that raised its capital in the form of limited shares, cotton textiles.

Veterans of the early days at Purdue, re-convened at Miami in 1992: left to right (rear) Tom Weiss, Jerry Flueckiger, Lou Cain, Dick Sylla and George Grantham; (front) Roger Ransom, Jeff Williamson and Sam Williamson.

The paper by Moshe Buchinsky (Chicago) and Ben Polak, presented by Polak, explored the integration of financial and real capital markets in the eighteenth and nineteenth centuries by comparing London interest rates with data on deed registrations in Middlesex and in Yorkshire, West Riding. Questions addressed the appropriateness of comparing rates on paper and real assets, the structure of British capital markets, and the problems introduced by institutional and political turbulence during the period.

David Wheelock (Texas) expressed serious doubt about the validity of using property transactions as a measure of real economic activity. White was concerned that Polak did not seem to recognize the enormous institutional change that occurred in British capital markets during the period under consideration and was concerned about the continuity of Polak's data series. White also noted that war was an important feature of the British economy, and war affected yields systematically. Kiesling wanted to know about the importance of informal loans in stimulating economic activity, and Field inquired about consumption loans in this period. Mokyr wanted Polak to measure the contribution of financial integration to the Industrial Revolution. Solomos Solomou (Stanford) asked about the importance of demographic factors in the building cycle. Borodkin suggested that Polak incorporate dummy variables into his model to represent crises. Eichengreen wanted more information about the structure of the land and deed markets. In particular, he noted that the deed series the authors used included turnover plus building, and that turnover might have been more interest-rate sensitive. Boal suggested Polak interact interest rates with a time trend in his estimations.

As part of their ongoing project on the political and economic history of workers' compensation in the United States, Price Fishback and Shawn Kantor examined evidence from the coal industry on the relationship between post-accident compensation and risk premiums in wage rates. Questions and comments once again broached a variety of topics, from speculation on employers' reactions to the law, to differences among countries' compensation legislation.

S. Williamson wanted Kantor and Fishback to incorporate the ways in which employers reacted to workers' compensation laws. Olney noted that in a world without such insurance, people would save more, and that the authors' results overstate the benefit of workers' compensation by assuming that workers had no alternatives. Rotella wondered if compensation increased accident rates and suggested weighting awards by the probability they were received. Sylla suggested risk aversion as a motivating force for the transition to a system of workers' compensation. Carter asked about the difference between new workers and the average worker. Calomiris asked the authors if they knew why private workers' compensation did not emerge in private markets in an important way. Field replied that workers' compensation laws might have been more efficient than a private system because it cut out the lawyers. Greif wanted to know how much of wages were compensating differentials. Wallis noted that in modelling the effect of workers' compensation laws, each law should be allowed to have a different effect. Hammaker suggested that the authors closely examine differences in conditions across industries to identify danger levels. Twarog suggested that a cross-country comparative study would be illuminating.

The final paper, by Thomas Maloney and Warren Whatley (Michigan), was presented by Maloney. They used personnel data from Ford Motors and census data for Detroit to investigate the labor market for blacks between the 1920s and 1940s. Much of the discussion was about specific policies at Ford and about the foundry, where Ford employed most of its black workers. Steckel wanted Maloney and Whatley to motivate the paper using the work of historians and sociologists and to compare their results with findings for blacks in late twentieth century. Johnston wanted evidence on the origins of Ford's policy of hiring blacks; did Ford see it as profitable, and for what reasons? Lazonick asked the authors to consider the evolution of the labor market, noting that after the $5/day system, Ford increased the pace. Carter, stressing the hard work associated with the foundry, asked why some people worked so much harder than others in some departments. Was it the nature of the work in the foundry, or that blacks would be driven hard in any department as a way of lowering their real wage? Wallis wanted the authors to give a breakdown of Detroit workers by Ford and non-Ford employees. Fishback suggested that wages for blacks at Ford were high because Ford could select the best workers; therefore, high wages reflected better-than-average workers and not discrimination. White suggested that Ford used marriage and family life as a screen to find the better workers, because information on education was unreliable, and Fenoaltea wondered about the differences in school quality by race. Fishback observed, however, that education beyond the sixth grade was not very important for productivity on the assembly line. Grantham wanted to know if blacks worked in foundries elsewhere, and Fenoaltea interjected that small, independent foundries would not hire blacks because they could not be assured of a large enough supply of black labor, whereas Ford was big enough to do this.

In its 32nd time at bat the Cliometrics Conference once again hit a home run as it featured the most stimulating papers and discussion in the profession. Many thanks to the participants, the organizers, and the National Science Foundation for keeping Clio batting 1.000.

Lebergott Interview *(Continued from page 6)*

It'll do it every time. Macroeconomists are all economic historians, but they do it on the cheap. If it's the last ten years, that's their history. The real difference between them and economic historians is we say there is a world longer than the last three recessions. In principle, I don't think they disagree, but they are still taking the last five or ten years.

The historians for a long while were far too respectful of cliometric history. I remember an article by Vann Woodward, that great gentleman. Apropos one of the real noisy presentations he said, in sort of a quiet way, this seems to be the wave of the future. Most good historians are very ambivalent about having an explicit model of anything; largely a fear of God, because they know in their guts there are too many variables – too many that are hard to observe. Wallerstein and whatnot can explain everything by throwing words around. But most historians are being cautious, leaning over backwards. Sometimes their humility turns to aggression, but, you know, they may not hate economists any more than others do.

You have a question here about "new directions." How has the attitude evolved in the past thirty years? I don't think the sparring has changed much. You can look at something like Bill Parker's conference volume with Arrow and Solow. Had you asked Arrow or Solow thirty years ago, they would have said the same thing, I think. These guys have such broad perspectives that you wouldn't have caught them being negative about the worst idiocies of what we did at Williamstown or at some of the cliometric sessions. They would not have said "this is immoral," the way poor Redlich did.

I want to push you a little on this. There are people, historians, traditional historians, who — I'm aware of this partly because I came into the field in the midst of questions about *Time on the Cross* — withdrew in some ways from taking economic history seriously because they felt cliometricians had this attitude that "we are going to come and help you."

Well, you have to partial this out. I spent maybe fifteen years in Washington at the Budget Bureau, working on income distribution and national income. In that time there was Simon Kuznets, a glorious, dedicated scholar. Now, Simon Kuznets was not the field of national income. He oriented the rest of us; he energized us; but he wasn't the field. *Time on the Cross* is not the field. That historians treated it as "the field," I can understand.

Remember that session in Rochester? Robert Silber from the *The New York Review of Books* called me up before it. He wanted to get a line on a silly rebuttal he had from a historian. I remember futilely trying to tell him: you don't have to agree with their work. I can tell you eight reasons why it is inadequate: but you have to understand what it does achieve before you can start cutting away. But I couldn't get through to him. If historians have pulled away because of the noise, so much the worse for them. Do you think the younger guys have done that?

My own perspective is that history departments seem to have pulled away from economic history in particular, but also more broadly from what I would call social science. I have been struck — as during your comments on the new social history — by an absence of a clear conceptual framework. It is an attempt to do social science history without the social science. If you don't make clear what your framework is and what your assumptions are, you're not going to be able to impose much real sense. I think your example is perfect; I think it says a tremendous amount about an enterprise when they have a line in there that says something about vaccinations. Now, you might want to argue they just want to take advantage of you by keeping you healthy ...

That's right. For an economist it is sort of "so what?" It depends on the terms of trade and what is involved. I once read a book where the author recreated Angers, a tiny French city in the eighteenth century, lovingly dealing with it, a chapter on the church, on the markets. It was like one of those Japanese paper flowers that opens out in water. You don't understand the town, but you are overwhelmed, living for a while in that rich reality. That is an historian's great accomplishment. Economists can do other, good things; historians can benefit from all that but they shy away from an analytic structure. Too bad.

There's been a resurgence of interest in labor-historical issues, with Margo, Sutch, Goldin. A lot of this flows from exciting theoretical developments in labor economics, but it brings us back to what we think was the large role played by spot markets for labor in the 1850's or 1870's. Now we have a much different kind of labor market. What do you think of what is going on?

These are creative economists who are moving things forward. As for spot markets, I guess William Julius Wilson is one of the commanding figures in the field. But he doesn't seem to know much about spot markets. You can't allocate labor unless they all have automobiles — that seems to be one of his central assertions. But that's not so in economic history — in the 1940's in Washington at the head of Georgia Avenue, a spot market functioned every morning. Domestic servants appeared. They took the trolley car; friends gave them a lift; taxi drivers, because many were black, dropped them off there. The ladies from the suburbs would drive up and take them to clean in their homes. Or the construction boys would say I need three men; three men would climb on. The market was allocating. And they *were* spot markets. If you need more examples, go back to the rolling harvests across the Midwest, in the 1870s, 80s, 90s; who the hell had cars? The guys didn't even have horses. The people on the demand side picked them up, moved them to the locus of production and moved them back. This technical imperfection sounds like a kinked-demand curve, everything works except at one critical point. Well, is there a kink? This is where economic history comes in. You can say we've had millions of people who had no facilities for transportation who have been moved decade after decade in all sorts of conditions that were far more difficult than those for people in Chicago today. Either you really have to explain why it is not being done, or you have to give up that nifty little kink.

History is a discipline on economics.

History should be a discipline on every new policy choice. All countries experiment. It must have been twenty or thirty years ago, I heard Allais describe time-of-day pricing in the French electric system. It was marvelous. Of all places, in France. Maybe the ghost of Cournot was involved. For policy in Washington, or in Hartford, you want to understand what other people have done. And you have two choices: you've got history and time, and you've got space. Then you can discover how spot markets work.

I guess I would say, given the recent labor economics work, we're going through a cyclical process in some sense. The last ten years I've thought

economic history was more exciting, more interesting, because we'd gotten back to doing what in many ways the early cliometricans had done, which is, let's get back to the archives, let's do the primary research. We had gone through a period when we were trying to use the technical power of economics to beat up on old data sets ...

Not doing our homework.

We were trying to do our history on the cheap.

That's right. So data had been around already. "Now we'll take an interest in economic history, and we know economics. Therefore, all we have to do is pick a few observations, and we can solve the problems." You're right, there was and there is still some of that, of course. But Margo and the others, these are people who are going back and thinking. Last week, I finally picked up Thorold Rogers, *History of Agriculture and Prices*. This guy spent a life turning up data for the rest of us. Among other things, this is why we should always be respectful of the historians. Especially younger people, who learn by grubbing and learn by getting their hands dirty. How much is temperament and so on? These young people are doing it. They are using their drive to get the relevant data, in some cases to go to the archives. They then try to fit it into some kind of analytic model. It works. It's a marvelous combination.

This reminds me of the kind of energy you have given to empirical work. Like the recent work on the 1920s, or going back to the income survey data that had been done, which nobody had used because, I think, they had been intimidated by the size of the samples. I've been gratified, maybe because economists when they get to work seriously aren't intimidated by looking at a 100,000 ...

With computers, it's a different world.

I know Bob Fogel commented in his interview a couple years ago about this incredible productivity enhancement. You go in with a little laptop, and you can enter the data directly and go through these enormous data sets and really construct a far more complex ...

Far richer, far safer to work with. I remember when I was working at the Budget Bureau, I would go down on Saturdays because there was a hand-crank calculating machine. It was great, because it didn't have the rounding problem that the slide-rule did. You know, I can't bear to throw away this beautiful ... [SL pulls out magnificent 24-inch slide-rule he keeps in his desk drawer.]

On a Saturday it took me virtually one whole day to run a regression with five explanatory variables. Worse still, to get five variables, I had to throw out forty that I considered. When we first started on this consumption job, we were entering all the work sheets in pencil, so you could erase. You had to do the arithmetic twice to check what you had. Now I can even check estimates for errors by regression — nearby states, nearby patterns, or a larger group. I'm not trying to analyze; I'm just trying to check. It's easy to pick up gross errors. Then there's the grey area where you say, "Maybe it's an error, but maybe it's the truth." In which case I leave it in. The estimates may be bum, but I haven't fiddled with them.

Then someone can figure out a better way to make the estimate.

Sure. The computer makes that editing possible. Of course, for 1900, my estimates, alas, differ from Dick Easterlin's.

Well, it's a whole new field for someone to sort out: resolving the Easterlin-Lebergott conundrum... On another matter, I was really struck by and enjoyed your article on why the South lost the Civil War, and the rather powerful argument that it's because they weren't paying attention.

Glad someone read it.

How did you get into that? It was a striking article and a little unexpected in terms of its authorship.

As a depository library, Wesleyan has the marvelous serial set, thousands of wonderful volumes. A large hunk of that is called *The War of the Rebellion*. After surgery, I was at home and had several volumes around. Well, as you know, they can be thirteen hundred pages each, with an enormous amount of documentation. So, just turning over the pages, reading all these wonderful despatches one occasionally sees something. I came across long discussions about international law: what's an effective blockade? I began to wonder how much cotton was exported, what the blockade amounted to. Later, I went into the National Archives, got into that wonderful Confederate document collection, and began to piece out average tonnage. Then I came upon, I guess this was in the 1866 Report of the Secretary of the Navy, lists of all the vessels captured. And that wonderful piece by the glorious antiquarians on itemizing every ship that slipped through the blockade. When that was sort of put together, I began wondering what the hell did it mean? When I saw the orders of magnitude, I began to consider the opportunity cost. Why are they *growing* cotton? Eventually I came to Tyler's 1861 address to the Virginia legislature, "King Cotton will have the monopoly of the world."

I was struck by it too because, when asking about current policy implications, there is a lot of discussion now about G & A in the 1980s — greed and avarice — and the degree to which we undercut ourselves in the long-term: investments in education; corporate strategy. The new book on debt, *The Debt Merchants*, asks how much damage the leveraged buyouts really did. It struck me as having some very powerful parallels ...

[SL turns around and reaches for his extraordinary collection — hundreds of copied articles in loose leaf binders.]

Just read the little insert.

"'It was an extraordinary thing to see our squires and poorer people split the bellies of those dead Saracens so that they might pick out the gold coins from their intestines, which they had swallowed down their horrible gullets while still alive.' Abbott Fulcher at the siege of Jerusalem." Where *do* you find these things? That is wonderful.

I would say that somebody in Wall Street M&A is pretty moderate compared to that. And he doesn't pretend to be carrying the Gospel.

That's right. I admire your paying attention to interesting questions which emerge and pursuing them, which I think all of us ought to do ... In closing, do you have any broad, sweeping insights that you want to share with the profession?

I am just optimistic about economic history.

Good.

There are three areas that would repay work. One is war. I mean, war as an overwhelming fact in human existence. Whether it is a small scale (like the headhunters in the Philippines) and it gets confounded by religion or values, or large scale (like World War II, or the Gulf), there are enormous impacts on the economy, on supply, relative prices, production. I don't just mean nifty innovations. There is a great future for economic historians, young historians, looking at that. There are many wars, all over the world, all through time. Many of them have records. There are military historians. They must have miles of records just for the U.S. Charlie Schultze had a marvelous piece on the effect of El Niño on the price of anchovies, hence the price of grain, hence inflation under Carter. Well, if El Niño can do something like that, what about war?

Well, of course, the Soviet case, with the huge casualties, fundamentally changed the structure of their labor market for two generations.

Sure. Number two is a real oddity. Consider a group of earnest, youngish economic historians setting to work. Then imagine an area with an immense set of publications, pretty well indexed, of all kinds, in English. Would you be interested in studying that world? Well, *they* ain't. Why, I don't know. Scots history! [Bangs the desk.] Who developed the British Empire? A few Brits, including incompetent second sons. But the Scots were all over the British Empire. Who do we think of as the father of economics? And David Hume and Hutchinson, the real visions of the enlightenment. Our library has records for Edinburgh going back to the fifteenth century. So here you have a language, you have events, brilliant minds, aggressive entrepreneurs, not to mention the Darien Company or clearing the highlands or the beginnings of modern industry. Robert Owen began in Scotland as a young guy. So I think Scots history is a natural because the stuff is there; it's in English; it's indexed; it's relevant to the development of the industrial revolution; and they have some of the most wonderful ballads.

That's a comment on welfare functions.

Yes, indeed. The third is the change in the public health in the last century in various countries. Preston and Haines have recently done something for the U.S., but I don't think we have any understanding of what brought about changes in mortality rates. For many of the countries, we have cause of death. Which means you don't have to talk vaguely about cleanliness, and rats, and cleaning up the Thames; you have particular causes. Granting problems with diagnosis, changing patterns of diagnostics, and all that, you have an enormous set of coherent, consistent data on major phenomena. When life expectation doubles, you get a major impact, not merely on welfare, but on all the economic processes. What is bringing this about? A great project!

Historical Labor Statistics Project Publishes Newsletter

by Susan Carter, Roger Ransom, and Richard Sutch, University of California

(Berkeley and Riverside, CA) The *Historical Labor Statistics Project*, a research program currently underway at the University of California's Berkeley and Riverside campuses, has inaugurated a newsletter. A major objective of the HLSP is to collect detailed data in machine-readable format on American labor markets, selected from over 150 separate investigations undertaken between 1874 and 1920 by the bureaus of labor statistics in 29 states. To date, 27 separate surveys have been collected, coded, and documented. Each one is a cross-section survey of workers or firms with information on wages, working conditions, living standards, family demography, and household economy.

The HLSP Newsletter is meant to inform interested scholars of our progress in data collection, to describe our distribution policy and to disseminate other information that might be of interest. The inaugural issue includes a report on a small conference held in June 1991, at the University of Kansas in Lawrence, on the use of the state BLS data sets for the study of economic history. Subsequent issues will appear irregularly as progress in data collection and research warrant.

If you would like to receive a copy of the Newsletter please write:

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University of California-Riverside
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FAX: 714-787-5299
e-mail: LHR21 @ UCRVMS

IN MEMORIAM

Cliometrics lost one of its creators, and I lost a friend, when Jon Hughes passed away on May 30th this year at the age of 64.

I first met Jon in his office at Purdue in 1960, when he suggested that I transfer out of engineering and become an economics major. From that day onward, he was one of the most supportive and loyal friends I had. I know many of you feel the same way.

I documented Jon's role in the formation of Cliometrics in my contribution to *The Vital One: Essays in Honor of Jonathan R. T. Hughes*. He, along with Lance Davis and Stan Reiter, were the first to use the term Cliometrics. [See "Aspects of Quantitative Research in Economic History," *Journal of Economic History*, December 1960, p. 540.] Jon also named our Society some 14 years before its formal founding when he said "we held the first meetings of what is now the defunct 'Purdue Cliometrics Society' in 1960." ["Is the New Economic History an Export Product," in D. N. McCloskey, ed., *Essays on a Mature Economy: Britain after 1840*, 1971, p. 403.] All that proved to be defunct was the site of the meetings.

A memorial service for Jon will be held at Northwestern on Friday, September 25th at 3:30 p.m. CDT. If you would like more details, please contact Lou Cain or Joel Mokyr.

The lines of poetry quoted below eloquently express some of my feelings about Jon as scholar and friend.

Sam Williamson

Lines for the life and work of Jonathan Hughes

List close my scholars dear,
Doctrines, politics and civilization exurge from you,
Sculpture and monuments and any thing inscribed anywhere are tallied
in you,
The gist of histories and statistics as far back as the records reach is in
you this hour, and myths and tales the same,
If you had not breathed and walked here, where would they all be?
The most renown'd poems would be ashes,
orations and plays would be vacuums.

From "A Song for Occupations," Book XV of *Leaves of Grass*
(1891-92 edition) by Walt Whitman

ANNOUNCEMENTS

Business History Conference

The 1993 annual meeting of the Business History Conference will be held in Boston, MA on March 19-21, with the theme "Theory and Business History." Proposals for sessions (2 or 3 papers) should be submitted by October 1, 1992, and should include a one-page abstract identifying authors and subjects of papers, commentator(s), moderator, and session theme, along with a brief CV for each participant. Proposals for single papers are also solicited. All proposals should be sent to the program organizer:

Richard H.K. Vietor
Harvard Business School
Soldiers Field
Boston, MA 02163
Telephone 617-495-6460

The Conference will also feature a dissertation session for which dissertations completed in the years 1990, 1991, and 1992 are eligible. Each dissertation selected will be considered for the Herman E. Krooss Prize, for the best dissertation presented at the meeting. Those wishing to present a dissertation summary should send a one-page abstract and a copy of the dissertation to:

David Hounshell
Department of History
Carnegie Mellon University
Pittsburgh, PA 15213-3890
Telephone 412-268-3753

European Historical Economics Society

With the financial support of the European Commission, Stimulation Plan for Economic Science, EHES has formed several networks which are intended to encourage reunification of European economics and economic history by systematic comparison and analysis of elements of European economies over the long term. Workshops have been planned for the coming year by various of the SPES networks:

Workshop on Long-Run Economic Growth in the European Periphery:

The aim of this workshop is to explore hypotheses concerning determinants of growth, retardation, and catching-up among European "latecomers" from a comparative perspective. Possible subjects include technology diffusion, natural resources, factor mobility, commercial policy, human capital accumulation, and, more generally, institutional change.

The workshop plans a conference to be held in Madrid in February 1993, with ten to fifteen papers and up to twenty-five participants. Transport and lodging expenses will be paid for those who are members of EHES. Although the initial submission deadline is now past, the organizer will continue to accept paper proposals and requests to attend through 1st October 1992. Correspondence should be addressed to:

Leandro Prados de la Escosura
Departamento de Economía
Universidad Carlos III de Madrid
28903 Getafe (MADRID)
Spain
FAX 34-1-624-9875
Tel. 34-1-624-9623

Workshop on the History of International Monetary Arrangements

A workshop at a site near Lisbon is planned for 7th - 9th May 1993 under the auspices of the New University of Lisbon, with support from the EC/SPES program. Topics for discussion are: a) formal international monetary unions, b) common international monetary standards (*e.g.*, gold or silver standards), c) international monetary cooperation in the inter-war period, and d) Bretton Woods and beyond. The focus will be on Europe and the North Atlantic in the 19th and 20th centuries, but other periods and regions may be addressed.

Participation will be limited to twenty five persons, and full board and travel expenses will be paid for those who are members of EHES. Those wishing to take part should send a two-page synopsis, no later than 15th October 1992, to:

Jaime Reis
Faculdade de Economia, UNL
Travessa Estevão Pinto
1200 Lisboa
Portugal
FAX: 351-1-387-1105

Eleventh International Economic History Congress, Milan, 1994

Young scholars are invited to present summaries of their doctoral research at the Milan Congress of the International Economic History Association in September 1994. Thesis summaries will be published in a volume of the Congress proceedings, and four diplomas and four prizes of $1,000 will be awarded.

To be eligible for these sessions, candidates must have been awarded their doctorates (or equivalent) after September 1, 1988, and not later than December 31, 1992. Scholars interested in participating in these sessions should specify the title of the thesis, its supervisor and assessors, and the institution awarding the degree. (NB: Do *not* send the thesis.) This information and requests for further details should be sent promptly to:

Professor Joseph Goy
General Secretary
International Economic History Association
Centre de Recherches Historiques
54 Bd Raspail
75270 Paris Cedex 06
France

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